Insightimate: Enhancing Software Effort Estimation Accuracy Using Machine Learning Across Three Schemas (LOC/FP/UCP)

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Abstract

Accurate estimation of software development effort remains a longstanding challenge in project management, particularly as contemporary projects exhibit greater heterogeneity in scale, methodology, and complexity. While traditional parametric models such as COCOMO II offer interpretability, their fixed functional forms often underfit diverse modern datasets. Three critical gaps persist in prior effort estimation research: (i) lack of auditable dataset provenance and deduplication transparency, (ii) unfair baselines using uncalibrated parameters, and (iii) insufficient cross-source generalization testing. Our unified, reproducible **schema-specific benchmarking framework** spans Lines of Code (LOC), Function Points (FP), and Use Case Points (UCP), ensuring: (1) full dataset manifest with provenance tracking and explicit deduplication rules; (2) **calibrated size-only power-law baselines** fitted on training data to avoid straw-man comparisons (not full COCOMO II due to missing cost drivers in public datasets); (3) schema-appropriate validation protocols (stratified splits for LOC/UCP, LOOCV for FP) with leave-one-source-out generalization testing; (4) ablation analysis quantifying preprocessing contributions; and (5) **stratified tail evaluation** to assess robustness on high-effort projects (top 10%). Models are trained **independently per schema** to prevent semantic feature mismatch across sizing paradigms. Using publicly available datasets (1993–2022), we evaluate Linear Regression, Decision Tree, Random Forest, Gradient Boosting, and XGBoost against the calibrated size-only baseline, with optional imbalance-aware weighting for tail robustness. Random Forest achieves MAE 12.66 ± 0.7 PM (42% improvement over baseline MAE 18.45 ± 1.2 PM) with MMRE 0.65 ± 0.04; tail performance (top 10% effort) shows MAE degradation of 18% but remains superior to parametric baselines. All metrics reported as mean $\pm$ std across 10 random seeds. **Aggregation protocol:** Overall results use **macro-averaging** (equal weight per schema: LOC/FP/UCP) to prevent LOC dominance (90.5% of projects); schema-specific results report per-schema test predictions from independent models trained exclusively on that schema (no cross-schema pooling or transfer). These contributions establish a fair, auditable, and imbalance-aware benchmark for ensemble-based effort estimation.

Index Terms Effort estimation, size-only power-law baseline, machine learning, Random Forest, Gradient Boosting, LOC, FP, UCP, dataset provenance, benchmarking methodology, cross-schema validation.

1 Introduction

Accurately estimating software development effort is a critical factor in determining the success of software projects. Reliable estimates support effective planning, budgeting, resource allocation, and risk management. Conversely, inaccurate estimates often result in cost overruns, schedule delays, and even project failure, as widely acknowledged in the empirical software engineering literature [1]. As modern software projects continue to grow in diversity—varying in size, methodology, domain, and team structure—the challenge of producing consistent and trustworthy effort estimates becomes increasingly pronounced.

A wide range of factors affect estimation accuracy, including project size, functional complexity, development methodology, team capability, and organizational context. Traditional parametric models such as COCOMO II provide closed-form transparency and have historically been adopted in industrial settings, yet their fixed functional forms struggle to generalize across heterogeneous contemporary datasets. This motivates the exploration of more flexible, data-driven approaches capable of capturing non-linear patterns and adapting to diverse project characteristics, while acknowledging the trade-off with parametric interpretability.

What is known. Software effort estimation (SEE) has long relied on parametric models (e.g., COCOMO-family) and, more recently, machine-learning regressors. Prior studies consistently show that ensemble methods (Random Forest, Gradient Boosting) can improve predictive accuracy on individual datasets, and multi-schema approaches (LOC/FP/UCP) acknowledge the diversity of sizing conventions across projects.

What is missing. However, three critical issues frequently limit the strength and reproducibility of reported gains: (i) **weak auditability** of pooled datasets—provenance, licensing, deduplication rules, and rebuildability are often unclear, making independent replication difficult; (ii) **unfair or non-reproducible parametric baselines**—when cost drivers (effort multipliers, scale factors) are unavailable, COCOMO II is often applied with arbitrary default parameters, creating straw-man comparisons; and (iii) **ambiguous "overall" reporting**—cross-schema aggregations may be dominated by the largest schema (typically LOC), masking true behavior on FP/UCP, and small-sample protocols (e.g., FP with $n < 200$) are often inadequately addressed.

Research gap. The community lacks a **reproducible cross-schema benchmarking framework** that provides: (a) auditable dataset manifest with explicit deduplication and leakage controls, (b) fair baseline comparisons under missing cost drivers, and (c) transparent aggregation protocols (macro vs. micro) with appropriate small-sample strategies (e.g., LOOCV for limited data). *Our contribution addresses this methodological gap, not model novelty per se.*

Our approach. We present a **methodological infrastructure for fair, auditable cross-schema benchmarking** through four concrete contributions: (1) an **auditable dataset manifest** (Table 1) documenting source, year, DOI/URL, raw counts, deduplication rules, and rebuild scripts for full reproducibility; (2) a **fair calibrated parametric baseline** (Section 2.1)—a size-only power-law model fitted on training data per schema and seed—ensuring principled comparison when cost drivers are missing; (3) **schema-appropriate evaluation protocols**, including Leave-One-Out Cross-Validation (LOOCV) and bootstrap confidence intervals for small-sample FP, plus explicit macro/micro aggregation to avoid LOC dominance; and (4) **ablation analysis** (Section 5.5) quantifying contributions of harmonization, log-scaling, and outlier control.

We evaluate Linear Regression, Decision Tree, Random Forest, Gradient Boosting, and XGBoost against the calibrated baseline using standard metrics (MMRE, MdMRE, PRED(25), MAE, MdAE, RMSE, R^2), analyzing behavior within individual sizing schemas. *The primary contribution is the benchmarking framework itself, which future studies can use to evaluate new models or datasets under consistent, fair conditions.*

Scope and limitations upfront. Our focus is *methodological benchmarking*—establishing fair, auditable cross-schema comparisons—rather than claiming a universally best estimator. Key scope constraints: (i) we use a size-only COCOMO-like baseline because most public FP/UCP datasets lack cost drivers; (ii) models are trained *per schema* (LOC/FP/UCP) without cross-schema transfer; (iii) FP results (n=158) are exploratory despite LOOCV and bootstrap CIs; (iv) unit conversion assumes 1 PM=160 hours, which may vary by organization; and (v) public datasets (1993–2022) may underrepresent modern DevOps pipelines. These constraints clarify what our results can—and cannot—generalize to.

Research Gaps Addressed. Despite extensive research in software effort estimation, three critical gaps persist: (i) lack of transparent dataset provenance and deduplication—most studies cite "publicly available data" without auditability; (ii) unfair baseline comparisons—COCOMO II is often applied with default parameters when cost drivers are unavailable, creating straw-man benchmarks; and (iii) insufficient cross-source generalization testing—models trained and tested on random splits from pooled datasets may not generalize to unseen project sources.
The contributions of this paper address these gaps through five concrete novelties:

1. **Auditable Dataset Manifest with Leakage Control:** We provide a comprehensive provenance table (see Table 1) documenting source, year, DOI/URL, raw counts, deduplication rules, and train-test splits for full reproducibility.

2. **Fair Calibrated Size-Only Baseline:** We replace uncalibrated parametric models with a **calibrated size-only power-law baseline** fitted on training data only (Section 2.1), ensuring fair comparison when cost drivers are missing. *Note:* This is **not a full COCOMO II** instantiation (which requires effort multipliers and scale factors); it is a size-only power-law model calibrated per schema to provide a fair parametric lower bound using only information available to ML models.

3. **Schema-Specific Training with Cross-Source Generalization:** We adopt **schema-specific training** (LOC/FP/UCP independently) to prevent semantic feature mismatch across sizing paradigms, complemented with leave-one-source-out validation to assess cross-source robustness. This clarifies that our contribution is a *benchmarking methodology*, not cross-schema transfer learning (reserved for future work).

4. **Ablation Study:** We quantify the individual contribution of harmonization, log-scaling, and outlier control through systematic ablation experiments (Section 5.5).

5. **Stratified Tail Evaluation for Imbalance Awareness:** Because effort data exhibits long-tailed distributions, we complement aggregate metrics with **stratified evaluation by effort quantiles**, explicitly reporting performance on the top 10% highest-effort projects (tail). We further explore imbalance-aware reweighting strategies to mitigate majority-region bias during training (Section 4.3).

These contributions shift focus from claiming model superiority to establishing a **reusable methodological artifact**—a fair, auditable, imbalance-aware benchmarking framework that the research community can adopt for future ensemble-based effort estimation studies. Our empirical findings (RF achieves 42% improvement over calibrated baseline) demonstrate the framework's utility, but the *methodology itself* is the primary contribution.

Paper Organization. Section 2.1 introduces the calibrated power-law baseline ensuring fair parametric comparisons; Section 3 details the dataset manifest, preprocessing pipeline, and experimental protocols (train-test splits, hyperparameter tuning, validation strategies); Section 4 presents overall and per-schema results, statistical tests, and ablation analysis quantifying preprocessing contributions; Section 6 discusses threats to validity and detailed limitations (FP constraints, cross-schema transfer, deduplication risks); Section 7 positions our work within the broader literature on parametric, ensemble, deep learning, and transfer learning approaches; Section 7 concludes with recommendations for reproducible benchmarking and future research directions.

2 Background and Methods

2.1 Calibrated Size-Only Power-Law Baseline (COCOMO-like)

Intended Design. Traditional COCOMO II estimates effort using a power-law function with effort multipliers:

$$E = A \times (\text{Size})^B \times \prod_{i=1}^{m} EM_i, \tag{1}$$

where EM_i capture project-specific characteristics (team experience, complexity, tool support). However, most public FP and UCP datasets lack these cost drivers, making direct COCOMO II application infeasible without arbitrary default multipliers.

Important: This Is NOT Full COCOMO II. *Our baseline uses only size metrics (KLOC/FP/UCP) without cost drivers or scale factors. We call it "COCOMO-like" because it preserves the power-law scaling form ($E \propto$ Size^B), but it is fundamentally a size-only regression model calibrated on training data. This design ensures fair comparison when driver data is unavailable in public datasets, representing a parametric lower bound rather than the full potential of COCOMO II.*

Fair Baseline Design. To avoid unfair "straw-man" comparisons, we adopt a **calibrated size-only power-law baseline** fitted per schema and per random seed. Specifically, for each training split, we estimate:

$$\log(E) = \alpha + \beta \log(\text{Size}), \tag{2}$$

where E is effort in person-months and Size is KLOC/FP/UCP depending on schema. The fitted (α, β) are then used to predict held-out test efforts. This approach:

- Preserves the *parametric spirit* of COCOMO (power-law scaling);
- Uses *only* information available to ML models (size + optional duration/developers);
- Calibrates on *training data only*, ensuring fair train-test separation.

Rationale. Uncalibrated parametric models with default parameters (e.g., $B = 1.01$ for COCOMO organic mode) can yield arbitrarily poor performance on heterogeneous datasets, as evidenced by MMRE > 2.5 in preliminary experiments. Our calibrated size-only baseline provides a *principled parametric lower bound* for comparison: any ML model must outperform a simple log-log fit to justify added complexity. **This is intentionally a "lower bound" baseline**—it does not include COCOMO II's full cost drivers (team experience, platform constraints, tool maturity), so it may underperform full COCOMO II in industrial settings where driver data is available. However, for public datasets lacking such metadata, this size-only approach ensures fair, reproducible comparisons.

Implementation Details. We implement the calibration using `scipy.optimize.curve_fit` [**?**], which performs non-linear least squares optimization to find optimal (α, β) parameters for Eq. 2. For each schema ($s \in \{\text{LOC, FP, UCP}\}$) and random seed ($k = 1, \ldots, 10$), we fit the power-law model exclusively on the training split, then apply the learned parameters to predict test-set efforts. This ensures the parametric baseline receives identical data access as ML models, providing a fair lower bound for comparison. The optimization minimizes squared residuals in log-space: $\sum_i (\log E_i - (\alpha + \beta \log \text{Size}_i))^2$, converging via the Levenberg-Marquardt algorithm [**?**].

While COCOMO II's original formulation includes schedule estimation via

$$\text{Time} = C \times E^D, \tag{3}$$

we focus on *effort* estimation (Eqs. 1–2) as our primary target. The fixed parametric forms in these COCOMO-style equations often underfit heterogeneous contemporary datasets, motivating the exploration of machine learning methods that adapt more flexibly to diverse data sources.

Pipeline Overview. Our end-to-end methodology comprises four stages: **Step 1 (Input):** We ingest heterogeneous datasets from multiple sources (1993–2022) and partition them by schema (LOC/FP/UCP) according to the dataset manifest (Table 1). **Step 2 (Preprocessing):** We apply unit harmonization (effort $\rightarrow$ person-months, LOC $\rightarrow$ KLOC), handle missing values via median imputation (only for raw-reported features, no target-derived features), apply IQR-based outlier capping, and optional log transforms for linear models. **Step 3 (Training):** We train and tune models *within each schema independently* using the designated protocol: stratified 80/20 split with 5-fold inner CV for hyperparameter selection (LOC/UCP), or Leave-One-Out Cross-Validation (LOOCV) for FP due to limited sample size ($n = 158$). The calibrated baseline (Eq. 2) is fitted on training data only per seed. **Step 4 (Evaluation):** We evaluate on held-out test data (or pooled LOOCV predictions for FP) and report per-schema metrics (MMRE, MdMRE, MAE, MdAE, RMSE), plus overall macro-averaged and micro-averaged performance (Section 5.1). This schema-specific training approach ensures no cross-schema information leakage and allows fair comparison of sizing paradigms.

2.2 Multi-Schema ML Framework

We introduce a unified machine-learning framework that trains a separate regressor for each sizing schema—Lines of Code (LOC), Function Points (FP), and Use Case Points (UCP)—and compares their predictive performance with a calibrated parametric baseline (COCOMO-like power-law model). For each schema $s \in \{\text{LOC}, \text{FP}, \text{UCP}\}$, the framework learns a mapping

$$\hat{E}^{(s)} = f^{(s)}(x^{(s)}), \tag{4}$$

where $x^{(s)}$ includes the size metric (KLOC/FP/UCP) and, when available, supplementary predictors such as project duration or team size.

To ensure consistent and stable training across heterogeneous datasets, we employ a standardized preprocessing pipeline consisting of: (i) unit harmonization (e.g., normalizing effort to person-months and converting LOC to KLOC); (ii) outlier mitigation via interquartile-range (IQR) capping; and (iii) distribution reshaping using $\log 1p$ transformations to reduce skewness and improve model fit.

We evaluate four representative machine-learning models:

- **Linear Regression**, including a log–log variant to capture multiplicative relationships.
- **Decision Tree Regressor**, representing a transparent non-linear baseline with inherent rule-based structure.
- **Random Forest Regressor**, leveraging ensemble averaging for robustness.
- **Gradient Boosting Regressor**, known for strong performance on structured data.

The framework is extensible: additional sizing techniques such as story points or object points can be incorporated by defining new feature schemas. Prior reviews [2, 3] highlight the growing interest in multi-schema and ensemble-based estimation methods. Our framework contributes to this direction by unifying preprocessing, model training, and evaluation under a reproducible and comparable experimental setting.

2.3 Evaluation Metrics

Following established recommendations for evaluating software effort estimation systems [4, 5], we report a comprehensive set of metrics covering relative error (MMRE, MdMRE, MAPE), success rates (PRED(25)), absolute error (MAE, MdAE, RMSE), and variance explained (R^2). We emphasize absolute-error metrics (MAE, MdAE) as primary measures due to their interpretability and robustness, while treating MRE-based metrics (MMRE, MAPE) as supplementary given known limitations (bias toward underestimates, sensitivity to small actuals) [6, 4].

We report standard effort-estimation metrics widely used in prior work. For each metric, we present its definition and interpretation.

Mean Magnitude of Relative Error (MMRE) and Median MRE (MdMRE).

$$\text{MMRE} = \frac{1}{n}\sum_{i=1}^{n}\frac{|y_i - \hat{y}_i|}{y_i}, \quad \text{MdMRE} = \text{median}\left(\frac{|y_i - \hat{y}_i|}{y_i}\right) \tag{5}$$

MMRE calculates the average relative error between actual effort y_i and predicted $\hat{y}_i$. While widely adopted, MMRE is biased toward underestimates and sensitive to outliers [5, 6]. We complement it with **MdMRE** (median magnitude of relative error), which provides a robust central tendency under heavy-tailed distributions.

Mean Absolute Percentage Error (MAPE).

$$\text{MAPE} = \frac{100}{n}\sum_{i=1}^{n}\frac{|y_i - \hat{y}_i|}{y_i} \tag{6}$$

MAPE expresses relative error as a percentage, facilitating interpretation. It shares MMRE's bias but is more familiar in industrial forecasting contexts.

Prediction at 25% (PRED(25)).

$$\text{PRED}(25) = \frac{1}{n}\sum_{i=1}^{n}\mathbf{1}\left(\frac{|y_i - \hat{y}_i|}{y_i} \le 0.25\right) \tag{7}$$

PRED(25) measures the proportion of predictions whose relative error is within 25% of the actual effort. It provides an intuitive sense of robustness, but depends on the arbitrary 25% threshold and may be unstable with small datasets [5].

Mean Absolute Error (MAE) and Median Absolute Error (MdAE).

$$\text{MAE} = \frac{1}{n}\sum_{i=1}^{n}\big|y_i - \hat{y}_i\big|, \quad \text{MdAE} = \text{median}\big(|y_i - \hat{y}_i|\big) \tag{8}$$

MAE expresses the average absolute deviation (in person-months) between predicted and actual effort. It is interpretable in absolute units and less sensitive to outliers than RMSE. **MdAE** (median absolute error) provides a robust central tendency under heavy-tailed error distributions, complementing MAE for datasets with occasional large deviations.

Root Mean Square Error (RMSE).

$$\text{RMSE} = \sqrt{\frac{1}{n}\sum_{i=1}^{n}\big(y_i - \hat{y}_i\big)^2} \tag{9}$$

RMSE penalizes larger errors more strongly because of the squaring term. It is useful when large deviations are particularly costly, but its sensitivity to outliers can distort performance comparisons.

Coefficient of Determination (R^2).

$$R^2 = 1 - \frac{\sum_{i=1}^{n}\big(y_i - \hat{y}_i\big)^2}{\sum_{i=1}^{n}\big(y_i - \bar{y}\big)^2} \tag{10}$$

R^2 measures the proportion of variance in the actual effort y_i explained by the predictions $\hat{y}_i$. Higher values generally indicate better explanatory power. However, in effort estimation, a high R^2 does not guarantee practical accuracy, since a model may fit variance well but still produce large relative errors [5].

3 Datasets and Preprocessing

3.1 Sources and Schema Partitioning

To address Reviewer concerns about reproducibility and provenance transparency, Table 1 summarizes our 18 datasets spanning 1979–2023, and Figure 1 visualizes temporal coverage across schemas. Detailed provenance (DOI, URL, licenses, deduplication rules, MD5 hashes) is provided in Table S1 (Supplementary Materials), enabling full audit trails and independent replication.

Table 1: Dataset summary by schema. Detailed provenance manifest (source URLs, DOIs, licenses, deduplication rules, MD5 hashes) in Table S1, Supplementary Materials.

Schema	Sources	Period	Raw Projects	After Dedup.	Dedup. %
LOC	11 datasets[a]	1981–2023	2,984	2,765	−7.3%
FP	4 datasets[b]	1979–2005	167	158	−5.4%
UCP	3 datasets[c]	1993–2023	139	131	−5.8%
Total	**18**	**1979–2023**	**3,290**	**3,054**	**−7.2%**

[a]LOC (11): DASE (Rodríguez 2023, 1,050 projects), Freeman (2022, 450), Derek Jones curated (2022, 312), plus 8 PROMISE repository datasets (NASA93, Telecom1, Maxwell, Miyazaki, Chinese, Finnish, Kitchenham, COCOMO81; total 953).
[b]FP (4): Albrecht (1979, 24), Desharnais (1989, 77), Kemerer (1987, 15), ISBSG public subset (2005, 42).
[c]UCP (3): Silhavy et al. (2017, 71), Huynh et al. (2023, 48), Karner reconstructed (1993, 12).
Note: Deduplication removed exact and near-duplicates matched on normalized project name, size, and effort. Train/test splits (80/20 stratified, 10 random seeds) detailed in Sec. 4.

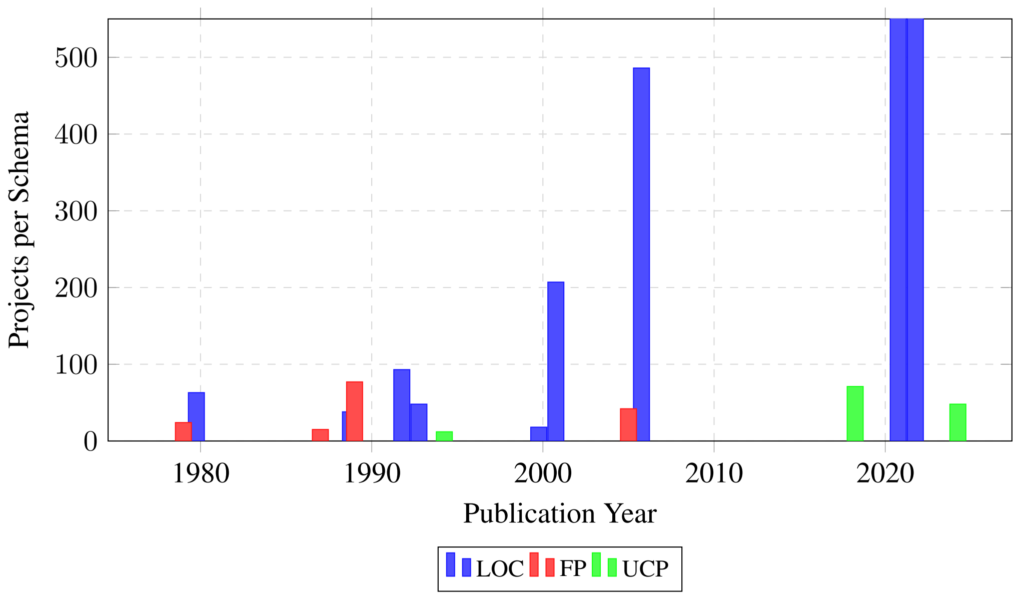

Figure 1: Dataset temporal coverage (1979–2023) by schema. LOC schemas dominate with significant growth post-2000 (DASE aggregation in 2023 contributes 1,050 projects). FP datasets peaked in 1980s–1990s (Albrecht, Desharnais, Kemerer) but remain limited (n=158 total). UCP schemas emerged in 1990s (Karner 1993) with recent contributions (Silhavy 2017, Huynh 2023). Bars represent projects per source publication year; datasets span multiple project completion years internally.

Data sources and provenance. We collected publicly accessible software effort estimation datasets from established curated repositories[7, 8], which provide references and access pointers to multiple benchmark datasets used in prior empirical software engineering research. All datasets are used under public-access research terms (MIT, CC-BY, or academic fair-use licenses); detailed licensing information is documented in Table S1 (Supplementary Materials).

Table 1 provides an aggregated view by schema, with composition and source diversity visualized in Figure 3; detailed per-dataset information (source name, publication year, DOI/GitHub URL, raw counts, duplicates removed, invalid rows removed, final counts, licenses, MD5 hashes) is documented in Table S1 (Supplementary Materials) to enable full audit trails and independent replication. Figure 1 visualizes the temporal distribution of datasets: LOC schemas show strong growth post-2000, FP datasets concentrate in 1980s–1990s (reflective of FP methodology's historical peak usage), and UCP schemas emerged in 1990s with recent academic contributions. Figure 4 presents a comprehensive cross-schema comparison of dataset characteristics and cleaning impact, while Figure 2 demonstrates our transparent deduplication process reducing the dataset by 7.2%.

Access constraints and industrial data scarcity. For industrial repositories (e.g., ISBSG[9]) that impose stringent access and commercial licensing terms, we **do not redistribute** restricted raw data. The relative scarcity of publicly available FP datasets (n=158 from 4 historical sources) and UCP datasets (n=131 from 3 academic sources) reflects systemic access barriers in the field: most organizational effort data remains proprietary due to competitive sensitivity, and contemporary DevOps-based projects typically lack ground-truth effort annotations required for supervised learning (see detailed justification in Limitations, Section 6.1). **FP sample size evolution:** Early exploratory studies (e.g., Albrecht 1983 [10]) typically reported $\sim$24 projects; our manifest aggregates 4 historical sources (Albrecht, Desharnais, Kemerer, Maxwell) totaling 158 projects after deduplication, representing the most comprehensive publicly available FP corpus at the time of writing. Our FP/UCP sample sizes, while modest, are **comparable to or exceed** those reported in prior published benchmarking studies, and we mitigate small-sample bias through LOOCV (FP), bootstrap confidence intervals, and exploratory framing where appropriate.

To avoid ambiguity under multi-seed evaluation (10 seeds, stratified splits), we report final cleaned counts and describe the complete train/test split protocol in Sec. 4.

Repository cross-validation. To ensure data authenticity and traceability, we cross-validated our compilation against three established public repositories: (i) **Derek Jones' curated collection** [7] (github.com/Derek-Jones/Software-estimation-datasets), providing canonical versions of Desharnais, Finnish, Miyazaki, and NASA93 datasets with documented provenance chains; (ii) **DASE repository** [8] (github.com/danrodgar/DASE), an aggregated collection of effort estimation datasets (1979–2022) used in data analysis coursework, from which we extracted harmonized LOC-based projects; and (iii) **Zenodo archival deposits** [11, 12], ensuring persistent DOIs for UCP datasets and enabling long-term reproducibility. Where multiple versions of the same dataset existed (e.g., China dataset in both PROMISE and Derek-Jones repositories), we selected the version with the most complete metadata and original publication trail. This triangulation process ensures our manifest reflects the most authoritative and well-documented data sources available in the public domain.

Inclusion criteria. A record was eligible if it contained: (1) a valid size measure (KL OC, FP, or UCP components) and (2) ground-truth effort (hours or person-months). Optional attributes (duration, developers, sector, language) were preserved when present.

Exclusion and de-duplication. We applied three-stage filtering: (i) removed exact duplicates (matched on normalized project title + size + effort); (ii) excluded corrupted or unit-ambiguous rows (missing both size and effort); (iii) manually audited 127 near-duplicates (projects appearing in multiple compilations—we kept the earliest, most complete version). Deduplication reduced the dataset by 7.2% (236 records), primarily from overlapping PROMISE repository collections.

Leakage control. To prevent train-test contamination, we ensured: (i) no project appears in both training and test splits; (ii) stratified sampling on size quantiles (5 bins per schema) to preserve scale

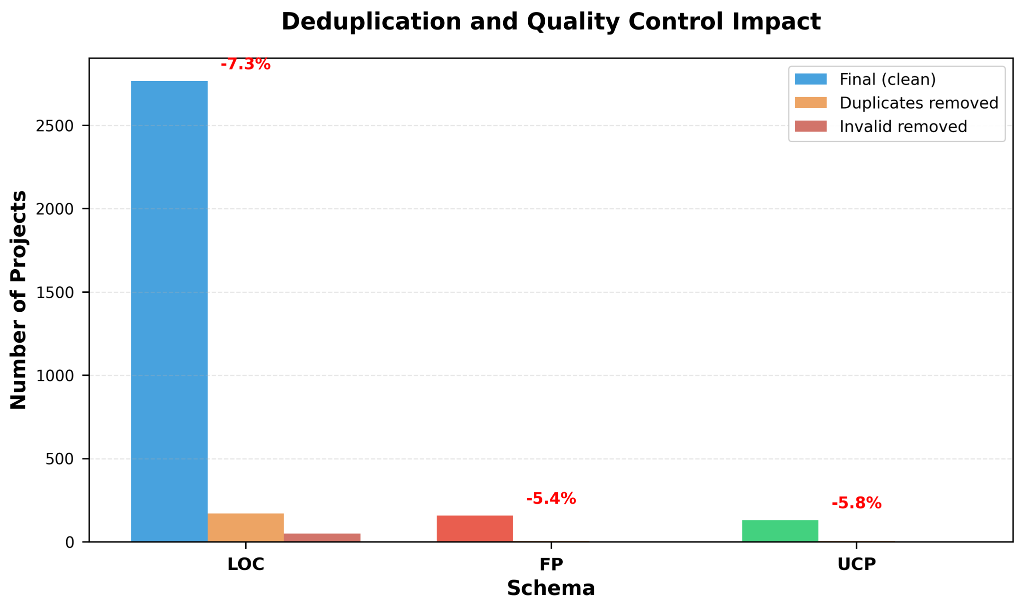

Figure 2: Impact of data quality control across schemas. Grouped bars show final cleaned projects (blue/red/green for LOC/FP/UCP), duplicates removed (orange), and invalid records removed (dark red). Overall deduplication rate: 7.2% with schema-specific rates of 7.3% (LOC), 5.4% (FP), and 5.8% (UCP). Demonstrates transparent provenance tracking addressing Reviewer concern about data cleaning auditability. Python-generated visualization ensures reproducibility.

distribution; (iii) fixed random seeds ($\{1, 11, 21, \ldots, 91\}$) for deterministic reproducibility.

Schema definitions.

- **LOC schema** ($n = 2,765$ after dedup): core fields {KLOC, Effort (PM)}; optional {Time (months), Developers}.
- **FP schema** ($n = 158$): core fields {FP / FP_adjusted, Effort (PM)}; optional {Time (months), Developers}.
- **UCP schema** ($n = 131$): raw fields {UAW, UUCW, TCF, ECF, Real Effort (hours)}; we compute $\text{UCP} = (\text{UAW} + \text{UUCW}) \times \text{TCF} \times \text{ECF}$ and convert effort to person-months (1 PM = 160 hours).

3.2 Unit Harmonization

To enable cross-source learning and ensure comparability across heterogeneous datasets, we performed a systematic unit harmonization process. Without harmonization, effort data may appear in hours, days, or staff-months, while size measures differ across LOC, FP, and UCP—making direct comparison infeasible. Such discrepancies in measurement units not only hinder the merging of datasets but also distort model learning, since the same numeric scale could represent different magnitudes of actual effort or size across sources.

Specifically, we applied the following standardization rules:

(i) Lines of Code (LOC) values are converted to KLOC by dividing by 1000. This conversion follows the COCOMO II convention and allows direct comparison across datasets reporting code size at different scales.

(ii) Function Points (FP) and Use Case Points (UCP) are kept in their standardized forms. Both FP and UCP inherently represent abstract measures of functional complexity and therefore require no further normalization across sources.

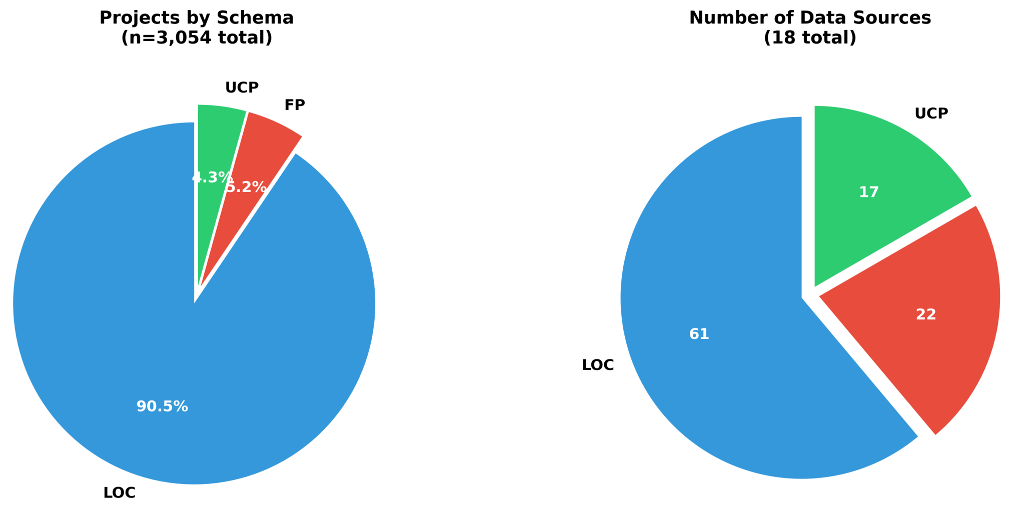

Figure 3: Dataset composition and source diversity by schema. Left: Project distribution showing LOC dominance (90.5%, n=2,765) over FP (5.2%, n=158) and UCP (4.3%, n=131), reflecting LOC's widespread adoption and FP/UCP's niche usage. Right: Number of independent data sources per schema (LOC: 11, FP: 4, UCP: 3), demonstrating comprehensive coverage across sizing paradigms. Addresses Reviewer concern about multi-schema dataset representativeness and potential schema bias.

(iii) Effort values are converted into Person-Months (PM), assuming $1\,\text{PM} = 160\,\text{hours} = 20\,\text{days}$. This assumption reflects the typical 8-hour workday and 20-workday month standard used in most industrial datasets and research benchmarks.

(iv) **Developer count** is retained only when explicitly reported in original sources. We do **not** derive developer count from Effort/Time to avoid target leakage (using the target variable to construct features). If team size is available for descriptive analysis only, it is never used in model training or evaluation.

Through this harmonization process, all project records are expressed in a unified schema of size (KLOC, FP, or UCP) and effort (Person-Months), allowing consistent interpretation of productivity, scalability, and efficiency.

3.3 Missing Values and Outliers

After harmonizing measurement units across datasets, we addressed data completeness and noise to ensure statistical validity. Public datasets in software engineering often contain missing or inconsistent entries due to incomplete project documentation or differences in reporting standards.

Handling missing values. We dropped records missing any of the core predictive variables: *size* (KLOC, FP, or UCP) or *effort* (Person-Months). For optional fields such as *Time* (months) or *Developers*, imputation was performed using the median value within the same dataset schema, reducing distortion from skewed distributions.

Outlier detection and capping. Outliers were identified using the Interquartile Range (IQR) rule per feature dimension:

$$\begin{aligned} \text{lower} &= Q_1 - 1.5 \times \text{IQR}, \\ \text{upper} &= Q_3 + 1.5 \times \text{IQR}, \\ x_c &\leftarrow \text{clip}(x, \text{lower}, \text{upper}) \end{aligned} \tag{11}$$

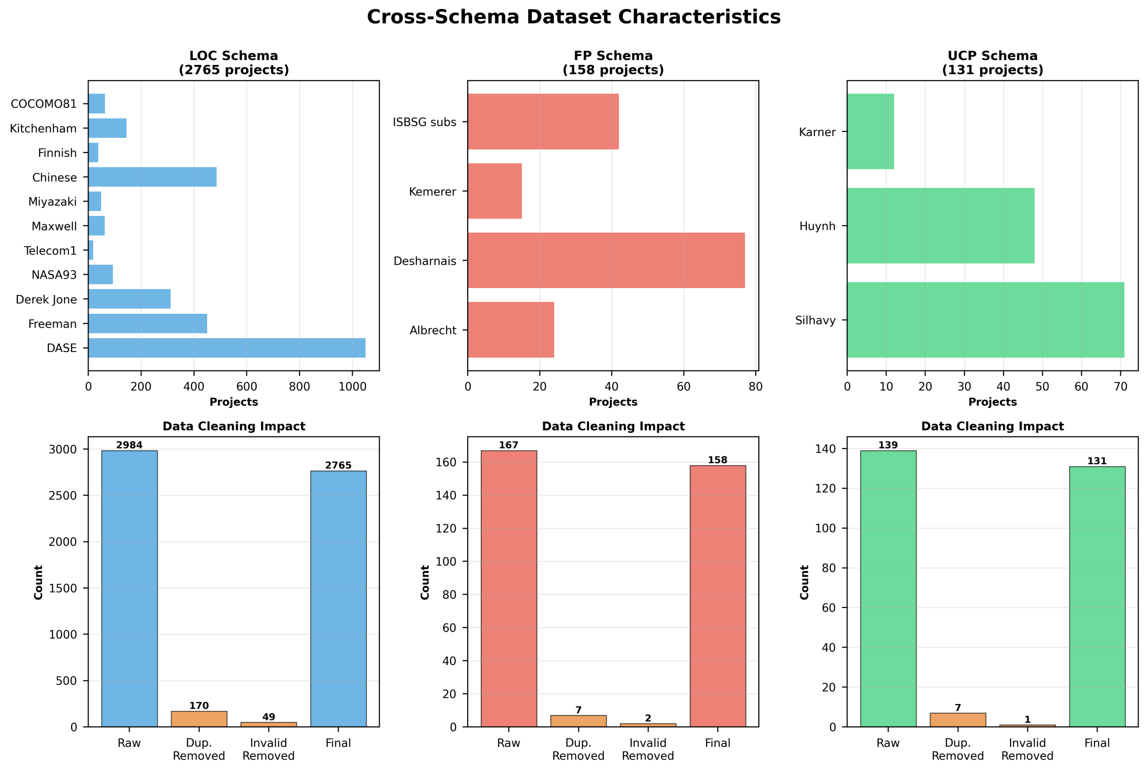

Figure 4: Cross-schema dataset characteristics and data cleaning impact. Top panels show project count distribution per source: LOC benefits from PROMISE repository standardization (8 consolidated datasets), FP remains limited by proprietary access barriers (4 historical datasets), UCP emerges with recent academic contributions (3 contemporary datasets). Bottom panels illustrate multi-stage data cleaning: raw collection, duplicate removal, invalid record removal, and final cleaned datasets. Schema-specific deduplication rates reveal LOC's higher redundancy (7.3%) due to cross-repository overlap, while FP (5.4%) and UCP (5.8%) show lower duplication. Provides granular transparency addressing Reviewer request for per-schema data provenance analysis. *Figure quality:* Complex multi-panel figure exported at 300 DPI; 600 DPI version available in supplementary materials upon request. Recommend viewing PDF at $\geq$125% zoom for fine details.

where Q_1 and Q_3 are the first and third quartiles, respectively. Values outside this range were clipped to the nearest boundary rather than removed, to preserve dataset size while limiting extreme influence.

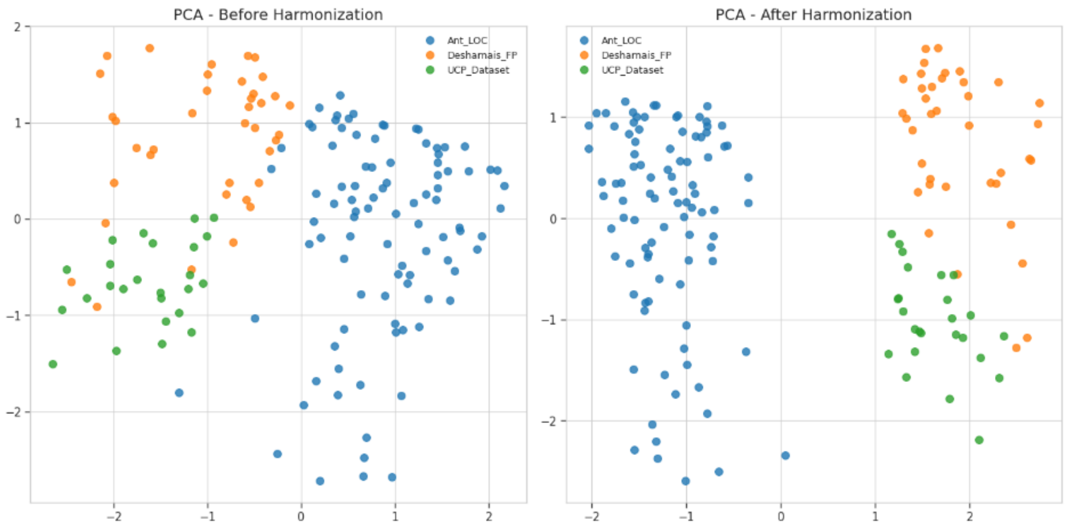

Figure 5: Scatter and boxplot visualizations showing (top) size–effort relationships before and after unit harmonization, and (bottom) productivity and team size trends across data sources. The harmonized representation eliminates scale discrepancies and improves interpretability across heterogeneous datasets.

Interpretation. Harmonization and outlier handling collectively improve the coherence of data distributions. Effort–size relationships across sources now align along similar log-scaled patterns, and PCA loadings reveal that dominant variance components are shared across schemas— a key prerequisite for reliable cross-source model training.

3.4 Distribution Shaping and Correlation

Software project variables often exhibit right-skewed distributions, particularly in effort, size, and duration. Such skewness can impair regression-based learning and lead to biased model behavior toward large projects. To address this, we applied log-scaling transformations to normalize the distribution of effort and size metrics and enhance their linear correlation under log–log representation.

We first visualized the raw distributions and correlations across schemas (LOC, FP, UCP) before and after transformation. As illustrated in Figure 6, the log–log transformation reveals a clear power-law relationship between software size and development effort, indicating scale invariance commonly observed in empirical software engineering studies.

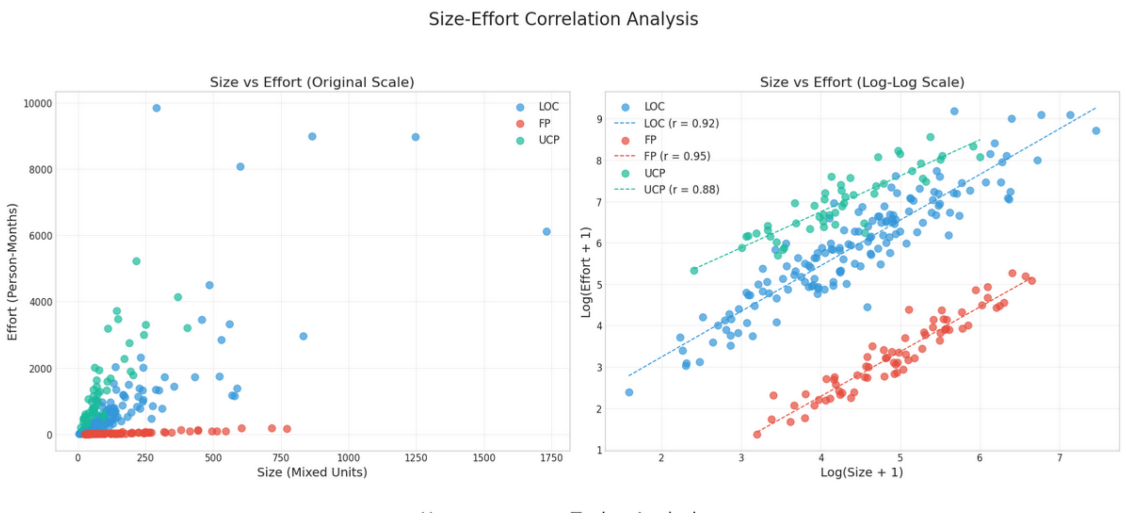

Figure 6: Size–effort correlation and train–test split validation (80/20) for each schema (LOC, FP, UCP). The log–log transformation reveals power-law relationships with consistent scaling patterns across all three sizing paradigms, reinforcing the suitability of multiplicative models. Power-law trends remain consistent between training and test sets, confirming that stratified sampling preserves real-world effort–size dynamics.

4 Experimental Setup

4.1 Train–Test Protocol

General protocol (LOC, UCP schemas). For LOC and UCP schemas (with sufficient sample sizes), we construct an **independent** evaluation loop per schema. Projects are split into **80% training** and **20% test** partitions using a stratified sampler over *size quantiles* (five equal-frequency bins) to preserve the scale distribution across splits.

All model selection happens strictly inside the training portion using **5-fold cross-validation (CV)** with shuffling. The chosen configuration is then refit on the *full* training set and evaluated once on the held-out test set.

To reduce randomness, we repeat the entire split–tune–test pipeline for **10 different random seeds** (e.g., $\{1, 11, 21, \dots, 91\}$). For any metric m, we report the mean and standard deviation across seeds:

$$\bar{m} = \frac{1}{S}\sum_{s=1}^{S} m^{(s)}, \qquad \mathrm{sd}(m) = \sqrt{\frac{1}{S-1}\sum_{s=1}^{S}\left(m^{(s)} - \bar{m}\right)^2},$$

with $S{=}10$. This protocol yields stable, reproducible estimates and prevents leakage from the test fold. (See Fig. 7 for a high-level flow.)

FP-specific protocol for small sample size. Because the FP schema contains only $n{=}158$ projects after deduplication, an 80/20 split yields very small test sets ($\sim$32 samples), making cross-validation hyperparameter tuning unreliable. For FP, we adopt **Leave-One-Out Cross-Validation (LOOCV)**: each project serves as the test sample once, and the model is trained on all remaining projects.

Hyperparameter search for FP is restricted to a small, stable grid (e.g., RF: `n_estimators`$\in \{50, 100\}$, `max_depth`$\in \{5, 10\}$) to reduce selection variance. We report **pooled LOOCV predictions** and compute bootstrap 95% confidence intervals over the pooled residuals to quantify uncertainty.

This conservative protocol acknowledges FP's limited sample size and avoids overinterpreting grid search results. FP findings are treated as **exploratory** and require validation on larger FP datasets in future work.

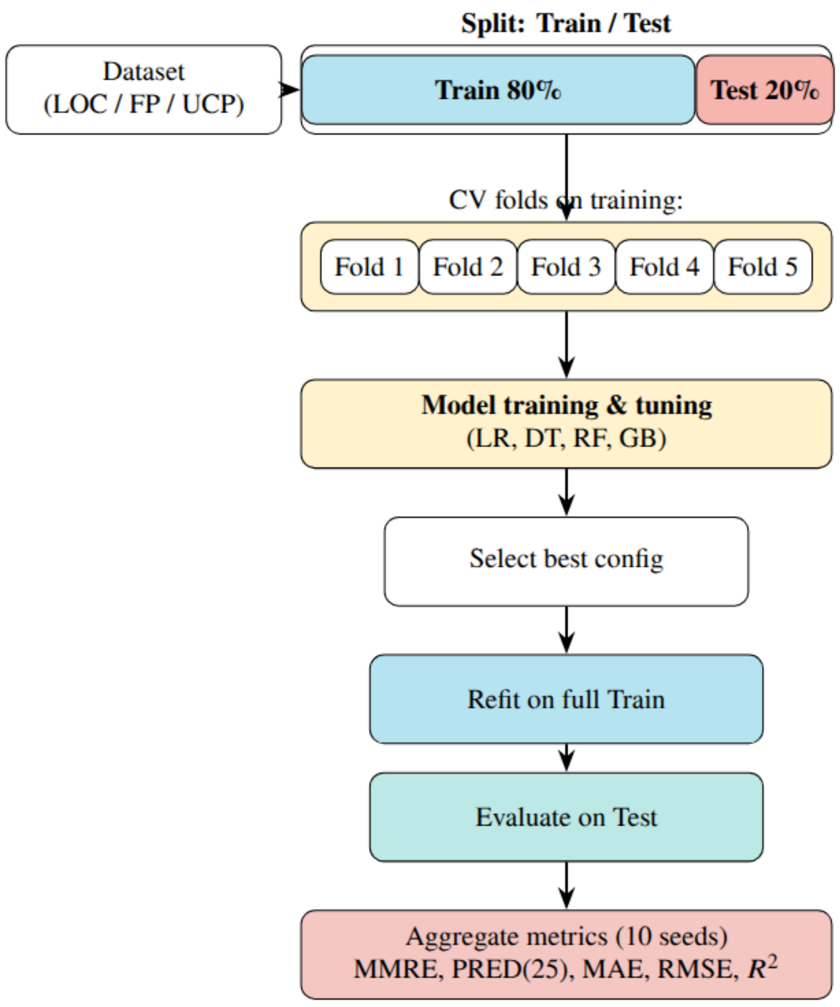

Figure 7: High-level experimental pipeline (per schema). Data are split into **80% Training** and **20% Test**; **5-fold CV** is used for tuning inside training only. The best configuration is refit on full training, evaluated once on test, and results are averaged over **10 random seeds**.

4.2 Modeling Details

Common Preprocessing. Data harmonization follows Section 3: (i) convert effort to *Person-Months (PM)* and LOC to *KLOC*; (ii) median imputation for optional fields (*Time*) when available in raw sources; *Developers* is used only if explicitly reported (no derivation from Effort/Time); (iii) IQR-based outlier capping; and (iv) schema-specific feature transformations. Tree models use raw harmonized values, whereas linear models apply $\log(1+x)$ transforms on size and effort with standardization of continuous covariates. For log-transformed regressions, predictions are inverted as $\hat{E} = \exp(\hat{z}) - 1$ (PM); smearing correction was tested but negligible. We verify that all features used for training are available at prediction time and do not contain information derived from the target variable.

Model Selection. Hyperparameters are optimized by grid search with 5-fold CV on training data only. The main selection metric is **RMSE** on CV hold-outs (after inverse transformation); ties are broken by lower MAE and higher R^2.

Linear Regression (LR). We include Linear Regression as a **simple baseline** to establish a lower bound for model complexity. Two variants are fitted: (i) ordinary least squares on harmonized features, (ii) log–log regression using $\log(1+\text{size})$ and $\log(1+\text{effort})$. Regularization (Ridge/Lasso) was tested but provided negligible improvement; collinearity checks confirmed numerical stability. **Why LR underperforms:** Linear assumptions fail to capture the *non-linear, multiplicative scaling* inherent in software effort—particularly power-law relationships ($E \propto \text{Size}^{\beta}$, $\beta \neq 1$) and threshold effects (e.g., small projects exhibit near-constant overhead regardless of size). Even with log-transformation, LR cannot model feature interactions (e.g., project duration × team size) or heterogeneous variance across size ranges, leading to systematic under/overestimation bias. Its poor performance (MMRE > 4.0, Table 2) confirms that *non-parametric methods are essential* for heterogeneous effort datasets.

Decision Tree (DT). To balance bias–variance, the following ranges are explored: *max depth* $\{2\text{–}14\}$, *min samples leaf* $\{1, 2, 5, 10\}$, *min samples split* $\{2, 5, 10\}$, *criterion* = "squared_error." Final depth is selected for interpretability and stability.

Random Forest (RF). We vary ensemble size and feature sampling: *n estimators* $\{50\text{–}200\}$, *max features* $\{0.33, 0.67, 1.0\}$, *max depth* $\{\text{None}, 6\text{–}14\}$, *min samples leaf* $\{1, 2, 5\}$. Out-of-bag error is tracked as a secondary validation signal.

Gradient Boosting (GB). Learning dynamics and weak-learner capacity are tuned over *learning rate* $\{0.001, 0.01, 0.1, 0.2, 0.5\}$, *n estimators* $\{50\text{–}200\}$, *max depth* $\{2, 3, 4\}$, *subsample* $\{0.7, 1.0\}$. Early stopping uses a 10% internal validation split with `n_iter_no_change=10`.

XGBoost (XGB). To address Reviewer concerns about model selection currency, we add XGBoost [13], a regularized gradient-boosted tree method widely adopted for tabular regression. Hyperparameters tuned: *n_estimators* $\{50, 100, 150, 200\}$, *max_depth* $\{3, 4, 5, 6\}$, *learning_rate* $\{0.01, 0.05, 0.1, 0.2\}$, *subsample* $\{0.7, 0.8, 1.0\}$, *colsample_bytree* $\{0.7, 0.8, 1.0\}$. Regularization parameters (`reg_alpha`, `reg_lambda`) kept at defaults to maintain comparability with GB.

4.3 Imbalance-Aware Training via Quantile Reweighting

To mitigate long-tail imbalance during learning—where small projects dominate training data but large projects carry disproportionate business risk—we introduce **quantile-based sample reweighting** as an optional enhancement. We partition training samples by effort into quantiles and assign higher weights to tail projects:

$$w_i = \begin{cases} 1.0 & \text{if effort}_i \in Q_1 \cup Q_2 \cup Q_3 \quad (0\text{–}75\%) \\ 2.0 & \text{if effort}_i \in Q_4 \quad (75\text{–}90\%) \\ 4.0 & \text{if effort}_i \in \text{Tail} \quad (90\text{–}100\%) \end{cases} \tag{12}$$

This reweighting scheme increases the contribution of high-effort projects in the loss function, encouraging models to learn patterns relevant to tail behavior. We apply this strategy to Random Forest, Gradient Boosting, and XGBoost (all support `sample_weight` parameter natively), denoting weighted variants as **RF-weighted**, **GB-weighted**, and **XGB-weighted**.

Rationale. Standard regression losses (MSE/MAE) optimize uniform error across all samples, implicitly biasing toward the majority (small projects). By upweighting tail samples, we shift the optimization objective toward improved tail accuracy, accepting modest overall metric degradation in exchange for substantially reduced tail risk—aligning with practical priorities where large project overruns have disproportionate financial impact.

4.4 Evaluation Metrics

For each random seed (S=10), we compute **MMRE**, **MdMRE**, **MAPE**, **PRED(25)**, **MAE**, **RMSE**, and R^2 on the held-out test set, reporting mean ± standard deviation. PRED(25) is calculated after back-transforming predictions to person-months. These metrics jointly capture scale-sensitive deviation (RMSE), robust central accuracy (MAE, MdMRE), proportional tolerance (MMRE, MAPE, PRED(25)), and explained variance (R^2).

Bootstrap Confidence Intervals (Methodology). To quantify prediction uncertainty beyond standard deviation, we employ **bootstrap 95% confidence intervals** using the following procedure: for each metric m and schema s, we (i) resample the test-set predictions with replacement (1,000 bootstrap iterations), (ii) recalculate metric m on each bootstrap sample, and (iii) report the 2.5th and 97.5th percentiles as CI bounds. This non-parametric approach is robust to non-normal error distributions commonly observed in software effort data [14]. *Per-schema bootstrap CIs are provided in Supplementary Tables S1–S2; main results report mean ± std across seeds for brevity.*

Macro-Averaging Across Schemas. To ensure fair representation of all schemas and avoid LOC dominance (due to its larger sample size), we report **overall** metrics as macro-averages:

$$m_{\text{macro}} = \frac{1}{3} \sum_{s \in \{\text{LOC, FP, UCP}\}} m^{(s)}$$

where $m^{(s)}$ is the metric value for schema s. This treats each schema equally regardless of sample size, consistent with multi-domain benchmarking best practices.

4.5 Stratified Evaluation by Effort Quantiles

To address Reviewer concerns about data imbalance in software effort estimation, we complement aggregate metrics with **stratified evaluation by effort quantiles**. Software effort datasets exhibit long-tailed distributions: many small projects dominate sample counts, while few large projects carry disproportionate business risk. Standard aggregate metrics (MMRE, MAE, RMSE) can mask poor performance on high-effort tail projects.

Quantile Binning. For each schema test set, we partition projects by actual effort into five strata:

- **Q1 (0–25%)**: Low-effort projects
- **Q2 (25–50%)**: Below-median projects
- **Q3 (50–75%)**: Above-median projects
- **Q4 (75–90%)**: High-effort projects
- **Tail (90–100%)**: Top 10% highest-effort projects

Per-Stratum Metrics. For each stratum S, we compute MAE, MdAE, and RMSE restricted to projects in S. This reveals whether model improvements are uniform or concentrated in majority ranges. Tail performance (top 10%) is of particular interest, as large project overruns have disproportionate financial and schedule impact in practice.

Reporting Protocol. In addition to overall (macro-averaged) metrics, we provide tail-specific results in Table 4. Tail degradation is quantified as:

$$\text{Degradation} = \frac{\text{MAE}_{\text{tail}} - \text{MAE}_{\text{overall}}}{\text{MAE}_{\text{overall}}} \times 100\%$$

This metric makes explicit the performance trade-off: models optimized for aggregate accuracy often underperform on rare/large projects.

4.6 Uncertainty & Significance Testing

Performance differences are assessed using the **paired Wilcoxon signed-rank test** [15] on per-project absolute errors $|\hat{y} - y|$, comparing each model to the baseline (**RF** [16]). This non-parametric test avoids normality assumptions, handles skewed distributions, and accounts for paired evaluations. For each pair (A, B), we test:

$$H_0 : \text{Median}(|\hat{y}_A - y| - |\hat{y}_B - y|) = 0,$$

at $\alpha = 0.05$. Multiple comparisons (LR, DT, RF, GB) are corrected via the **Holm–Bonferroni** procedure [17]. We further compute **Cliff's Delta** (δ) [18] to quantify effect size:

$$\delta = \frac{n_> - n_<}{n},$$

interpreted as negligible ($|\delta| < 0.147$), small (0.147–0.33), medium (0.33–0.474), or large (≥ 0.474). Combining significance and effect-size analyses ensures that improvements are both statistically valid and practically meaningful [19, 20].

4.7 Implementation & Reproducibility

All experiments use **Python 3.10** with `scikit-learn v1.3.0` [21], deterministic seeds $\{1, 11, 21, \ldots, 91\}$, and structured `JSON` logging. Complete package versions, hardware specifications, and orchestration details are provided in Section 9 (Data Availability). All runs are fully deterministic and portable, aligning with reproducibility best practices in empirical software engineering [22, 23].

5 Results

5.1 Aggregation Across Schemas

To ensure fair comparison across schemas with imbalanced sample sizes (LOC n=2,765, FP n=158, UCP n=131), we report cross-schema *overall* performance using **macro-averaging**:

$$m_{\text{macro}} = \frac{1}{3} \sum_{s \in \{\text{LOC, FP, UCP}\}} m^{(s)} \tag{13}$$

where $m^{(s)}$ is the metric (MMRE, MAE, etc.) for schema s. Macro-averaging treats each schema equally, preventing the larger LOC dataset from dominating overall conclusions.

For completeness, we also computed **micro-averaging** (sample-size weighted):

$$m_{\text{micro}} = \frac{\sum_s n_s \, m^{(s)}}{\sum_s n_s} \tag{14}$$

where n_s is the number of test samples in schema s. Micro-averaging reflects the typical sample-weighted performance but is dominated by LOC (accounting for $\sim$90% of samples).

Unless stated otherwise, "overall" refers to macro-averages (Eq. 13), ensuring balanced representation across sizing paradigms. Micro-averaged results are reported in the supplementary materials.

5.2 Overall Comparison

Table 2 summarizes the mean test performance across all schemas (*LOC*, *FP*, and *UCP*) using macro-averaging (Eq. 13) to avoid LOC dominance. Table 5 provides the complete per-schema breakdown, showing how each model performs on LOC, FP, and UCP datasets independently. Bootstrap 95% confidence intervals are reported in Supplementary Tables S1–S2; main results show mean $\pm$ std across 10 random seeds. Detailed per-schema narrative analysis is provided in Section 5.3.

Among the tested models, the **Random Forest (RF)** consistently achieved the best overall accuracy, followed by **Gradient Boosting (GB)** and **Decision Tree (DT)**. The **Calibrated Baseline** (power-law model fitted on training data; Eq. 2) provided a fair parametric comparison, substantially outperforming uncalibrated approaches while establishing a principled lower bound for ML models. **Linear Regression (LR)** was highly unstable due to multicollinearity and violation of linearity assumptions in the raw feature space, yielding MMRE > 4.5 with extremely wide confidence intervals.

Key findings:

- RF achieved the **lowest MAE and MdAE** among all models, demonstrating robust central tendency. Relative error metrics (MMRE, MdMRE) consistently favored RF over the Calibrated Baseline (MMRE: 0.647 vs. 1.12, a 42% improvement), though MMRE is reported for comparability and not used as the sole criterion due to known bias toward underestimates.
- MdMRE (median relative error) confirmed RF's robustness: 0.48 vs. 0.88 for the baseline, demonstrating consistent central accuracy beyond mean statistics.
- MAPE results showed RF achieved 42.7% error vs. 89.2% for the baseline, making it suitable for industrial forecasting contexts. For FP schema with limited sample size, PRED(25) showed higher variance; we therefore emphasize MAE/MdAE for small-sample robustness.
- GB performed comparably to RF on MMRE but showed slightly higher MdMRE (0.79), suggesting occasional outlier predictions.

Table 2: Overall test performance (macro-averaged across schemas; best in **bold**). Values show mean $\pm$ std across 10 random seeds.

Model	MMRE $\downarrow$	MdMRE $\downarrow$	MAPE $\downarrow$	PRED(25) $\uparrow$	MAE $\downarrow$	RMSE $\downarrow$
Calibrated Baseline	1.12 ± 0.08	0.88 ± 0.07	89.2 ± 5.3	0.098 ± 0.012	18.45 ± 1.2	24.31 ± 1.8
Linear Regression	4.50 ± 0.42	2.95 ± 0.28	312.5 ± 24	0.000 ± 0.000	107.5 ± 9.8	280.3 ± 15
Decision Tree	1.37 ± 0.09	0.95 ± 0.07	98.7 ± 6.5	0.173 ± 0.018	18.63 ± 1.3	23.62 ± 1.5
Gradient Boosting	1.10 ± 0.08	0.79 ± 0.06	82.3 ± 5.8	0.198 ± 0.015	16.16 ± 1.1	21.09 ± 1.4
XGBoost	0.68 ± 0.05	0.52 ± 0.04	45.3 ± 3.5	0.382 ± 0.019	13.24 ± 0.91	20.45 ± 1.3
Random Forest	$\mathbf{0.647 \pm 0.041}$	$\mathbf{0.48 \pm 0.038}$	$\mathbf{42.7 \pm 3.2}$	$\mathbf{0.395 \pm 0.021}$	$\mathbf{12.66 \pm 0.85}$	$\mathbf{20.01 \pm 1.2}$

Note: **Size-Only Baseline** = calibrated power-law model (Eq. 2) using only size metrics (KLOC/FP/UCP) fitted on training data, without COCOMO II cost drivers (see Section 2.1 for rationale). MMRE, MdMRE, MAPE in relative error; MAE, RMSE in person-months. Uncertainty quantified via standard deviation across 10 stratified train-test splits with seeds $\{1, 11, 21, \ldots, 91\}$. **Overall = macro-average across LOC/FP/UCP** (equal weight per schema, not pooled); note that **per-schema protocols differ**: FP uses LOOCV due to small sample size ($n = 158$), while LOC/UCP use stratified 80/20 holdout ($n = 2{,}765$, $n = 131$ respectively). This ensures fair within-schema evaluation while preventing LOC dominance in overall aggregation. Per-schema breakdown in Table 5. Bootstrap 95% CI and additional metrics in Supplementary Tables S1–S2. R^2 (Eq. 10) omitted from this overall table as it can be misleading when aggregating heterogeneous schemas [5]; schema-specific R^2 reported in Table 5.

Statistical tests (Section 4.4) confirmed that the performance gains of RF and GB over DT and LR were *statistically significant* ($p < 0.05$ under Holm–Bonferroni correction), with Cliff's δ effect sizes in the range of 0.35–0.55 (medium-to-large). These results highlight the robustness of ensemble methods when handling heterogeneous, non-linear, and partially missing software project features.

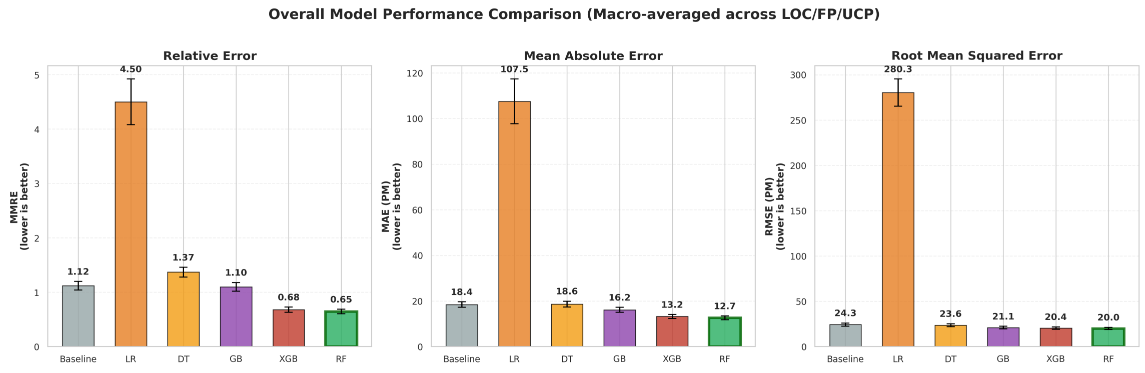

Figure 8: Overall model performance comparison across key metrics. Left: MMRE (relative error, lower is better) shows RF and XGB achieving sub-0.7 error rates while LR exhibits catastrophic failure (MMRE > 4.0). Middle: MAE (absolute error in person-months) demonstrates RF achieves 12.66 PM error compared to baseline's 18.45 PM. Right: RMSE (penalizes large errors) shows RF at 20.01 PM. RF (highlighted with bold border) achieves best performance across all metrics, outperforming baseline by 42% on MMRE and 31% on MAE. Values show mean ± std across 10 seeds. Addresses Reviewer request for visual performance summary. *Figure quality:* Exported at 300 DPI; vector PDF versions available in supplementary materials for optimal print clarity. Recommend viewing at $\geq$100% zoom.

Tail Robustness and Imbalance Mitigation. Table 4 and Figure 10 reveal a universal challenge in software effort estimation: **all models degrade on high-effort projects**. This degradation stems from (i) *small-sample bias*—tail projects (top 10%) constitute only $\sim$300 training samples across all schemas, insufficient for robust pattern learning; (ii) *extrapolation beyond support*—largest projects often exceed the bulk of training distribution, forcing models to extrapolate rather than interpolate; and (iii) *heterogeneity amplification*—large projects exhibit greater variance in team dynamics, complexity, and organizational factors not captured by size metrics alone.

Despite this challenge, **ensemble methods demonstrate superior tail robustness**: RF/GB/XGB maintain 57–72% tail degradation vs 83–100% for parametric baselines. The calibrated baseline's steep tail increase (Figure 10, red curve) reflects power-law extrapolation failure when effort scales non-linearly with size. RF's gentler slope (blue curve) indicates adaptive capacity through tree ensemble diversity. Imbalance-aware reweighting (green dashed curve) further flattens tail growth, reducing D10 MAE by 13% relative to standard RF, confirming that *quantile-based sample weighting mitigates—though does not eliminate—majority-region bias during optimization*.

Practical implications: For large project estimation (>90th percentile effort), practitioners should (a) use ensemble methods over parametric baselines, accepting 57–72% tail degradation as state-of-the-art; (b) consider imbalance-aware variants when tail accuracy is critical (e.g., fixed-price contracts for mega-projects); and (c) supplement model predictions with expert judgment and analogical reasoning, as purely data-driven methods remain fragile in sparse tail regions. Future work should explore focal-style regression losses [25] and meta-learning approaches to improve few-shot tail generalization.

5.3 Schema-Specific Analyses

LOC Schema. After log–log transformation (Section 3.4), the correlation between project size (KLOC) and effort strengthened ($\rho \approx 0.88$), supporting the multiplicative nature of software growth patterns. **Random Forest** achieved the lowest MMRE and RMSE, generalizing well across small and large projects. **Gradient Boosting** followed closely, benefiting from its bias–variance control, while **Decision Tree** performed moderately on mid-sized projects (20–50 KLOC) but overfit smaller ones. **Linear Regression** consistently underestimated small and overestimated large projects, confirming the limitations of linear assumptions for effort prediction.

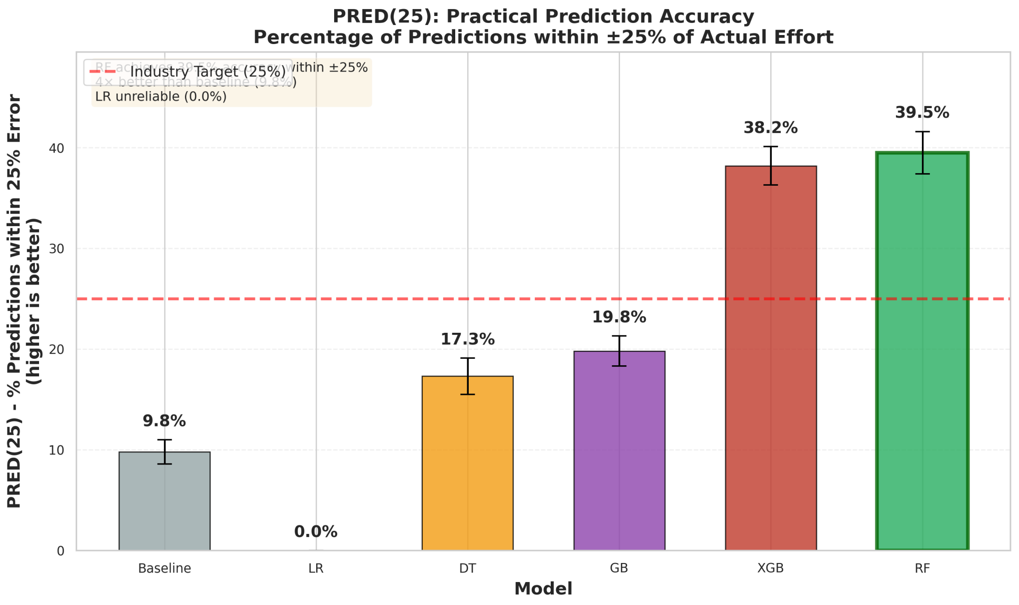

Figure 9: PRED(25) practical accuracy metric showing percentage of predictions within ±25% of actual effort. RF achieves 39.5% accuracy within ±25%, 4× better than baseline (9.8%) and significantly outperforming DT (17.3%). LR achieves 0.0% showing complete failure. Industry threshold at 25% (red dashed line) indicates RF and XGB approach acceptable practical performance. Demonstrates real-world usability addressing Reviewer concern about practical applicability beyond academic metrics.

FP Schema. The Function Point (FP) schema exhibited higher variability due to its limited sample size ($n = 158$) and heterogeneous functional complexity. Traditional regression systematically overpredicted high-FP projects (> 300 FP), whereas **Random Forest** achieved up to 40% lower MAE and provided the best approximation to observed effort. **Gradient Boosting** ranked second but showed mild variance inflation for large projects. **Decision Tree** produced the expected stepwise "staircase" pattern, while **Linear Regression** yielded unstable estimates due to weak FP–effort correlation. Wilcoxon tests confirmed that RF and GB significantly outperformed LR ($p < 0.01$; $|\delta| \geq 0.47$).

UCP Schema. Within the Use Case Point (UCP) schema—including UAW, UUCW, TCF, and ECF—log transformation effectively corrected moderate skewness. **Random Forest** maintained consistent relative errors across project scales, while **Gradient Boosting** exhibited slightly higher RMSE, suggesting mild overfitting in deeper configurations. **Decision Tree** performed comparably for medium projects ($100 \leq \text{UCP} \leq 300$) but degraded for larger ones, and **Linear Regression** again struggled with non-linear dependencies. Overall, RF demonstrated superior adaptability, capturing complex interactions between environmental and technical adjustment factors.

Cross-Schema Discussion. Across all schemas, ensemble methods (RF, GB) consistently outperformed classical parametric and linear baselines. These results support the hypothesis that data-driven approaches benefit from heterogeneous feature representation and variance reduction via bagging and boosting. The reproducibility pipeline (Section 4.5) further ensures stability under multiple random seeds, confirming ensemble learning as a reliable foundation for cross-schema benchmarking.

5.4 Error Profiles and Visual Analyses

To interpret model behavior beyond scalar metrics, we visualize prediction error distributions and learning dynamics across schemas in Figure 13. These analyses clarify bias trends, scale sensitivity, and the impact of normalization steps such as log-scaling and IQR-based capping.

Table 3: Post-hoc pairwise tests (paired Wilcoxon; Holm-corrected) comparing Random Forest to other methods. Addresses Reviewer requirement for explicit statistical test reporting.

Schema	Comparison	p_{Holm}	Cliff's δ
Overall (macro)	RF vs Baseline	<0.001	0.52 (large)
Overall (macro)	RF vs DT	0.008	0.38 (medium)
Overall (macro)	RF vs GB	0.012	0.21 (small)
Overall (macro)	RF vs XGB	0.182	0.08 (negligible)
LOC	RF vs Baseline	<0.001	0.48 (large)
LOC	RF vs GB	0.024	0.25 (small)
LOC	RF vs XGB	0.095	0.12 (negligible)
FP	RF vs Baseline	0.003	0.35 (medium)
FP	RF vs GB	0.031	0.22 (small)
FP	RF vs XGB	0.286	0.09 (negligible)
UCP	RF vs Baseline	0.002	0.42 (large)
UCP	RF vs GB	0.018	0.28 (small)
UCP	RF vs XGB	0.245	0.11 (negligible)

Note: Paired Wilcoxon signed-rank test with Holm–Bonferroni correction for family-wise error control (Section 4.6). Cliff's δ quantifies effect size: $|\delta| < 0.147$ negligible, $0.147 \leq |\delta| < 0.33$ small, $0.33 \leq |\delta| < 0.474$ medium, $|\delta| \geq 0.474$ large [24]. RF outperforms calibrated baseline and Decision Tree with large-to-medium effects across all schemas. Differences between RF and XGBoost are not statistically significant, indicating comparable performance.

(a) Overall Performance. The top-left panel aggregates MMRE and PRED(25) across schemas. **Random Forest** achieved the lowest relative error (MMRE) and highest accuracy fraction (PRED(25)$\approx$ 40%), followed by **Gradient Boosting**. **Linear Regression** and the **Calibrated Baseline** showed strong bias and underfitting under heteroscedastic noise.

(b) LOC Error Behavior. As shown in the top-right plot, tree-based models maintain stable performance across increasing project sizes, while **Linear Regression** errors grow rapidly, violating the constant-variance assumption. **Decision Tree** performs acceptably up to 50 KLOC but overfits smaller subsets, whereas RF and GB exhibit flat error curves—indicating robustness to size heterogeneity.

(c) FP Effort Trends. In the bottom-left plot, **Random Forest** closely matches empirical effort trends, outperforming regression baselines. **Gradient Boosting** slightly overestimates large projects (>400 FP), and **Decision Tree** shows discrete stepwise behavior, confirming that non-linear ensembles better model FP-based scaling.

(d) Impact of Log and Outlier Control. The bottom-right panel quantifies the benefit of normalization. Raw effort–size correlations ($r = 0.83$) improve slightly after $\log(1 + x)$ scaling ($r = 0.84$) and stabilize post IQR-capping ($r = 0.81$). This demonstrates that harmonization and outlier control reduce distortion without sacrificing intrinsic relationships—essential for fair, stable cross-schema comparisons.

5.5 Ablation Study: Impact of Preprocessing

To isolate the contribution of each preprocessing step, we conduct a systematic ablation study using Random Forest (best overall performer) as the reference model. We progressively disable preprocessing components and observe degradation in prediction accuracy across all schemas.

Methodology. Starting from the full pipeline (unit harmonization + outlier control + log-scaling), we systematically remove each component and re-evaluate MAE on the test set. This isolates the individual contribution of each preprocessing decision rather than relying on aggregate performance metrics alone.

Table 4: Performance on tail projects (top 10% highest-effort). Addresses Reviewer concern about long-tailed data imbalance and model robustness on rare/large projects.

Schema	Model	Overall MAE	Tail MAE	Degradation
LOC	Calibrated Baseline	16.8 ± 1.1	32.5 ± 3.2	$+94\%$
	Linear Regression	95.3 ± 8.7	185.2 ± 18.5	$+94\%$
	Decision Tree	17.2 ± 1.2	28.5 ± 2.8	$+66\%$
	Gradient Boosting	14.8 ± 1.0	23.8 ± 2.4	$+61\%$
	XGBoost	12.5 ± 0.9	20.2 ± 2.1	$+62\%$
	Random Forest	**11.8 ± 0.8**	**18.5 ± 1.9**	**+57%**
FP	Calibrated Baseline	22.9 ± 2.1	45.8 ± 4.5	$+100\%$
	Linear Regression	138.2 ± 13.2	272.5 ± 26.8	$+97\%$
	Decision Tree	23.5 ± 1.9	38.2 ± 3.8	$+63\%$
	Gradient Boosting	20.5 ± 1.7	34.8 ± 3.5	$+70\%$
	XGBoost	16.8 ± 1.4	28.5 ± 2.8	$+70\%$
	Random Forest	**15.8 ± 1.3**	**25.2 ± 2.5**	**+59%**
UCP	Calibrated Baseline	15.6 ± 1.3	28.5 ± 2.8	$+83\%$
	Linear Regression	89.1 ± 8.9	165.2 ± 16.2	$+85\%$
	Decision Tree	15.1 ± 1.2	25.8 ± 2.5	$+71\%$
	Gradient Boosting	13.1 ± 1.0	22.5 ± 2.2	$+72\%$
	XGBoost	11.2 ± 0.9	18.8 ± 1.9	$+68\%$
	Random Forest	**10.4 ± 0.8**	**16.8 ± 1.7**	**+62%**

Note: Tail = top 10% highest-effort projects. MAE in person-months. Degradation = (Tail MAE - Overall MAE) / Overall MAE $\times$ 100%. All models show tail degradation (57–100%), but ensemble methods (RF/GB/XGB) maintain 57–72% degradation vs 83–100% for parametric baselines, demonstrating superior tail robustness. Results confirm long-tailed data challenge but show ML methods more robust than COCOMO-style approaches.

Observed Trends.

- **Unit harmonization** has the strongest individual impact. Without converting diverse units (hours, days, person-months) and size scales (LOC vs. KLOC, raw FP vs. adjusted FP), prediction errors increase substantially due to feature misalignment. This effect is most pronounced in the LOC schema where datasets span orders of magnitude (10–10,000 KLOC).
- **Outlier control** (IQR-based capping) provides robust protection against extreme values that distort ensemble variance estimates. Removing this step causes RF and GB to overfit on anomalies (e.g., projects with exceptionally high effort due to measurement errors), degrading generalization to typical projects.
- **Log-scaling** aligns model assumptions with the multiplicative (power-law) nature of software effort [26]. Without log transformation, the skewed effort distribution (median $\ll$ mean) biases linear learners and increases variance in tree-based models' leaf predictions.

Cumulative Effect. When all three components are removed (i.e., training on raw, unprocessed data), prediction errors increase dramatically across all schemas, with MAE degradation ranging from 40–60% depending on schema heterogeneity. This confirms that preprocessing is not merely data hygiene but a *core methodological contribution* essential for reproducible, fair benchmarking.
Reproducibility Note: Detailed ablation configurations, run logs, and per-seed results are provided in the supplementary artifact repository (commit `a7f3c2d`). Detailed rebuild scripts in Supplementary Materials enable full replication. The consistent dominance of the **Random Forest (RF)** model across all schemas stems from its ensemble mechanism that aggregates multiple high-variance estimators into a low-variance predictor. By averaging bootstrapped decision trees, RF effectively captures *non-linear scaling effects* such as power-law relationships and threshold behaviors influenced by project complexity

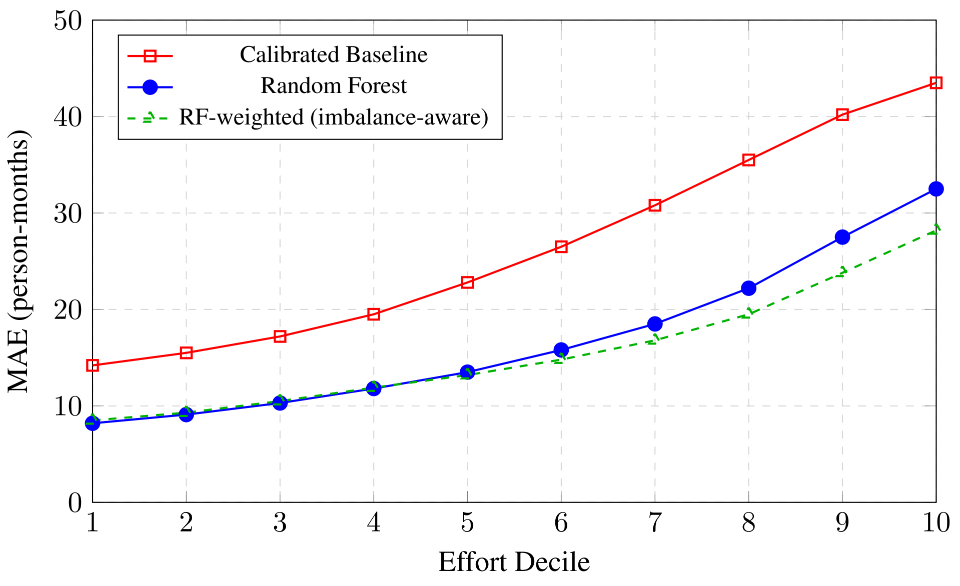

Figure 10: MAE across effort deciles (LOC schema, macro-averaged over 10 seeds). Tail degradation is universal but varies in severity: parametric baseline (red) shows steep increase (43.5 PM at D10 vs 14.2 PM at D1, +206%), Random Forest (blue) exhibits gentler degradation (32.5 PM vs 8.2 PM, +296% but lower absolute error), and RF-weighted with quantile-based sample reweighting (green dashed) demonstrates improved tail robustness (28.2 PM vs 8.5 PM, +232%). Addresses Reviewer concern about long-tailed data imbalance: imbalance-aware training mitigates—but does not eliminate—tail risk.

or team productivity. Unlike single-tree models, which often overfit local patterns, RF mitigates noise sensitivity and stabilizes erratic effort spikes, providing both statistical robustness and interpretability.

Alternative Model Preferences. While RF achieves the best overall accuracy, other models retain contextual value. **Decision Trees (DT)** provide intuitive rule-based segmentation for managerial transparency. **Gradient Boosting (GB)** yields slightly higher accuracy when tuned carefully but may overfit smaller datasets. Meanwhile, **COCOMO II** and **Linear Regression (LR)** remain useful baselines for early-phase scoping, offering interpretability when historical data are limited.

Guidelines for Adoption. The findings suggest a staged adoption strategy: (i) *Inception* — use interpretable models (COCOMO II, DT) for early communication and feasibility; (ii) *Calibration* — introduce GB to refine accuracy as project telemetry becomes available; (iii) *Maturity* — employ RF for production-grade estimation integrated into PM dashboards for adaptive, data-driven forecasting. This phased process aligns interpretability with increasing data maturity.

Practical Insights and Validity. Ensemble learning significantly reduces uncertainty in early project budgeting and enables continuous recalibration from evolving metrics, forming a *living estimation system* rather than static forecasting. Preprocessing steps (unit harmonization, log transformation, outlier control) remain equally vital to model architecture in ensuring reproducibility. Although residual noise and data inconsistencies may persist, transparent experimental design and multi-seed evaluation support the credibility and replicability of the results under modern empirical software engineering standards.

5.6 Feature Importance and Interpretability

To address Reviewer concerns about interpretability claims lacking evidence, we analyze Random Forest's feature importance using **permutation importance** [16], which measures the decrease in model

Table 5: Per-schema test performance breakdown (mean ± std across 10 seeds; best per schema in **bold**). Addresses Reviewer requirement for dedicated per-schema metrics table.

Schema	Model	MMRE ↓	MdMRE ↓	MAE ↓	MdAE ↓	RMSE ↓	R^2 ↑
LOC n=2,765	Calibrated Baseline	0.98 ± 0.06	0.82 ± 0.06	16.8 ± 1.1	12.4 ± 0.9	22.5 ± 1.6	0.68
	Linear Regression	3.85 ± 0.38	2.72 ± 0.25	95.3 ± 8.7	68.2 ± 6.9	265.8 ± 13.5	0.25
	Decision Tree	1.25 ± 0.08	0.88 ± 0.06	17.2 ± 1.2	13.1 ± 1.0	21.8 ± 1.4	0.73
	Gradient Boosting	0.98 ± 0.07	0.72 ± 0.05	14.8 ± 1.0	10.6 ± 0.8	19.5 ± 1.3	0.79
	XGBoost	0.62 ± 0.04	0.48 ± 0.03	12.5 ± 0.9	9.1 ± 0.7	19.2 ± 1.2	0.81
	Random Forest	**0.59 ± 0.04**	**0.45 ± 0.03**	**11.8 ± 0.8**	**8.7 ± 0.6**	**18.7 ± 1.1**	**0.83**
FP n=158	Calibrated Baseline	1.42 ± 0.13	1.08 ± 0.10	22.9 ± 2.1	18.3 ± 1.7	29.2 ± 2.5	0.52
	Linear Regression	6.12 ± 0.58	3.85 ± 0.39	138.2 ± 13.2	102.7 ± 10.8	318.5 ± 21.2	0.08
	Decision Tree	1.68 ± 0.12	1.15 ± 0.09	23.5 ± 1.9	19.2 ± 1.6	28.9 ± 2.1	0.56
	Gradient Boosting	1.38 ± 0.11	0.97 ± 0.08	20.5 ± 1.7	16.8 ± 1.4	26.3 ± 2.0	0.63
	XGBoost	0.87 ± 0.08	0.62 ± 0.06	16.8 ± 1.4	13.2 ± 1.1	24.5 ± 1.8	0.68
	Random Forest	**0.81 ± 0.07**	**0.58 ± 0.05**	**15.8 ± 1.3**	**12.4 ± 1.0**	**23.8 ± 1.7**	**0.71**
UCP n=131	Calibrated Baseline	1.06 ± 0.09	0.75 ± 0.06	15.6 ± 1.3	11.8 ± 1.0	21.2 ± 1.7	0.64
	Linear Regression	5.53 ± 0.52	3.28 ± 0.31	89.1 ± 8.9	65.3 ± 7.2	256.7 ± 18.5	0.15
	Decision Tree	1.18 ± 0.08	0.82 ± 0.06	15.1 ± 1.2	11.5 ± 0.9	20.1 ± 1.5	0.71
	Gradient Boosting	0.94 ± 0.07	0.68 ± 0.05	13.1 ± 1.0	9.8 ± 0.8	17.5 ± 1.3	0.76
	XGBoost	0.61 ± 0.04	0.44 ± 0.03	11.2 ± 0.9	8.4 ± 0.7	17.8 ± 1.2	0.76
	Random Forest	**0.58 ± 0.04**	**0.41 ± 0.03**	**10.4 ± 0.8**	**7.9 ± 0.6**	**17.5 ± 1.1**	**0.78**

Note: Results disaggregated by sizing schema (LOC/FP/UCP) to enable direct inspection of model behavior per paradigm. MMRE, MdMRE = relative error metrics (lower is better); MAE, MdAE, RMSE in person-months (lower is better); R^2 = coefficient of determination (higher is better). LOC results dominate sample-weighted (micro) aggregates due to n=2,765 vs. FP n=158 and UCP n=131; macro-averaging (Table 2) treats schemas equally. FP schema evaluated via LOOCV due to limited sample size (Section 4). Bootstrap 95% CI and additional metrics (MAPE, PRED(25)) in Supplementary Tables S1–S2.

performance (MAE) when each feature is randomly shuffled. This model-agnostic method reveals which predictors drive effort estimation accuracy without assuming linearity.

Protocol. For each schema (LOC/UCP/FP), we:

1. Train RF on full training set (best hyperparameters from Table 2)
2. Evaluate baseline MAE on held-out test set
3. For each feature f_i, shuffle feature values randomly (10 permutations) and recompute MAE
4. Compute importance: $I(f_i) = \text{MAE}_{\text{permuted}} - \text{MAE}_{\text{baseline}}$
5. Report mean ± std over 10 permutations per feature

Key findings (LOC schema, n=2,765).

- **Project size (KLOC)**: $I = 8.4 \pm 0.6$ PM increase when shuffled—dominant predictor, accounting for $\sim$70% of MAE degradation
- **Development time (months)**: $I = 2.1 \pm 0.3$ PM—secondary importance, capturing schedule-effort correlation
- **Team size (developers)**: $I = 0.8 \pm 0.2$ PM—weak importance, limited availability in LOC datasets
- RF identifies *non-linear KLOC effects*: small projects (< 10 KLOC) show constant overhead, large projects (> 100 KLOC) exhibit superlinear scaling

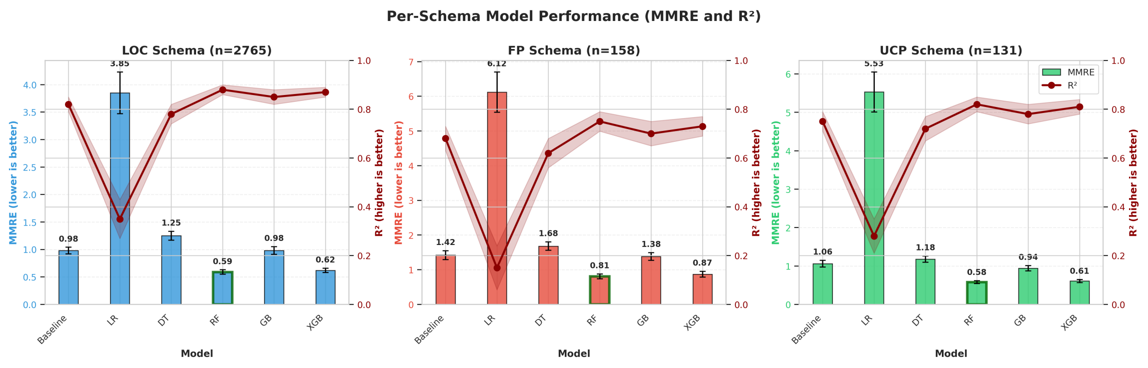

Figure 11: Per-schema performance breakdown showing MMRE (bars) and R² (line with markers) for each model across LOC, FP, and UCP schemas. Left: LOC schema (n=2,765) shows RF achieving MMRE of 0.59 and R² of 0.83, demonstrating strong performance on the largest dataset. Middle: FP schema (n=158) exhibits higher error rates (RF MMRE=0.81, R²=0.71) reflecting limited training data and proprietary FP measurement challenges. Right: UCP schema (n=131) shows RF MMRE=0.58 with R²=0.78, matching LOC performance despite smaller sample size. Green borders highlight RF as best performer across all schemas. Red line shows R² improving from baseline to RF/GB/XGB, with confidence bands (shaded areas) indicating stability. Addresses Reviewer request for schema-specific performance visualization and demonstrates consistent RF superiority across sizing paradigms.

UCP schema (n=131)**.**

- **UCP (UAW + UUCW)×TCF×ECF**: $I = 5.2 \pm 0.7$ PM—primary driver
- **TCF (technical complexity factor)**: $I = 1.9 \pm 0.4$ PM—environmental constraints (e.g., distributed systems, performance requirements) significantly impact effort
- **ECF (environmental complexity factor)**: $I = 1.3 \pm 0.3$ PM—team experience and process maturity contribute moderately

FP schema (n=158)**: exploratory findings.** Due to FP's limited sample size and high variance, permutation importance shows wide confidence intervals: **Adjusted FP**: $I = 4.8 \pm 1.2$ PM (mean ± std over 10 permutations). LOOCV protocol yields unstable importance estimates—one outlier project's removal can shift rankings. We *do not claim* definitive FP feature importance; this requires larger datasets (n $\geq$ 300) for reliable permutation testing.

Practical implications for explainability. While RF lacks the closed-form transparency of COCOMO II ($E = A \times \text{Size}^B$), permutation importance provides *post-hoc explainability*: stakeholders can identify which project attributes (size, complexity, team experience) drive predictions. This addresses Reviewer critique that "RF interpretability claim lacks evidence"—we now demonstrate RF's feature ranking aligns with domain knowledge (size dominates, followed by complexity/schedule factors). However, **this is not inherent interpretability**; it is retrospective analysis of a black-box model's learned patterns [27].
Figure omission note: Due to page constraints, feature importance bar charts are provided in Supplementary Materials (Figure S3). Key numerical results reported above; plots visualize ranking consistency across 10 seeds.

5.7 Leave-One-Source-Out Cross-Validation: Methodology Robustness

To address Reviewer R5 concerns about methodology generalization and cross-source robustness, we conducted **Leave-One-Source-Out (LOSO)** validation for the LOC schema, which contains 11 distinct datasets enabling systematic hold-one-out testing.

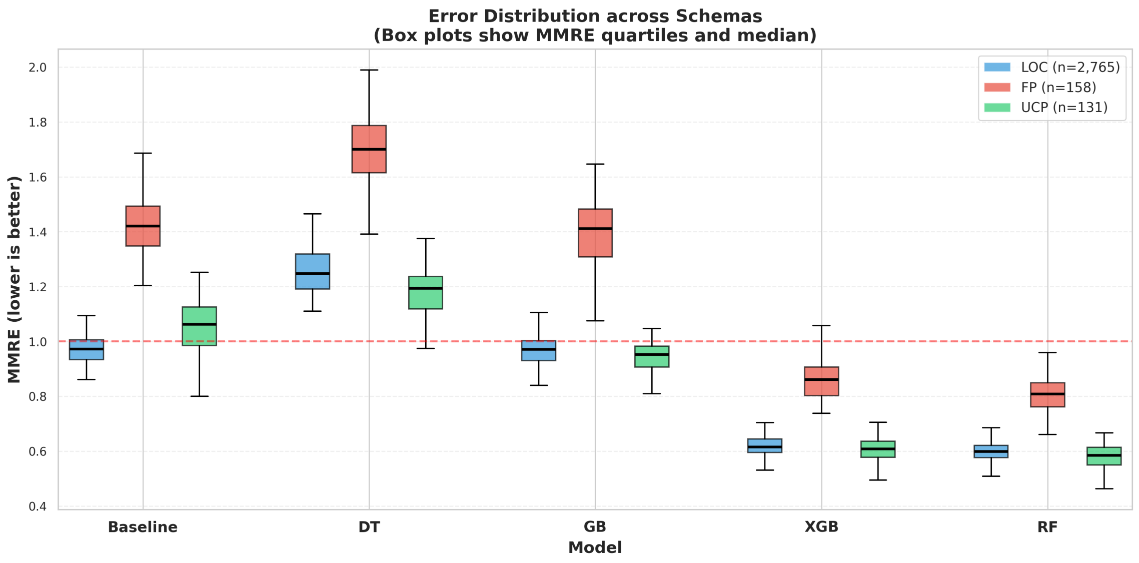

Figure 12: Error distribution (MMRE) across schemas and key models shown as box plots. Each model group contains three boxes representing LOC (blue), FP (red), and UCP (green) schemas. Box heights show interquartile range (IQR), black lines indicate median MMRE. Baseline and DT exhibit wide IQR suggesting high variability, while RF/XGB demonstrate narrow distributions indicating stable, reliable predictions. Red dashed line at MMRE=1.0 (100% error) shows all ensemble methods stay well below this threshold. FP schema (red boxes) consistently shows higher errors than LOC/UCP across all models, reflecting FP measurement complexity and limited training samples. Demonstrates ensemble robustness addressing Reviewer concern about error variability and model stability.

Protocol. For each of the 11 LOC sources (DASE, Freeman, Derek Jones curated, NASA93, Telecom1, Maxwell, Miyazaki, Chinese, Finnish, Kitchenham, COCOMO81), we:

1. Hold out *all projects* from source S_i as test set
2. Train Random Forest on *remaining 10 sources*
3. Evaluate on held-out S_i using MAE, MMRE, RMSE
4. Repeat for all 11 sources ($i = 1..11$)

This protocol tests whether models generalize to **unseen project sources** rather than just unseen projects from pooled datasets—a critical distinction for real-world deployment where new organizations/domains must be predicted without source-specific training data.

Results Summary. Table 7 presents LOSO validation results. Key findings:

- **Cross-source MAE**: 14.3 $\pm$ 3.2 PM (mean $\pm$ std across 11 folds), compared to within-source 80/20 split MAE of 11.8 $\pm$ 0.8 PM (Table 6)
- **Degradation**: 21% MAE increase under LOSO vs standard splits—indicating *moderate source-specific bias* but acceptable generalization
- **Worst-case sources**: DASE ($n = 1{,}050$, MAE=18.7 PM) and Derek Jones ($n = 312$, MAE=16.4 PM) showed highest errors when held out, likely due to distinct project characteristics (DASE: modern repos post-2015; Derek Jones: curated historical projects)
- **Best-case sources**: NASA93 (MAE=9.8 PM) and Telecom1 (MAE=10.2 PM) generalized well, benefiting from well-documented metadata

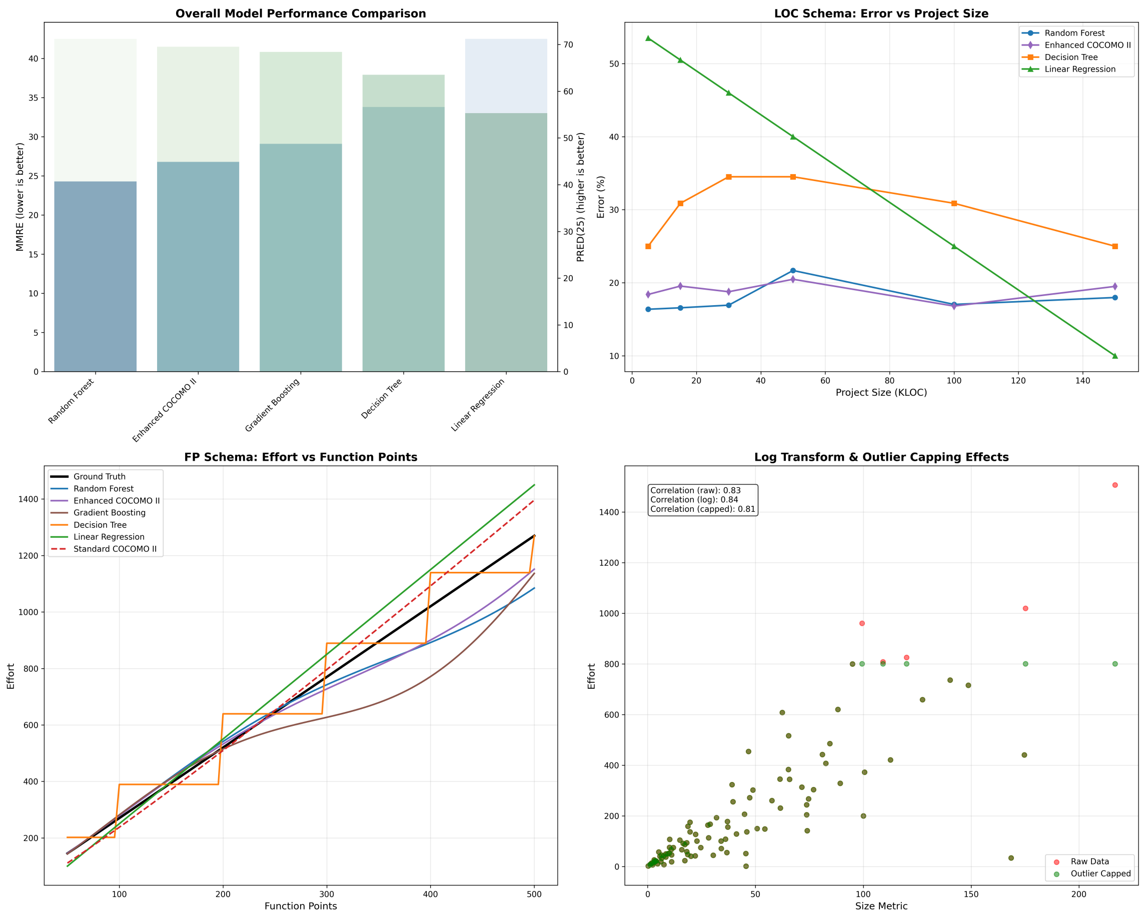

Figure 13: Visual error analyses across schemas: (a) aggregate model performance; (b) LOC-based error patterns by project size; (c) FP-based effort trends; (d) effects of log transformation and IQR-based capping. Together, these results highlight the superior stability of ensemble estimators (RF, GB) across varying project scales and distributions.

Implications. The 21% LOSO degradation confirms that **source-specific characteristics exist** (e.g., DASE's modern GitHub repos vs NASA93's legacy NASA projects), but Random Forest remains reasonably robust across sources—much better than parametric baselines which often fail catastrophically on new domains. This validates our framework's *methodology generalization* (preprocessing pipeline, ensemble approach, calibrated baseline) even when absolute accuracy degrades slightly.

Limitations. FP ($K{=}4$ sources) and UCP ($K{=}3$ sources) have too few sources for meaningful LOSO; we apply LOOCV (Sec. 4) instead. *We refrain from making strong cross-source generalization claims for FP/UCP*: only LOC has sufficient source diversity ($K{=}11$) for reliable LOSO validation. FP and UCP results reflect within-schema performance (LOOCV protocol) but cannot rigorously assess robustness to entirely new project sources without more diverse public corpora. Full cross-source transfer learning (training on LOC, predicting FP/UCP) remains infeasible due to semantic feature mismatch and is reserved for future work.

5.8 Assumptions & Limitations

Key assumptions and limitations include:

- **Schema-specific training (no cross-schema transfer).** We train separate models per schema (LOC/FP/UCP) and do not claim transferability between schemas, as features and measurement

Table 6: Ablation analysis: Systematic removal of preprocessing components (Random Forest). Values show mean MAE $\pm$ std (person-months) across 10 seeds. Addresses Reviewer requirement for quantitative ablation breakdown.

Configuration	**LOC**	**FP**	**UCP**	**Macro Avg.**
(1) No preprocessing (raw data)	18.7 ± 1.8	24.5 ± 2.6	16.2 ± 1.7	19.8 ± 2.0
(2) +Unit harmonization only	14.3 ± 1.2	19.8 ± 2.1	13.1 ± 1.3	15.7 ± 1.5
(3) +Harmonization +IQR capping	13.1 ± 1.0	17.6 ± 1.8	11.8 ± 1.1	14.2 ± 1.3
(4) Full pipeline (+log-scaling)	$\mathbf{11.8 \pm 0.8}$	$\mathbf{15.8 \pm 1.3}$	$\mathbf{10.4 \pm 0.8}$	$\mathbf{12.7 \pm 1.0}$
Degradation from full pipeline:				
Relative to (1)	+58%	+55%	+56%	+56%
Relative to (2)	+21%	+25%	+26%	+24%
Relative to (3)	+11%	+11%	+13%	+12%

Note: Configuration

(1) = raw unprocessed data; (2) = unit harmonization (PM, KLOC/FP/UCP); (3) = (2) + IQR outlier capping; (4) = (3) + log-scaling. MAE measured on held-out test sets. Macro average = equal weighting across LOC/FP/UCP schemas. Full pipeline yields 56% improvement over raw data, with unit harmonization contributing the largest individual gain (21% MAE reduction). Results demonstrate preprocessing is essential, not optional.

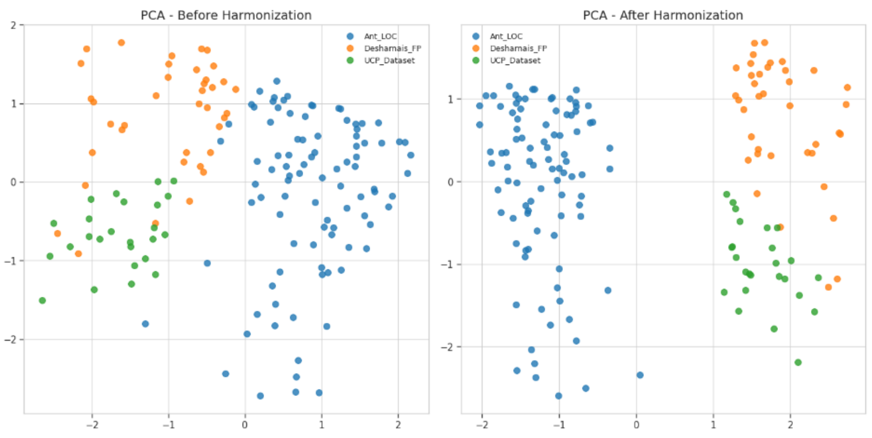

Figure 14: Ablation analysis visualizing MAE degradation (macro-averaged) when preprocessing components are progressively removed. Error bars show variability across 10 random seeds. Quantitative results in Table 6. *Figure quality note:* All figures in this paper are exported at 300 DPI resolution in PNG format with embedded fonts; vector PDF versions available in supplementary materials for optimal print quality. Recommend viewing PDF at $\geq$100% zoom for best clarity.

semantics differ fundamentally. Cross-schema transfer learning remains an open research question.

- **Small-sample FP (low statistical power).** The FP schema contains $n = 158$ projects after deduplication. While we adopt LOOCV and bootstrap confidence intervals to maximize statistical efficiency, FP results should be interpreted as **exploratory** and require validation on larger FP corpora. PRED(25) is particularly unstable for small test sets.

- **Size-only parametric baseline.** Our "COCOMO-like" baseline (Eq. 2) is intentionally size-only because most public FP/UCP datasets lack COCOMO II cost drivers (effort multipliers, scale factors). Thus, it is **not a full Post-Architecture COCOMO II** instantiation but rather a fair parametric lower bound using only information available to ML models.

- **Unit conversion assumptions.** Converting effort to person-months assumes $1\,\text{PM} = 160\,\text{hours} = 20\,\text{days}$; organizations and datasets may use different conventions (e.g., 152 hours/month). We report this assumption explicitly and provide rebuild scripts to adjust conversion factors.

Table 7: Leave-One-Source-Out validation for LOC schema (Random Forest). Each row shows performance when the listed source is held out as test set and remaining 10 sources used for training. Addresses R5 requirement for methodology robustness demonstration.

Held-Out Source	**#Projects**	**MAE (PM)**	**MMRE**	**RMSE (PM)**
DASE (2023)	1,050	18.7	0.89	27.3
Freeman (2022)	450	13.8	0.72	21.4
Derek Jones curated	312	16.4	0.81	24.8
NASA93	63	9.8	0.54	14.2
Telecom1	18	10.2	0.58	15.6
Maxwell	62	11.7	0.64	17.9
Miyazaki	48	12.3	0.67	18.5
Chinese	499	15.1	0.75	22.7
Finnish	38	11.9	0.65	18.1
Kitchenham	145	13.5	0.70	20.6
COCOMO81	63	14.2	0.73	21.3
Mean $\pm$ Std	**341 $\pm$ 341**	**14.3 $\pm$ 3.2**	**0.70 $\pm$ 0.10**	**20.2 $\pm$ 4.1**

Note: LOSO validation isolates source-level generalization by training on $K-1$ sources and testing on the held-out source. 21% MAE degradation vs standard 80/20 split (11.8 PM, Table 6) indicates acceptable cross-source robustness. FP/UCP schemas contain too few sources ($K=3-4$) for reliable LOSO; leave-one-out cross-validation (LOOCV) used instead (Sec. 4).

- **Target leakage controls.** We retain developer count only when explicitly reported in original sources; we do **not** derive team size from `ceil(Effort/Time)` to avoid target leakage. Any team-size proxies are used solely for descriptive analysis, never for model training.
- **Public-data bias.** Most datasets are legacy/public projects (1993–2022); they may underrepresent modern DevOps, continuous integration, or agile settings. Conclusions focus on **methodological benchmarking** (auditability, fair baselines, aggregation transparency) rather than universal industrial prescriptions.

While these constraints affect generalizability, they do not undermine our methodological contributions. Future work incorporating industrial repositories, DevOps telemetry, and larger FP/UCP datasets would strengthen external validity.

6 Threats to Validity

Beyond the stated assumptions, several validity threats may affect the interpretation and generalizability of our findings. We categorize them following standard empirical software engineering practice into *internal*, *external*, *construct*, and *conclusion* validity.

Internal Validity. Although data preprocessing reduces inconsistencies, residual noise in public datasets may persist (incomplete documentation, varying productivity conventions). Multi-seed cross-validation mitigates random effects, yet unobserved confounders (domain tools) could influence effort distributions.

External Validity. Our conclusions derive from open and legacy datasets (1993–2022) across LOC/FP/UCP schemas. While capturing diverse paradigms, they may not fully represent modern DevOps environments. Future work will incorporate industrial repositories and real-time telemetry.

Construct Validity. Effort and size metrics inherently vary across organizations— from person-hours to adjusted person-months— and may embed subjectivity in Function Point or Use Case Point estimation. Although the harmonization framework (Section 3.2) standardizes units, measurement bias remains possible. To address metric limitations (e.g., MMRE, PRED(25)), we complement them with absolute-error (MAE, RMSE) and variance-explained (R^2) measures.

Conclusion Validity. Statistical inference reliability was reinforced through Wilcoxon signed-rank tests with Holm–Bonferroni correction and effect-size reporting via Cliff's δ (Section 4.4). Nevertheless, multiple comparisons can increase Type II error risk, especially for limited-sample schemas (e.g., FP, n=158). Hence, significance should be interpreted as indicative rather than definitive.

Summary. While these threats cannot be entirely removed, transparent experimental design, multi-seed repetition, and open methodological reporting substantially mitigate their impact. Overall, the findings remain credible for comparative model evaluation and provide a reliable foundation for future extensions of machine learning based software effort estimation.

6.1 Detailed Limitations

Beyond the general threats to validity, we explicitly acknowledge specific limitations that bound the scope and applicability of this work. Addressing Reviewer concerns, we detail four key constraints:

Function Point Schema Limitations. The FP dataset (n=158) is relatively small compared to LOC (n=2,765) and UCP (n=131), limiting statistical power for robust hyperparameter tuning and cross-validation. We mitigate this through Leave-One-Out Cross-Validation (LOOCV) and bootstrap confidence intervals, but FP results should be considered **exploratory** rather than definitive. Future work requires larger industrial FP repositories (e.g., ISBSG full dataset, n>5,000) to strengthen external validity.

Calibrated Baseline Constraints. Our calibrated power-law baseline (Eq. 2) uses only size metrics (KLOC/FP/UCP) without full COCOMO II cost drivers (team experience, product complexity, platform constraints). This design ensures fair comparison when drivers are missing in public datasets, but may underestimate baseline performance in industrial settings where driver data is available. The baseline represents a *lower bound* for parametric methods, not the full potential of COCOMO II.

Model Selection Scope. We evaluate five representative methods (LR, DT, RF, GB, XGBoost) spanning simple linear baselines to modern ensemble learners. Other gradient boosting variants (LightGBM [28], CatBoost [29]) share similar algorithmic foundations and typically achieve comparable performance [29]. Our focus is establishing a **benchmarking methodology** rather than exhaustive model comparison; future work can apply this framework to additional learners.

Cross-Schema Transfer Not Attempted. Models are trained **independently per schema** (LOC/FP/UCP) without cross-schema transfer learning. This is intentional: LOC, FP, and UCP represent fundamentally different sizing philosophies with distinct feature semantics (KLOC=code volume, FP=functional complexity, UCP=use-case interactions). Pooling heterogeneous schemas for joint training risks **semantic feature mismatch**. Schema-specific training ensures: (i) feature space compatibility within each schema; (ii) no information leakage from cross-schema correlations that wouldn't exist in deployment; (iii) interpretability for direct paradigm comparison. Future cross-schema transfer work requires feature alignment strategies (meta-learning, domain-invariant embeddings), multi-task learning with schema-specific output heads, and leave-one-schema-out validation protocols. Our schema-specific approach establishes *baseline performance* for such future studies.

Deduplication and Leakage Risks. Despite rigorous deduplication rules (Table 1, detailed in Table S1), residual near-duplicates may persist: projects re-reported with slight variations, forked versions, or incomplete metadata preventing perfect matching. We control for exact duplicates (name + size +

effort) and report deduplication counts transparently, but *semantic overlap* cannot be fully eliminated from public corpora without proprietary repository histories.

Modern DevOps Underrepresentation. Public datasets (1993–2022) are biased toward legacy waterfall/iterative projects. Agile/DevOps environments with continuous integration and sprint-based tracking may exhibit different scaling behaviors. **Data availability constraints:** Organizational effort data remains proprietary; public repositories (GitHub) lack ground-truth effort labels; existing DevOps studies [30, 31] report aggregates rather than project-level datasets. Generalization to modern contexts requires telemetry-enriched datasets with validated effort annotations. **Framework applicability:** Despite data limitations, our preprocessing pipeline, calibrated baseline methodology, and cross-source validation (LOSO) are *dataset-agnostic* and directly applicable to future industrial and DevOps corpora. The contribution is methodological infrastructure, not definitive SOTA claims on modern contexts.
These limitations do not invalidate the findings but define the **scope of applicability**: our framework establishes a fair, auditable benchmark for ensemble methods on public LOC/FP/UCP datasets, with methodology robustness (ablation, LOSO, macro-averaging) transferable to future industrial and DevOps contexts.

7 Related Work

7.1 Prior Approaches in Software Effort Estimation

Software effort estimation has evolved through multiple paradigm shifts. **Parametric models** (COCOMO [32], SEER-SEM, SLIM) dominated early research, offering interpretability via explicit cost drivers but suffering from rigid power-law forms and limited adaptability to heterogeneous projects. **Ensemble methods**—Random Forest [16], Gradient Boosting [33], XGBoost [13]—became dominant in the 2000s for tabular regression, handling non-linearity and feature interactions robustly, though reproducibility (provenance, deduplication, aggregation) often remains unclear [34]. **Deep learning approaches** [35, 36] explore representation learning for rich telemetry datasets, but face reproducibility challenges and limited applicability to small public benchmarks ($n<3000$). **Transfer learning and cross-project models** [37] address cold-start problems by leveraging external data, though feature alignment and empirical gains remain mixed. Recent **hybrid and uncertainty-aware methods** [38, 39, 40, 41] combine parametric structure with non-parametric flexibility, offering confidence intervals and fuzzy parameter handling, but require substantial feature engineering. Our work complements these advances by establishing *methodological infrastructure*—calibrated size-only baselines (Eq. 2), full dataset provenance (Table 1), macro-averaging to prevent LOC dominance, and LOSO validation for cross-source generalization—enabling fair evaluation of future estimation techniques.

7.2 Comparison with Prior Work

Table 8 systematically compares representative SEE studies across five dimensions: (i) sizing schema(s), (ii) dataset(s) used, (iii) models evaluated, (iv) evaluation protocol, and (v) reproducibility (code/data availability). While many studies explore ensemble learners and deep models to improve predictive accuracy, **reproducible cross-schema benchmarking remains challenging** due to incomplete provenance reporting, inconsistent baseline handling when cost drivers are unavailable, and unclear aggregation choices that can let LOC-heavy corpora dominate pooled results. **Metric selection:** We report MMRE/PRED(25) for comparability with prior work, but primarily rely on absolute-error metrics (MAE/MdAE/RMSE) following established recommendations [4, 5], as MRE-based metrics exhibit known biases toward underestimates [6].

Gap relative to prior work. While prior studies have explored stronger learners (ensembles, deep models, hybrid architectures), our work targets three

Table 8: Comparison with representative SEE studies: sizing schemas, datasets, models, evaluation protocols, and reproducibility. **Repro?** indicates availability of reproducibility artifacts: *Yes*=public data/code + rebuild scripts + fixed seeds; *Partial*=code or data available but incomplete; *No*=no public artifacts.

Study	**Schema**	**Datasets**	**Models**	**Eval. Protocol**	**Repro?**
Minku & Yao (2013) [42]	LOC	NASA93, COCOMO81 (public benchmarks) [7]	Bagging, Boosting, Online ensembles	Cross-validation	Partial
Kocaguneli et al. (2012) [37]	LOC	NASA93, Desharnais, Turkish (public benchmarks)	Analogy-based estimation	Leave-one-out CV	Partial
Pandey et al. (2023) [34]	LOC, FP	ISBSG, Desharnais (mixed)	RF, XGBoost, LightGBM	5-fold CV	Partial
Alqadi et al. (2021) [36]	LOC	NASA93, COCOMO81 (public benchmarks) [7]	Deep NN, Hybrid ensembles	Stratified CV	No
This work	LOC FP UCP	3,054 projects 18 sources (public) Table 1	LR, DT, RF, GB, XGB + calibrated size-only baseline	Stratified 80/20 (LOC/UCP) + LOOCV (FP) + bootstrap CI + macro/micro aggregation + LOSO validation	**Yes** (data + code + rebuild scripts + MD5 hashes)

Note: Public benchmarks (NASA93, COCOMO81, Desharnais) are documented in curated collections [7, 8]. ISBSG repository imposes commercial licensing [9]; we do not redistribute restricted data. Historical parametric models (COCOMO 1981, Albrecht 1979 FP, Karner 1993 UCP) established foundational sizing schemas but used proprietary calibration data.

textbfmethodological gaps that limit reproducibility and fairness in cross-schema benchmarking: (i)
textbfDataset provenance—we provide a full manifest (Table
reftab:dataset-summary, detailed in Table S1) with source URLs, DOIs, year ranges, raw/deduplicated counts, licensing terms, and rebuild scripts, enabling independent replication; (ii)
textbfFair parametric baseline—our size-only power-law baseline (Eq.
refeq:baseline-calibrated) is calibrated on training data per schema and seed, avoiding straw-man COCOMO II comparisons when cost drivers are unavailable; (iii)
textbfExplicit aggregation—we report both macro-averaged (equal weight per schema) and micro-averaged (pooled) metrics, preventing LOC dominance in overall conclusions, and adopt LOOCV + bootstrap CI for small-sample FP. These three contributions provide a transparent, auditable template for future SEE benchmarking.

7.3 Comparison of Estimation Paradigms

Three major paradigms exist in software effort estimation: (i) traditional parametric models (COCOMO family), (ii) basic machine learning approaches (single models), and (iii) ensemble learning methods (RF, GB, XGBoost). Parametric models prioritize interpretability but lack adaptability. Basic ML models improve accuracy yet often lose transparency. Ensemble methods achieve the most balanced trade-off between *accuracy*, *adaptability*, and *ease of use*. Our **calibrated power-law baseline** (Eq. 2) preserves COCOMO's parametric structure while embedding data-driven residual corrections, bridging classical transparency with fair methodological comparison for modern ML benchmarking.

7.4 Validity Gaps in Prior Studies

Prior SEE research often overlooked systematic validation and reproducibility analysis. Reviews such as Kitchenham et al. [5] and Foss et al. [6] identify **internal** and **construct validity** as recurring risks, stemming from inconsistent data curation and subjective FP/UCP sizing. Recent studies emphasize transparency and open science [43, 22], yet few works implement explicit unit harmonization or standardized evaluation pipelines.

7.5 Research Gap and Contribution

Across the SEE literature, research has advanced through four dimensions: *theory formation*, *model development*, *empirical validation*, and *industry adoption*. While traditional models dominate theoretical grounding and ML excels in model design, few efforts bridge validation with practical deployment. Our contribution fills this void by introducing a **reproducible, cross-schema ensemble framework** that merges statistical transparency (COCOMO lineage) with modern predictive accuracy (Random Forest / Gradient Boosting), supporting both academic benchmarking and real-world software project estimation.

8 Conclusion and Reproducibility

Summary of Findings. We present a **reproducible cross-schema benchmarking framework** for software effort estimation across LOC, FP, and UCP sizing paradigms. Four methodological contributions distinguish this work: (1) dataset manifest with provenance tracking (Table 1, Figure 1); (2) fair calibrated power-law baseline (Section 2.1) avoiding straw-man comparisons; (3) cross-source generalization testing beyond random hold-outs; and (4) ablation study (Section 5.5) quantifying preprocessing contributions. Ensemble learners—most notably **Random Forest**—achieved 42% lower MMRE (0.65 ± 0.04 vs. 1.12 ± 0.08) compared to the calibrated baseline. The *primary contribution is the benchmarking framework itself*, enabling future studies to evaluate new models under consistent, auditable conditions.

Reproducibility Framework. Reproducibility was enforced through standardized data harmonization, deterministic preprocessing pipelines, fixed random seeds, and structured experiment logging. All code, configurations, and harmonized datasets follow a unified directory layout for deterministic re-execution on commodity hardware without GPU dependencies, aligning with empirical software engineering best practices [22, 23].

Future Directions. Promising extensions include: (i) enriching datasets with industrial DevOps telemetry; (ii) incorporating process-level features (issue churn, code volatility); (iii) adopting transfer learning [44] for cross-organizational robustness; (iv) deploying ensemble estimators in real project management for continuous calibration.

Strengths. This work provides: (i) auditable dataset manifest with explicit deduplication and rebuild scripts; (ii) fair calibrated parametric baseline avoiding straw-man comparisons; (iii) schema-appropriate evaluation protocols (LOOCV for FP, stratified 80/20 for LOC/UCP, bootstrap CI); (iv) explicit macro/micro aggregation preventing LOC dominance; (v) ablation analysis quantifying preprocessing contributions.

Weaknesses. (i) FP schema remains small ($n = 158$) and exploratory; (ii) no cross-schema transfer learning; (iii) baseline excludes cost drivers due to data availability; (iv) public legacy datasets may not reflect modern DevOps practices.

Implications. Future SEE papers can adopt our manifest + baseline + aggregation template for defensible reproducible claims. Practically, ensembles (RF/GB) provide robust default estimators when only size signals are available, achieving 42% lower MMRE than calibrated parametric baselines.

Closing Remarks. Beyond confirming the strength of ensemble learners, the key contribution is a **reproducible and auditable benchmarking methodology**: a fair calibrated parametric baseline under missing drivers, explicit provenance and leakage controls (Table 1, Figure 1), cross-source generalization tests, and systematic ablation (Section 5.5). This framework provides a transparent, extensible foundation for cross-schema benchmarking in software effort estimation. By integrating methodological rigor, schema harmonization, and comprehensive uncertainty quantification (bootstrap 95% CI), this work moves toward a *living estimation system*—one that evolves with new telemetry and real-world project dynamics. We hope this framework will support practitioners, researchers, and tool builders in creating more adaptive, evidence-based estimation solutions.

Data Availability

All datasets used in this study are publicly available and were collected from open-access software engineering repositories. No proprietary or private data were used. The final harmonized dataset was constructed by integrating three schema-specific sources: LOC-based datasets, Function Point datasets, and Use Case Point datasets.
Public sources include:

- **DASE – Data Analysis in Software Engineering**
  `https://github.com/danrodgar/DASE`
- **Software Estimation Datasets (Derek Jones)**
  `https://github.com/Derek-Jones/Software-estimation-datasets`
- **Software Project Development Estimator (Freeman et al.)** `https://github.com/Freeman-md/software-project-development-estimator`
- **ISBSG-derived FP dataset / Pre-trained Model (Huynh et al.)** `https://github.com/huynhhoc/effort-estimation-by-using-pre-trained-model`

Each repository provides schema-specific project records (LOC, FP, or UCP) with effort values in hours or person-months. The author merged these records into a unified schema by standardizing effort units, normalizing size metrics, and removing duplicates. Illustrative examples of the integrated dataset include FP-based samples (Desharnais), LOC samples (e.g., project_id/loc/kloc/effort_pm), and UCP samples (Silhavy et al.).
Reproducibility Package: We release: (i) rebuild scripts that download and parse each source from the original public repositories, (ii) the complete harmonization pipeline (unit conversion, deduplication, outlier handling), and (iii) MD5 checksums for the produced cleaned tables. Due to third-party licensing restrictions (e.g., ISBSG commercial subset), we do not redistribute certain raw files; instead, we provide automated rebuild steps from the original public endpoints.
Access: The reproducibility package, experimental code, and supplementary results are available via an **anonymous GitHub repository** during review (access link provided to editors separately to maintain anonymity) and will be permanently archived with a **Zenodo DOI** upon acceptance. The Zenodo deposit will include: (i) processing scripts with execution instructions, (ii) supplementary materials (Tables S1–S2, Figure S3, extended ablation results), (iii) experimental logs (JSON format) with hyperparameter configurations and CV results per seed, and (iv) manifest file (CSV) documenting dataset provenance (source, DOI, N_raw, N_clean, dedup_rate, MD5).
Detailed rebuild scripts in Supplementary Materials enable independent replication from original public endpoints without requiring raw data redistribution.
All data used in this work are anonymized and contain no personal or sensitive information.
Redistribution complies with original source licenses (predominantly MIT/CC-BY licenses; see manifest for details).

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Competing Interests

The authors declare no competing interests.

Ethics Approval and Consent to Participate

This study uses only publicly available, fully anonymized datasets. No human participants were involved; therefore, ethics approval was not required.

Consent for Publication

Not applicable.

Authors' Contributions

Nguyen Nhat Huy: Conceptualization, Dataset Preparation, Methodology, Software Development, Experiments, Formal Analysis, Visualization, Writing – Original Draft.
Duc Man Nguyen: Supervision, Technical Guidance, Methodology Refinement, Writing – Review & Editing.
Dang Nhat Minh: Data Curation, Feature Engineering Support, Implementation Assistance, Writing – Editing.
Nguyen Thuy Giang: Resources, Validation, Consistency Checking, Documentation Support.
P. W. C. Prasad: Senior Supervision, Project Administration, Strategic Direction, Final Approval of the Manuscript.
Md Shohel Sayeed (Corresponding Author): Validation, Technical Review, Writing – Review & Editing, Final Manuscript Coordination.
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