The Effectiveness of Supervised Machine Learning Algorithms in Predicting Software Refactoring

Mauricio Aniche*, Erick Maziero†, Rafael Durelli‡, Vinicius H. S. Durelli†
*Delft University of Technology - The Netherlands
†Federal University of Lavras - Brazil
‡Federal University of São João del Rei - Brazil
M.F.Aniche@tudelft.nl, Erick.Maziero@ufla.br, Rafael.Durelli@ufla.br, durelli@ufsjs.edu.br

Abstract—Refactoring is the process of changing the internal structure of software to improve its quality without modifying its external behavior. Empirical studies have repeatedly shown that refactoring has a positive impact on the understandability and maintainability of software systems. However, before carrying out refactoring activities, developers need to identify refactoring opportunities. Currently, refactoring opportunity identification heavily relies on developers’ expertise and intuition. In this paper, we investigate the effectiveness of machine learning algorithms in predicting software refactorings. More specifically, we train six different machine learning algorithms (i.e., Logistic Regression, Naive Bayes, Support Vector Machines, Decision Trees, Random Forest, and Neural Network) with a dataset comprising over two million refactorings from 11,149 real-world projects from the Apache, F-Droid, and GitHub ecosystems. The resulting models predict 20 different refactorings at class, method, and variable-levels with an accuracy often higher than 90%. Our results show that (i) Random Forests are the best models for predicting software refactoring, (ii) process and ownership metrics seem to play a crucial role in the creation of better models, and (iii) models generalize well in different contexts.

Index Terms—software engineering, software refactoring, machine learning for software engineering.

1 INTRODUCTION

Refactoring, as defined by Fowler [1], is “the process of changing a software system in such a way that does not alter the external behavior of the code yet improves its internal structure”. Over the years, empirical studies have established a positive correlation between refactoring operations and code quality metrics (e.g., [2], [3], [4], [5], [6]). All these evidence indicates that refactoring should be regarded as a first-class concern of software developers.

However, deciding when and what (as well as understanding why) to refactor have long posed a challenge to developers. Software development teams should not simply refactor their software systems at will, or decide not to refactor a piece of code that causes technical debt, as any refactoring activity comes with costs [7, 8].

Besides all the effort our community has made in providing tools that automatically refactor source code (e.g., JDeodorant [9]), identifying refactoring opportunities (i.e., identifying which classes or methods need to be refactored) is an important stage that precedes the refactoring process. We argue that the task of identifying relevant refactoring opportunities, which currently heavily relies on developers’ expertise and intuition, should be supported by sophisticated recommendation algorithms. After all, modeling all the context that a developer faces when deciding what to refactor, is a complex problem.

Researchers have been indeed experimenting with different techniques to recommend refactoring, e.g., code smells detection strategies [10], invariant mining [3], logic meta programming [11], search-based [12, 13], and pattern mining [14]. In this paper, we explore how machine learning (ML) can be harnessed to predict refactoring operations. ML algorithms have been showing promising results when applied to different areas of software engineering, such as defect prediction [15], code comprehension [16], and code smells [17].

We formulate the prediction of refactoring opportunities as a binary classification problem. More formally, given a set $R$ of possible refactorings for a source code element (class, method, and variables; the full list of refactoring operations can be found in Table 2), we learn a model $M_r(e)$, $r \in R$, that predicts whether a source code element $e$ should be refactored by means of refactoring operation $r$.

To probe into the effectiveness of supervised machine learning algorithms in predicting refactoring opportunities, we apply six machine learning algorithms (i.e., Logistic Regression, Naive Bayes, Support Vector Machines, Decision Trees, Random Forest, and Neural Network) to a dataset containing more than two million labelled refactoring operations that happened in 11,149 open-source projects from the Apache, F-Droid, and GitHub ecosystems. The resulting models are able to predict 20 different refactoring operations at class, method, and variable-levels, with an average accuracy often higher than 90%.

Understanding the effectiveness of the different models is the first and necessary step in building tools that will help developers in drawing data-informed refactoring
decisions. This paper provides the first solid large-scale evidence that ML techniques can model the refactoring recommendation problem accurately.

In summary, this paper makes the following contributions:

(i) A large-scale in-depth study of the effectiveness of different supervised machine learning algorithms to predict software refactoring, showing that ML methods can accurately model the refactoring recommendation problem.

(ii) A dataset containing more than two million real-world refactorings from more than 11k projects of F-Droid, GitHub, and Apache ecosystems.

(iii) A future research agenda to further explore the use of machine learning to recommend software refactorings.

2 Research Methodology

The goal of this paper is to evaluate the feasibility of using supervised machine learning approaches to identify refactoring opportunities. To this end, we framed our research around the following research questions (RQs):

RQ1: How accurate are supervised machine learning approaches in predicting software refactoring? In practice, some prediction models perform better than others, depending on the task. In this RQ, we explore how accurate different supervised machine learning algorithms (i.e., SVM, Naive Bayes, Decision Trees, Random Forest, and Neural networks) are in predicting refactoring opportunities at different levels (i.e., refactorings at class, method, and variable-levels), using the Logistic Regression as a baseline for comparison.

RQ2: What are the important features in the refactoring prediction models? Features play a pivotal role in the quality of the obtained models. In RQ1, we build the models using all the features we had available (for a method-level refactoring, for example, we use 58 different features). In this RQ, we explore which features are considered the most relevant by the models. Such knowledge is essential because, in practice, models should be as simple as and require as little data as possible.

RQ3: Can the predictive models be carried over to different contexts? Understanding whether refactoring prediction models should be trained specifically for a context or whether a generic refactoring prediction model is accurate enough in different contexts can significantly reduce the cost of applying these models in practice. We set out to study whether prediction models, devised in one context (e.g., the Apache ecosystem), are able to generalize to different contexts.

3.1 Experimental Sample

We selected a very large and representative set of Java projects from three different sources:

- The Apache Software Foundation (ASF) is a non-profit organization that supports all Apache software projects. The ASF is responsible for projects such as Tomcat, Maven, and Ant. Our tools successfully processed 844 out of their 860 Java-based projects. We discuss why the processing of some projects have failed in Section 2.6.
- F-Droid is a software repository of Android mobile apps. The repository contains only free software apps. Our tools successfully processed 1,233 of their 1,352 projects.
- GitHub provides free hosting for open source projects. GitHub has been extensively used by the open source community. As of May 2019, GitHub has 37 million users registered. We collected the first 10,000 most starred Java projects. Note that ASF and F-Droid projects might also exist in GitHub; we removed duplicates. In the end, our tools were able to process 9,072 projects.

The three different sources of projects provide the dataset with high variability in terms of size and complexity of projects, domains and technologies used, and community.
The resulting sample can be seen in Table 1. It comprises the 11,149 projects (844 from Apache, 1,233 from F-Droid, and 9,072 from GitHub). These projects together a history of 8.8 million commits, measured at the moment of data collection, in March of 2019.

### 2.2 Extraction of Refactoring Instances

In a nutshell, our data collection process happens in three phases. In the first phase, the tool clones the software repository, uses RefactoringMiner [18] to collect refactoring operations that happened throughout the history of the repository, and collects the code metrics of the refactored classes. In the second phase, where all the refactoring operations and their respective files are already known, the tool then collects the process and ownership metrics of the refactored classes. Finally, the tool collects instances of non-refactored classes (as well as their code, process, and ownership metrics).

After cloning repositories, the history of each repository is analyzed using RefactoringMiner [18]. RefactoringMiner is the current state-of-the-art tool to identify refactorings applied throughout the history of a Java project. Currently, RefactoringMiner has the highest recall and precision rates (98% and 87%, respectively) among all currently available refactoring detection tools [18].

To better illustrate this step, let $C_p = \{c_1, c_2, \ldots, c_n\}$ be the set of commits for a given cloned project $p$. Also, let $J_c = \{j_1, j_2, \ldots, j_n\}$ be the set of modified Java files in commit $c$. For each commit in $C$ (i.e., $\forall c_i, c_i \in C_p$), RefactoringMiner goes over all Java files modified in $i$ (i.e., $\forall j_i, j_i \in J_c$) and identifies whether a refactoring (or refactorings) have happened in that file. Note that a refactoring can happen at three different levels: class, method, or variable-level.

We collect the information at the precise level of the refactoring (e.g., if a method is refactored, we collect the class name and the method that was refactored). To each identified refactoring, our tool extracts code metrics of the refactored element in its version before the refactoring had been applied. Our approach ignores test code (e.g., JUnit
files) and only captures refactoring operations in production files. Test code quality has been the target of many studies (e.g., [19][20][21]). In this work, we assume that refactorings that happen in test code are naturally different from the ones that happen in production code; our future agenda includes the development of refactoring models for test code.

After all the refactorings were identified, our tool collects the process and ownership metrics of the refactored classes. Note that these metrics are also collected at the version before the refactoring had been applied. Finally, our tool collects instances of non-refactored classes. We consider a class to be a non-refactoring instance if and only if that class was modified (i.e., a change committed in the Git repository) precisely \( k \) times without a single refactoring operation being applied in between this time. After manual experimentation, we set \( k = 50 \) (we discuss the influence of \( k \) in Section 4.1). The tool, therefore, collects all classes that were modified precisely \( k \) times and did not go through any refactoring operation. We then extract its source code, process, and ownership metrics. The same element can appear more than once in this dataset (although always with different metric values), as whenever we collect an instance of non-refactoring, we restart its counter and continue to visit the repository.

In Table 3, we show the number of refactored and non-refactored instances we collected per dataset. We highlight the fact that the number of instances varies per refactoring, a reflex of how much developers apply each of these refactorings. For example, the dataset contains around 327k instances of Extract Method, but only 654 instances of Move and Rename Class. We see this as a positive point to our exploration, as the model will have to deal with refactoring where the number of instances is not high.

### 2.3 Feature Selection

We extract source code, process, and ownership metrics of all refactored and non-refactored instances. These three types of metrics have been proven useful in other prediction models in software engineering (e.g., [22][23][13]). Besides, as aforementioned, empirical studies over the years have established a positive correlation between refactoring operations and code quality metrics (e.g., [3][4][5][6]). Table 4 lists all the metrics we chose to train predictive models. In our online appendix [24], we show the distribution (i.e., descriptive statistics) of the values of each feature. The following subsections detail the source code, process, and code ownership metrics we collect.

#### Source Code Metrics

Features in this category are derived from source code attributes. We collect CK metrics [25] as they express the complexity of the element. More specifically, CBO, WMC, RFC, and LCOM. We also collect several different attributes of the element, e.g., number of fields, number of loops, number of return statements. These metrics are collected at class (38 metrics), method (20 metrics), and variable-levels (1 metric).

#### Process Metrics

Process metrics have been proven useful in defect prediction algorithms [26][27]. We collect five different process metrics: quantity of commits, the sum of lines added and removed, number of bug fixes, and number of previous refactoring operations. The number of bug fixes is calculated

| Class-level refactorings | All | Apache | GitHub | F-Droid |
|--------------------------|-----|--------|--------|---------|
| Extract Class | 41,191 | 6,658 | 31,729 | 2,804 |
| Extract Interface | 10,495 | 2,363 | 7,775 | 357 |
| Extract Subclass | 6,436 | 1,302 | 4,929 | 205 |
| Extract Superclass | 26,814 | 5,228 | 20,027 | 1,559 |
| Move And Rename | 654 | 87 | 545 | 22 |
| Class | 49,815 | 16,413 | 32,259 | 1,143 |
| Rename Class | 3,991 | 557 | 3,287 | 147 |

| Method-level refactorings | All | Apache | GitHub | F-Droid |
|---------------------------|-----|--------|--------|---------|
| Extract And Move | 9,723 | 1,816 | 7,273 | 634 |
| Method | 327,493 | 61,280 | 243,011 | 23,202 |
| Inline Method | 53,827 | 10,027 | 40,087 | 3,713 |
| Move Method | 163,078 | 26,592 | 124,411 | 12,075 |
| Pull Up Method | 155,076 | 32,646 | 116,953 | 5,477 |
| Push Down Method | 62,630 | 12,933 | 47,767 | 1,930 |
| Rename Method | 427,935 | 65,667 | 340,304 | 21,964 |

| Variable-level refactorings | All | Apache | GitHub | F-Droid |
|-------------------------------|-----|--------|--------|---------|
| Extract Variable | 6,709 | 1,587 | 4,744 | 378 |
| Inline Variable | 30,894 | 5,616 | 23,126 | 2,152 |
| Parameterize Variable | 22,537 | 4,640 | 16,542 | 1,355 |
| Rename Parameter | 33,675 | 61,246 | 261,186 | 14,319 |
| Rename Variable | 324,955 | 57,086 | 250,076 | 17,793 |
| Replace Variable w/ Attr. | 25,894 | 3,674 | 18,224 | 3,996 |

| Non-refactoring instances | All | Apache | GitHub | F-Droid |
|---------------------------|-----|--------|--------|---------|
| Class-level | 10,692 | 1,189 | 8,043 | 1,460 |
| Method-level | 293,467 | 38,708 | 236,060 | 18,699 |
| Variable-level | 702,494 | 136,010 | 47,811 | 518,673 |

| Class-level (total of 46 metrics) | All | Apache | GitHub | F-Droid |
|-----------------------------------|-----|--------|--------|---------|
| Source Code (37 metrics): CBO, WMC, RFC, LCOM, number of methods, number of static methods, number of public methods, number of private method, number of protected method, number of abstract methods, number of final methods, number of synchronized methods, number of fields, number of static fields, number of public fields, number of private fields, number of protected fields, number of default fields, number of final fields, number of synchronized fields, number of static invocations, lines of code, number of ‘return’ statements, number of loops, number of comparison expressions, number of try catch expressions, number of expressions with parenthesis, number of string literals, number of ‘number constants’, number of assignments, number of mathematical operators, number of declared variables, max number of nested blocks, number of anonymous classes, number of sub classes, number of lambda expressions, number of unique words. |

| Method-level (total of 20 metrics + 37 code metrics at class-level) | All | Apache | GitHub | F-Droid |
|------------------------------------------------------------------|-----|--------|--------|---------|
| Source Code (20 metrics): CBO, WMC, RFC, lines of code, number of ‘return’ statements, number of variables, number of parameters, number of loops, number of comparison operators, number of try/catch expressions, number of expressions with parenthesis, number of string literals, number of ‘number constants’, number of assignment, number of mathematical operators, max number of nested blocks, number of anonymous classes, number of sub-classes, number of lambda expressions, number of unique words. |

| Variable-level (total of 1 metric + 57 method+class level) | All | Apache | GitHub | F-Droid |
|-----------------------------------------------------------|-----|--------|--------|---------|
| Source Code (1 metric): Number of times the variable is used. |
by means of an heuristic: Whenever any of the keywords \{bug, error, mistake, fault, wrong, fail, fix\} appear in the commit message, we count one more bug fix to that class. The number of previous refactoring operations is based on the refactorings we collect from RefactoringMiner. Given that we extract this information directly from Git, these metrics are only calculated at class-level.

**Code Ownership Metrics.** We adopt the suite of ownership metrics proposed by Bird et al. [28]. The quantity of authors is the total number of developers that have contributed to the given software artifact. The minor authors represent the number of contributors that authored less than 5% (in terms of the number of commits) of an artifact. The major authors represent the number of developers that contributed at least 5% to an artifact. Finally, author ownership is the proportion of commits achieved by the most active developer. Given that we extract this information directly from Git, these metrics are also only calculated at class-level.

### 2.4 Model Training

In this step, we apply different machine learning algorithms to yield models that predict refactoring opportunities. Given that the models resulting from the learning algorithms should be able to predict refactoring opportunities at class, method, and variable-levels, the cardinality of the set of features we use to train each model varies. The feature set for training models whose desired output is to predict class-level refactoring comprises 46 features: 37 source code metrics, 5 process metrics, and 4 ownership metrics. As for training method-level models, we use a set of features that comprises all the 37 class-level source code metrics plus 20 method-level features, totaling 57 features. The same holds for variable-level models, all class, method, and variable-level source code metrics features are used to fit these models.

Subsequently, the input to a trained model is a feature vector that represents the class, method, or variable one wants to predict whether one of the 20 refactoring operations we study should happen, and the output is whether the specific refactoring should or should not be applied to that given class, method, or variable.

We make use of six different (binary classification) supervised machine learning algorithms, all available in the scikit-learn [29] and keras: (i) Logistic Regression [30], (ii) (Gaussian) Naive Bayes [31], (iii) Support Vector Machines [32], (iv) Decision Trees [33], (v) Random Forest [34], and (vi) Neural Networks [35]. We refer the reader to the papers cited above for a better understanding of each algorithm. Our training pipeline works as follows:

(i) We collect the refactoring and the non-refactoring instances for a given dataset \(d\) and a refactoring \(r\). We merge them in a single dataset, where refactoring instances are marked with a true value and non-refactoring instances are marked with a false value.

(ii) The number of refactoring instances vary per refactoring; thus, the number of refactoring instances might be greater than or smaller than the number of non-refactoring instances. Thus, we balance the dataset. To that aim, we use scikit-learn’s random under sampling algorithm, which randomly selects instances of the over-sampled class.

(iii) We scale all the features to a \([0, 1]\) range to speed up the learning process of the algorithms [36]. We use the Min-Max scaler provided by the scikit-learn framework.

(iv) We tune the hyper parameters of each model by means of a random search. We use the randomized search algorithm provided by the scikit-learn. We set the number of iterations to 100 and the number of cross-fold validations to 10. Thus, we create 1,000 different models before deciding the model’s best parameters. For the SVM in particular, we use number of iterations as 10 and number of cross-fold validations to 5, given its slow training time (which we discuss more below). For each algorithm, we search the best configuration among the following parameters:

- **Logistic Regression**: C: This parameter specifies, inversely, the strength of the regularization. Regularization is a technique that diminishes the chance of overfitting the model.
- **Naive Bayes**: Smoothing: It specifies the variance of the features to be used during training.
- **Support Vector Machines**: C: This parameter informs the SVM optimization method how much it is desired to avoid misclassifying training instances. Like the C parameter in the Logistic Regression, it helps in avoiding overfitting. Moreover, given that our goal is to also understand which features are important to the model (RQ\(_2\)), we opt only for the linear kernel of the SVM. Future research should explore how non-linear kernels perform.
- **Decision tree**: Max depth: It specifies the maximum depth of the generated tree. The deeper the tree, more complex the model becomes; Max features: It defines the maximum number of features to be inspected during the search for the best split, generating inner nodes; Min sample split: It indicates the minimum number of instances needed to split an internal node, supporting the creation of a new rule; Splitter: It defines the strategy in choosing the split at each node, varying from “best to random” strategies; Criterion: It defines the function to measure the quality of a split.
- **Random Forest**: The max depth, max features, min samples split, and criterion parameters have similar goals as to the ones in the Decision Tree algorithm; Bootstrap: It specifies whether all training instances or bootstrap samples are used to build each tree; Number of estimators: It indicates the number of trees in the forest.
- **Neural Network**: As we intend to explore sophisticated, and more appropriated, Deep Learning architectures in the future work (Section 4), here we compose a sequential network of three dense layers with 128, 64, and 1 units, respectively. Also, to avoid overfitting, we added dropout layers between sequential dense layers, keeping the learning in 80% of the units in dense layers. The number of epochs was set to 1000. This architecture is similar to a Multilayer Perceptron, in the sense that it is a feedforward deep network.

(v) Finally, we perform a stratified K-fold cross-validation using the hyper parameters established by the search. We return the precision, recall, and accuracy of all the
2.5 Evaluation

To answer RQ1, we report and compare the mean precision, recall, and accuracy among the different models after the 10 stratified cross-fold executions. We apply stratified sampling in all the cross-fold executions to make sure both training and test datasets contain the same amount of positive and negative instances. For SVM and the Neural Network, we set the number of cross-folds to 5. The SVM and the Neural Network models training and validation processes took 237 and 232 hours, respectively. Nevertheless, the precision, recall, and accuracy across the five folds of both models were highly similar, indicating that the models are stable (numbers can be found in our appendix [24]), and thus, we have no reason to believe that the smaller number of cross-fold validations for the SVM and Neural Network affected their results.

To answer RQ2, we report how often each feature (from Table 4) appears among the top-1, top-5, and top-10 most important features of all the generated models. We use scikit-learn’s ability to extract the feature importance of the Logistic Regression, SVM, Decision Trees, and Random Forest models. The framework does not currently have a native way to extract feature importance of Gaussian Naive Bayes and Neural Networks. We intend to extend the feature importance of both algorithms via “permutation importance” in future work. Given the high number of different models we build (we extracted the feature importance of 320 out of the 480 models we created), we have no reason to believe the lack of these two models would affect the overall findings of this RQ. Given that the number of features vary per refactoring level, we generate different rankings for the different levels (i.e., different ranks for class, method, and variable-level refactorings). Some models (e.g., SVM) might return the importance of a feature as a negative number, indicating that the feature is important for the prediction of the negative class. We consider such a feature also important to the overall model, and thus, we build the ranking using the absolute value of feature importance returned by the models.

Finally, to answer RQ3, we test each of our dataset-specific models on the other datasets. For example, we test the accuracy of all Apache’s models in the GitHub and F-Droid datasets. More formally, for each combination of datasets $d_1$ and $d_2$, where $d_1 \neq d_2$, and refactoring $r$ we: 1) load the previously trained $r$ model of the $d_1$ dataset, 2) open the data we collected for $r$ of the $d_2$ dataset, 3) apply the same pre-processing steps (i.e., sampling and scaling), 4) use $d_1$’s model to predict all data points of $d_2$’s dataset, 5) and report the precision, recall, and accuracy of the model.

2.6 Implementation and Execution

The data collection tool is implemented in Java and stores all its data in a MySQL database. The tool integrates natively with RefactoringMiner [18] (also written in Java) as well as with the source code metrics tool.

The tool gives RefactoringMiner a timeout of 20 seconds per commit to identify a refactoring. We define the timeout as RefactoringMiner performs several operations to identify refactorings, and these operations grow exponentially, according to the size of the commit. Throughout the development of this study, we observed some commits taking hours to be processed. The 20 seconds was an arbitrary number decided after experimentation. In practice, most commits are resolved by the tool in less than a second. Given that its performance is related to the size of the commit and not to the size of the class under refactoring or the number of refactorings in a commit, we do not believe that ignoring commits where RefactoringMiner takes a long time influences our sample in any way.

Given that our tool integrates different tools, there are many opportunities for failures. We have observed (i) the code metrics tool failing when the class has an invalid structure (and thus, ASTs can not be built), (ii) our tool failing to populate process and ownership metrics of refactored classes (often due to files being moved and renamed multiple times throughout history, which our tool could not track in 100% of the cases), (iii) RefactoringMiner requiring more memory than what is available in the machine. To avoid possible invalid data points, we discard all data points that were involved in any failure.

We had 30 Ubuntu 18.04 LTS (64bits) VMs, each with 1 GB of Ram, 1 CPU core, and 20 GB of disk available for data collection. These machines, altogether, spent a total of 933 hours to collect the data. We observe that the majority of projects (around 99% of them) took less than one hour to be processed. 159 of them took more than one hour, and 70 of them more than two hours. A single project took 23 hours.

The machine learning pipeline was developed in Python. Most of the code relies on the scikit-learn framework [29] and keras for the Neural Networks training. To the machine learning training, we had under our disposition two machines: one Ubuntu 18.04.2 LTS VM, 396GB of RAM, 40 CPU cores, and one Ubuntu 18.04.2 LTS VMS with 14 CPUs and 50 GB of RAM. Given the hyperparameter search and cross-validations, our machine learning pipeline experimented with a total of 404,080 models. The overall computation (training and testing) time was approximately 500 hours.

2.7 Reproducibility

Our online appendix [24] contains: (i) the list of the 11,149 projects analyzed, (ii) a spreadsheet with the full results, (iii) the source code of the data collection and the machine learning tools, and (iv) a two million refactorings dataset.

3 Results

In the following subsections, we answer each of the RQs.

3.1 RQ1: How accurate are supervised machine learning approaches in predicting software refactoring?

In Table 5, we show the precision, recall, and accuracy of each machine learning algorithm in each one of the 20 refactoring operations, when training and testing in the entire dataset. Due to the lack of space, we show the results of training and testing in individual datasets, as well as the confusion matrix, in our appendix [24].

Observation 1: Random Forest models are the most accurate in predicting software refactoring. Random Forest has
the highest overall accuracy among all types of refactorings. Its average accuracy for class, method, and variable-level refactorings, when trained and tested in the entire dataset, are 0.93, 0.90, and 0.94, respectively. The only three refactorings that are below the 90% threshold are Extract Class, Extract and Move Method, and Move Extract. Its average accuracy among all refactorings in all the datasets together, as well as Apache, GitHub, and F-Droid datasets only, are 0.93, 0.94, 0.92, and 0.90, respectively. As a matter of comparison, the second best model is Decision Trees, which achieves an average accuracy of 0.89, 0.91, 0.88, and 0.86 in the same datasets.

Observation 2: Random Forest was outperformed only a few times by Neural Networks. In the F-Droid dataset, Neural Networks outperformed Random Forest 4 times (in terms of accuracy). Neural Networks also outperformed Random Forest in two opportunities in both the Apache and GitHub datasets. However, we note that the difference was always marginal (around 1%).

Observation 3: Naive Bayes models present high recall, but low precision. The Naive Bayes models presented recalls of 0.94, 0.93, 0.94, and 0.84 in the entire dataset, Apache, GitHub, and F-Droid datasets, respectively. These numbers are often slightly higher than the ones from Random Forest models, which were the best models (on average, 0.01 higher). Nevertheless, Naive Bayes models presented the worst precision values: 0.62, 0.66, 0.62, and 0.67 in the same datasets. Interestingly, no other models presented such low precision.

Observation 4: Logistic Regression, as a baseline, shows good accuracy. Logistic Regression being, perhaps, the most straightforward model in our study, presents a somewhat high overall accuracy, always outperforming Naive Bayes models. The average accuracy of the model in all the refactorings in the entire dataset is 0.83. Its best accuracy was in the Move Class refactoring: 0.94 (which also presented high values in the individual datasets: in F-Droid, 0.94, in GitHub, 0.93, and in Apache, 0.95), and its worst accuracy, 0.77, was in the Extract and Move Method and Inline Method refactorings. The overall averages are similar in the other datasets: 0.85 in Apache, 0.83 in GitHub, and 0.78 in F-Droid.

3.2 Q2: What are the important features in the refactoring prediction models?

In Table 6 we show the most important features per refactoring level. The complete ranking of features importance can be found in the online appendix [24].

Observation 5: Process metrics are highly important in class-level refactorings. Metrics such as quantity of commits, lines added in a commit, and number of previous refactorings appear in the top-1 ranking very frequently. In the top-5 ranking, seven out of the first ten features are process metrics; six out of the first ten are process metrics in the top-10 ranking. Ownership metrics are also considered important by the models. The author ownership metric appears 32 times in the top-1 ranking; the number of major authors and number of authors metrics also appear often in the top-5 and top-10 rankings.

Observation 6: Class-level features play an important role in method-level and variable-level refactorings. Method-level refactoring models often consider class-level features (e.g., lines of code in a class, number of methods in a class) to be more important than method-level features. In the top-1 ranking for the method-level refactoring models, 13 out of the 17 features are class-level features. In variable-level refactoring models, the same happens in 11 out of 17 features. Interestingly, the most fine-grained feature we have, the number of times a variable is used appears six times in the top-1 ranking for the variable-level refactoring models.

Observation 7: Some features never appear in any of the rankings. For class-level refactoring models, the number of
Class-level refactorings
Top-1: quantity of commits (68), author ownership (32), lines added (6)
Top-5: quantity of commits (108), lines added (63), previous refactorings (63), author ownership (56), unique words in the class (47)
Top-10: quantity of commits (111), lines added (90), previous refactorings (90), unique words in the class (78), class LOC (70)

Method-level refactorings
Top-1: class LOC (39), number of unique words in a class (15), number of methods in a class (13), class LCOM (9), number of fields in a class (6)
Top-5: class LOC (74), number of methods in a class (55), number of unique words in a class (52), class LCOM (37), number of final fields in a class (25)
Top-10: number of methods in a class (90), class LOC (88), class LCOM (71), number of unique words in a class (54), class CBO (54)

Variable-level refactorings
Top-1: class LOC (27), class LCOM (10), number of unique words in a class (9), method LOC (7), number of public fields in a class (7)
Top-5: class LOC (61), number of unique words in a class (48), number of string literals in a class (38), number of variables in the method (30), number of public fields in a class (24)
Top-10: number of string literals in a class (72), class LOC (71), number of unique words in a class (66), number of variables in a class (55), number of variables in a method (49)

TABLE 6
Most important features for the models at different refactoring levels. Top-1, Top-5, and Top-10 indicate the number of times (in parenthesis) a specific feature appeared in the top-N ranking. For class and method level refactorings, a feature can at most appear 112 times; 96 times for a variable level refactoring. We show only the first five features per ranking; full list in the online appendix [24].

default fields, and the number of synchronized fields do not appear even in the top-10 ranking. Nine other features never appear in the top-10 feature importance ranking of method-level refactoring models (e.g., number of comparisons, math operations, and parenthesized expressions), and ten features never make it in the variable-level refactoring models (e.g., number of loops, and parenthesized expressions).

3.3 RQ3: Can the predictive models be carried over to different contexts?
We show the precision and recall of each model and refactoring, in all the pairwise combinations of datasets in our appendix [24]. In Table 2, we show the overall average precision and recall of the Random Forest models (the best model, according to RQ1 results) when trained in one dataset and tested in another dataset.

Observation 8: Random Forest still presents excellent precision and recall when generalized, but smaller when compared to previous results. Random Forest models achieve precision and recall of 0.87 and 0.84, when trained using the GitHub repository, the largest repository in terms of data points, and tested in Apache. When trained in the smallest dataset, F-Droid, Random Forest still performs reasonably well: precision and recall of 0.77 and 0.73 when tested in Apache, and 0.81 and 0.76 when tested in GitHub. Nevertheless, we remind the reader that in terms of accuracy, Random Forest achieved average scores of around 90%. In other words, models seem to perform best when trained with data collected from different datasets.

Observation 9: Method and variable-level refactoring models perform worse than class-level refactoring. In general, class-level refactoring models present higher precision and recall than the method and variable-level refactoring models. Using a model trained with the GitHub data set and tested in the F-Droid data set, the average precision and recall for Random Forest models at class-level are 0.92 and 0.92. On the other hand, the average precision and recall for Random Forest models at method-level are 0.77 and 0.72, respectively; at variable-level, we observe precision and recall of 0.81 and 0.75.

Observation 10: SVM outperforms Decision Trees when generalized. We observed Decision Trees being the second best model in RQ1. When carrying models to different contexts, however, we observe that SVM is now the second best model, and only slightly worse than the Random Forest. For example, in the appendix, we see that for a model trained in GitHub and tested in Apache, the average precision and recall of SVM models is 0.84 and 0.83 (in contrast, Random Forest models have 0.87 and 0.84). The difference between both models is, on average, 0.02.

Observation 11: Logistic Regression is still a somewhat good baseline. Logistic Regression baseline models, when carried to different contexts, still present somewhat good numbers. As an example, the models trained with GitHub data and tested in the Apache dataset show an average precision and recall of 0.84 and 0.83. The worst averages happen in the models trained with the Apache dataset and tested in the F-Droid dataset (precision of 0.75 and recall of 0.72).

Observation 12: Heterogeneous datasets might generalize better. More homogeneous datasets (i.e., the Apache and F-Droid datasets), when carried to other contexts, present lower precision and recall. This phenomenon can be seen whenever Apache and F-Droid models are cross tested; their precision and recall never went beyond 0.78. This phenomenon does not happen when GitHub, a more heterogeneous dataset in terms of different domains and architectural decisions, is tested on the other two datasets. On the other hand, the GitHub dataset also happens to be the largest individual dataset. Future work should explore whether models need to be trained on heterogeneous and/or large datasets.

4 DISCUSSION
In the following, we extensively discuss the ramifications of our work. More specifically, we discuss:

1) the challenges in defining k as a constant to collect non-refactored instances,
We note that the distribution of the features values of the 20 refactoring prediction Random Forest models, when trained in one dataset and tested in another dataset. Rows represent datasets used for training, and columns represent datasets used for testing.

|       | Apache | GitHub | F-Droid |
|-------|--------|--------|---------|
| Pr    | Re     | Pr     | Re      |
| Apache | -      | -      | 0.84    | 0.79    | 0.77    | 0.70    |
| GitHub | 0.87   | 0.84   | -       | -       | 0.84    | 0.80    |
| F-Droid| 0.77   | 0.73   | 0.81    | 0.76    | -       | -       |

The average precision (Pr) and Recall (Re) of the 20 refactoring prediction Random Forest models, when trained in one dataset and tested in another dataset. Rows represent datasets used for training, and columns represent datasets used for testing.

2) the possible use of anomaly detection techniques to predict software refactoring,
3) the importance of process and ownership metrics (and the need for fine-grained metrics),
4) field-level refactorings as another type of refactoring that can be explored,
5) the use of multi-classification models,
6) the need for more fine-grained refactoring recommendations,
7) the recommendation of high-level refactorings,
8) taking the developers’ motivations into account,
9) the use of Deep Learning (and Natural Language Processing methods) for software refactoring, and
10) deploying refactoring recommendation models in the wild.

4.1 Collecting non-refactored instances via an heuristic

We used $k = 50$ (i.e., 50 commits in a row without being refactored) as a constant to determine whether a class, its methods, and its variables should be considered an instance of a non-refactoring. The number 50 was chosen after manual exploration in the dataset.

To measure the influence of $k$ in our study, we re-executed our data collection procedure in the entire dataset (11,149 projects) with two different values for $k$:

- $k = 25$. The half of the value used in the main experiment.
  - A threshold of 25 means that we are less conservative when considering instances for the non-refactoring dataset. In this dataset, we have a total of 7,210,452 instances (at class, method, and variable levels). This represents an increase of 7.1 times when compared to the dataset in the main experiment.
- $k = 100$. The double of the value used in the main experiment. A threshold of 100 means that we are more conservative when it comes to considering a class as an instance of a non-refactoring. In this dataset, we have a total of only 120,775 instances. This represents around 12% of the dataset in the main experiment.

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We trained Random Forest models (given that it was the algorithm with the best accuracy in RQ1) in both $k = 25$ and $k = 100$ datasets. In $k = 25$, the average of the absolute difference in the precision and recall of the 20 refactoring models, when compared to $k = 50$, are 0.0725 and 0.099, respectively. In $k = 100$, the average of the absolute difference in precision and recall when compared to $k = 50$ are 0.0765 and 0.064, respectively. The precision and recall of each refactoring is in our online appendix [24].

In $k = 25$, however, in only four (Move Class, Move and Rename Class, Extract Method, and Rename Method; out of 20) models, the precision values were better than in the $k = 50$. Similarly, only a single model (Rename Method) had a better recall when compared to $k = 50$. This might indicate that $k = 25$ is not a good threshold, as it might be too small. In $k = 100$, while it is hard to distinguish whether it has a better precision than $k = 50$ (11 models did better with $k = 50$ and 9 models did better with $k = 100$), models in $k = 100$ had almost always a better recall (16 models out of 20). This might indicate that more conservative thresholds might help in increasing the recall, but maybe at the price of precision.

This discussion shows the importance of finding the right threshold to determine classes, methods, and variables that can serve as non-refactoring instances. We also highlight an even deeper point: while our community has been working on several approaches to point developers to problematic pieces of code, less research is dedicated to revealing exemplary pieces of code; such examples might be useful for machine learning studies in code quality.

4.2 The use of anomaly detection techniques to predict software refactoring

Given that the current state-of-the-art enables us to precisely identify refactoring operations that have happened in software systems, but the identification of non-refactoring instances is challenging, we suggest the possibility of modeling the refactoring prediction problem as an anomaly detection problem.

In this case, one would consider the “refactoring instances” (that we can easily collect) to be the normal case, and the “non refactoring instances” to be the abnormal cases. Note that, in this way, there is no need for collecting non-refactoring data points, which would avoid the problem discussed in the sub-section above.

We therefore suggest future work to explore the performance (as well as the drawbacks) of anomaly detection models in recommending software refactoring.

4.3 The importance of process and ownership metrics (and the need for fine-grained metrics)

We currently use process and ownership metrics to support the prediction of class-level refactorings only. These metrics are naturally collected at file-level, and collecting them in a more fine-grained manner (i.e., method and variable ownership) would require tooling to be developed.

Interestingly, we observed that process metrics are indeed considered important by the models (see RQ2). For example, the number of commits metric that appeared as the most important variable in the class-level refactoring models. Our
intuition is that changing a class several times eventually leads to brittle design, which drives developers to remedy the situation by refactoring the class (following Lehman's laws of software evolution [37][38]). We can only conjecture that process metrics would also help models in better predicting refactoring at method and variable levels. To that aim, it is our goal to (i) develop a tool that is able to collect process and ownership metrics at method and variable levels, (ii) feed our models with these new features, (iii) re-execute our research pipeline and study how the accuracy changes.

4.4 Field-level refactorings
RefactoringMiner also supports field level refactorings. More specifically, Move Attribute, Pull Up Attribute, Move and Rename Attribute, Push Down Attribute, Replace Attribute, and Rename Attribute. In this paper, we focused on three different levels; it is part of our future plans to also build models to predict field level refactorings. We conjecture models will have similar accuracy as the variable level models, as the features that can be extracted are similar.

4.5 Multi-classification models
All our models currently perform binary classification. Given that a binary model has only two options (true or false), a “bad model” might learn that guessing one of them brings the highest accuracy. Our conjecture is that, in a multi-classification model, the model would have to choose between “no refactoring” or one of the 20 refactorings we currently predict. This would force the model to take a concrete decision, reducing its chances of biasing the accuracy due to guessing. The next step is to carry out a follow-up experiment, using the same algorithms, but now with their multi-classification versions. Additionally, we plan on examining the accuracy of the binary classification algorithms in comparison to their multi-classification versions.

4.6 The need for more fine-grained refactoring recommendations
In this first step, we have showed that machine learning can model the refactoring recommendation problem. And, although we provide recommendations at different levels of granularity (i.e., class, method, and variable levels), we see room for even more fine-grained recommendations. Take as an example the Extract Method refactoring. Our models can identify which method would benefit from an extraction; however, it currently does not point to which parts of that method should be extracted (i.e., initial token and end token). Another example is the Move Method refactoring: to which class should this method be moved to?

We see a future where, for each of the refactorings we studied, a highly-specific model, able to provide fine-grained recommendations, is devised. We conjecture that models that learn precisely, e.g., what tokens to extract out of a method, would need to be deep. Therefore, we believe that deep learning will play an important role in the field of software refactoring in the near future. We discuss deep learning later in this section.

4.7 The recommendation of high-level refactorings
In this study, we explore recommendations of low-level refactorings, i.e., small and localized changes that improve the overall quality of the code. We did not explore recommendations of high-level refactorings, i.e., larger changes that improve the overall quality of the design.

We see that the great challenge of recommending high-level refactorings is that the model requires even more context to learn from. Before applying a design pattern to the source code, developers often think about how to abstract the problem in such a way that the pattern would fit.

As an initial step, the book of Kerievsky [39] might serve as a guide. In his book, the author shows how to move code, that is often implemented in a procedural way, to a design pattern oriented solution, by means of low-level refactorings. Our next step is to explore how we can “aggregate” several low-level refactoring recommendations in order to provide developers with high-level refactoring suggestions.

4.8 Taking the developers’ motivations into account
Empirical research indeed shows that developers refactor for several reasons, other than to “just improve the quality of the code” (e.g., [21]). In this first exploration, we do not add “motivation” in our models. Nevertheless, note that our large dataset of refactorings contains refactorings that have happened for whatever reasons (given that we never filtered refactoring based on motivation from the projects). Interestingly, our generic models still show high accuracy. Exploring whether models built specifically for, e.g., “refactor to add new functionality”, “refactor to fix bug”, would provide even better results, not only in terms of accuracy, but also in terms of “developer satisfaction”, is an interesting future work.

4.9 The use of Deep Learning (and Natural Language Processing methods) for software refactoring
Programming languages have phenomena like syntax and semantics [40][41][42]. Motivated by several recent works that use advanced ML algorithms on source code with the goal of (semi) automating several non-trivial tasks of SE such as suggesting method and class names [43], code comments [44], generation of commit messages [45], and defect prediction [46], we intend to experiment NLP-specific deep learning architectures to deal with code refactorings. Using models like Seq2Seq [47] and Code2Vec [48], both refactoring predictions and refactored code can be outputs of the model, having the source code only as input. To facilitate the work of future researchers interested on the topic, our online appendix [24] contains a dataset with all the refactored classes studied here.

4.10 Deploying refactoring models in the wild
Deploy such models in the wild should be considered the final goal of this line of research. We foresee two practical challenges. First, collecting process and ownership metrics requires complex tooling. Future work should also evaluate what to do in cases where the developer does not have access to these metrics, i.e., when offering consultancy to a company that does not provide the consultant with the full repository,
or when the project is in earlier stages and the repository still does not contain useful data. Second, there might be too many recommendations to give; we intend to explore ways to prioritize such recommendations. A first step would be to use the internal probabilities that our models use as a way to prioritize the list of recommendations.

Moreover, a production-ready model should be as lean as possible. In RQ2, we show that some features never make to the top-10 ranking features; others were never even used. Future work should work on identifying the simplest model that works by, e.g., performing feature reduction. As a reference, we refer the reader to Kondo et al’s work [49]. Authors explored the impact of eight different feature reduction techniques on defect prediction models; we suggest the same line of work for refactoring recommendation models.

5 Related Work

After the publication of Fowler’s seminal book [1], refactoring went mainstream and many surveys and literature reviews on the subject were performed. One of the early surveys that brought refactoring into the limelight of researchers was carried out by Mens and Tourwé [50]; their survey is centered around refactoring activities, supporting techniques, and tool support. Specifically, their discussion is organized around software artifacts and how refactoring applies to them, so the authors emphasize requirement refactoring, design refactoring, and code refactoring. Additionally, Mens and Tourwé briefly share their outlook on the impact of refactoring on software quality. Their survey, however, took only a few studies on identifying refactoring opportunities into account and did not follow a systematic approach. As mentioned, since then several systematic literature reviews have been conducted on refactoring.

The existing literature discusses different automatic refactoring approaches whose purpose is helping practitioners in detecting code smells, some of which are even able to suggest the refactoring activities that should be performed by the practitioners in order to remove the detected code smells. Most approaches are either based on rules, employ search-based algorithms, or machine learning approaches. A recent systematic literature review [51] shows that there has been an increase in the number of studies on automatic refactoring approaches. According to the results of such literature review, source code approaches have been receiving more attention from researchers than model based approaches. In addition, the results indicate that search-based approaches are gaining increasing popularity and researchers have recently begun exploring how machine learning can be used to help practitioners in identifying refactoring opportunities. The concepts and rule-based approaches proposed by early researchers that laid the theoretical foundation for more recent advances in the area are presented in Subsection 5.1. Related work on search-based approaches applied to refactoring is discussed in Subsection 5.2 and related work on machine learning is reviewed in Subsection 5.3.

5.1 Code Smell Detection

In hopes of providing a insightful understanding of code smells, the goals of studies on code smells, approaches used to probe into code smells, and evidence that bolsters the fundamental premise that code smells are symptoms of issues in the code, Zhang et al. [52] carried out a systematic literature review in which they synthesized the results of 39 studies on code smells. Since we consider the identification of code smells and the detection of refactoring opportunities two related problems, it is also worth mentioning the systematic literature review performed by Al Dallal [53]. Al Dallal discusses studies that consider both code smells and refactoring opportunities from a different perspective: the main focus of their literature review is providing an overview of code smell identification approaches. Based on an analysis of 47 studies, Al Dallal concluded that although there was a sharp increase in the number of studies on identifying refactoring opportunities, up to 2013 the results of these studies were derived mostly from relatively small datasets. Singh and Kaur [54] extended the systematic literature review carried out by Al Dallal focusing on code smells identification and anti-patterns. The two main contributions of their survey is highlighting the datasets and the tools employed in the selected studies and the identification of the code smells that were most used in these studies.

Recently, Santos et al. [55] performed a systematic literature review to summarize knowledge about how code smells impact software development practices, which the authors termed “smell effect”. Santos et al. selected and analyzed 64 studies that were published between 2000 and 2017. One of the main findings reported by the authors is that human-based evaluation of smells is not reliable: a trend in the selected studies seems to indicate that developers have a low level of consensus on smell detection. Furthermore, their analysis of the selected studies suggests that demographic data as developers’ experience can significantly impact code smell evaluation.

Fernandes et al. [56] carried out a systematic literature review on code smell detection tools. Their study is centered around the identification of code smell detection tools, their main features, and the types of code smells that these tools are able to identify. Fernandes et al. also performed a comparison of the four most widely used tools (i.e., most frequently mentioned in the selected studies). It is worth mentioning that considering the selected studies, which were published from 2000 to 2016, no tool implements a machine learning based approach: this indicates that only recently researchers have begun investigating machine learning models in this context. Rasool and Arshad [57] also performed a systematic literature review on tools and approaches to mining code smells from the source code. Essentially, Rasool and Arshad classified tools and approaches based on their detection methods. Rasool and Arshad emphasized mining approaches, thus they did not take machine learning based approaches into account.

5.2 Search-based Refactoring

Mariani and Vergilio [12] carried out a systematic literature review of how search-based approaches have been applied to refactoring. Mariani and Vergilio found that evolutionary algorithms and, in particular, genetic algorithms were the most commonly used algorithms in the analyzed studies. In addition, they found that the most widely used and
investigated refactorings are the ones in Fowler’s catalog [1]. More recently, Mohan and Greer [38] also looked at search-based refactoring. However, differently from the literature survey by Mariani and Vergilio, Mohan and Greer give a more in-depth review of the selected studies in the sense that Mohan and Greer also cover other aspects of the literature. For instance, Mohan and Greer also discuss the tools used in the selected studies as well as provide an investigation of how some metrics have been tested and discussed in the selected literature. In addition, Mohan and Greer detail how the search-based approaches described in the selected studies have evolved over time. Similarly to the results presented by Mariani and Vergilio, Mohan and Greer also found that evolutionary algorithms are the most commonly used algorithms in the selected studies.

5.3 Machine Learning

To our best knowledge, only one systematic literature review [59] has been conducted with the purpose of summarizing the research on machine learning algorithms for code smell prediction. Azeem et al. selected 15 studies that describe code smell prediction models. Azeem et al. analyzed the selected studies in terms of (i) code smells taken into account, (ii) setup of the machine learning based approaches, (iii) how these approaches were evaluated, and (iv) a metaanalysis on the performance of the code smell prediction models described in the selected studies. According to the results, God Classes, Long Methods, Functional Decomposition, and Spaghetti Code are the most commonly considered code smells. Decision Trees and Support Vector Machines are the most widely used machine learning algorithms for code smell detection. Additionally, JRip and Random Forest seem to be the most effective algorithms in terms of performance.

6 Threats to validity

This section outlines the threats to the validity of our study.

6.1 Construct validity

Threats to construct validity concern the relation between the theory and the observation, and in this work are mainly due to the measurements we performed.

- Our strategy for gathering the large amount of data we investigated entailed mining a large number of software repositories for instances of class, method, and variable refactorings. Thus, the main internal validity threat is the data collection process. We cannot rule out the issues that arise when performing large scale data extraction (issues indeed happened, as discussed in Section 2.6). We provide a replication package containing all experimental scripts and datasets used in our study so that researchers and practitioners can fully replicate and confirm our results.

- Owing to the fact that, in most cases, there are more instances of the non-refactoring class in our dataset than instances of the refactoring class (see Table 3), we had to cope with an imbalanced dataset. Given that there is no reliable estimate on the distribution of refactoring and non-refactoring instances “in the wild” (as we discuss in Section 4), we decided to perform under-sampling. That is, we chose to remove instances from the over-represented class by means of random under-sampling. This means that the dataset for each refactoring operation is bounded by the minimum between the number of positive and the number of negative instances (e.g., as we see in Table 3 although we have 41,191 instances of Extract Class refactorings, we have only 10,692 instances of non-refactored classes, and thus, our model is trained on all the 10,692 instances of non-refactored classes + 10,692 randomly sampled instances of refactored classes).

To better measure the impact of this choice, we re-created the Random Forest models using a “Near Miss” under-sampling strategy [60]. While the average absolute difference in precision and recall are 0.116 and 0.052, respectively, it is hard to distinguish which strategy helps the model in improving accuracy. In nine out of 20, Near Miss improved the precision when compared to the random sampling strategy (and thus, random performed better in 11 models), whereas in 12 out of 20, Near Miss improved the recall. As we conjecture that, in the refactoring problem, classes will always be unbalanced by nature, future research is necessary to better understand how to under (or even over) sample.

- Our machine learning pipeline performs scaling and undersampling. Improving the pipeline, e.g., by applying better feature reduction, different balancing strategies, and extensive hyperparameter search, will only make our results better. While developing production-ready models was not the main goal of this paper, we note that our open-source implementation available in our appendix [24] enables it effortlessly. In other words, any researcher or company can download our implementation and datasets, use their available infrastructure, and train (even more accurate) models.

- Our data collection mechanism makes use of RefactoringMiner [18], a tool that is able to identify refactoring operations in the history of a repository. Therefore, the soundness of our method hinges on the effectiveness of refactoring detection tool we used. RefactoringMiner presents a precision and recall of 98% and 87%, respectively, in detecting the refactoring operations we study. We did not re-evaluate the precision and recall of RefactoringMiner in the studied sample, as this was already established in their research. Given how RefactoringMiner works internally and that RefactoringMiner was evaluated on projects with similar characteristics (in fact, 65% of the projects in RefactoringMiner’s evaluation dataset are in our dataset), we have no reason to believe that the accuracy reported in the literature would not apply to our study.

- When comparing the performance of models that were trained and tested on different datasets, we used the best model out of the hyperparameter selection process. Given that we used five folds during hyperparameter selection, the best model was trained with 80% of the available data points (the other 20% was used for testing the model). We can only expect better performance if one trains a model on the entire dataset. Future research should precisely measure this improvement. To facilitate future research, we provide models trained on the full dataset in our online appendix [24].

- Finally, one of the metrics we also used to train our models was the “number of default methods” (at class-level).
However, later in one of our inspections, we observed that, due to a bug in the metric collection tool, the number of default methods was always zero. All the learning algorithms ignored this metric, as it indeed added no value to the learning process; in fact, it appeared on the list of features that were never used by our model. Therefore, we affirm that this bug does not influence the overall results of our paper. Moreover, we have no reason to believe that the adding this feature would bring significant improvements. Nevertheless, we propose researchers to use this feature in future replications of this paper.

6.2 Internal validity

Threats to internal validity concern external factors we did not consider that could affect the variables and the relations being investigated.

- We removed projects that failed during data collection. As we discussed in Section 4.1 our pipeline is composed of several tools, all of them being prone to failures, e.g., RefactoringMiner running out of memory. The percentage of failed projects is small (8%), and does not affect the representativeness of our final dataset.

- Code smells are symptoms that might indicate deeper problems in the source code [1]. While code smells have been shown to greatly indicate problematic pieces of code, in this work, we did not use them as features to our model. However, we note that code smells are detected by combinations of proxy metrics (i.e., detection strategies [10]). These proxy metrics are commonly related to the structure of the source code (e.g., complexity/WMC, coupling/CBO) and are highly similar to the structural metrics we use as features (see Table 4). In other words, we train our models with metrics that are similar to the metrics used by the code smells detection strategies. Therefore, we conjecture that using code smells as features would add only a small amount of information for the models to learn from. That being said, making sure that all catalogued code smells are covered by our features is interesting future work which might increase the accuracy of refactoring recommendation models. Our own previous research shows that code smells might be architecture-specific [61, 62, 63, 64], e.g., MVC systems might suffer from different and specific smells than Android systems.

- As we discuss in Section 4 we did not take into account the different reasons a developer might have when deciding to refactor, e.g., to add a new functionality, or to improve testability. These motivations might indeed change (or even help the developer to prioritize) which refactorers to apply. Nevertheless, we affirm that the goal of this first study was to explore whether ML can model the refactoring recommendation problem. Given that we observed high accuracy, we can only conjecture that taking the motivation into consideration will only increase the accuracy (or again, help in prioritization) of the models. We leave it as future work.

- We consider our dataset as a set of unordered refactorings. As a contrast, studies in defect prediction consider datasets as a set of ordered events, e.g., they do not mix “past” and “future” when evaluating the accuracy of their models (e.g., [65]). We argue that there is no need for such design, given that we devise a single cross-project model, based on hundreds of thousands of data points from more than 11k projects altogether. In other words, we do not devise one model per project, as commonly done in defect prediction. Thus, we affirm that the model has little chance of memorizing specific classes. Our 10-fold random cross validation (and the individual precision and recall of each fold, that can be seen in our appendix [24]) also gives us certainty that this is not a threat.

Despite these limitations, we observe that our results carry over to other programming languages. Thus, replications of this study are needed for different programming languages. However, we cannot think of any reason why the results would be different for other imperative object-oriented languages.

6.3 External validity

Threats to external validity concern the generalization of results.

- Our results are based only on open source projects, which might affect their generalizability to industrial settings. It is worth mentioning, however, that our sample contains many industrial-scale projects that span different domains. To the best of our knowledge, this is the most extensive study of machine learning algorithms for the prediction of refactorings to date. Nevertheless, replicating this research in a large dataset of industry projects is necessary.

- Moreover, since we considered Java as the language of choice, we cannot be sure that our results carry over to other programming languages. Thus, replications of this study are needed for different programming languages.

7 Conclusion

Supervised machine learning methods are effective in predicting refactoring opportunities and might indeed support developers in making faster and more educated decisions concerning what to refactor.

Our main findings show that:

1) Random Forest models outperform other machine learning models in predicting software refactoring.

2) Process and ownership metrics seem to play a crucial role in the creation of better models.

3) Models, especially when built on top of heterogeneous projects, generalize well and achieve good performance.

More importantly, this paper shows that machine learning models can accurately model the refactoring recommendation problem. We hope that this paper will pave the way for more data-driven refactoring recommendation tools.

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