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Incremental Development of Software Quality Prediction Models

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Abstract The identification of fault-prone modules has a significant impact on software quality assurance. In addition to prediction accuracy, one of the most important goals is to detect fault prone modules as early as possible in the development life cycle. Requirements, design, and code metrics have been successfully used for predicting fault-prone modules. In this paper, we investigate the benefits of the incremental development of software quality models. We compare the performance of these models as the volume of data and their life cycle origin (design, code, or their combination) evolve during project development. We analyze fourteen data sets from publicly available software engineering data repositories. These data sets offer both design as well as code metrics. Using a number of modeling techniques and statistical significance tests, we confirm that increasing the volume of training data improves model performance. Further, models built from code metrics typically outperform those that are built using design metrics only. However, both types of models prove to be useful as they can be constructed in different phases of the life cycle. Code-based models can be used to improve the effectiveness of assigning verification and validation activities late in the development life cycle. We also conclude that models that utilize a combination of design and code level metrics outperform models which use either one metric set exclusively.

Keywords Design metrics, Code metrics, Software quality prediction, Model performance evaluation

1 Introduction

The accuracy of software quality models, which identify the modules where faults may hide, has not improved significantly over the past decade. Menzies et. al. call this the "ceiling effect." [34]. Despite intensive research, the current generation of models is not finding

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new information in the data sets available in public repositories, such as PROMISE [9] and NASA MDP [3]. Therefore, we hypothesize that future fault prediction research should *change* its focus from designing better modeling algorithms towards improving (a) the information content of the training data, or (b) the model evaluation functions which would inject additional knowledge regarding context in which software is used into the modeling process. This paper explores option (a) by thoroughly evaluating the information content offered by design level and code level metrics.

Recently, we have had success with augmenting code metrics with features extracted from requirements documents via lightweight text parsing [25]. When modeling was applied to features extracted from *both* code *and* requirements, we observed a remarkable improvement in the probability of correctly detecting fault-prone modules while reducing the probability that a fault-free module is wrongly classified as fault-prone. While these results are promising, the conclusions were based on a limited sample size. We had access to requirements metrics from only three defect data sets. Nevertheless, we hypothesize that using metrics that reflect software artifacts from different phases in the life cycle offer the opportunity for improving the performance of fault prediction models. When it comes to the availability of design level and code level metrics, public repositories offer a much larger number of data sets, allowing us to analyze this hypothesis.

A decade ago, Zhao *et. al.* explored the differences between fault prediction models developed using code level and design level metrics [56]. They conclude that “the design metrics are as good as the code metrics; little improvement can be achieved if both design metrics and code metrics are used...”. However, their conclusions were drawn from the data analysis from a single software project using only one fault prediction modeling technique (logistic regression). One of our goals is to reexamine their conclusion. In this study, we use 14 publicly available project data sets. Each of these projects offers both design and static code metrics. We built fault prediction models using each metrics set separately (models denoted as $design$ and $code$, respectively), as well as from a combined metrics data set (denoted as all). Unlike Zhao *et. al.*, we will utilize five modeling techniques frequently used in the current research.

If the conclusion from [56], which states that “design metrics are as good as code metrics” holds, the implication is that software projects should build fault prediction models as soon as the design metrics become available. Further, if “little improvement can be achieved if both design metrics and code metrics are used”, updating design-based models makes no sense. However, this paper will demonstrate that *incremental* development of fault prediction models is beneficial and should not be ignored. In this paper, incremental development implies that models are updated as the project artifacts are developed and their metrics become available. Our results show that models benefit from the increased size of the training data as well as from the ability to combine design and code metrics.

The remainder of the paper is organized as follows. Section 2 describes the data sets and metrics used in the study. Section 3 outlines the experimental design in terms of the chosen modeling techniques, selected evaluation methods, and the statistical testing procedure. Section 4 presents the experimental results and discusses their implications. Section 5 provides a background of related work, and Section 6 concludes with a summary and future work.

Table 1 Datasets used in this study

Data	Modules	% Faulty	# Metrics			Project description	lang.
			all	design	code		
CM1	505	16.04%	40	16	20	Spacecraft instrument	C
KC1	2,407	13.9%	21	4	17	Storage management for receiving/processing ground data	C++
KC3	458	6.3%	40	16	20	Storage management for ground data	Java
KC4	125	48%	40	16	20	A ground-based subscription server	Perl
PC1	1,107	6.59%	40	16	20	Flight software from an earth orbiting satellite	C
PC3	1,563	10.43%	40	16	20	Flight software for earth orbiting satellite	C
PC4	1,458	12.24%	40	16	20	Flight software for earth orbiting satellite	C
MW1	433	6.7%	40	16	20	A zero gravity experiment related to combustion	C
MC2	161	32.30%	40	16	20	A video guidance system	C++
JM1	10,878	19.3%	21	4	17	A real time predictive ground system	C
MC1	9,466	0.64%	39	15	20	A combustion experiment of a space shuttle	(C)C++
PC2	5,589	0.42%	40	16	20	Dynamic simulator for attitude control systems	C
PC5	17,186	3.00%	39	15	20	A safety enhancement of a cockpit upgrade system	C++
ar4	107	18.69%	29	9	18	Control software for washing machines	C

2 Metrics Description

2.1 Used metrics

The data sets used in this study originate from the Metrics Data Program (MDP) repository [3] and PROMISE repository [9]. The fourteen projects shown in Table 1 are used in the experiments.

These data sets offer module metrics that describe 14 diverse projects. Thirteen come from NASA MDP repository and $ar4$ comes from PROMISE repository [9]. These are highly diverse projects. NASA typically subcontracts software development and it is likely that no two projects originate from the same site or development organization. The criticality of these projects varies from low cost utilities to highly sensitive mission critical applications [27]. Their only similarity is that they are developed for a specific purpose and, while some level of software reuse is likely, there is no information regarding previous releases. $ar4$ does not come from NASA.

Projects JM1 and KC1 offer 24 total metrics (model attributes), MC1 and PC5 have 42 total attributes, while the remaining 9 data sets have 43 metrics describing individual modules. MDP data sets contain a $module\_id$ field and two error-related attributes: $error\_count$ and $error\_density$. We removed $module\_id$ and $error\_density$ attributes prior to modeling. The $error\_count$ attribute we converted into a boolean attribute called $DEFECT$. If the $error\_count$ attribute is greater than or equal to 1, then the value of $DEFECT$ is $TRUE$, otherwise it is $FALSE$. $DEFECT$ becomes the predicted variable. After removing and replacing these attributes, JM1 and KC1 have 21 attributes that can be used as independent (predictor) variables, MC1 and PC5 have 39, the other data sets from MDP have 40. Project $ar4$ has 29 attributes.

The metrics shown in Table 2 have been extracted using McCabe IQ 7.1, a reverse engineering tool that derives software quality metrics from code, visualizes flowgraphs and generates report documents [2]. It is important to note that McCabe IQ reverse engineers design metrics from code flowgraphs, rather than from design documentation. It is not unusual to analyze software design quality from design artifacts reengineered from the code [5,6,11,12,49,50]. However, we recognize that this fact is one of the validity threats for our experiment and will be discussed later.

In all the data sets available metrics are classified into three groups: $design$, $code$, and $other$, as indicated in Table 2. What separates design metrics from code metrics is the opportunity to extract them from design specification diagrams such as UML. For example, Ohlsson and Alberg extract design metrics from Formal Description Language (FDL) graphs [43]. Their design metrics include $node\_count$, $branch\_count$, and McCabe cyclomatic complexity measures [33], also produced by McCabe IQ tool. McCabe complexity metrics are also used as design metrics in [56], the study that provides motivation and a point of comparison for this research. As a quick reminder, if graph G represents module's flowgraph, its cyclomatic complexity $v(G)$ is calculated as $v(G) = e - n + 2$, where e is the number of edges and n is the number of nodes.

The code metrics in our study are the features that can only be extracted from the source code. Static code metrics, such as $num\_operators$, $num\_operands$, and Halstead metrics are calculated from program statements [19]. In this paper, $other$ metrics are those related to both the design and code. Most data sets have four metrics we classified as $other$. We do not use $other$ metrics in isolation to build models, but we include them in the experiments in which fault prediction models are developed from all available attributes.

$ar4$ is the only data set that does not come from the MDP collection. This data set has 29 attributes including nine $design$ metrics, eighteen $code$ metrics, and two metrics classified as $other$. Although the names of some of the $ar4$ metrics are different from MDP, they in fact are equivalent to a subset of metrics used in the MDP repository. For example, metrics "total_loc", "unique_operands", and "halstead_time" in $ar4$ correspond to "loc_total","num_operands", and "halstead_prog_time" in MDP. The metrics describing $ar4$ project are presented in bold font in Table 2.

3 Experimental Design

As a reminder to readers, the goal of our experiments is support for the conjuncture that the incremental development of fault prediction models throughout the design and implementation phases of the life cycle. There are two interesting aspects of incremental model development:

1. Utilization of the increasing size of the training data set, and
2. Timely (sequential) utilization of design and code metrics.

To reach these goals, we will compare the performance of models derived from:

- different percentages of data for model development (training): 10%, 25%, 50%, 75%, and 90%. In each experiment, the reminder of the data set will be used for model evaluation.
- different metric groups: $design$, $code$, and all.

The data sets we used do not include development schedule information. Therefore, we cannot faithfully emulate the actual "arrival" schedule of modules and their metrics. However, we will deploy cross-validation, the statistical practice of randomly partitioning a sample of data into two subsets: training and testing subset ten times. Reporting the median result from the ten experiments should minimize the impact a development sequence could have on the prediction results. The predicted variable is $DEFECT$, that is, the models predict whether a module is likely to contain fault(s). As mentioned earlier, we will deploy five well known classification algorithms for software quality prediction, listed in Table 3. These classification algorithms have consistently been recommended to practitioners and

Table 2 Software metrics used in this study

group	metrics	description or formula
code	**parameter_count**	Number of parameters to a given module
	num_operators:N1	The number of operators contained in a module
	num_operands:N2	The number of operands contained in a module
	num_unique_operators:μ_1	The number of unique operators contained in a module
	num_unique_operands:μ_2	The number of unique operands contained in a module
	halstead_content:μ	The halstead length content of a module $\mu = \mu_1 + \mu_2$
	halstead_length:N	The halstead length metric of a module $N = N_1 + N_2$
	halstead_level:L	The halstead level metric of a module $L = \frac{(2*\mu_2)}{\mu_1 * N_2}$
	halstead_difficulty:D	The halstead difficulty metric of a module $D = \frac{1}{L}$
	halstead_volume:V	The halstead volume metric of a module $V = N * \log_2(\mu_1 + \mu_2)$
	halstead_effort:E	The halstead effort metric of a module $E = \frac{V}{L}$
	halstead_prog_time: T	The halstead programming time metric of a module $T = \frac{E}{18}$
	halstead_error_est: B	The halstead error estimate metric of a module $B = \frac{E^{2/3}}{1000}$
	number_of_lines	Number of lines in a module
	loc_blank	The number of blank lines in a module
	loc_code_and_comment:NCSLOC	The number of lines which contain both code and comment in a module
	loc_comments	The number of lines of comments in a module
	loc_executable	The number of lines of executable code for a module (not blank or comment)
	percent_comments	Percentage of the code that is comments
	loc_total	The total number of lines for a given module
design	edge_count:e	Number of edges found in a given module from one module to another
	node_count:n	Number of nodes found in a given module
	branch_count	Branch count metrics
	call_pairs	Number of calls to functions in a module
	condition_count	Number of conditionals in a given module
	cyclomatic_complexity: v(G)	The cyclomatic complexity of a module $v(G) = e - n + 2$
	decision_count	Number of decision points in a module
	decision_density	$Condition\_count/Decision\_count$
	design_complexity:iv(G)	The design complexity of a module
	design_density	Design density is calculated as: $\frac{iv(G)}{v(G)}$
	essential_complexity:ev(G)	The essential complexity of a module
	essential_density	Essential density is calculated as: $\frac{(ev(G)-1)}{(v(G)-1)}$
	maintenance_severity	Maintenance Severity is calculated as: $\frac{ev(G)}{v(G)}$
	modified_condition_count	The effect of a condition affect a decision outcome by varying that condition only
	multiple_condition_count	Number of multiple conditions within a module
	pathological_complexity	A measure of the degree to which a module contains extremely unstructured constructs
others	**normalized_cylomatic_complexity**	$\frac{v(G)}{number\_of\_lines}$
	global_data_complexity:gdv(G)	the ratio of cyclomatic complexity of a module's structure to its parameter_count
	global_data_density	Global Data density is calculated as: $\frac{gdv(G)}{v(G)}$
	cyclomatic_density	$\frac{v(G)}{NCSLOC}$

provide adequate performance [30,25,34,20,26]. More importantly, the classifiers are implemented in publicly available machine learning toolkit Weka [42]. The use of multiple machine learning models allows us to compare the results with the recent study conducted by Lessmann $et.\ al.$ [30], in which they compare the performance of two dozen classification algorithms on MDP data sets. We use the default parameters in all the classifiers except in Random Forest, in which we follow the recommendation of algorithm's creator L. Breiman [10] and use 500 tree ensemble (rather than the Weka default of 10 trees, which optimizes run-time).

Table 3 Classification algorithms used in the study.

Classifier	**Abbreviation**
Random Forest	rf
Bagging	bag
Logistic regression	lgi
Boosting	bst
Naivebayes	nb

In total, we conducted $10,500$ experiments utilizing *14 data sets*, *5 different sizes of training subset*, *3 metric groups*, *10 cross validation runs*, all of these repeated using *5 classification algorithms*.

3.1 Model Evaluation and Comparison

Conducting such a large number of experiments, we have to define the criteria for their evaluation and comparison. For model evaluation, we decided to use Receiver Operating Characteristic (ROC) curves. Model performance will be visually compared in Box-plot diagrams, followed by a rigorous nonparametric statistical significance testing.

ROC curves provide an intuitive way to evaluate the classification performance of different models. An ROC curve is a plot of the Probability of Detection (pd) as a function of the Probability of False alarm (pf) across all the possible experimental threshold settings. Many classification algorithms allow users to define and adjust the threshold parameter in order to generate an appropriate classifier [54]. When modeling software quality, a higher pd can be produced at the cost of increased pf and vice versa. A typical ROC curve has a concave shape with (0,0) as the beginning and (1,1) as the end point. Figure 1 shows three example ROC curves representing models built using all, $code$, and $design$ metric sets over the data set MW1 with 90% data as training subset. The legend provides a link between the classification algorithm and individual curves.

The **A**rea **U**nder the ROC **C**urve, referred to as *AUC*, is a numeric performance evaluation measure directly associated with an ROC curve. It is very common to use AUC to compare the performance of different classifiers. From Figure 1, we can see that the performance of $design$ metrics is the worst among the three. And the performance of all and $code$ metric models are tangled making it difficult to tell which one is better. The values of AUCs provide a direct answer: the AUCs of all, $code$, and $design$ are 0.840, 0.824, and 0.782, respectively. The differences among the values are small: $all - code = 0.016$, $code - design = 0.042$, $all - design = 0.058$. Thus, without investigating the significance of our observations, we conclude that $all > code > design$.

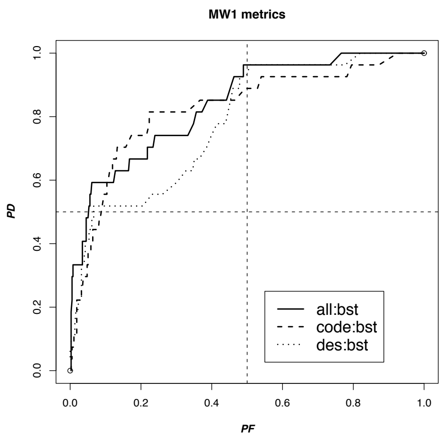


Fig. 1 ROC curves of MW1, developed using three different metric groups.

The area under the curve of the upper left corner of ROC, referred as AUC_{UL}, also provides a meaningful performance index for software engineering studies [26]. From AUC, we know that the performance of all is better than that of $code$. Looking closely at Figure 1, we can see in the region of the upper left hand corner of ROC, it seems that the performance of $code$ metrics is better than that of all. The values of AUC_{UL} are 0.429, 0.476 and 0.247 for all, $code$ and $design$ groups respectively. Using AUC_{UL}, we have $code > all > design$. Using AUC_{UL} as the measurement for the performance in this case appears meaningful. For this reason, we decided to use both AUC and AUC_{UL} as measurements in our studies. We calculate AUC and AUC_{UL} using the trapezoid rule.

Due to 10-way cross validation, comparing a large number of ROC curves becomes difficult. For this reason, we visualize the ten corresponding values of AUC (or AUC_{UL}) in a Box-plot diagram.

A Box-plot, also known as a box and whisker diagram, graphically depicts numerical data distributions using the five first order statistics: the smallest observation, lower quartile (Q1), median, upper quartile (Q3), and the largest observation. The box is constructed based on the interquartile range (IQR) from Q1 to Q3. The line inside the box depicts the median which follows the central tendency. The whiskers indicate the smallest observation and the largest observation. Figure 2 shows an example Box-plot of three groups of metrics on MW1 data set using 90% data as training subset. The models developed using all metrics have the best performance (the largest median values of AUCs) while the performance of models from $design$ metrics group is the worst of the three.

3.2 Statistical Significance Test Procedure

We will use statistical hypothesis testing and a rigorous statistical significance test procedure to understand the outcome of our fault prediction experiments. Our hypotheses are:

H_0: *There is no difference in the performance of fault prediction models resulting from the* 5 *different sizes of training subsets (or among the* 3 *different metrics groups).*
vs.

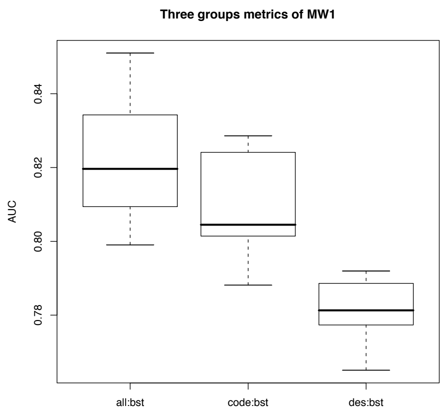


Fig. 2 Boxplots of MW1 data set

H_α: *At least two different sizes of training subset(or two different metrics groups) result in fault prediction models that have significantly different performance.*

Model performance, as described above, is measured through the two similar Area Under the ROC Curve parameters. To address these hypotheses, we will use "multiple hypothesis testing" procedure. Multiple hypothesis testing (also called multiple comparisons) is a well-known procedure. The critical problem is to control the "family-wise error", that is, to control Type I error during the comparison. There are two methods to test the significant differences among multiple samples: parametric analysis of variance (ANOVA) and and its nonparametric counterpart, the Friedman test.

In [17], Demsar overviewed the theoretical work on statistical tests for the multiple comparisons problems in machine learning. Whenever the comparison includes more than two samples, Demsar recommends the Friedman test with the corresponding post-hoc Nemenyi test. Demsar advocates these tests largely due to the fact that nonparametric procedures make less stringent demands on the data. However, nonparametric tests do not utilize all the information available, as the actual data values (in our case AUC or AUC_{UL}) are not used in the test procedure. Instead, the ranks of the observations are used.

The Friedman test ranks the performance of samples. For example, in Table 4, the best performing sample receives the rank 1, the second best is ranked 2, etc. In case of ties, average ranks are assigned to both samples. For example, in the first row of Table 4 the last two cells have the same AUC values. Unable to rank them as 1 and 2, they are assigned the average rank of $\frac{1+2}{2} = 1.5$.

Let k represent the number of different samples. For example, $k = 5$ represents the five sizes of training subsets. Let N represent the number of different experiments which utilize the samples. In our case $N = 5$ represents the five classification algorithms used for building fault prediction models. R_j is then the average rank of the size of a training subset j over different classifiers. This rank is in the last row of Table 4). The Friedman statistic test is distributed according to the F-distribution:

$$F_F = \frac{(N-1)\chi_F^2}{N(k-1) - \chi_F^2}, \tag{1}$$

Table 4 Comparison of fault prediction models on PC3 *design* metrics. The values inside the parentheses are the ranks.

	10%	25%	50%	75%	90%
bag	0.66(5)	0.704(4)	0.733(3)	0.734(1.5)	0.734(1.5)
bst	0.659(5)	0.698(4)	0.724(3)	0.728(2)	0.736(1)
log	0.625(5)	0.704(4)	0.727(3)	0.734(2)	0.738(1)
nb	0.635(5)	0.688(3)	0.682(4)	0.695(2)	0.703(1)
rf	0.676(5)	0.694(4)	0.728(2)	0.721(3)	0.733(1)
rank	5.0	3.8	3.0	2.1	1.1

with $k-1$ and $(k-1)(N-1)$ degrees of freedom where $\chi_F^2 = \frac{12N}{k(k+1)}[\Sigma_j R_j^2 - \frac{k(k+1)^2}{4}]$. The critical value of F-distribution can be found in any statistical book.

If the null hypothesis of Friedman test is rejected, the Nemenyi test is further used as the after-the-fact test. The performance of two samples is significantly different if their average ranks differ by at least the Critical Difference, denoted by CD:

$$CD = q_\alpha \sqrt{\frac{k(k+1)}{6N}}, \tag{2}$$

where q_α is the critical value of the Studentized range statistic divided by $\sqrt{2}$, provided in Demsar's paper [17].

The procedure is illustrated through an example as follows. Table 4 lists median values of AUC from models built from five different sizes as training subset(10%, 25%, 50%, 75% and 90%) using five classification algorithms using project PC3 design metrics. The rank of each cell is indicated inside the parenthesis along with the actual AUC value. The average rank is listed in the last row. In the Friedman test, we use ranks instead of the actual values.

In this paper, we use the 95% confidence interval ($\alpha = 0.05$) as a threshold to judge the significance. The Friedman test and the Nemenyi test are implemented in the statistical package R (http://www.r-project.org/). First, the Friedman test calculates the F distribution:
$\chi_F^2 = \frac{12*5}{5(5+1)}[(5.0^2 + 3.8^2 + 3.0^2 + 2.1^2 + 1.1^2 - \frac{5(5+1)^2}{4})] = 18.12$
$F_F = \frac{(5-1)\chi_F^2}{5(5-1)-\chi_F^2} = \frac{(5-1)*18.12}{5(5-1)-18.12} = 38.55$.
With five training subsets and 5 classifiers, F_F is distributed according to the F distribution with $5-1=4$ and $(5-1)(5-1)=16$ degrees of freedom. The critical value of $F(4,16)$ for $\alpha = 0.05$ is 3.01. Because $3.01 < 38.55$, we reject the null-hypothesis and conclude that there is significant difference among models developed from different sizes of the training data on PC3. The critical value of q_α is 2.728 [17], consequently:
$CD = q_\alpha\sqrt{\frac{k(k+1)}{6N}} = 2.728\sqrt{\frac{5(5+1)}{6*5}} = 2.728$.

Figure 3 shows the results of the Nemenyi test. The numbers in the scale represent the average ranks: the higher the rank, the worse the performance of the model based on the given training sample. Not surprisingly, on PC3 data, the performance of fault prediction models increases with the size of the training subset. When the difference between the average ranks of two samples is smaller than the value of $CD = 2.728$, the difference in their performance is not significant. This is indicated by straight line connections, which indicate performance $clusters$. From Figure 3, we can identify two $clusters$, one includes 10%, 25% and 50% models; the other includes 25%, 50%, 75%, and 90%.

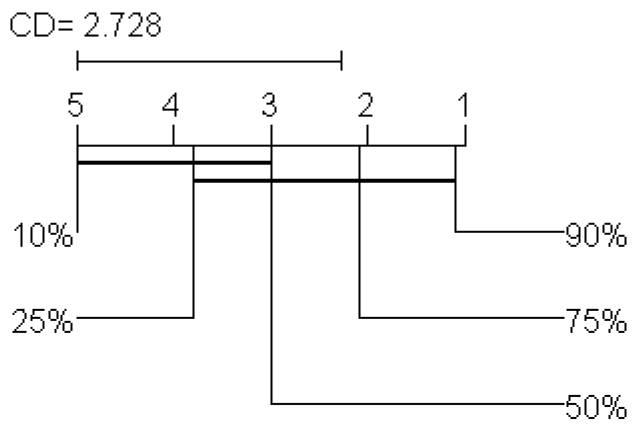


Fig. 3 Comparison of models which use different training subset sizes on PC3 design metrics, using Demsar's procedure.

4 Experimental Results

4.1 Increasing the Size of Training Set

One of the questions that repeatedly surfaces in discussions about fault prediction modeling deals with the amount of data needed to build reasonably accurate models. The question is not critical when models from earlier releases are used in the quality assurance of the new version or product release. Presumably, such systems are part of the product line and fault prediction models from earlier releases are adequate for the new release [44]. However, when an organization develops one-of-a-kind system or the first system release with no substantial history (and limited reuse), the amount of data needed to develop the model is the real issue in practice. This is certainly the case for most of the projects described in the NASA MDP repository. We will not directly address the "minimal" data set requirement for model development. Rather, following the idea of incremental model development, we would like to know the rate of model improvement as additional modules and their metrics descriptors become available.

For these reasons, we compare the performance of models generated using five training subset, containing 10%, 25%, 50%, 75%, and 90% of project modules. Although these proportions represent different sizes of training data for different projects, they match realistic milestones in the development life cycle.

We will test the following statistical hypotheses:
H_0: *The size of the model's training set has no influence on model performance*
vs.
H_α: *Some (at least two) models developed using different training subset sizes result in significantly different model performance.*

For this experiment, we utilize 14 data sets, 5 different training subsets, 3 groups of metrics, 5 classifiers and two measurement methods of AUC and AUC_{UL}, summarized in 420 box-plot diagrams. Each box-plot diagram reports the median result from 10 cross validation repetitions of each experiment. Due to the sheer volume of information, $Demsar's$

statistical analysis procedure [17] will provide acceptable summarization of results. The results are shown separately for models that utilize *design* metrics only, *code* metrics only, and *all* metrics.

The general trends to be reported are not surprising:

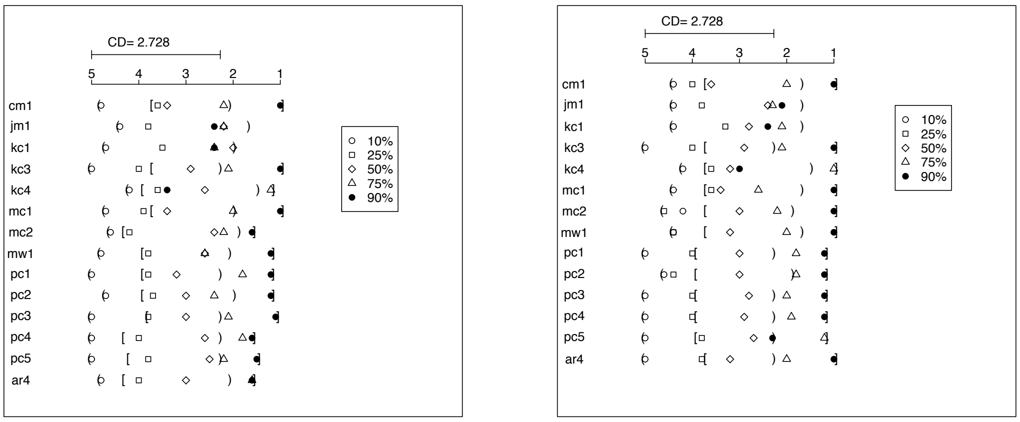
- 10% training subset results in the weakest performance.
- In most cases, the "best" performance can be expected from models which use 90% of data for training. Unfortunately, these models are the least useful as only 10% of the modules are left for fault prediction.
- Models increase their performance as the size of the training data set grows. However, these increases are rather minimal and statistical significance must be considered.

A more detailed analysis of the experimental results, including their statistical significance and practical implications follows.

4.1.1 Design metrics models

We test the null hypothesis first on fault prediction models that use only *design* metrics.

In Section 3.2, Figure 3 depicts an example of statistical testing procedure applied to the five models developed from the design metrics of PC3 project. We applied the the same statistical procedure to the design metric fault prediction models of the remaining 13 projects. To avoid visual clutter when presenting the results, we modified the ranking diagram of Figure 3. Figure 4 presents the ranking of models for all projects.



(a) AUC as measurement **(b) AUC_{UL} as measurement**

Fig. 4 The Friedman test on *design* metrics models using (a) AUC and (b) AUC_{UL} for performance evaluation

Panel (a) presents the model rankings using AUC as performance measure, while panel (b) uses AUC_{UL} for performance evaluation. CD represents the critical difference value for this statistical test: if the difference between two ranks is greater than the value of CD, they are statistically different, otherwise, they are not.

Referring to the graphic in Figure 4 the higher the rank (on a horizontal scale from 1 to 5) the worse the performance of the model. To account for the statistical significance, we enclose the five models from each project into the two pairs of brackets. Each bracket pair encloses the distance equivalent to the value of $CD = 2.728$. The round brackets enclose models which form a performance cluster with the worst performing model (typically the model which trains from the 10% subset). We will call this cluster the *lower rank cluster*. The square brackets enclose the performance cluster which includes the best performing model (typically inferred from the 90% subset). This will be our *higher rank cluster*. These bracket pairs replace the horizontal lines from Figure 3.

Let us look closely into the performance of the five models built from $cm1$ in Figure 4(a). For the increasing sizes of training subsets, the corresponding ranks are 4.8, 3.6, 3.4, 2.2, and 1. These ranks form two performance clusters. Models built from 25%, 50%, and 75% subsets are in the intersection between the lower and higher performance rank clusters. The fault prediction models for projects $jm1$ and $kc1$ exhibit no statistically significant differences regardless of the size of the training subset. Therefore, they are all included in a single performance rank cluster. The models for all other projects form two performance rank clusters. It is also interesting to note that in project $kc4$, the best performing model is developed using 75% of modules for training. In smaller projects ($kc4$ contains only 125 modules), training from 90% may result in over-fitting.

The summary of $design$ metrics based models evaluated through AUC_{UL}, from Figure 4(b), is similar:

1. In 2 data sets, jm1 and kc1, there is no significant difference amongst the 5 models.
2. In 6 data sets the lower cluster includes models built from 10% to 50% of data. These projects are $kc3, pc1, pc2, pc3, pc4, ar4$.
3. In 4 data sets the lower cluster includes models built from 10% to 75% of data. These projects are $cm1, mc1, mc2$, and $mw1$.
4. In $kc4$ and $pc5$, models which used 75% of data for training ranked the best.
5. In 3 data sets the higher cluster includes models built from 25% to 90% of data. These projects are $kc4, mc1$, and $pc5$.
6. In 9 data sets the higher cluster includes models built from 50% to 90% of data. These projects are $cm1, kc3, mc2, mw1, pc1, pc2, pc3, pc4$, and $ar4$.

These results indicate that the model evaluation using AUC and AUC_{UL} are, generally, very similar. Minor differences appear in average ranks and clustering, but they are not likely to impact our conclusions. Further, there are never more than two performance clusters. Therefore, more detailed result comparisons in Table 5 and in Figure 5 will describe only 10%, 50%, and 90% training subsets, and use AUC for model evaluation.

Table 5 shows median values of AUC and variances for 10%, 50%, and 90% training subsets. The median value is rounded to two digits, variance to 4 digits. The last three columns show the percentage increase in model performance. If the performance increases, the value positive. Table 5 provides a closer look into the actual performance differences between models. The gains in fault prediction vary from project to project and they are generally difficult to anticipate. Clearly, the significance results indicate that building only one or two models over the development life cycle, given that only design metrics are utilized, should be sufficient. Figure 5 visualizes the corresponding box-plot diagrams.

Table 5 Median and variance of 10%, 50%, and 90% training subset models from $design$ metrics, measured by AUC

data	$m_{10\%}$	$v_{10\%}$	$m_{50\%}$	$v_{50\%}$	$m_{90\%}$	$v_{90\%}$	$\frac{m_{50\%}}{m_{10\%}} - 1$	$\frac{m_{90\%}}{m_{10\%}} - 1$	$\frac{m_{90\%}}{m_{50\%}} - 1$
cm1	0.54	0.0035	0.57	0.002	0.6	0.0133	5.56%	11.11%	5.26%
jm1	0.66	9e-04	0.67	8e-04	0.66	0.0013	1.52%	0	-1.49%
kc1	0.72	0.0027	0.73	9e-04	0.73	0.0028	1.39%	1.39%	0
kc3	0.61	0.0194	0.74	0.0121	0.77	0.017	21.31%	26.23%	4.05%
kc4	0.75	0.0109	0.78	0.0017	0.76	0.0175	4%	1.33%	-2.56%
mc1	0.53	0.0054	0.62	0.0091	0.68	0.0214	16.98%	28.3%	9.68%
mc2	0.6	0.0087	0.63	0.0035	0.63	0.0271	5%	5%	0
mw1	0.66	0.0163	0.76	0.0038	0.78	0.0282	15.15%	18.18%	2.63%
pc1	0.57	0.0067	0.66	0.0028	0.69	0.0118	15.79%	21.05%	4.55%
pc2	0.5	0.0269	0.67	0.0174	0.75	0.0503	34%	50%	11.94%
pc3	0.65	0.0033	0.73	8e-04	0.73	0.0041	12.31%	12.31%	0
pc4	0.73	0.0029	0.78	0.001	0.78	0.0036	6.85%	6.85%	0
pc5	0.94	9e-04	0.95	1e-04	0.95	3e-04	1.06%	1.06%	0
ar4	0.62	0.0185	0.7	0.0085	0.72	0.0803	12.9%	16.13%	2.86%

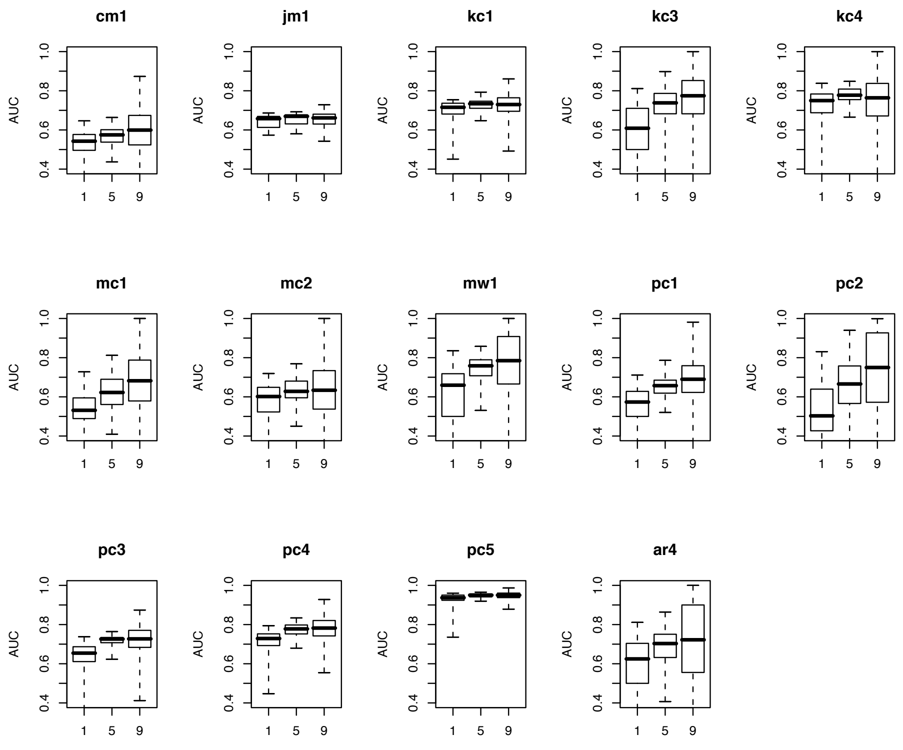
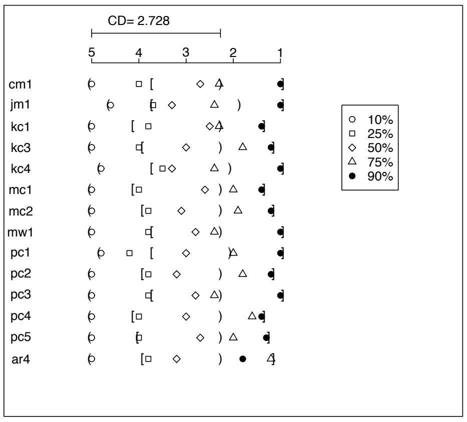
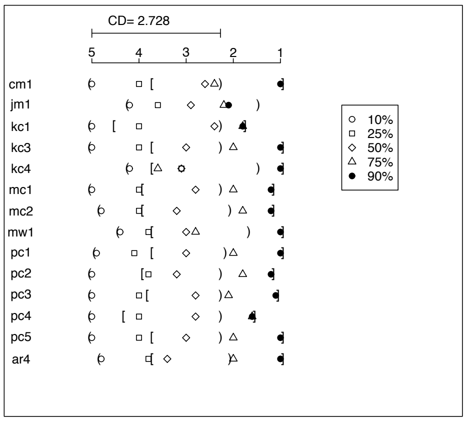


Fig. 5 Box-plots of 10%, 50%, and 90% training subset models built from $design$ metrics, measured by AUC. On x-axis, “1” stands for 10%; “5” stands for 50%; “9” stands for 90%.

4.1.2 Code metrics models



(a) AUC



(b) AUC_{UL}

Fig. 6 The Friedman test on *code* metrics models evaluated using (a) AUC and (b) AUC_{UL}

We repeated the same statistical analysis procedure for the models derived from code metrics.

Using AUC (AUC_{UL}) as measurement, from Figure 6, we can see that:

1. For 8 (10) data sets lower cluster includes models developed from 10% to 50% of modules.
2. For 6 (3) data sets lower cluster includes models developed from 10% to 75% or modules.
3. For 5 (9) data sets the high cluster includes models developed from 50% to 90%.
4. For 9 (4) data sets the high cluster includes models developed from 25% to 90%.
5. In $jm1$, all five models are included in the same performance cluster

Similar to $design$ metrics based models, $code$ metrics models evaluated through AUC and AUC_{UL} result only in minor differences in ranks and clustering.

Table 6 shows median values of the AUC and variances for the models built from 10%, 50%, and 90% training subsets, as well as their comparison. Figure 7 shows the corresponding box-plot diagrams. We note that performance increases due to the growing size of training samples in $code$ metrics based fault prediction models are more modest than in case of models built from $design$ metrics. However, the magnitude of performance improvement varies between different projects and is difficult to anticipate.

Table 6 AUC median and variance for models built from 10%, 50%, and 90% of modules for training using $code$ metrics

data	$m_{10\%}$	$v_{10\%}$	$m_{50\%}$	$v_{50\%}$	$m_{90\%}$	$v_{90\%}$	$\frac{m_{50\%}}{m_{10\%}} - 1$	$\frac{m_{90\%}}{m_{10\%}} - 1$	$\frac{m_{90\%}}{m_{50\%}} - 1$
cm1	0.61	0.0054	0.69	0.0019	0.72	0.0112	13.11%	18.03%	4.35%
jm1	0.69	1e-04	0.72	2e-04	0.72	7e-04	4.35%	4.35%	0
kc1	0.77	0.0011	0.8	3e-04	0.8	0.0017	3.9%	3.9%	0
kc3	0.68	0.0212	0.78	0.0041	0.84	0.0273	14.71%	23.53%	7.69%
kc4	0.57	0.0088	0.62	0.0082	0.65	0.024	8.77%	14.04%	4.84%
mc1	0.79	0.015	0.92	0.0065	0.94	0.0092	16.46%	18.99%	2.17%
mc2	0.61	0.0066	0.67	0.0032	0.7	0.0224	9.84%	14.75%	4.48%
mw1	0.71	0.016	0.79	0.0047	0.84	0.0281	11.27%	18.31%	6.33%
pc1	0.7	0.0103	0.8	0.0037	0.83	0.0113	14.29%	18.57%	3.75%
pc2	0.59	0.0341	0.8	0.0139	0.89	0.0338	35.59%	50.85%	11.25%
pc3	0.75	0.0025	0.81	5e-04	0.82	0.0036	8%	9.33%	1.23%
pc4	0.84	0.0046	0.9	0.0036	0.9	0.0055	7.14%	7.14%	0
pc5	0.93	5e-04	0.95	3e-04	0.95	5e-04	2.15%	2.15%	0
ar4	0.7	0.0293	0.79	0.0089	0.81	0.0853	12.86%	15.71%	2.53%

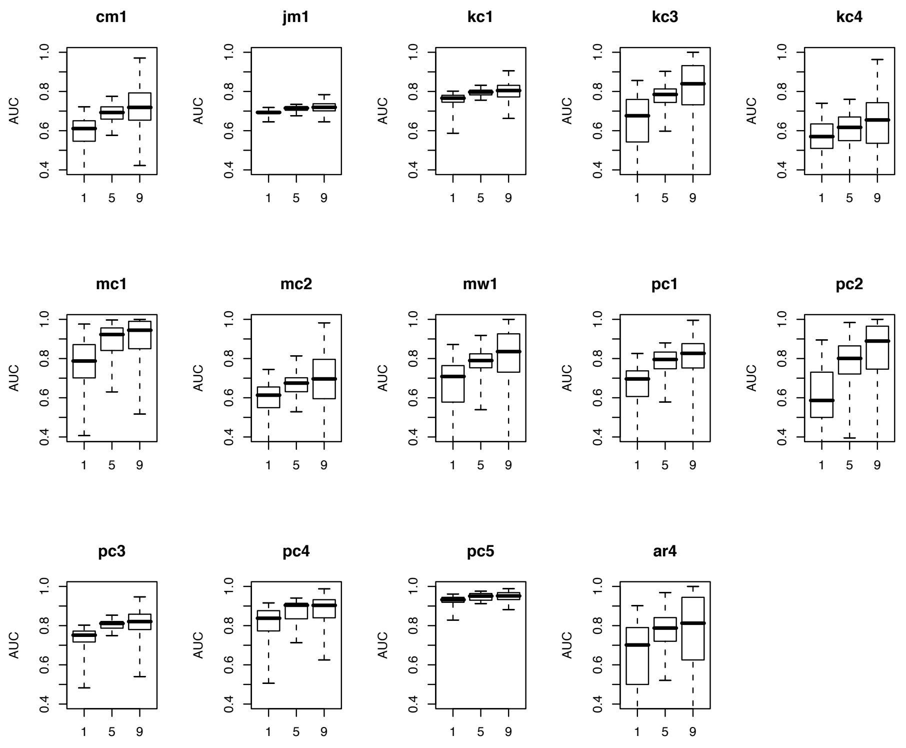
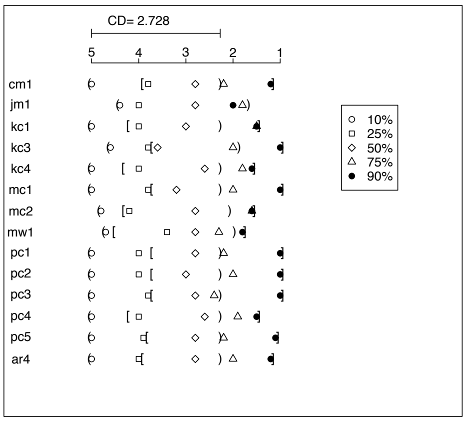
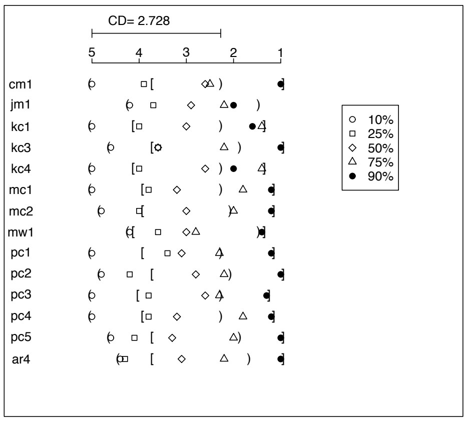


Fig. 7 Box-plot diagrams of fault prediction models built from 10%, 50%, and 90% of data using $code$ metrics, measured by AUC. On x-axis, "1" stands for 10%; "5" stands for 50%; "9" stands for 90%.





(a) AUC as measurement **(b) AUC_{UL} as measurement**

Fig. 8 The Friedman test on all metrics (a) AUC and (b) AUC_{UL}

4.1.3 All metrics

All metrics refers to the entire set of module attributes available for each fault prediction data set. These attributes include design and code metrics, as well as some software measures that combine them.

The results of the ranking analysis for the incremental learning experiment with all metrics, using AUC (and AUC_{UL}) as performance evaluation measures, are shown in Figure 8(a) and (b). A quick summary follows:

1. Regardless of the evaluation technique, $jm1$ does not show statistically significant difference for any training method.
2. There are 10 (5) data sets for which the lower cluster includes models trained from 10%, to 50% subsets of available modules.
3. There are 3 (8) data sets for which the lower cluster includes models trained from 10%, to 75% subsets of available modules.
4. There are 7 (5) data sets for which the higher performance cluster includes models trained from 50%, to 90% subsets of available modules.
5. There are 6 (8) data sets for which the higher performance cluster includes models trained from 25%, to 90% subsets of available modules.

Table 7 shows median values of the AUC and variances for the models built from 10%, 50%, and 90% training subsets, as well as their comparison. Figure 9 shows the corresponding box-plot diagrams. An interesting observation inferred from Table 7 is that in case of a comprehensive metrics attributes, fault prediction models do not degrade when the size of the training set grows. This is similar to $code$ metrics models, but different from models which use $design$ information only.

The analysis of all the experiments leads to the following perspective. Regardless of the type of metrics used for model development ($design$, $code$, all), fault prediction models

Table 7 AUC median and variance for models built from 10%, 50%, and 90% of modules for training using all metrics

data	$m_{10\%}$	$v_{10\%}$	$m_{50\%}$	$v_{50\%}$	$m_{90\%}$	$v_{90\%}$	$\frac{m_{50\%}}{m_{10\%}} - 1$	$\frac{m_{90\%}}{m_{10\%}} - 1$	$\frac{m_{90\%}}{m_{50\%}} - 1$
cm1	0.61	0.0054	0.69	0.0019	0.72	0.0112	13.11%	18.03%	4.35%
jm1	0.69	1e-04	0.72	2e-04	0.72	7e-04	4.35%	4.35%	0
kc1	0.77	0.0011	0.8	3e-04	0.8	0.0017	3.9%	3.9%	0
kc3	0.68	0.0212	0.78	0.0041	0.84	0.0273	14.71%	23.53%	7.69%
kc4	0.57	0.0088	0.62	0.0082	0.65	0.024	8.77%	14.04%	4.84%
mc1	0.79	0.015	0.92	0.0065	0.94	0.0092	16.46%	18.99%	2.17%
mc2	0.61	0.0066	0.67	0.0032	0.7	0.0224	9.84%	14.75%	4.48%
mw1	0.71	0.016	0.79	0.0047	0.84	0.0281	11.27%	18.31%	6.33%
pc1	0.7	0.0103	0.8	0.0037	0.83	0.0113	14.29%	18.57%	3.75%
pc2	0.59	0.0341	0.8	0.0139	0.89	0.0338	35.59%	50.85%	11.25%
pc3	0.75	0.0025	0.81	5e-04	0.82	0.0036	8%	9.33%	1.23%
pc4	0.84	0.0046	0.9	0.0036	0.9	0.0055	7.14%	7.14%%	0
pc5	0.93	5e-04	0.95	3e-04	0.95	5e-04	2.15%	2.15%	0
ar4	0.7	0.0293	0.79	0.0089	0.81	0.0853	12.86%	15.71%	2.53%

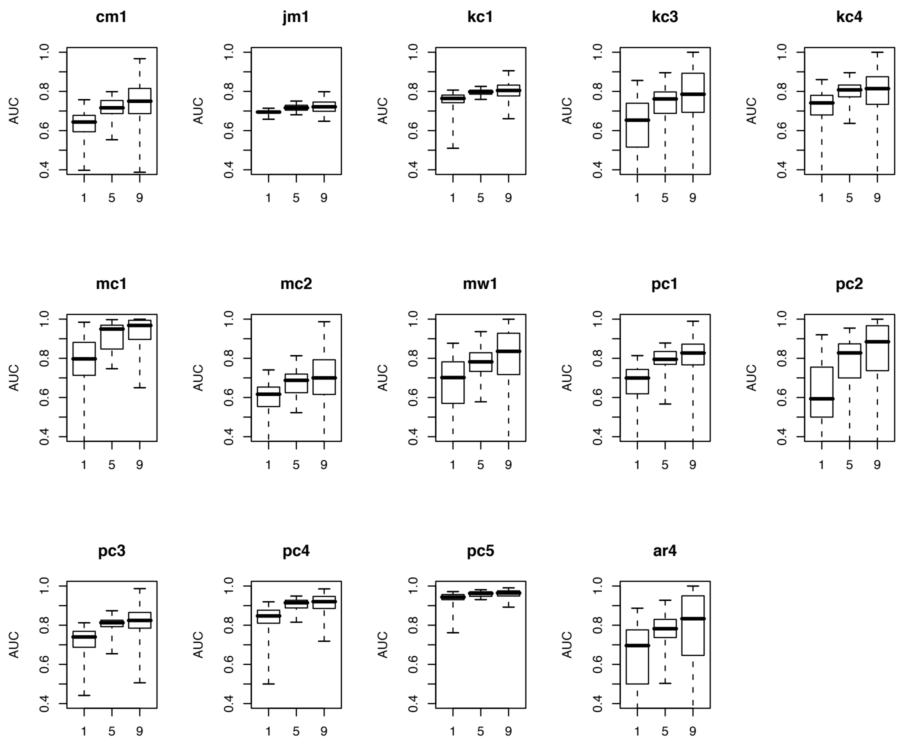


Fig. 9 Box-plot diagrams of fault prediction models built from 10%, 50%, and 90% of data using all metrics, measured by AUC. On x-axis, “1” stands for 10%; “5” stands for 50%; “9” stands for 90%.

should be built as early as an initial set of modules becomes available. Using 10% of project modules for this purpose results in models that are statistically as good as those developed from 50% or sometimes even 75% of project modules. Larger training set generally helps improve model performance, but the cost of incremental model rebuilding should be compared with the performance benefits. When justified, model rebuilding does not need to be a continual or a frequent activity. Rather, models could be built early and, possibly, updated at the mid point of the development.

These experiments further revealed that, in general, AUC and AUC_{UL} offer similar performance evaluation indices. For this reason, in the remaining experiments, we will only report AUC.

4.2 Comparison of $design$, $code$, and all metrics models

The increase in size of available data for model definition is only one aspect in the incremental development of fault prediction models. The other opportunity comes from the fact that design artifacts and their metrics are typically available before the modules are implemented and code metrics can be computed. In rapid prototyping or agile processes, a few modules will be developed, implemented and tested in the first few process cycles. Their fault proneness status can be used to build models predicting the quality of design or code artifacts. Therefore, the question of comparing the performance of fault prediction models built from different types of metrics is important.

To guide statistical analysis comparing $design$, $code$, and all metrics based models, we define the following hypotheses:
H_0: *There is no difference in the performance of fault prediction of models developed using the three metrics groups.*
vs.
H_α: *Fault prediction models built from (at least) one of the three metrics groups offer significantly different performance.*

The lesson learned in the previous section is that fault prediction models do not need frequent updates. For this reason, we decided it will be sufficient to compare $design$, $code$, and all models built from 10% and 50% of the modules only.

Figure 10 depicts box-plot diagrams for experiments with all the 14 data sets. The diagrams compare the performance of models which use $design$, $code$, and all metrics built from 10% subsets. The performance measure in these experiments is AUC. The box-plots indicate that the majority of models built from all metrics outperform those built from $code$ metrics, which in turn outperform $design$ metrics models. The statistical significance of these results will be tested following the Demsar's procedure [17]. It is worth mentioning here that the reported performance reflects models which use all five classification algorithms. While there are differences between classification algorithms, reporting median AUC across all of them (following 10-way cross validation of each) minimizes the impact of the classifier and emphasizes the inherent properties of metrics data sets. We study the impact of classification algorithms in the next section, but if Lessman's results [30] are correct, models developed using different algorithms are not likely to have statistically significant performance differences.

Following the notation introduced earlier, Figure 11 introduces performance clusters based on the value of the Critical Distance, which for this experiment assumes value $CD = 1.48$. Enclosure of two or more models within either round brackets (lower performance

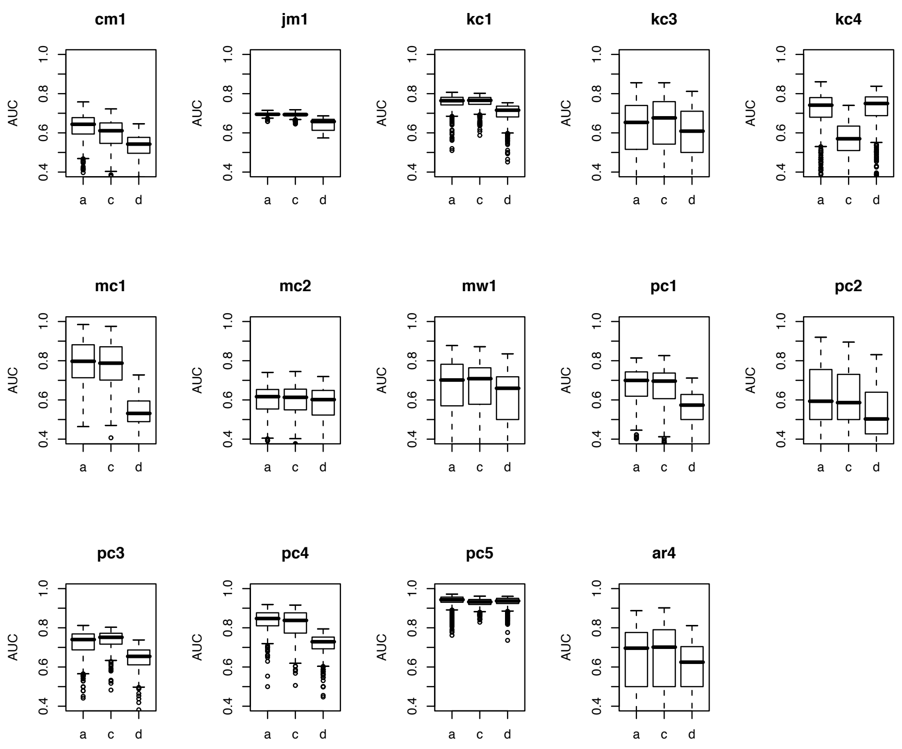


Fig. 10 Box-plot diagrams compare the performance of models built from $design$ (d), $code$ (c), and all (a) metrics using 10% subset.

cluster) or square brackets (higher performance cluster) indicate that they do not exhibit statistically distinguishable performance. We summarize the findings below.

- The performance of $design$ metrics models is ranked the lowest in 13 out of 14 data sets. The exception is project $kc4$ in which $code$ metrics model was the weakest one. In $pc5$, the rank of $design$ and $code$ models overlaps.
- The performance of all metrics models is ranked the best in 9 out of 14 data sets.
- The performance of $code$ metrics models is ranked the best at 5 out of 14 data sets.
- The rank distance between $design$ and $code$ models is typically greater than the distance between $code$ and all models.

From the statistical significance point of view:

- In 6 projects, $design$, $code$ and all models demonstrate no significant difference.
- In 5 data sets, $design$, $code$ and all fault prediction models form two performance clusters: $design$ and $code$ models form the lower cluster, $code$ and all form the higher performance cluster.
- In 3 data sets the $lower$ cluster includes $design$ and all models, while all and $code$ form the $higher$ cluster.

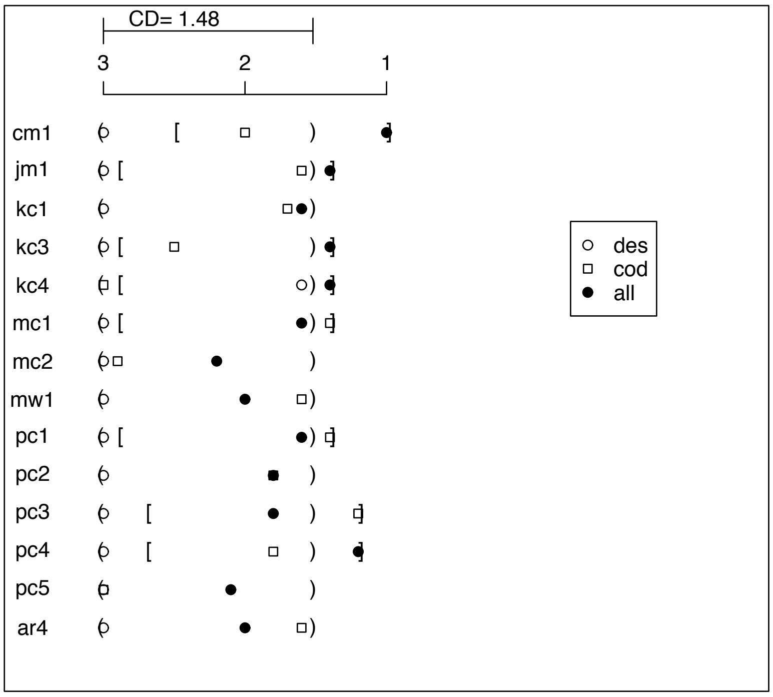


Fig. 11 Statistical performance ranks of $design$, $code$, and all models built using 10% of data for training.

We repeated the same analysis using models built from 50% subsets of data sets. The box-plot diagrams are presented in Figure 12. The summary of the findings about models built from 50% data subsets is very similar to those we had about models built from 10% subsets.

More interesting observations emerge from the diagram in Figure 13. Using 50% of data for model development seems to stabilize performance trends. For example, except in $kc4$, in all other data sets $design$ metrics models offer the inferior performance. Even more interestingly, models built from all metrics outperform other models in all data sets. Please note that this was not the case with models built from 10% of data, where in 5 projects $code$ models outperformed those drawing from all metrics as attributes.

Further statistical analysis of models built from the 50% subsets reveals that:

1. Only one data set, $mw1$, offers $design$, $code$ and all models with statistically indistinguishable performance.

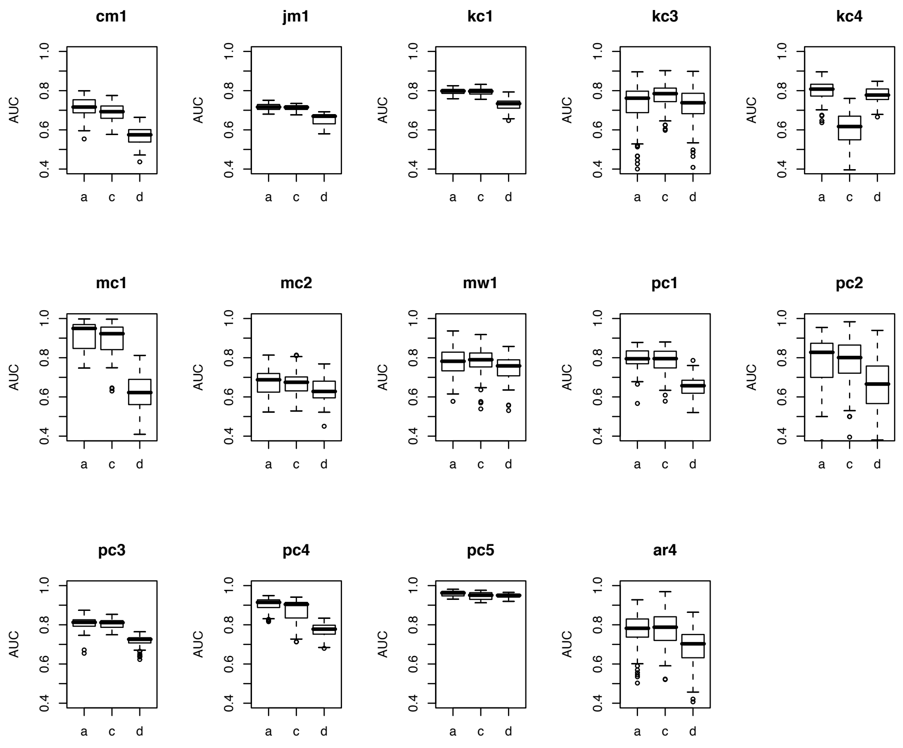


Fig. 12 Box-plot diagrams compare the performance of models built from $design$ (d), $code$ (c), and all (a) metrics using 50% of data for training.

2. In the remaining 13 data sets, the three models form two performance clusters. Except in $kc4$, all and $code$ models form the higher performance cluster. In $kc4$, all and $design$ models form the higher performance cluster.

We summarize the findings emerging from experimentation with $design$, $code$ and all metrics models built from 10% and 50% subsets as follows:

1. Although $code$ metrics models typically outperform $design$ metrics models, the difference in their performance measured by AUC index is not statistically significant.
2. Whenever possible, fault prediction models should be developed using a combination of $design$ and $code$ metrics. all metrics models typically outperform $design$ and $code$ metrics models. The difference in performance between all metrics models and $design$ metrics models is typically statistically significant.
3. Larger model training data sets, in this case 50% subsets, stabilize the expected model performance. More specifically, models built from 50% subsets almost uniformly offer two rank performance clusters ($design$ and $code$ vs. $code$ and all). In almost half of the models built from 10% data subsets, the performance of $design$, $code$ and all forms one rank performance cluster, i.e., their performance is statistically indistinguishable.

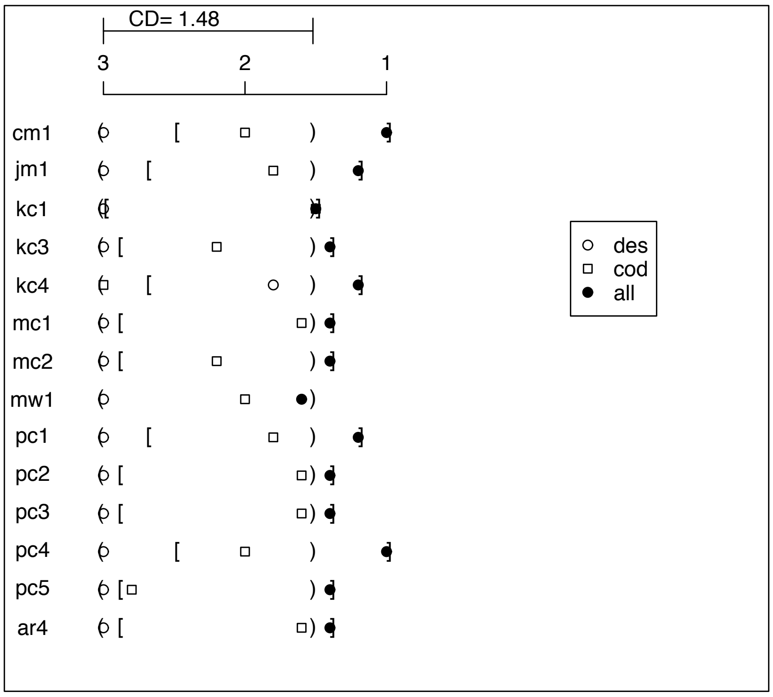


Fig. 13 Statistical performance ranks of *design*, *code*, and *all* models built using 50% of data for training.

An interesting question arises regarding $kc4$ data set, in which $design$ metrics outperform $code$ metrics. While all other projects use C, C++ or Java, $kc4$ relies on the scripting language Perl. The use of Perl is an obvious project anomaly (in the context of the data sets included in this study) and a possible cause for much higher proportion of faulty modules in $kc4$ (48%). While we cannot offer a more detailed analysis of the causes of anomalous manifestations in fault prediction models for this data set, we are inclined to suggest a more thorough research consideration be given to software quality for projects which use scripting languages like Perl.

4.3 Comparing models developed using different classifiers

In the experiments reported so far, our results reflect median model performances achieved using all the five classification algorithms listed in Table 3. As we mentioned, by collapsing the performance of all the classifiers and models into a single performance index (AUC), we intended to emphasize the inherent properties of metrics data sets. In this section, we reveal the performance impact of different classification algorithms.

A recent study by Lessmann et. al. [30] indicates that most classification algorithms do not offer models which exhibit statistically significant performance differences. Sixteen out of 19 algorithms included in their study belong to a "higher" rank cluster. In addition to the 10 data sets included in Lessmann's study, we use four additional project data sets: $mc1$, $mc2$, $pc5$, and $ar4$. Out of the five classifiers in our study three (random forest, logistic regression and Naive Bayes) were reported by Lessmann to achieve statistically indistinguishable performance. Further, Lessmann reports results only from all metrics models using $\frac{2}{3}$ of the data for training. We find some very interesting observations by experimenting with the three types of metric sets ($design$, $code$, all) and by increasing the size of the training subset (10% to 90%). An additional difference between the two studies reflects our opinion that software quality practitioners are most likely to use off-the-shelf classifiers with their default parameter values. We follow this principle in our study and report median results from 10-way cross validation. Lessmann et. al. use grid-search approach to find an optimal combination of algorithmic hyper-parameters which maximize the performance of each classifier and report the mean values of AUC.

Our statistical hypotheses for this experiment can be formulated as follows:
H_0: *Fault prediction models developed from the same data sets using five different classification algorithms do not result in statistically different performance.*
vs.
H_α: *At least two classification algorithms provide models whose performance is significantly different.*

Figure 14 depicts the outcome of our experiments analyzed through $Demsar$'s procedure. The 15 experiments (lines in Figure 14) reflect the three metrics groups and the five sizes of training subsets.

These results can be summarized as follows:

– Regardless of the classification algorithm, for all sizes of training subsets, models developed from $design$ metrics have statistically indistinguishable performance.
– For all sizes of training subsets, models developed from all metrics are grouped in two performance rank clusters; Random forest models are consistently in the higher rank cluster.
– $Code$ metrics models fall in between. When trained on 10% to 50% of the data, they result in two rank clusters. When trained on larger subsets, all models fall into the single performance rank clusters.

These are very interesting insights. For all and $code$ metrics models, we can infer that the choice of classification algorithm in mature data sets (those where we train from larger subsets), consistent with Lessmann's results, matters less than when they are built early. When the samples come earlier in the development life cycle from a smaller number of completed modules (10% or 25%), the choice of the classification algorithm matters more.

To gain better insights into these experiments, in Tables 8, 9, and 10 we preview median AUC and variances from models developed for each data set. The top median AUC for each project is marked in bold. We decided to report these values from experiments in which models are trained using 50% of the available data. Models build from 50% subsets are almost always statistically similar to those developed from larger and smaller data subsets. These tables report the results which support box-plot diagrams shown in Figure 12. The box-plots do not differentiate the performance of five classification algorithms, while the tables do.

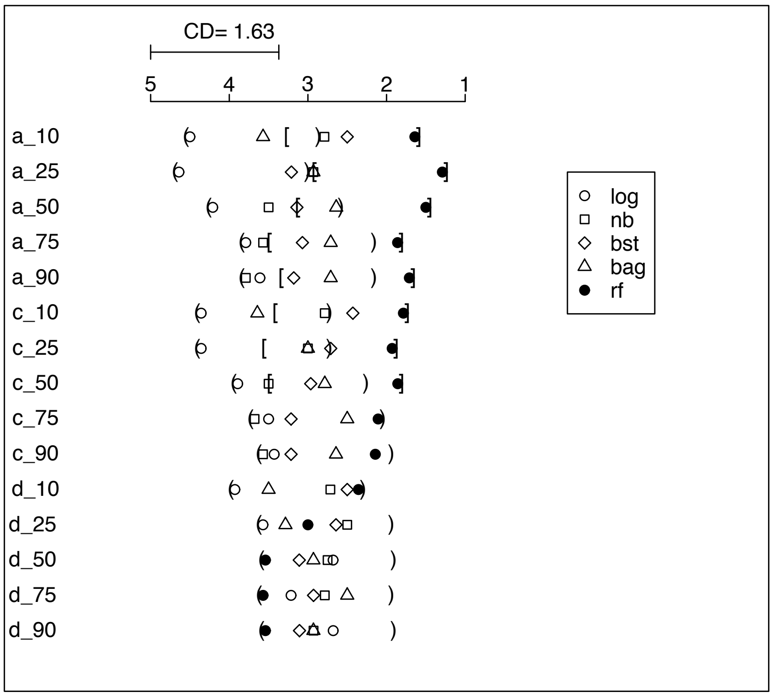


Fig. 14 Comparison of classification algorithms over different sizes of training subset and three metric groups. Labels "a_10" (or "d_50"), for example, stand for *all* (*design*) metrics and 10% (50%) training subset. The reported results reflect performance ranks over all 14 data sets.

From performance ranks in Figure 14, we find that random forest classifier achieves the highest average rank for *all* and *code* metrics models. Of course, this does not mean that random forests perform the best for each data set and training subset size. Tables 10) and 9 reveal that almost half of *code* metrics models and a quarter of *all* metrics models feature a different classifier as the best one. This is not surprising and practitioners should be prepared to try out different classification algorithms. Given that applying off-the-shelf classifiers with (mostly) default parameter values is not costly or difficult, the exercise may be worth the effort. However, looking back on Figure 14, in many cases the differences will be at the margin of statistical significance.

The three tables also reveal that AUC indices across the five classifier models (within each row) are similar to each other. in For example, in Table 10, all classifiers almost perform very well on the $pc5$ data set and quite poorly for $mc2$ data set. It is evident that the performance of a fault prediction model is determined by the characteristics of the data sets,

Table 8 Median and variance AUC from $design$ metrics models built from 50% training subsets.

data	**bag**		**bst**		**log**		**nb**		**rf**	
	median	var	median	var	median	var	median	var	median	var
cm1	**0.59**	0.0023	0.57	0.0026	0.57	0.0017	**0.59**	9e-04	0.54	0.0018
jm1	0.67	0	0.67	0	**0.68**	0	0.63	4e-04	0.63	1e-04
kc1	0.73	5e-04	**0.74**	4e-04	**0.74**	5e-04	**0.74**	3e-04	0.68	4e-04
kc3	0.71	0.0167	0.73	0.0108	0.7	0.0151	**0.79**	0.004	0.74	0.0073
kc4	**0.79**	0.0011	0.76	0.0018	0.77	0.002	**0.79**	9e-04	0.75	0.0017
mc1	0.65	0.0067	0.6	0.0035	0.63	0.002	0.52	0.0034	**0.74**	0.0023
mc2	0.65	0.0022	0.62	0.0029	0.59	0.0027	**0.68**	0.0027	0.62	0.0026
mw1	0.76	0.0028	0.77	0.0019	0.75	0.0063	**0.79**	0.0016	0.71	0.0028
pc1	**0.68**	0.0023	0.66	0.0012	0.62	0.0021	0.63	0.004	**0.68**	0.0024
pc2	0.6	0.0091	0.71	0.0035	0.61	0.0202	**0.79**	0.0041	0.57	0.0094
pc3	**0.73**	4e-04	0.72	3e-04	**0.73**	4e-04	0.68	0.0016	**0.73**	4e-04
pc4	0.79	4e-04	0.79	3e-04	**0.81**	2e-04	0.75	0.0011	0.74	4e-04
pc5	**0.96**	1e-04	**0.96**	0	0.94	1e-04	0.94	1e-04	0.95	1e-04
ar4	0.7	0.01	0.68	0.0071	0.64	0.0108	**0.76**	0.0051	0.72	0.0041

Table 9 Median and variance AUC from $code$ metrics models built from 50% training subsets.

data	**bag**		**bst**		**log**		**nb**		**rf**	
	median	var	median	var	median	var	median	var	median	var
cm1	0.71	0.0018	0.66	0.0014	0.69	9e-04	0.67	0.0013	**0.72**	9e-04
jm1	**0.72**	0	0.71	1e-04	0.71	0	0.69	0	**0.72**	0
kc1	**0.8**	3e-04	0.79	3e-04	0.79	3e-04	0.79	2e-04	**0.8**	2e-04
kc3	0.77	0.0037	0.76	0.0037	0.76	0.0071	**0.82**	0.0022	0.79	0.0014
kc4	**0.67**	0.0021	0.66	0.0011	0.53	0.0026	0.55	0.0105	0.63	0.0039
mc1	0.94	0.0046	0.95	1e-04	0.88	0.0017	0.8	0.0046	**0.96**	0.0012
mc2	0.63	0.0038	0.64	0.0024	**0.69**	0.0046	**0.69**	0.0011	0.67	0.002
mw1	0.78	0.0051	0.8	0.0022	0.75	0.0089	**0.82**	0.002	0.79	0.0024
pc1	0.81	0.0017	0.79	0.0011	0.8	0.0015	0.7	0.0036	**0.84**	0.001
pc2	0.73	0.0226	**0.87**	0.0064	0.72	0.0093	0.85	0.0044	0.81	0.007
pc3	0.81	3e-04	0.81	3e-04	0.81	5e-04	0.78	5e-04	**0.82**	4e-04
pc4	0.91	1e-04	0.91	1e-04	0.84	4e-04	0.77	0.0012	**0.92**	1e-04
pc5	0.96	0	0.95	0	0.93	1e-04	0.93	0	**0.97**	0
ar4	0.78	0.0079	**0.82**	0.0069	0.67	0.0087	0.81	0.0054	0.8	0.0052

rather than the differences between the classification algorithms. Similar variances represent further evidence.

It is also interesting to note that random forest classifier does not appear to rank well on $design$ metrics models, although its results belong to the same rank cluster with the other four algorithms. The number of attributes representing $design$ metrics is lower than the number of attributes in $code$ and, especially, all models. It is known [10] that the strength of random forest classifier are the data sets with a large number of attributes and a large number of instances.

4.4 Discussion

Projects which follow iterative development processes, such as rapid prototyping or even extreme programming, offer the opportunity for the incremental development of fault prediction models. Our experiments indicate that:

– Fault prediction models from relatively small subsets of project modules (10%, for example) achieve meaningful performances.

Table 10 Median and variance AUC from all metrics models built from 50% training subsets.

data	**bag**		**bst**		**log**		**nb**		**rf**	
	median	var	median	var	median	var	median	var	median	var
cm1	0.73	0.001	0.71	0.0019	0.70	0.0024	0.70	0.0011	**0.76**	8e-04
jm1	0.73	0	0.71	1e-04	0.71	0	0.69	0	**0.74**	0
kc1	0.8	1e-04	0.79	3e-04	0.79	2e-04	0.79	1e-04	**0.81**	1e-04
kc3	0.72	0.0052	0.76	0.0027	0.53	0.0166	**0.80**	0.0023	0.78	0.0015
kc4	**0.82**	0.0014	0.80	0.0024	0.81	0.0036	0.78	0.0025	**0.82**	0.0014
mc1	0.96	0.0048	0.95	2e-04	0.94	0.0039	0.80	0.0019	**0.98**	8e-04
mc2	0.67	0.0032	0.65	0.0047	0.64	0.0049	**0.72**	0.0014	0.70	0.0031
mw1	0.77	0.0048	0.79	0.0047	0.72	0.0063	**0.82**	0.0019	0.79	0.003
pc1	0.82	0.0013	0.80	9e-04	0.76	0.0047	0.77	7e-04	**0.85**	5e-04
pc2	0.74	0.0347	0.85	0.0048	0.65	0.0185	0.87	0.004	**0.83**	0.0071
pc3	0.81	3e-04	0.81	3e-04	0.82	7e-04	0.78	0.0017	**0.83**	3e-04
pc4	0.92	2e-04	0.92	1e-04	0.90	3e-04	0.84	3e-04	**0.94**	1e-04
pc5	**0.97**	0	0.96	0	0.95	1e-04	0.94	0	**0.97**	0
ar4	0.78	0.0036	**0.84**	0.0058	0.68	0.0067	0.80	0.0054	0.81	0.0035

– These early models do not need to be updated frequently. Models built from 50% of the modules almost always belong to the best performance rank cluster, i.e., their performance is statistically indistinguishable from the models that could be built later in the project development cycle.
– Models built from $design$ metrics metrics achieve meaningful performance. In principle, design metrics of reused modules can be utilized to evaluate the designs of new modules to identify problem areas possibly even before implementation.
– Models which combine design and code metrics attributes typically outperform $design$ metric based models by a statistically significant margin. Therefore, we conclude that combining design and code metrics when modeling fault prediction is desirable. The type of metrics used for modeling impacts model performance more than the selection of the classification algorithm.
– Using multiple classification algorithms to build fault prediction models should be a recommended practice. Especially when models are built from smaller subsets of available modules, different classifiers are likely to offer statistically meaningful performance differences.

5 Related Work

Although the choice of software metrics is important in the prediction of fault-proneness, comparing the effectiveness of design and code metrics received limited attention. To the best of our knowledge, the paper by Zhao *et. al.* is unique as it compares the performance of design and code metrics in the prediction of software fault content. They compared fault prediction models built from design metrics, code metrics, and the combination of design and code metrics in the context of a large real-time telecommunication system. Their design metrics are a modified version of McCabe's cyclomatic complexity, extracted from Specification Description Language (SDL). They used regression equations to fit the three groups of metrics and R^2 statistic to evaluate the performance of the models. Their findings are similar to ours: (1)the design and code metrics are correlated with the number of faults; (2) some improvement can be achieved if both design metrics and code metrics are used for prediction. While their findings are based on the analysis of a single data set, we use 14 data sets and our conclusions, based on sound statistical testing, indicate significant difference gained from the application of all metrics.

Menzies *et. al.* [36] select the "best" two or three attributes from 10 MDP data sets (the same data sets are included in our experiments). Halstead metrics, which we classify as $code$ metrics, appear on the list of "best" attributes much mode often than McCabe metrics (which we classify as design metrics). A study conducted by Xu *et. al.* [55] compare twelve metrics are extracted from the Large Sky Area Multi Object Spectroscopic Telescope project in China. They conclude that the most useful individual metrics for fault prediction are Halstead program difficulty, the number of executable statements, and Halstead program volume. Cyclomatic complexity and related metrics fared low on their list. In these studies, Halstead metrics, part of our $code$ metrics suite appear to be better predictors of fault content than the metrics from the McCabe group.

Requirement metrics have been used to predict fault prone software modules [24,25, 32]. Malaiya *et. al.* examined the relationship between requirement changes and fault density and found a positive correlation [32]. Javed *et al.* [24] investigate the impact of requirement instability on software faults. In 4 industrial e-commerce projects and 30 releases they found: (1) a significant relationship between pre/post release change requests and overall software faults; (2) insufficient and inadequate client communication during system design phase cause requirements changes and, consequently, software faults. Jiang and colleagues [25] found that combining requirements level metrics with code level metrics significantly improves the performance of fault prediction models.

One of the earliest studies of design metrics was conducted by Ohlsson and Alberg [43]. They predicted fault-prone modules prior to coding in Telephone Switches system of 130 modules at Ericsson Telecom AB. Their design metrics are derived from graphs where functions and subroutines in a module are represented by one or more graphs. These graphs, called Formal Description Language (FDL) graphs, offer a set of direct and indirect metrics based on the measures of complexity. The examples of direct metrics are the number of branches, the number of graphs in modules, the number of possible connections in a graph, and the number of paths from input to the output signals etc. The indirect metrics are the metrics calculated from the direct metrics such as McCabe cyclomatic complexity, etc.

The suite of object oriented (OO) metrics, referred as CK metrics, has been first proposed by Chidamber and Kemerer [15]. They proposed six CK design metrics including Weight Method Per Class (WMC), Number of Children (NOC), Depth of Inheritance Tree (DIT), Coupling Between Object class (CBO), Response For a Class (RFC), and Lack of Cohesion in Methods (LCOM). Basili et. al. [8] were among the first to validate these CK metrics using 8 C++ systems developed by students. They demonstrated the benefits of CK metrics over code metrics. In 1998, Chidamber, Darcy and Kemerer explored the relationship between the CK metrics and productivity, rework effort or design effort [14]. They show that CK metrics have better explanatory power than traditional code metrics.

Predicting fault-prone software modules using metrics from design phase has recently received increased attention [46,57,37,48]. In these studies, metrics are either extracted from design documents or, like in our work, by mining the source code using the reverse engineering techniques. Subramanyam and Krishnan predict faults from three design metrics: Weight Method Per Class (WMC), Coupling Between Object Class (CBO), and Depth of Inheritance Tree (DIT) [48]. The system they study is a large B2C e-commerce application suite developed using C++ and Java. They showed that these design metrics are significantly related to defects and that defects are strongly related to the language used. Nagappan, Ball and Zeller [37] predict component failures using OO metrics in five Microsoft software systems. Their results show that these metrics are suitable to predict software defects. They also show that the predictors are project specific, the suggestion also mentioned by Menzies *et. al.* [35].

Recovering design from source code has been a hot topic in software reverse engineering [12,6,5]. Systa [49] recovered UML diagrams from source code using static and dynamic code analysis. Tonella and Potrich [50] were able to extract sequence diagrams from source code. Briand *et. al.* demonstrated sequence diagrams, conditions and data flow can be reverse engineered from Java code through transformation techniques [11].

Recently, Schroter, Zimmermann, and Zeller [46] applied reverse engineering to recover design metrics from source code. They used 52 ECLIPSE plug-ins and found usage relationships between these metrics and past failures. The relationship they investigate is the usage of import statements within a single release. The past failure data represents the number of failures for a single release. They collected the data from version archives (CVS) and bug tracking systems (BUGZILLA). They built predictive models using the set of imported classes of each file as independent variables to predict the number of failures of the file. The average prediction accuracy of the top 5% is approximately 90%. Zimmermann, Premraj and Zeller [57] further investigate ECLIPSE open source, extract object oriented metrics along with static code complexity metrics and point out their effectiveness to predict fault-proneness. Their data set is now posted in the PROMISE [9] repository. Neuhaus, Zimmermann, Holler and Zeller examine Mozilla code to extract the relationship of imports and function calls to predict software components' vulnerabilities [41].

Besides requirement metrics, design metrics, and code metrics, various other measures have been used to predict fault prone software modules. Historical project characteristic, developer information and social networks have all been reported as effective predictors. Ostrand, Weyuker, and Bell [44] predict the files most likely to contain the largest numbers of faults in the next release using modification history from previous release and the code in the current release. Illes-Seifert and Paech investigate the relationship between software history characteristics and the number of defects [23]. After analyzing 9 open source Java projects, they conclude that some history characteristics, such as the number of changes and the number of distinct authors performing changes to a file, highly correlate with faults. Code churn, defined as the amount of code change taking place within a software unit [39], also called cached history [29], was also reported as an effective predictor of faults.

The role of developer social networks is currently receiving significant attention . Weyuker, Ostrand, and Bell [52] found that the addition of developer information improves the accuracy of fault prediction models. Li *et. al.* analyzed 139 metrics collected from software product, development, deployment, usage, software and hardware configurations in OpenBSD [31]. They found that the number of messages to the technical discussion mailing list during the development period is the best predictor of the number of field defects. Nagappan *et. al.* [40] collect 8 organizational structure complexity metrics which relate code binary to the organizational social networks, i.e, the number of engineers, the number of ex-engineers, edit frequency of source code, and organization intersection factors to predict failure-proneness. They compare this model to models which use five groups of traditional metrics (code churn, code complexity, code coverage, dependency, and pre-release bugs). The use of organizational structure complexity metrics appears to hold a significant promise for fault prediction.

6 Summary

The experiments reported in this paper have been motivated by the hypothesis that combining software metrics from different stages in the development benefits the accuracy of fault prediction models. This motivation includes the utilization of different metrics types, those available from software design or code, as well as the proactive use of measurements from software modules for the development of fault prediction models when they become available during project development. Publicly available NASA MDP and Promise repositories offer a substantial number of data sets, 14 of which we have used in the experiments. The large number of project data sets and a rigorous statistical test procedures we applied offer strong evidence in support of our conclusions, presented below.

The starting position for our analysis of the relative strengths of design and code metrics in building fault prediction models comes from Zhao *et. al.* [56]. They claimed that $design$ and $code$ models perform comparatively well and that little improvement can be achieved if design and code metrics are jointly used in fault prediction. Our results confirm that the performance of $design$ and $code$ models, measured through the area under the ROC curve, is typically statistically indistinguishable. In other words, although code metrics based models outperform design metrics models, the performance margin is not statistically significant. On the other hand, the performance of models built from all metrics (which include design and code metrics) typically outperform $design$ models by a statistically significant margin. Our experiments, therefore, offer support for utilizing a combination of design and code metrics in building fault prediction models. However, if design metrics are available earlier, $design$ models should not be discarded as they offer meaningful fault prediction performance.

The impact the size of the fault data set used for training has on model performance is also significant and interesting. One of the basic questions regarding the practicality of fault prediction is: When does the project have sufficient amount of data to build a model? Before we describe our recommendations, it is necessary to mention that in many organizations fault information from early (or earlier) product releases has been successfully used for fault prediction for the new release [44,52]. In organizations which practice software product lines or those where upgrades form the majority of project releases, data sufficiency is not a major problem. But there are many other organizations and projects which develop one-of-a-kind systems. Thirteen out of 14 data sets we analyzed come from such environments. These organizations must rely on the metrics from reused modules and those delivered and tested early in the development life cycle for building fault prediction models.

Most results from our experiments which test the impact of the training data size on the fault prediction performance are not surprising. When derived from a larger data set, model performance improves. The interesting aspect of our results comes from statistical hypothesis testing. In simple terms, the performance margin between models derived from 50% data subsets and those derived from just 10% is not statistically significant. Further, models built from 50% data subsets and 90% data subsets typically belong the the same performance cluster too (but models built from 10% and 90% do not). The implication of this result is, we believe, very positive. Models developed from small data sets, presumably early in the project life time, offer fault prediction capability comparable with models that can only be developed much later. Therefore, while updating the fault prediction model is a good idea, it does not have to be practiced often. This conclusion offers the real chance to optimize the cost of fault prediction model development. Fault prediction models, in turn, optimize the cost of verification and validation activities.

We believe the results from both experiments support the general hypothesis: defect detectors can be improved by increasing the information content of the training set. Therefore continuing to explore the effects of combining the attributes from multiple phases of the development life cycle, process and business related attributes, appears to be the most promising research direction [25, 40, 52].

In the third group of experiments, we examined the impact of the selection of the classification algorithm in fault prediction modeling. In the recent paper, Lessmann [30] offers convincing arguments that most classification algorithms offer statistically the same performance (i.e., their performance differences are not significant). One limitation of Lessmann's analysis is the uniform use of $\frac{2}{3}$ of the data for model training. In our experiments we varied the size of the training subset and the type of metrics used for training. We observed that statistically significant differences between classification algorithms do occur when models are developed from smaller training subsets. Further, significant differences are more likely to occur when training from all and $code$ metrics then from $design$ metrics. Consequently, we recommend experimentation with several classification algorithms, recommended in the literature (for example [30]) for fault prediction modeling.

Combined, the outcomes of our experiments provide a good guidance for an incremental process for software fault prediction modeling. Models can be built early, from design metrics and/or from relatively small subsets of available fault data. Updating such early models is recommended. The frequency of such updates can be optimized as model performance gains justify intermittent (sporadic) upgrades, rather than recurring ones.

We believe the results reported here have been obtained following a valid experimental methodology, using publicly available data sets. As any other experimental study we are aware of potential validity threats too. For example, we mentioned that the design metrics used in the experiments have been reengineered from the code. While in principle similar metrics can (and have been) extracted from design documentation, it is likely that the metrics used in our experiments reflect the code more faithfully than the metrics collected at the design stage would. Had $design$ models demonstrated better fault prediction performance than $code$ models, one could argue that any reduction in the code-level details from attributes would have a tendency to improve performance. But, $design$ models in our experiments do not perform as well as $code$ models. Therefore, it seems logical that if design metrics do not reflect code structure as close as they do in our data sets, this would likely deteriorate the performance even further. Without additional research, we cannot offer further assurances.

It is also worth repeating here that the data sets we analyzed do not contain information about when in the project development life time modules became available. To advocate incremental model development, we made the assumption that random selection of data subset used for training (repeated 10 times in each experiment) represents a valid sample of modules as if they became available before the modules we used for model evaluation. We are aware that software modules that become available early might suffer from quality deficiencies which projects reduce as their processes mature. If fault introduction is reduced over the life time of the project, our results about the suitability of fault prediction models built from smaller data subsets may be overly optimistic. Also, this might imply that updating models more frequently during the project's life time is warranted. Unfortunately, the data sets we use do not allow us to study this problem further.

Lastly, we did not investigate the impact of feature selection on model performance. Feature selection algorithms minimize the number of attributes used in model development based on some measure of their information content. Our experience with MDP datasets indicates that a smaller number of attributes (metrics), typically a dozen or less, could offer models that perform almost as well as the models which use the entire set of attributes [36]. This could have an impact on the effort invested in metrics collection. But, we have never been able to develop a model from a reduced set of attributes which outperforms models developed from comprehensive attribute sets. For this reason, we believe that feature selection is not likely to impact the validity of our results.

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