Software Quality Journal (2021) 29:197–237
https://doi.org/10.1007/s11219-021-09550-5

Exploratory study of the impact of project domain and size category on the detection of the God class design smell

Khalid Alkharabsheh[1] · Yania Crespo[3] · Manuel Fernández-Delgado[2] · José R. Viqueira[2] · José A. Taboada[2]

Accepted: 5 March 2021 / Published online: 31 March 2021

Abstract
Design smell detection has proven to be an efficient strategy to improve software quality and consequently decrease maintainability expenses. This work explores the influence of the information about project context expressed as project domain and size category information, on the automatic detection of the *god class* design smell by machine learning techniques. A set of experiments using eight classifiers to detect *god classes* was conducted on a dataset containing 12, 587 classes from 24 Java projects. The results show that classifiers change their behavior when they are used on datasets that differ in these kinds of project information. The results show that *god class* design smell detection can be improved by feeding machine learning classifiers with this project context information.

Keywords Design smell detection · Machine learning · Software metrics · Project context information · God class

✉ Yania Crespo
yania@infor.uva.es

Khalid Alkharabsheh
khalidkh@bau.edu.jo

Manuel Fernández-Delgado
manuel.fernandez.delgado@usc.e

José R. Viqueira
jrr.viqueira@usc.e

José A. Taboada
joseangel.taboada@usc.e

[1] Department of Software Engineering, Al-Balqa Applied University (BAU), As-Salt 19117, Jordan

[2] CiTIUS, Centro Singular de Investigación en Tecnoloxías Intelixentes, Universidad de Santiago de Compostela, Santiago de Compostela 15782, Spain

[3] Departamento de Informática. Escuela de Ingeniería Informática. Campus Miguel Delibes, Universidad de Valladolid, Paseo de Belén 15, Valladolid 47011, Spain

1 Introduction

Software quality is an important concern for practitioners in software factories, as well as for academics and researchers. To maintain software quality, the main processes that need to be undertaken are identifying bad pieces, modifying code, correcting bugs, adding new functionalities into the software structure, and testing.

The poor and bad pieces in a software structure (either in the source code or design) are termed "*design smells* ". *Design smells* do not produce compilation or runtime errors (Alkharabsheh et al., 2018), but they negatively affect software quality factors such as reusability, stability, flexibility and maintainability (Brown et al., 1998). We use the term *design smell*, as (Alkharabsheh et al., 2018) does, because we consider that it integrates different smells which can manifest in the code but are caused by deficiencies in the design. Note that other alternative terms are used in the literature, such as code smells (coined by Kent Beck in Ward's Wiki[1], as a hint that something has gone wrong somewhere in the code), anti-patterns (Brown et al., 1998), bad smells (Fowler & Beck, 1999) or architectural smells (Azadi et al., 2019), among others.

Great interest has been observed in the state of the art with respect to the identification and correction of *design smells* using different approaches and techniques (Alkharabsheh et al., 2018; Rasool & Arshad, 2015; Zhang et al., 2011). Many researchers have attempted to tackle this problem by using metric-based, rule-based approaches (Choinzon & Ueda, 2006; Fourati et al., 2011; Marinescu et al., 2005; Moha, 2007; Moha & Gueheneuc, 2007; Munro, 2005; Shatnawi, 2015; Tahvildar & Kontogiannis, 2004) and machine learning approaches (Hassaine et al., 2010; Khomh et al., 2011; Kreimer, 2005; Maneerat & Muenchaisri, 2011). All these studies depend on the definition of the *design smells*, which is mapped into rules manually or automatically; these rules are specified by using a combination of metrics that is directly related to the definition of the *design smells*. However, a substantial effort is needed to identify the right threshold value for each metric.

For example, the *god class*, which is known in the literature as the *god class or the blob* anti-pattern (Brown et al., 1998), god class disharmony (Lanza & Marinescu, 2007), and that is similar in definition to the *large class* bad smell (Fowler & Beck, 1999), is characterized by a class that tends to be very large and complex and also has low cohesive functionality. Therefore, the rule that is used to detect the *god class* should include a set of metrics related to class size, complexity and functionality. According to Riel (1996), the *god class* is defined as the class of an object controlling too many objects in the system, which has grown beyond all logic to become *the class that does everything*. Fowler and Beck (1999) described the bad smell *large class* in similar definition, where the class tries to do much tasks. In good object-oriented designs, the logic of the system is uniformly distributed across multiple classes. A *god class* has too many instance variables (attributes), a large number of methods, relations with other classes, i.e., too much code, which increases the danger of duplicated code.

The *god class* is one of the *design smells* which has attracted more attention of the research community (Alkharabsheh et al., 2016; Alkharabsheh et al., 2016; Counsell & Mendes, 2007; Fontana et al., 2012; Li & Shatnawi, 2007; Santos et al., 2013; Yamashita et al., 2015). Moreover, it is one of the most frequently detected and hence deserve efforts devoted to improving detection and management of this *design smell*, as revealed in the

[1] https://wiki.c2.com/?CodeSmell.

systematic mapping we conducted (Alkharabsheh et al., 2018). According to (Moha et al., 2010) and (Yamashita & Moonen, 2013), who investigated the relationships between different types of *design smells*, they found a relationship between the presence of *god class* and of other *design smells*, such as *data class, duplicated code, feature envy, large class* and *god method*. Besides, *god class* is one of the *design smells* that most negatively influence on a broad set of software quality factors such as *maintainability, understandability, changeability, stability, performance, complexity, reusability,* and *evolvability* (Alkharabsheh et al., 2018).

Despite the diversity of approaches, tools and techniques in the literature that could be used to improving the detection of *design smells*, the adoption of appropriate techniques remains a challenge for the software industry. From our review of the state of the art (Alkharabsheh et al., 2018), we observed the following. Firstly, differences in understanding the precise definition of the *design smell* lead to inconsistency: 1) in the results obtained by different software detection tools (Alkharabsheh et al., 2016) or by human evaluators (Alkharabsheh et al., 2016); 2) in the real impacts that *design smells* have on a particular software quality factor; and 3) in identifying the most important smells to remove from a software. Secondly, the process of mapping the definition of the *design smell* into accurate and efficient detection algorithms is not exploited adequately due to the ambiguities in the definition of the *design smell* and its mapping to metrics, but also because some adaptation to the context, the project or the organization, may be required.

Most of the research community has not taken into account the impact of project context factors in *design smell* detection (Santos et al., 2018). Our research assumption is that the criteria used by experts to identify *god classes* are influenced by some features of the assessed projects which are not explicitly defined. As an example, larger size or complex classes are more likely to be acceptable (i.e., not considered as *god classes*) in certain sophisticated application **domains**. Similarly, large **size** projects can have the same type of impact in the size and complexity of acceptable classes. The present research investigates whether both project features, domain and size category, have a positive influence on the detection of *god class design smell*. This is further elaborated in Subsection 2.2.

On the basis of this assumption, in this paper, we investigate whether the detection heuristics of the *design smell* has to be tuned according to the domain of the assessed project and its size classification, which are frequent features when analyzing different types of projects. The main contribution of this study is to test the influence of these features by evaluating the performance of eight machine learning classifiers in the automatic detection of *god classes* with and without project domain and project size category information. We use a large dataset where the *god classes* are labeled by a committee of five *design smell* detection software tools. First we study jointly the influence of project domain and size category, and afterwards we analyze each feature independently. This is followed by an analysis on a dataset in which the classification has been carried out by human experts in order to show whether the results vary.

The rest of this paper is organized as follows. Section 2 presents some related work, particularly Subsection 2.2 deals with the project context information. Next, Section 3 describes the problem to be solved, the machine learning techniques (Subsection 3.1), data collection (Subsection 3.2) and methodology (Subsection 3.3). Section 4 discusses the results of the experiments designed in the previous section. After that, a replication of the Step 1 of the methodology, using a different dataset, is described in Section 5 together with a joint analysis by classifier of all the experiments conducted. Section 6 presents the main threats to their validity. Finally, Section 7 presents our conclusions and the direction we intend to take in future work.

2 Related work

2.1 On *design smells* detection and machine learning techniques

Several studies have focused on *design smell* detection using different strategies, which range from manual to semiautomated and fully automated inspection. The software tools that deal with detecting *design smells* automatically in the software structure include *iPlasma* (Marinescu et al., 2005), a tool that uses a metrics-based approach to detect *design smells* (named disharmonies) at different abstraction levels. In their methodology, more than 80 metrics related to size, complexity, coupling and cohesion can be applied to analyze different software artifacts. The works (Moha & Gueheneuc, 2007) and (Moha et al., 2010) introduced *DECOR*, a tool that uses a domain-specific language to specify *design smells* at a high level of abstraction, where the specification can be defined manually by the practitioners in order to detect *design smells* using software context and vocabulary. In (Tsantalis et al., 2008) *JDeodorant* is presented, an Eclipse plug-in tool that identifies *design smells* in source code and applies a set of refactoring operations to remove them automatically. *JDeodorant* uses several methods and techniques to identify four types of *design smell* (*feature envy, type-checking, long method*, and *god class*). Some other detection tools such as *Checkstyle* (http://www.checkstyle.sourceforge.net) and *FXCop* (https://msdn.microsoft.com/en-us/library/bb429476.aspx) are related to bugs, dead code or unused code, and coding style problems, but they do not address higher level *design smells*.

The authors of (Palomba et al., 2015; Rapu et al., 2004; Tufano et al., 2015) employed the changing of software history information for *design smells* detection. In (Palomba et al., 2015), they proposed the *HIST* approach to detect five *design smells* (*feature envy, blob, divergent change, shotgun surgery* and *parallel inheritance*) by analyzing the changes among different software artifacts. This approach was evaluated empirically by two studies and the accuracy was assessed in terms of precision and recall. In (Rapu et al., 2004), the historical information was used to refine the concepts of detection strategy for *god class and data class design smells*, by defining a new historical measurement to increase the detection accuracy. In (Tufano et al., 2015), a metric-based methodology is developed to analyze the source code artifacts for identifying different types of *design smells*. In order to investigate when and why *design smells* are introduced in code, this work conducted a large empirical study of the change history of 200 open source projects from different domains to detect five types of *design smells* that include *the blob, class data should be private, complex class, functional decomposition,* and *spaghetti code*. The results conclude that *design smells* are introduced mainly during the maintenance tasks.

Other studies exploited the developers context for improving the *design smells* detection. In (Palomba et al., 2014), the aim was to ascertain to what extent developers understand *design smells* as problems that should be solved. Moreover, they wanted to check which *design smells* are considered the most harmful. The results showed that several *design smells* were not acknowledged as problems to solve. Nevertheless, *design smells* related to size and complexity such as *god class* were always seen as problems. ReCon (Morales et al., 2017) is an example of refactoring approach related to the developer task context, which uses a metaheuristics technique to compute the best order of refactoring operation that influences only the code artifacts that are relevant to the developer context.

On the other hand, several studies used machine learning approaches to detect *design smells* compared with metric-based and rule-based. For instance, (Kreimer, 2005) presented a method to detect design flaws by combining object-oriented metrics and machine

learning techniques. The proposed approach was validated using five design flaws (*the blob, feature envy, long method, lazy class and delegator*) and two software systems (IYC, with 91 classes, and Weka, with 597 classes). More recently, (Maneerat and Muenchaisri, 2011) proposed a methodology that uses seven machine learning algorithms and seven state-of-the-art datasets consisting of 27 design model metrics to predict seven smells (*lazy class, feature envy, middle man, message chains, long method, long parameter list and switch statement*) using software design models.

The Bayesian detection expert (BDTEX) is a goal question metric-based approach that detects anti-patterns using Bayesian belief networks founded on rule-based representation (Khomh et al., 2011). This approach was validated using three anti-patterns (*the blob, functional decomposition,* and *spaghetti code*) and two Java projects (Xerces v2.7.0, with 589 classes, and GanttProject v1.10.2, with 188 classes). SVMDetect (Maiga et al., 2012) uses the support vector machine and object-oriented metrics for each class to detect four anti-patterns (*the blob, functional decomposition, spaghetti code, and swiss army knife*) on three open source software solutions (ArgoUML v0.19.8, with 1,230 classes, Azureus v2.3.0.6, with 1,449 classes, and Xerces v2.7.0, with 513 classes). Afterwards, (Maiga et al., 2012) proposed *SMURF*, an approach to detect anti-patterns also based on the support vector machine that takes into account feedback from developers. More recently, (Peiris & Hill, 2014) proposed *NiPAD*, a non-intrusive machine learning approach for identifying and classifying anti-patterns. Classifiers were trained on two datasets of metrics, and then used to predict new metrics. The results showed that the approach can detect *one lane bridge* anti-pattern with 0.94 accuracy.

In (Fontana et al., 2016), a previous work (Fontana et al., 2013) is extended, performing a large-scale comparison of 32 machine learning techniques and 74 projects to detect four *design smells*: *data class, god class, feature envy, and long method*. According to the authors, most of the selected classifiers obtained accuracy and F-measures values above 95%, being random forest and J48 the best-performing ones. These results show that machine learning approaches can improve *design smells* detection. Recently, in (Di Nucci et al., 2018), a criticism to the current state of the research in ML about *design smells* detection is developed, concluding that more work is required regarding to methodology, datasets and metric analysis in classes with and without smells. They also analyze the impact of some dataset properties such as class unbalancing, and of feature selection, on the detection of several *design smells* types, using F-measure as performance measurement.

A systematic literature review and meta-analysis of machine learning techniques for smell detection was conducted in (Azeem et al., 2019). This study concludes, among other aspects, that other sources of information for the classification of smelly instances should be investigated.

The main difference between our approach and previous works that employed machine learning is that they only included numerical information (metrics) in the dataset. Also, they used just two or three projects in the validation process, conducting experiments with less than eight machine learning technique, with the exception of (Fontana et al., 2016) and (Di Nucci et al., 2018), who used 74 projects and 32 techniques. In contrast, the current work extends numerical information (metrics) with project context information (its domain and size category expressed in nominal scales) and our objective is to evaluate its influence on the *god class* detection, considering 12,587 classes from 24 open source Java projects, and eight machine learning classifiers arising from different families. Another important difference is in the results evaluation: the current paper uses the Cohen Kappa and Matthews correlation coefficient (MCC) as performance measurements. Studies as (Bekkar et al., 2013) shown that problems with unbalanced data should be better evaluated

measuring some indicators such as MCC, Cohen Kappa and Receiving Operating Characteristic (ROC) curve (Powers, 2011). When assessing a project for *god class design smell* detection, it is normal that data are imbalanced because just a small ratio of the project classes can be considered as *god classes*.

Recently, in (Pecorelli et al., 2019) a comparison between heuristic-based detection and machine learning detection techniques is conducted. The experiment analyzes five machine learning algorithms in front of an heuristic-based tool (DECOR). The study was conducted with five different smells. The authors use as performance indicators precision, recall, F-Measure and MCC. A dataset with different versions of 13 projects was used. The results shows that the best classifier of the five used was Naive Bayes, and that the heuristic approach in DECOR still perform better, yet have low performance. The problem with unbalance datasets in *design smells* detection is also highlighted.

As a consequence of the last conclusion, in (Pecorelli et al., 2020) an empirical study comparing the performance of five data-balancing techniques for code smell detection with respect to a no-balancing baseline was conducted. The data-balancing techniques included those which perform training only on the minority class such as Cost-Sensitive Classifier, and One-Class Classifier, resampling techniques such as Oversampling and Undersampling, and Creating synthetic instances (SMOTE). The results obtained shows that SMOTE is the best data balancing technique among the other five. However, authors state that if the dataset is exceptionally imbalanced, this technique fails because, although SMOTE allows the model to be more accurate, in many cases, it is not applicable because of the few instances belonging to the minority class. The authors concluded, on one hand, that existing data balancing techniques are inadequate for code smell detection and, on the other hand, that their results indicate that structural metrics alone are not adequate for code smell detection, which is in the line of the ground assumption that motivates our work.

Finally, a multi-label classification (MLC) machine learning technique is analyzed in (Guggulothu & Moiz, 2020) for two method-level code smells: Feature Envy and Long Method. The authors use the smell correlation and experimented with three different MLC techniques, analyzing with and without the usage of the correlation between smells.

2.2 Project context information

In this subsection the software context on the identification of design smells (code smells, bad smells, anti-patterns, ...) is explored in depth.

The antecedents of using context information to detect smells are found in what it is known as “relative metrics thresholds”. In (Crespo et al., 2006) it is stated that most of the tools for metric-based smells detection use generic values for thresholds. Authors dealt with the question of whether using generic vs. relative thresholds affects the detection of bad smells and argue that product domain and size could also affect the results. In (Alves et al., 2010) a method that determines metric thresholds using measurements in a benchmark of software systems is designed. Machine learning and data mining techniques were utilized in (Herbold et al., 2011) to define a data-driven methodology for the calculation of thresholds for a metric set using Rectangle Learning. Authors stated that an important aspect of thresholds for metrics is that they are often dependent on the properties of the project environment. In order to evaluate results they used a case study assessed by measuring Matthews Correlation Coefficient (MCC) and F-score (F-measure). Moreover, in (Fontana et al., 2015), a benchmark-based methodology for deriving metrics’ thresholds for code smell detection is presented.

In the conclusions of the study presented at (Fontana et al., 2012), the authors stated that to improve the smells' detection techniques, information related to the domain of the analyzed systems should be taken into account. A large-scale study that investigated the relationships between the presence of smells and quality-related metrics computed over Java Mobile apps in different application domains was conducted in (Linares-Vasquez et al., 2014). The authors found that some smells are commonly present in all the domains while others are most prevalent in certain domains. DECOR was used as anti-patterns & smells detection tool. Although the referred work is not intended to improve detection by adapting to the domain as part of the project context, the authors state, on one side, that "domain matters" and, on the other side, that "metrics are not enough". This is the same statement that can be found in (Simons et al., 2015) regarding a very close topic which is refactoring to improve quality. Through qualitative analysis of a survey of professionals, answered by 50 engineers, authors find that a simple static view of software is insufficient to assess software quality, and that software quality is dependent on factors that are not amenable to measurement via metrics. As part of this qualitative analysis, authors found that "The Problem Domain Matters" and also that "Qualities Have Meaning only in a Given Context".

Based on (Crespo et al., 2006), in (Liu et al., 2016) a method with genetic algorithms to dynamically adapt thresholds in smells detection was designed and tested. This method captures the developer/project context through incorporating engineers feedback in the process. Engineers could adapt metrics' based smell detection tools with the so calculated thresholds. The method includes collecting feedback automatically from the engineers who manually check and resolve smells. Hence, it collects information about which smells have been confirmed manually, and which smells have been denied. Five open-source projects were used in evaluation. Authors assessed results using precision and recall. Manual application of detection algorithms was achieved by three engineers and JDeodorant was used for metric collection. With this approach, we cannot infer what are the important context factors that influence in improving smell detection. In contrast, (Lopez Nozal, 2012) used J48 algorithm in order to obtain decisions trees and find that domain and project size can influence on improving detection because rules regarding this data appear in the obtained decisions trees. It inspired us to conduct this exploratory study to assess the positive influence of introducing project context information, mainly domain and size category, in machine learning based automatic *god class* detection.

Moreover, Chapter 1 of (Mistrik et al., 2015) states that metric's thresholds may vary considerably depending on software size, cost, nature of team, etc. In (Mori et al., 2018) authors observed that software domain and size are relevant factors to be considered when building benchmarks for metric's threshold derivation. Recently, this work was continued in (Vale et al., 2019), calculating thresholds for a benchmark with three different methods as a comparison with the Vale's threshold calculation method. Authors use an SPL called MobileMedia. They tune smell detection such as *god class* and lazy class with the so calculated thresholds, and observe that using the Vale's thresholds improves the detection of those smells in terms of precision and recall when applying Lanza & Marinescu strategy for detecting *god class* and lazy class manually with the different thresholds in the same line as (Liu et al., 2016).

3 Study design

This section is organized as follows, first of all, in Subsection 3.1 we describe the machine learning classification techniques used in this study to automatically detect *god classes*. After that, we describe the data collection in Subsection 3.2. Finally, in Subsection 3.3, the methodology applied is explained.

The scope of the study is limited to *god class* in first place because of the nature of the smell. The hypotheses on the influence of the domain and size of the project on the detection of *god class* can be intuitively explained before conducting the experiments. In second place, the design of the methodology uses combinations of labeling by the five tools, as is detailed in next section: only one tool detects the *god class*, two of the tools detect the *god class*, three, four or all five detect the *god class*. This implies that the five tools must be able to analyse the same smell. In the systematic mapping study we conducted on smells detection (Alkharabsheh et al., 2018), it is shown that it is not usual to find that several tools detect a common subset of smells. Human validation on the cases of the God Classes detected simultaneously by four of the tools and the five tools to check the false positive reduction are performed. In the first case we detect 8 false positives among the 79 god classes and, in the second case only one false positive was detected, particularly strange, among the 24 god classes detected. This is explained in Section 4 when describing results of step 4 of the methodology (Section 4.1). At the end, a replication experiment is conducted using a manually classified dataset which is well known in the literature (Pecorelli et al., 2019; Pecorelli et al., 2020).

3.1 Machine learning techniques

We selected several classification techniques that are widely applied for smell detection (Khomh et al., 2011; Kreimer, 2005; Maneerat & Muenchaisri, 2011; Fontana et al., 2013). These techniques use information about the code of the classes (which in the literature is called an input pattern, see below) to decide whether it is a *god class*, i.e., there are only two possible labels: "*god class* " and 'not *god class* ". We use several techniques in order to avoid that the conclusions of our study may be biased by the specific classifier. These classifiers represent different approaches such as Bayesian networks, support vector machines, rule learners and decision trees. Implementations of the chosen techniques are available in the well-known data mining software Weka version 3.7 (http://www.cs.waikato.ac.nz/ml/weka). Most classifiers have one or several tunable hyper-parameters, which usually have strong influence on their performance. The set of values used for the hyper-parameter tuning is extracted from the classifier documentation (Witten et al., 2016). The following list briefly describes each classifier and its hyper-parameter tuning.

1. LibSVM is an integrated library for support vector classification, regression and distribution estimation. The library is available with different software interfaces and extensions, such as Weka, Matlab, C, Java and Python. In our experiments, the classifier used is the Gaussian kernel SVM, whose high performance has been widely tested in the literature. The tunable hyper-parameters are regularization (C) and the inverse of the squared Gaussian spread (γ), tuned by selecting 20 values in the ranges 2^{2i+1}, with $-3 \leq i \leq 7$, and 2^{2i+1}, with $-8 \leq i \leq 1$, respectively.
2. IBK is the instance-based k-neighbor classifier, which classifies each input pattern to the label of the majority of the k closest neighbors for training. The LinearNNSearch is the selected search algorithm with IBK. The number of nearest neighbors (k) is tuned using eight odd values between 1 and 41.
3. J48 is the Weka implementation of the C4.5 decision tree algorithm, which is also a popular classification method. It can produce rules that are understandable and intelligi-

ble for classifying new instances. The confidence interval (C) is tuned using the values 0.1, 0.25 and 0.5.

4. JRip is a learning algorithm based on association rules with reduced error pruning (REP). It produces logical rules for classification that are easy to understand and that can be converted into source code. It has two tunable hyper-parameters: number of folds for REP ($F \geq 3$, 4 values) and number of optimization runs ($O \geq 2$, 3 values).
5. SMO, uses the sequential minimal optimization algorithm to solve the quadratic programming problem which appears through the training of a support vector machine. It uses heuristics to partition the training dataset into smaller problems that can be solved analytically. In the experiment, the SMO used a square polynomial kernel given by $K(\mathbf{x}, \mathbf{y}) = (\mathbf{x}^T\mathbf{y} + 1)^2$. The complexity hyper-parameter (C) is tuned by selecting 20 values in the set 2^{2i+1}, $(-3 \leq i \leq 7)$.
6. NB is the Naive Bayes based on the Bayesian theorem with strong assumptions on the independent features. It belongs to the family of probabilistic classifiers and is suitable for large-size input data. The NB has no tunable hyper-parameter, and we use supervised discretization (D parameter).
7. RC (Random Committee) is an ensemble of random base classifiers, trained using different random seeds on the same data whose output is the average prediction of the base classifiers, in this case random classification trees. The number of iterations (I) is tuned with five values ($I \geq 10$).
8. RF is the well-known random forest classifier, an ensemble ("forest") of random decision trees, where each tree has a particular subset of input features. It classifies each pattern by selecting the tree that contains the majority of classifications among all the existing trees. The number of iterations (I) is tuned with four values ($I \geq 100$).

3.2 Data collection

As mentioned above, we built a dataset composed by 24 open source Java projects containing 12,587 classes and 1,852 KLOC, which were obtained from the SourceForge source code repository. Only projects written in Java were selected because it is the only programming language which is common to all the detection tools used. Furthermore, Java is one of the most common mainstream object-oriented languages, particularly in the context of open source software and *design smell* detection tools and it allows to evaluate many metrics. We also selected projects belonging to different domains and sizes in order to allow us complete the study. Finally, only projects in beta, mature or production/stable status were selected to discard prototypes or experimental software. Specifically, we obtained a set of projects with a long-life cycle (before 2013) with a history of several versions. Given the large number of available projects meeting these criteria in the SourceForge repository, we randomly selected 24 projects, provided that projects of all the domains and sizes are included. We selected the SourceForge repository because it is one of the most widely known, it is used in the context of open source software and it supplies useful metadata for projects.

The classification of a project into a specific domain is not straightforward because there is no general schema available that is universally agreed. Therefore, we followed the classification proposed in (Tempero et al., 2010) in which the projects are distributed into four main domain categories (Table 1), each one containing a set of different sub-domains which are not used in this study. With respect to project size, the categories refer to the TLOC (total lines of code) written for the whole project. This methodology is the most

Table 1 Domain categories and sub-domains

Category	Sub-domain
Software Development (Dev)	Parsers/Generators/Makers, Testing, Integrated Development Software (IDE), Software Development Kit (SDK)
Application Software (App)	Word Processor, Web Browser, Accounting, Graphic, Player
Diagram Generator/Data Visualization (Vis)	Graphical User Interface (GUI) Design Tool
Client Server Software (Cli)	Database, Application Server, Middleware, Content Management System (CMS)

commonly used in software sizing. In the dataset, the project size is divided into five categories from small to large, similarly to the study (Fontana et al., 2013) of project classification. The categories are defined as follows: S (small) for projects where $TLOC < 5,000$; $5,000 \leq$ SM (small-medium) $< 15,000$; $15,000 \leq$ M (medium) $< 40,000$; $40,000 \leq$ ML (medium-large) $< 100,000$; $100,000 \leq$ L (large) $< 500,000$. This is an ordinal scale, but we are going to treat it as nominal.

In order to classify the 24 projects into the 4 domains, we perform an independent survey with four of the five authors. This first round included a survey question for each project with the url of the project in SourceForge and asking for a rapid answer. After first round, a Kappa inter-rated agreement of 24 subjects and 4 raters was calculated 0.441 as moderate agreement with $p - value = 0$. An analysis of the survey's answers shows that 9 of the projects have 4/4 coincidence and other 7 have 3/4 coincidence. The rest of the projects have 2, 1, 1 but one of the answers was "Can't classify". The four authors open a discussion about "Can't classify" answer that shows that some authors knows previously these projects but other authors do not. Just visiting the SourceForge page quickly was not enough. Hence, the second round starts with an open discussion and sharing more information on the projects. The agreement was achieved and the fifth author review, check and agree.

Table 2 provides, for each project, its name and version. This table also characterizes the projects in terms of domain and size category. The TLOC is included because it is used to obtain size category, while the number of classes (NOC) is included for further comparison with the number of *god classes*.

In order to analyze the chosen projects, we carefully selected a source code analyzer tool that matched the following criteria: open source, measures Java source code and includes a high number of object-oriented metrics. The tool that best met these criteria and suited the needs of this study was *RefactorIT* version 2.7 (https://sourceforge.net/projects/refactorit/). It uses semantic rules and produces a set of 30 metrics at the levels of project, package, class and method, but in our case we exclude the 14 method metrics because they are not relevant for our study. *RefactorIT* offers great functionality in terms of the management of the metrics profile and user interface. Also, it supplies a catalog of refactoring operations that assists in the maintenance process. *RefactorIT* is available in two forms: as an integrated plug-in of Eclipse or as a standalone desktop tool.

The dataset requires a particular formalization so that it can be input into the machine learning classifiers. Each class in the dataset is represented by a row of 19 variables (X_1 to X_{19}), see Table 3, widely used in the literature (Khomh et al., 2011; Kreimer, 2005; Maneerat & Muenchaisri, 2011) for *design smell* detection. Variables X_1 to X_{16} involve

Table 2 Characteristics of the projects in the dataset.

Project name	Domain	Size Category (TLOC)	NOC
jAudio-1.0.4	App	L (117615)	416
JDistlib-0.3.8	App	M (32081)	78
Freemind-1.0.1	Vis	L (106396)	782
JCLEC-4-base	Dev	M (37575)	311
JasperReports-4.7.1	Dev	L (350690)	1797
Java graphplan-1.0	Dev	SM (1049)	50
SQuirreL SQL Client-3.7.1	Cli	ML (71626)	1138
Mpxj-4.7	App	L (261971)	553
KeyStore Explorer-5.1	Vis	ML (83144)	384
Apeiron-2.92	App	SM (8908)	62
DigiExtractor-2.5.2	App	M (15668)	80
FullSync-0.10.2	App	M (24323)	169
Angry IP Scanner-3.0	App	M (19965)	270
OmegaT-3.1.8	App	L (121909)	716
Plugfy-0.6	Dev	S (2337)	28
Lucene-3.0.0	App	ML (81611)	606
Matte-1.7	App	ML (52067)	603
Ganttproject-2.0.10	App	ML (66540)	621
sMeta-1.0.3	App	M (30843)	222
JFreechart-1.0.X	App	L (206559)	499
xena-6.1.0	Dev	ML (61526)	1975
JHotDraw-5.2	App	M (17807)	151
pmd-4.3.x	Dev	ML (82885)	800
checkstyle-6.2.0	Dev	ML (41104)	277

App Application Software; *Dev* Software Development; *Vis* Diagram Generator/Data Visualization; *Cli* Client Server Software; *L* Large; *ML* Medium-Large; *M* Medium; *SM* Small-Medium; *S* Small

numerical attributes which represent software metrics and include project ($X_1 \ldots X_5$), package ($X_6 \ldots X_9$) and class ($X_{10} \ldots X_{16}$). Some of these metrics are particularly related to *god class* characteristics such lack of cohesion (LCOM), number of attributes (NOA), and complexity (weighted methods per class, WMC).

Variables X_{17} and X_{18} represent the two types of project context information (domain, size category) as factors. Finally, variable X_{19} represents the class label (*god class* or not) as a binary scale (true or false). Its value should be defined manually by a team of experts. However, it would be very costly, so we used a combination of five *design smell* detection tools which are very popular and cited in the literature according to the results of our systematic mapping study (Alkharabsheh et al., 2018), commonly employed in *god class* detection (Khomh et al., 2011; Lanza & Marinescu, 2007; Fontana et al., 2012; Yamashita et al., 2015; Maiga et al., 2012), which offer a feasible and repeatable way to analyze our dataset. The five tools are *iPlasma* (Marinescu et al., 2005), *DECOR* (Moha, 2007), *JDeodorant* (Tsantalis et al., 2008), PMD (Copeland, 2005) and Together (Borland, 2008). PMD and iPlasma use the same strategy, which is a metric-based strategy. Moreover, both tools use the same three metrics defined in Lanza and Marinescu (Lanza & Marinescu, 2007) strategy but with different thresholds. These three metrics

Table 3 Definition of variables

Variable	Metric	Definition	Level
X_1	TLOC	Total Lines of Code	Project
X_2	NCLOC	Non-Comment Lines of Code	Project
X_3	CLOC	Comment Lines of Code	Project
X_4	EXEC	Executable Statements	Project
X_5	DC	Density of Comments	Project
X_6	NOT	Number of Types	Package
X_7	NOTa	Number of Abstract Types	Package
X_8	NOTc	Number of Concrete Types	Package
X_9	NOTe	Number of Exported Types	Package
X_{10}	RFC	Response for Class	Class
X_{11}	WMC	Weighted Methods per Class	Class
X_{12}	DIT	Depth in Tree	Class
X_{13}	NOC	Number of Children in Tree	Class
X_{14}	DIP	Dependency Inversion Principle	Class
X_{15}	LCOM	Lack of Cohesion of Methods	Class
X_{16}	NOA	Number of Attributes	Class
Project level information (nominal scale)			
Variable	Information	Value	
X_{17}	Project domain	App, Dev, Cli, Vis	
X_{18}	Project size category	L, ML, M, MS, S	
Smell detection (binary)			
Variable	Design Smell	According to	
X_{19}	God Class	PMD, iPlasma, JDeodorant, Décor, Together	

are Weighted Methods per Class (WMC), Access to Foreign Data (ATFD), Tight Class Cohesion (TCC). The general strategy is defined as:

```
WMC >= WMC_VERY_HIGH &&
       ATFD > FEW_ATFD &&
       TCC < TCC_VERY_LOW
```

PMD considers, according to the coded rule obtained from Github (https://github.com/pmd/pmd/blob/master/pmd-java/src/main/java/net/sourceforge/pmd/lang/java/rule/design/GodClassRule.java):

```
WMC_VERY_HIGH as 47,
FEW_ATFD as 5, and
TCC_VERY_LOW as 1/3
```

iPlasma considers, according to (Mihancea & Marinescu, 2005):

```
WMC_VERY_HIGH as 20,
FEW_ATFD as 4, and
TCC_VERY_LOW as 1/3
```

JDeodorant, according to (Fokaefs et al., 2009), uses a hierarchical agglomerative clustering algorithm on each class, obtaining clusters with attributes and methods of a class. This method could be named as feature (attribute and methods) clustering analysis to find opportunities for "Extract class" refactoring.

DECOR is rule based, as a mixture of semantic rules, metrics based rules, structural rules and association rules. DECOR generates the detection code automatically from a definition of the anti-pattern expressed in a domain-specific language (DSL) as shown in the rule card below, according to (Moha et al., 2010; Palomba et al., 2014). The metric rules deal with Number of Methods Declared (NMD), Number of Attributes Declared (NAD) and LCOM5 (a version of Lack of Cohesion of Methods proposed by Henderson-Sellers). Compared with iPlasma and PMD, it is based in different types of rules, not just metrics and it not even uses the same metrics set.

```
RULE_CARD: Blob {
  RULE: Blob {ASSOC: associated FROM: mainClass ONE
                                    TO: DataClass MANY}
  RULE: mainClass {UNION
                   LargeClassLowCohesion
                   ControllerClass}
  RULE: LargeClassLowCohesion {UNION
                   LargeClass
                   LowCohesion}
  RULE: LargeClass {(METRIC: NMD+NAD, VERY_HIGH, 20)}
  RULE: LowCohesion {(METRIC: LCOM5, VERY_HIGH, 20)}
  RULE: ControllerClass {UNION
          (SEMANTIC: METHODNAME,
          {Process, Control, Command,
          Manage, Drive, System}),
          (SEMANTIC: CLASSNAME,
          {Process, Control, Command,
          Manage, Drive, System})}
  RULE: DataClass {(STRUCT: METHOD_ACCESOR, 90)}
};
```

Together ControlCenter (Borland Together 12.6) calculates 55 quality assurance metrics for use in analyzing Java and C++ source code (Gronback, 2003). It includes Chidamber and Kemerer (CK) suite and it is also inspired in Lanza & Marinescu (LM). According to different papers in literature, Together's *god class* detection (as shown manually in (Gronback, 2003)) is metric-based using a combination of different metrics, including LM and CK suites but, to the best of our knowledge (see for example (Li & Shatnawi, 2007b)), the precise strategy and thresholds used are not published.

Note that in our data collection the *god class* labeling and the class metrics are independent, because the former is defined by these five detection tools as described before, while the laters are computed using *RefactorIT*. On the other hand, there is a lack of agreement on the detection (Paiva et al., 2017; Fenandes et al., 2016) and in the values of the metrics delivered by the different tools. This suggests that, although some of the software

Table 4 Characteristics of selected design smell detection tools

Tool	DECOR	JDeodorant	iPlasma	PMD	Together
Version	1.0	5.0.13	6.1	5.3.2	12.6.0
Type	Open source	Open source	Open source	Open source	Commercial
Strategy	Rule-based (mix)	Clustering analysis	Metrics-based	Metrics-based	Metrics-based
Language	Java	Java	Java, C++	Java, C, C++	Java, C++, C#
Refactoring	No	Yes	No	No	No
Environment	Standalone	Eclipse Plug-in	Standalone	Eclipse Plug-in, Standalone	Eclipse Plug-in
Metrics Report	No	No	Yes	No	Yes
Inputdata	Source code	Source code	Source code	Source code	Source code, UML
Outputdata	Textual	Textual	Textual, Visual	Textual	Textual
Commandline	No	No	No	Yes	No

detection tools might use code metrics similar to the ones used by us, rarely are exactly the same metrics, calculated the same and used in the same way, so the probability of biasing in *god class* labeling is very low.

All the tools support Java projects and the input data are the source code, while the output is text in different formats (CSV, TXT or XML). Also, most of the tools focus only on smell detection, except *JDeodorant*, which includes refactoring operations that are performed after the *design smells* have been identified. Nearly all the tools have a GUI except *PMD*, which only provides a CLI (command line interface). Some tools can be executed in different environments such as IDE plugins or standalone and generate metric reports. Table 4 summarizes the most important characteristics of each detection tool. In our experiment these five detection tools are used, each of which must be operated in its way, some adapting the project to be analyzed by the tool, obtaining and processing the result manually by the authors. The aspect named CLI value "No" (in Table 4) describes that the tool should be used throughout the graphical interface. The full dataset used in this study is available in XLS and CSV formats from https://citius.usc.es/investigacion/datasets/project-nominal-information.

Each single tool is not perfect, and specifically it is widely known in the literature that they are prone to detect false *god classes*. Additionally, each detection tool usually performs better on its own developing context, defined by the types of project used, experience of the experts who designed the tool, etc. However, combining several detection tools in a committee with an adequate fusion policy for the outputs of the tools may enhance the detection performance. Specifically, by requiring less tools to detect a *god class*, the undetected *god class* rate is expected to reduce. Conversely, by requiring more tools to detect a *god class*, false detection rate is expected to reduce, specially considering the low agreement among tools. In fact, we performed a manual analysis of cases labeled as *god class*, with extreme policies of 4 and more tools and 5 tools coinciding in detection. In the first case, we found 10% of false positives (8 cases out of 79) while, in the second, 4% (only one case, particularly strange, out of 24). These values are low compared to those reported for these tools in other studies (Moha & Gueheneuc, 2007; Fontana et al., 2011). We develop several experiments using different policies to combine the *god class* detection

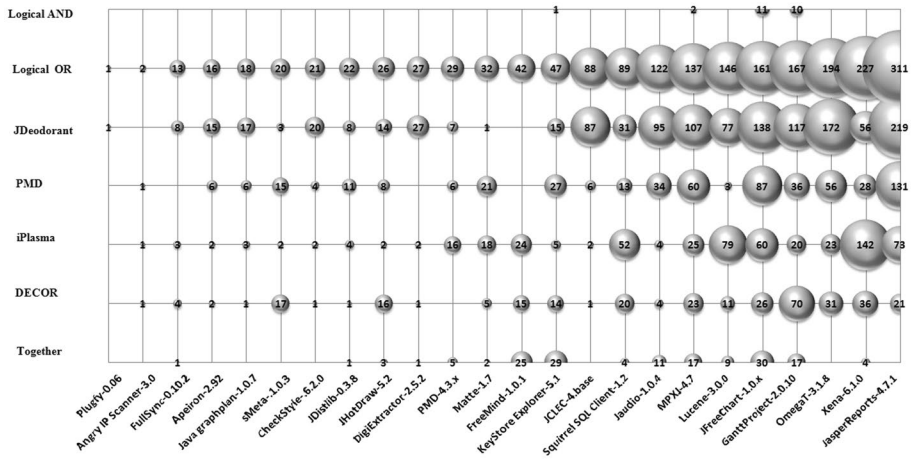


Fig. 1 God class distribution over the dataset using the selected tools

tools (e.g., a logical AND/OR of the five tools). These experiments also evaluate whether the influence on the *god class* detection of the information concerning the project context is consistent throughout the different policies.

Figure 1 shows the distribution of *god classes* detected over the dataset (24 projects) by the selected detection tools. The bubbles in the figure represent the number of *god classes* detected in each project by each tool. The two upper bubble rows represent the "Logical AND" of all the tools, i.e., the number of *god class* identified simultaneously by the 5 tools, and the "Logical OR" of all tools, i.e., the *god classes* identified by some tool, in other words, by one tool and more. The largest number of *god classes* was detected by *JDeodorant*, which is an open source tool, while the lowest number was detected by Together, which is a commercial tool. The majority of *god classes* were detected in projects that mostly belonged to the App domain and/or had a L size category, such as OmegaT-3.1.8, Lucene-3.0.0, GanttProject-2.0.10, Xena-6.1.0, Jasperreport-4.7.1, JAudio-1.0.4 and JFreeChart-1.0.x. For the same project, different *god class* detection results were obtained by different tools. Studying the agreement between the five selected detection tools, a Fleiss' Kappa test provides a Kappa value of 0.202, which means poor agreement among the five tools, with a p value of 0, which means that the differences between the five tools are statistically significant. Therefore, there exists a lack of agreement among the tools, which may be caused by the variety of techniques, algorithms and metric thresholds used by the tools.

From Table 5, it can be seen that the *god classes* are distributed among different categories of the investigated project context information. Since well-designed projects are expected to have zero or few *god classes*, in our dataset this label is low-populated, so the classification problem is unbalanced. For example, the S size category only includes one *god class*, because small-size projects should not include more than one or two *god classes*. In general, there exists a positive relation between the number of classes and the number of detected *god classes* in the same category (domain or project size), although there are some exceptions. For example, the ML size category has 6,404 classes and 758 *god classes*, while the L category that has 4,762 classes but 967 *god classes*. This behavior also happens considering projects instead of classes. The row labeled **Correlation** in

Table 5 Distribution of *god classes* among different nominal categories (N.Cat.) requiring one and more tools, two and more tools, three and more tools, four and more tools and five tools. Correlations between the number of *god classes* and the number of classes over the domain and size categories are also reported. The last line reports the number of projects, classes, *god classes* and *god class* percentages of the whole dataset for each number of tools

			#God Classes				
N.Cat.	Project	Class	≥1-tool (%)	≥2-tools (%)	≥3-tools (%)	≥4-tools (%)	5-tools (%)
Dev	7	5238	695 (13.3)	155 (2.9)	42 (0.8)	3 (0.1)	0 (0)
App	14	5046	1085 (21.5)	321 (6.4)	171 (3.4)	70 (1.4)	23 (0.5)
Vis	2	1165	89 (7.6)	43 (3.7)	18 (1.5)	3 (0.3)	1 (0.1)
Cli	1	1138	89 (0.8)	20 (1.8)	8 (0.7)	3 (0.3)	0 (0)
Correlation			0.93	0.85	0.69	0.54	0.53
L	6	4762	967 (20.3)	326 (6.8)	158 (3.3)	48 (1)	13 (0.3)
ML	8	6404	758 (11.8)	165 (2.6)	62 (1)	27 (0.4)	11 (0.2)
M	7	1281	198 (15.5)	36 (2.8)	15 (1.2)	2 (0.2)	0 (0)
SM	2	112	34 (30.4)	12 (10.7)	4 (3.6)	2 (1.8)	0 (0)
S	1	28	1 (3.6)	0 (0)	0 (0)	0 (0)	0 (0)
Correlation			0.93	0.80	0.74	0.84	0.94
Whole dataset	24	12,587	1,958 (15.5)	539 (4.3)	239 (1.9)	79 (0.6)	24 (0.2)

Table 5 reports the correlations between the number of total classes and the number of *god classes* for each number of tools: their high positive values show that the latter generally raises with the former. The unbalance between *god classes* and non-*god classes* is also clear in this table (the *god class* percentages never overcome 30.4%, 10.7%, 3.6%, 1.8% and 0.5% for each number of tools used simultaneously to detect the smell). The last row of Table 5 reports the total number of projects, classes and *god classes* and the percentage of *god classes*, for each combination policy (≥ 1 tool, ≥ 2 tool and so on). These percentages never overcome 16%, and they decrease very fast with the number of tools required to label a class as *god class*, increasing very much the classification unbalance.

3.3 Methodology

In this section we describe the experimental methodology, based on the use of several supervised classifiers to predict the presence of *god class design smell*. This methodology includes several steps which are related to the following **research question (RQ)**:

RQ: Does the project context information, specifically project domain and size categories, influence on the detection of the *god class design smell*?

Firstly, we identify *god class design smell* using a detection tool (*DECOR, JDeodorant, iPlasma, PMD,* and *Together*), listed in the Section 3.2. Our problem is modeled as a two-class classification problem: label 0 corresponds to "normal class" and label 1 corresponds to "*god class* ". We use the classifiers listed on the Section 3.1 to learn this classification problem. In order to build each classifier, the dataset is randomly splitted into a training set (60%), a validation set (20%) and a testing set (20%). Specifically, five randomly splitted groups of training/validation/test sets are generated (keeping the relative populations of both classes in each set), and the classifier performance is averaged over the five trials. Note that, since different policies of combining detection tools are considered, each policy

creates a different *god class* labeling, and thus a different dataset. In each trial, the training set is used to train the classifiers and the validation set is used to select appropriate values for their tunable hyper-parameters (e.g., the spread of Gaussian kernel for LibSVM), listed in Section 3.1. The hyper-parameter values are selected to provide the best average classification performance over the 5 validation sets. Finally, the classifier, trained on the 5 training sets using the selected values for the tunable hyper-parameters, runs on the 5 test sets, and the final performance is the average over these test sets.

The classification performance is measured by the Cohen Kappa (Blackman & Koval, 2000) statistic, in %, which measures the agreement between the true label and the one predicted by the classifier discarding the agreement by chance (e.g., caused by class unbalance, when all the patterns are classified in the majority class). This property has led us to select kappa over other statistics because the information in our problem is strongly unbalanced. The interpretation of Kappa values, denoted by K, which range from −100% to 100%, is as follows: excellent ($80\% < K < 100\%$), good ($60\% < K < 80\%$), moderate ($40\% < K < 60\%$), fair ($20\% < K < 40\%$) and poor ($K < 20\%$). According to this interpretation, in our study, we consider the classifier we built is good when the Kappa value is greater than 40%. The value of K is calculated as:

$$\begin{aligned} K =& 100\frac{a-e}{s-e}, \\ a =& TN+TP, \quad s = TN+FP+FN+TP \end{aligned} \tag{1}$$

$$e = \frac{(TN+FP)(TN+FN)+(FN+TP)(FP+TP)}{s} \tag{2}$$

where TN, FP, FN and TP are the number of true negatives, false positives, false negatives and true positives, respectively, and positives correspond to *God Class*. Note that K is always equal or lower than accuracy. The Matthews correlation coefficient (MCC, also in %) is another performance measure of the quality of binary classifications that is used to evaluate the classifiers (Matthews, 1975). The MCC has been evaluated in all the experiments, but for brevity we report mainly the Kappa values, although both measurements lead to the same conclusions.

In our experiments, we use the Wilcoxon rank sum test (Hollander et al., 2013) to evaluate the difference between the Kappa (resp. MCC) values achieved by the eight classifiers in two cases, e.g. with and without project context information. The null hypothesis of this test is that Kappa (resp. MCC) values in both cases come from distributions with equal mean. When the p value is below (resp. above) 0.05, this hypothesis can be rejected (resp. cannot be accepted), so the difference between cases is (resp. is not) statistically significant. The test also allows hypothesis such as "one distribution is greater than or equal to the other", but the procedure for rejecting the hypothesis is the same. This test is evaluated in R by the `wilcoxsign_test` function of the `coin` package which also provides a Z-factor that divided by $\sqrt{N}$, where N is the number of pairs of data, provides the effect size (notated by r) in whose interpretation we use the criteria widely accepted in the literature (Cohen, 2013): small effect size means $0.1 \leq r < 0.3$; moderate is $0.3 \leq r < 0.5$; and large is $0.5 \leq r$. These performance measures and this statistical test are widely used in the literature for similar purposes (Maneerat & Muenchaisri, 2011; Hall et al., 2011; Fontana et al., 2012; Fontana et al., 2013; Di Nucci et al., 2018).

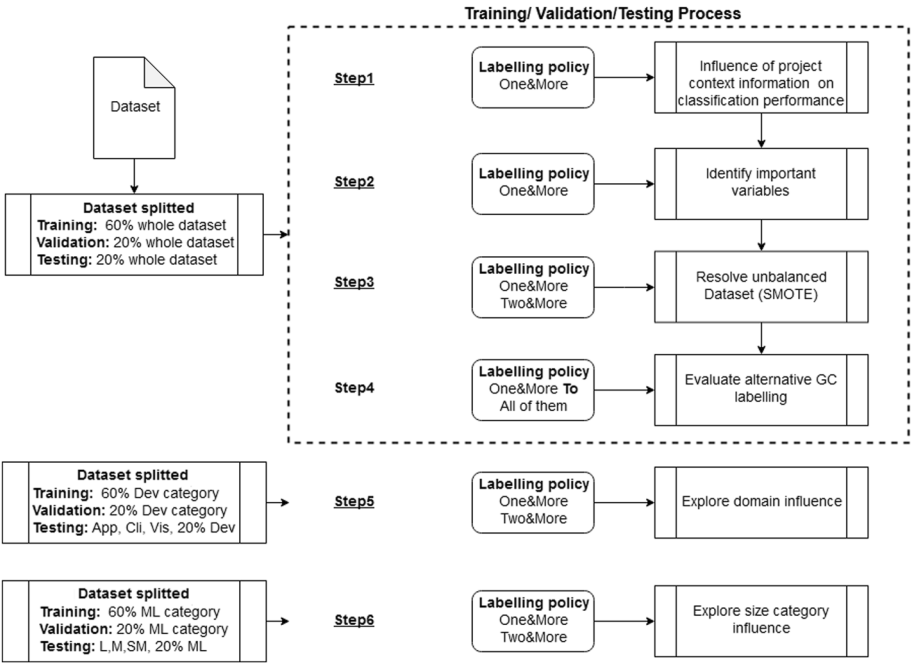


Fig. 2 Steps of the experimental work

The current study is organized in six steps (Fig. 2). The first four steps are applied on the whole dataset, while the last two divide the dataset in different project domains (step 5) and size categories (step 6).

Step 1. In this step we look at the following question RQ.S1: Is the performance of classifiers affected by the use of project context information? We simplify this preliminary study considering only the criterion of one tool and more for *god class* labeling because the problem of unbalanced information is less severe. In order to answer RQ.S1, we train, validate and test the eight classifiers using all numeric variables ($X_1, \ldots, X_{16}$, see Table 3) with and without the project context information (domain X_{17} and size category X_{18}). There are two null hypotheses for step 1: H_0^{Kappa} (resp. H_0^{MCC}) is that Kappa (resp. MCC) achieved by the classifiers without using project context information is greater or equal than using it, since our alternative hypothesis is that the performance of the classifiers improves using project context information. The comparison is performed according to the experimental methodology explained above. For the following steps, we select the configuration (with or without project context information) which provides the best classification performance (Kappa and MCC) over all the classifiers, according to a Wilcoxon test comparing the performances with and without project context information.

Step 2. Here, the question to explore is RQ.S2: Do we use unnecessary information which complicates the analysis without influence on the results? As in step 1, we use again one and more for *god class* labeling. First, we analyze the correlated variables, such as NOT (X_6) and NOTc (X_8), developing a test whose null hypothesis H_0^{NOT} is that classifiers achieve similar performance with and without the NOT variable. Two additional experiments train linear regressors with and without project context information in order to find

co-linear variables. Finally, the importance of the variables $X_1, \ldots, X_{16}$ and of the project context information is estimated, and a set of important variables is selected and compared to the whole set of variables. Three tests are developed: 1) the null hypothesis $H_0^{ALL,IMP+PCI}$ is that classifiers achieve equal Kappa using all the variables (ALL) and using the important variables (IMP) plus project context information (PCI); 2) the null hypothesis $H_0^{ALL,IMP}$ is that classifiers achieve equal Kappa using all variables and using only the important variables (IMP); and 3) the $H_0^{IMP,IMP+PCI}$ is that classifiers achieve the same Kappa using important variables plus project context information and using just important variables. The best configuration (all variables, important variables or important variables plus project context information) is selected for the next step.

Step 3. Since the *god classes* represent a small fraction of the available classes, their detection can be considered an unbalanced classification problem. The synthetic minority over-sampling technique (SMOTE) (Chawla et al., 2002) is a popular oversampling method for unbalanced classification problems. This method balances both labels by creating artificial training patterns of the minority label (in our case, *god class*), in the set used to train the classifiers. The step 3 investigates RQ.S3: Is the SMOTE technique useful to reduce the unbalance problem? A first experiment evaluates the null hypothesis H_0^{SMOTE}. It states that classifiers, using one and more tools as labeling criterion, achieve greater or equal Kappa without SMOTE than with it.

Step 4. This step investigates RQ.S4: What effect does *god class* labeling have on our problem? This question is analyzed from two angles. First, RQ.S4.1: Is the classifier performance influenced by the labeling policy? Second, RQ.S4.2: Does the project context information increase the classifier performance in all the labeling policies? When classes are labeled as *god class* by one and more *design smell* detection tools, it is expectable to have a high (resp. low) rate of false positives (resp. negatives). The increase in the number of tools required to label a class as *god class* change these rates and leads to a reduction in the number of *god classes*, so the classification problem becomes even more unbalanced. In order to take this fact into account, we develop experiments with different policies to combine the outputs of the detection tools. Specifically, we considered that a *god class* must be detected by two and more tools, by three and more tools, by four and more tools, and by the five tools (i.e., a logical AND of the five tools). To answer RQ.S4.1, we evaluate how these labelings influence the classifier performance. In order to answer RQ.S4.2, we compare the classifier performance without and with project context information using each of the previous alternative *god class* labelings. The null hypotheses of the tests associated to each experiment are listed in Table 6: hypotheses H_0^i, with $i = 2, \ldots, 5$, and H_0^{iC}, with $i = 1, 3, 4, 5$, correspond to RQ.S4.1, while H_0^{iW}, with $i = 1, \ldots, 5$, correspond to RQ.S4.2.

Step 5. The steps 5 and 6 look separately at each of the parameters of the project context information. First, we check RQ.S5: Can we discard the influence of project domain (X_{17}) in classifiers performance? In order to measure its impact on the *god class* detection, each classifier is trained using 60% of the classes of domain Dev, and validated for parameter tuning using 20% of its classes. We select Dev for training because it is the only domain whose classes belong to all the available size categories (SM, M, ML and L), so the project size is not expected to bias the results of this experiment. Afterwards, the classifier is tested: 1) using the remaining 20% of the classes from the same Dev domain; 2) using the classes in the App domain; 3) using the classes in the Vis domain; and 4) using the classes in the Cli domain. The objective of this experiment is to check whether the classifier performance is different on the classes of the same domain (Dev) used during training and for the remaining domains, in which case the classification is

Table 6 Null hypotheses of step 4 (PCI stands for project context information)

Name	Meaning
Classifiers achieve equal Kappa without PCI using One&More and using ...	
H_0^2	Two&More tools
H_0^3	Three&More tools
H_0^4	Four&More tools
H_0^5	Five tools
Classifiers achieve equal Kappa with PCI using Two&More and using ...	
H_0^{1C}	One&More tools (C means "context")
H_0^{3C}	Three&More tools
H_0^{4C}	Four&More tools
H_0^{5C}	Five tools
Classifiers achieve greater or equal Kappa without PCI than with it using ...	
H_0^{1W}	One&More tools (W means "with and without")
H_0^{2W}	Two&More tools
H_0^{3W}	Three&More tools
H_0^{4W}	Four&More tools
H_0^{5W}	Five tools

strongly influenced by the domain information. For example, assuming that domain is not relevant for *god class* detection, a classifier trained for domain Dev should behave similarly on the remaining 20% of Dev classes and on the classes of domains App, Cli and Vis. Otherwise, we must assume that the domain information is relevant. Because we do not use the entire dataset in this step, we selected one and more tools and two and more tools as *god class* labeling criteria, because they provide more cases and because, as we will see in Section 4.1, step 4 shows that the project context information improves the results with any criteria. The null hypotheses using one and more tools are that classifiers, trained using classes of domain Dev, achieve greater or equal performance in classifying classes from the App, Cli and Vis domains than with their own training domain, Dev. These hypotheses are denoted H_0^{DA1}, H_0^{DC1}, H_0^{DV1} respectively. Using two and more tools, the corresponding null hypotheses are denoted as H_0^{DA2}, H_0^{DC2} and H_0^{DV2}.

Step 6. Analogously to the previous step, we investigate RQ.S6: Can we discard the influence of project size (X_{18}) in classifiers performance? In order to answer this question, the classifiers are trained (resp. validated for parameter tuning) using 60% (resp. 20%) classes of size ML. Analogously to the previous step, we selected ML because it is the only size that contains classes of all the domains (App, Cli, Dev and Vis), in order to avoid that project domain may bias this experiment. The trained classifiers are tested: 1) using the remaining 20% of classes of size ML; 2) using classes of size SM; 3) using classes of size M; and 4) using classes of size L. Thus we evaluate whether the classifier performance is influenced by the class size. The same labeling criterion (one and more tools, and two and more tools) as in the previous step was taken for the same reasons.

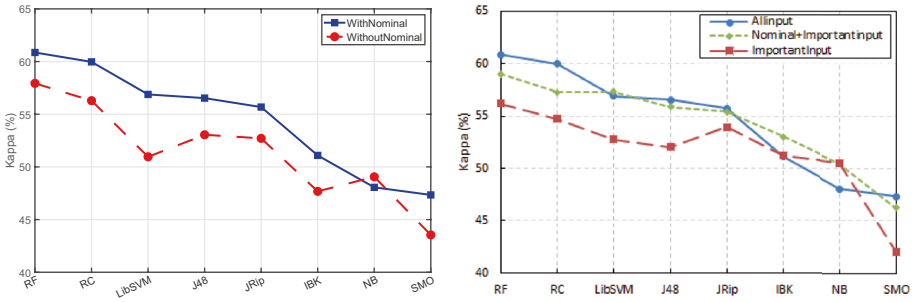


Fig. 3 Left: Kappa values of classifiers with and without project context information, sorted by decreasing Kappa values of the former to increase visibility. Right: Kappa values with all the variables, important variables and project context information, and important variables, sorted by decreasing values of the former

The null hypotheses with one and more tools are that classifiers, trained using size category ML, achieve greater or equal Kappa testing with projects classes in size category L ($H_0^{ML,L,1}$), size category M ($H_0^{ML,M,1}$), and size category SM ($H_0^{ML,SM,1}$), respectively, than with their own training size category, ML. With two and more tools, the analogous null hypotheses are denoted as $H_0^{ML,L,2}$, $H_0^{ML,M,2}$ and $H_0^{ML,SM,2}$.

4 Result and discussion

The results of the steps 1-6 described in the previous section are presented here. Section 4.1 compares the classification results with and without project context information (step 1), with the whole variables and only important variables (step 2), with and without SMOTE (step 3) and with several policies of tool combination as oracle (step 4). Sections 4.2 and 4.3 discuss the experiments in steps 5 and 6 of the methodology performed for each factor (project domain and size category) independently.

4.1 Training classifiers with the whole dataset

The left panel of Fig. 3 plots the Kappa achieved by the classifiers with and without factors (blue and red lines, respectively), sorted by decreasing values, according to **step 1** in the methodology. Excepting NB, with project context information the classifiers achieve higher Kappa (the red line is about 3% above the blue line). Therefore, the project context information, domain and size category (X_{17} and X_{18}) have a positive influence on *god class* detection. The best Kappa (above 60%) is achieved by RF using project context information. An asymptotic Wilcoxon-Pratt signed-rank test, and the same test with continuity correction, compare the Kappa values with and without the project context factors. The null hypotheses H_0^{Kappa} can be rejected, with p value 0.007812 (r =0.8416), assuming the alternative hypothesis "Kappa values are greater using project context information ", so the difference is statistically significant with a large effect size (r>0.5), which evidences that the project context information influences *god class* detection. The null hypotheses with MCC, H_0^{MCC}, is also rejected, with p value 0.01043. This is an example that Kappa and MCC lead to the same conclusion. For brevity, henceforth only p values of tests applied on Kappa will be reported.

Table 7 Kappa values (in %) achieved by the classifiers with all the variables (first row) and removing the NOT variable (second row)

	RF	RC	J48	JRip	IBK	NB	SMO	LibSVM
All variables	63.08	57.37	57.91	56.77	50.18	48.88	54.49	56.88
Without NOT	63.04	62.25	58.17	55.85	50.66	48.75	54.70	56.44

Step 2 of methodology identifies the group of important input variables, discarding multi-collinearity problems that lead to misleading in the classification results using the whole input variable set. For example, some variables might be related to each other from the first view, such as TLOC, CLOC, NCLOC or project size (see Table 3 above for the meaning of variables). However, in *RefactorIT* the sum of CLOC plus NCLOC is not equal to TLOC due to blank lines. Besides, the project size is given by the criterion defined in (Fontana et al., 2012) and not by *RefactorIT*. In fact, the Pearson correlation between TLOC and NCLOC is 0.90. However, other variables are more correlated, such as NOT and NOTc (0.99),X_6 and X_8 resp., because most types are concrete (see Table 3). Table 7 reports the Kappa achieved by the classifiers with and without the NOT variable. The values are very similar for all the classifiers (a Wilcoxon test gives a p value of 0.7422, and 0.6744 when asymptotic with continuity correction is applied). Thus, the null hypothesis H_0^{NOT} cannot be rejected and the difference is not statistically significant, which means that indeed the variable NOT can be excluded, but the dependence between NOT and NOTc is not a problem for classification. The correlations between NOC (X_{13}) and NOT, and between NOC and NOTc, are lower (0.94). The remaining correlations between variables are below 0.9. On the other hand, we trained two linear regressors, with and without project context information, and we checked that the coefficients of both linear models are finite, so the collinearity problems are not very important.

Additionally, we determined the importance of each variable using the rminer package (Cortez, 2015) written in the R statistical computing language. Specifically, we selected as important variables those which importance, given by the Importance function of the rminer package, overcomes 0.5. Ten variables were selected: WMC, EXEC, RFC, TLOC, NOTc, DIT, NOA, NCLOC, DC and CLOC. The variables NOT and NOC were excluded due to their high correlations with NOTc, which was included. Surprisingly, the nominal variables size category and domain achieved low importance values and were also excluded. The reason is that Importance ranks variables according to its relevance, but variables with low relevance (domain and size among others, in this case) can still influence on the god class detection, as step 1 evidenced. The right panel of Fig. 3 shows the classifier performance using all the variables, the important ones plus the project context information and only the important variables. A slightly better performance is achieved using the whole input set (blue line), followed very closely by the important variables with project context information (green line), and the worst Kappa values are achieved by the case of using just important variables without project context information (red line). Although the addition of project context information (green line) works similarly to the whole input set, the reduction in performance caused by feature selection, alongside with the low number (16) of features, suggests to keep all the variables for the next experiments. The differences between Kappa values of blue and green, blue and red, and green and red lines where analyzed with both a Wilcoxon and a Wilcoxon-Pratt test with continuity correction, used to avoid data tie warnings. Table 8 shows the null hypothesis, p value and effect size

Table 8 Null hypotheses, p values and effect size r of the tests in step 2

	p value (effect size r)	
Null hypothesis	Wilcoxon Test	Wilcoxon-Pratt
$H_0^{ALL,IMP+PCI}$	0.7422 (0.1485)	0.6744 (0.1485)
$H_0^{ALL,IMP}$	0.05469 (0.6931)	0.04995 (0.6931)
$H_0^{IMP+PCI,IMP}$	0.01562 (0.8416)	0.01729 (0.8416)

(r) of each test. The difference between all the features (ALL) and the important features (IMP) with project context information (PCI), denoted as IMP+PCI, is not significant (first row). This suggests that the use of PCI avoids a high performance reduction compared to all features, although the effect size is small ($r < 0.3$ in line 1 of Table 8). The difference between ALL and IMP (second row) is significant, so removing PCI the performance (Kappa) degrades significantly (IMP). Finally, the difference between PCI+IMP and IMP is also significant, so the PCI raises significantly the performance compared to IMP, in the last two cases with large effect size. These three tests evidenced that domain information brings the difference between the best performance (achieved with ALL or PCI+IMP, which are not significantly different) and alternatives without PCI such as IMP, which is significantly different with lower performance.

The left part of Table 9 reports the average confusion matrix over the 5 trials, the Kappa and MCC (both in %) achieved by the best classifier (RF) using all the variables. The diagonal values are high, but the percentage of non-detected *god classes* (false negatives) is relatively high (7.16%) compared to the percentage of *god classes* correctly detected (9.89%) although the false positive percentage is low (2.79%). The Kappa and MCC are high (60.86% and 61.76%, respectively). Since the performance degrades using the selected important features, in the following steps the experiments will use the whole set of metrics.

The experiment of **step 3** develops classification using SMOTE to balance both classes in the training set. Figure 4 shows that most classifiers obtained slightly better performances when the training set is balanced, excepting RC, LibSVM and IBK. The right part of Table 9 reports the confusion matrix of RF, which achieves the best Kappa, using SMOTE with one and more tools. Compared to the left part, the percentage of true positives is raised from 9.89% to 11.37%, whole the false negatives reduces from 7.16% to 5.68%. However, the false positive arise from 2.79% to 4.47%, while Kappa and MCC raise slightly. The conclusion of the experiment is that classifiers only perform slightly better using SMOTE, because the difference between classifications with and without SMOTE is not statistically significant. A Wilcoxon test comparing both collections of Kappa values gives a p value (resp. r value) of 0.1719 (resp. 0.3722), above 0.05 and a moderate effect size, between 0.3 and 0.5, so the hypothesis H_0^{SMOTE} of that performance without SMOTE

Table 9 Confusion matrix, Kappa and MCC (in %) of the best classifier (RF) using all input variables including project context information (PCI), left part, using SMOTE (S) with one and more tools, right part

	RF (PCI)		RF (S,≥ 1 tool)	
	Non-GC	GC	Non-GC	GC
Non-GC	80.17	2.79	78.49	4.47
GC	7.16	9.89	5.68	11.37
	Kappa(%)	MCC(%)	Kappa (%)	MCC (%)
	60.86	61.76	63.08	63.14

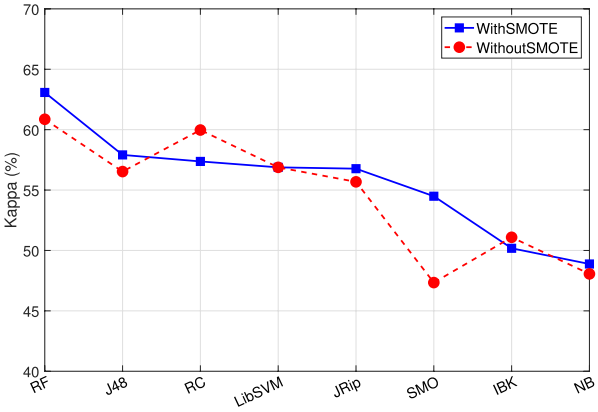


Fig. 4 Kappa values of classifiers with project context information using one and more tools with and without SMOTE, sorted by decreasing blue values

is greater or equal to with it cannot be rejected. However, given that class unbalancing will increase in the following experiments, the SMOTE method will be applied in order to achieve the best, although realistic, available performance.

According to **step 4**, we also developed experiments considering alternative class labels where a class is considered *god class* only when it is labeled as *god class* by: 1) three and more tools; 2) four and more tools; and 3) five tools. The objective of these experiments is to analyze the classifiers behavior related to the number of tools required to label a class as *god class*. Obviously, the more tools required to label a *god class*, the lower number of *god classes*, so the classification problem will be more unbalanced and the performance is expected to be reduced. From Table 5, the *god classes* vary from 15.55% of the classes with one and more tools to 0.19% with five tools. In other words, requiring less tools to detect a *god class* (e.g., by performing a logical OR of the tool outputs) the undetected *god class* rate is reduced, while requiring more tools (e.g., by performing an logical AND of the tool outputs) the false *god class* rate is reduced. In order to check this false positive reduction, a manual analysis of the *god class* detected was performed with the criteria of 4 tools and more and in the case of 5 tools. In the first analysis we detect 8 false positives among the 79 *god classes* and, in the second one, there is only one case, particularly strange, among 24 *god classes*, as mentioned in the introduction to the study design.

Figure 5 reports the Kappa of the different classifiers using the whole variables without (left panel) and with (right panel) project context information, SMOTE and one tool and more, two tools and more, three tools and more, four tools and more and five tools. The classifier's sortings by decreasing Kappa are very similar in both panels, being RC and RF the bests, followed by J48 and JRip. The Kappa values with one and more tools (black line) is the highest in the left plot, while the Kappa with two and more tools (blue line) is the highest in the right plot.

Inside each panel, a comparison among lines shows that Kappa degrades when the number of tools is increased. This degradation is faster without project context information (left plot) than with it (right plot). In fact, in the left plot the black, blue, red and gray lines are fairly separated, while in the right plot the blue, black and red lines are very near, the gray line is near them for the best classifiers, and the performance degrades dramatically with five tools (green line). This graceful degradation of Kappa on the right plot with increasing number of tools supports the positive contribution of project context information to the *god class* detection. The Wilcoxon test comparing

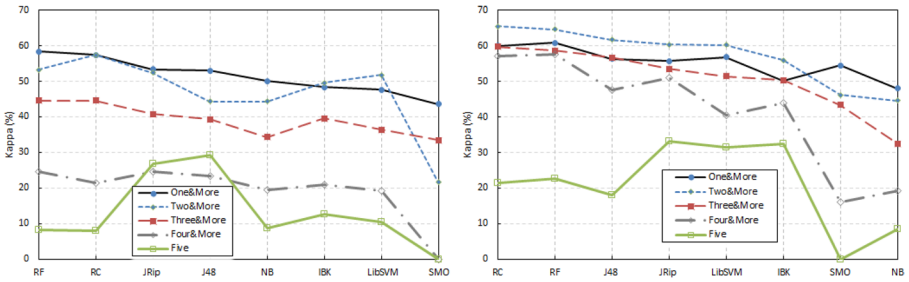


Fig. 5 Left panel: Kappa values of classifiers without project context information using SMOTE with one and more tools, two and more tools and so on. Right panel: Kappa values of classifiers with project context information using SMOTE with one and more tools, two and more tools and so on. Classifiers are sorted by decreasing Kappa of the best line

the difference between the best series and the others in each panel produce similar results. By comparing in the left panel the best line (black) with the other lines, which gives p values below 0.05 (statistically significant difference) and large effect size ($r > 0.5$) for red, gray and green lines are 0.007812, with r =0.8911 for H_0^3, H_0^4 and H_0^5, which are rejected (the values are equal because all the points in each series are below the black line). In the right panel, the differences between the blue line (the best one) and the others are statistically significant for red, gray and green lines, with the same p and r values as in the left panel for H_0^{3C}, H_0^{4C} and H_0^{5C}, respectively, which are rejected, so these lines are clearly different. In both plots, blue, black and red lines achieve the best Kappa values. This suggests that using four and more and five tools (gray and green lines) reduces too much the number of *god classes*, leading to severely unbalanced problems where the conclusions of our study would be biased by the low classification performance.

Comparing both figures, which share the vertical scale, Kappa is higher on the right plot (i.e., with project context information) for each line, and the differences are statistically significant using One&More, Two&More, Three&More and Four&More tools and test has large effect size, with p values (r) of 0.01172 (0.7921), 0.003906 (0.8911), 0.007812 (0.8416), 0.007812 (0.8416), respectively, for the null hypotheses H_0^{1W}, H_0^{2W}, H_0^{3W} and H_0^{4W}. The difference is not statistically significant with five tools, probably due to the strong unbalance problem, with p value (r) of 0.05323 (0.5707), using a Wilcoxon-Pratt test due to ties, for H_0^{5W}, which cannot be rejected. Nevertheless, the p value is too close to 0.05.

Therefore, the following experiments are developed using one and more tools and two and more tools labeling policies.

4.2 Influence of project domain

We conducted the experiment described in **step 5** of the methodology using SMOTE and applying both criteria (one and more tools, two and more tools) separately on the domain category in order to test again the influence of domain information on the *god class* detection. Figure 6 shows the Kappa achieved by the classifiers when they are: 1) trained and tuned using Dev category (using 60% and 20% of Dev classes, respectively); 2) tested using the

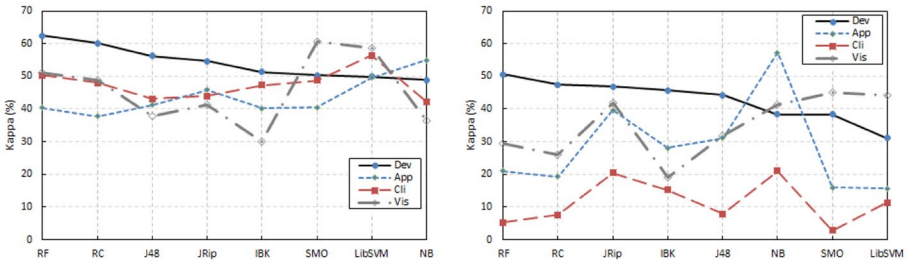


Fig. 6 Left: Kappa values of classifiers with domain information using one and more tools with SMOTE. Right: same plot using two and more tools. Classifiers are sorted by decreasing Kappa of the domain Dev

remaining 20% of Dev classes (black line), and using also the classes of App, Cli and Vis categories using one and more tools (left panel) and two and more tools (right panel). According to the left panel, the highest Kappa values are obtained in almost all the classifiers on the Dev category (SMO, LibSVM and NB are exceptions). Using the Wilcoxon test, the performance of the classifiers on the Dev domain in which they are trained is significantly superior to that obtained when used on the other App, Cli and Vis domains, presenting in all cases low p values and large effect sizes (r): 0.01172 (0.792118), 0.01953 (0.7426) and 0.01953 (0.7426). Thus, their respective null hypotheses H_0^{DA}, H_0^{DC} and H_0^{DV} are rejected.

Table 10 (two left parts) reports the confusion matrix achieved by RF training and testing with Dev classes with one and more tools, and with two and more tools: the Kappa value using one and more tools (62.43%) is lower than the value in the right part of Table 9 (63.08%), so despite of training and testing with classes of the same domain (Dev), the results are worse than using the whole dataset.

Applying the second criterion (*god class* is detected by two and more tools, right part of Fig. 6), the Kappa values are clearly lower than using one and more tools, but for domain Dev (black line) they overcome again the other domains for five of eight classifiers. This seems to confirm the influence of the project domain category on the *god class* detection. However, using two and more tools the Kappa values achieved by NB, SMO and LibSVM for domains App and Vis (blue and gray lines in the right panel of Fig. 6) overcome the corresponding Dev values. We will see in the next subsection that this effect also happens, even with more intensity, with the project size category and NB, although this classifier, alongside with SMO and LibSVM, achieve the globally worst Kappa values (see Subsection 5.3). The specific Kappa and MCC values are reported by rows 3-10 of Table 11 using SMOTE and two and more tools and varying the domain category (first two rows report the Kappa and MCC

Table 10 Confusion matrix, Kappa, and MCC (in %) of the best classifier (RF, JRip) using all the input variables and SMOTE (S): training and testing with Software Development (Dev) domain with one tool and more (1T) and two tools and more (2T); training and testing with Medium Large (ML) size with 1T and 2T

	RF(Dev, S, 1T)		RF(Dev, S, 2T)		RF(ML, S, 1T)		JRip(ML, S, 2T)	
	Non-GC	GC	Non-GC	GC	Non-GC	GC	Non-GC	GC
Non-GC	83.18	3.37	95.82	1.03	84.80	3.27	95.51	1.92
GC	4.94	8.5	1.68	1.47	4.83	7.09	1.5	1.08
	Kappa (%)	MCC(%)	Kappa (%)	MCC (%)	Kappa (%)	MCC (%)	Kappa (%)	MCC (%)
	62.43	62.59	50.66	62.53	59.3	59.10	36.91	37.07

Table 11 Kappa (K) and MCC (in %) achieved by each classifier using SMOTE and two and more tools: with all project context nominal variables (upper part); training with Dev domain and testing with the domain categories (middle part); training with ML size and testing with the size categories (lower part). Best values are in bold

Classifier	RF	RC	J48	JRip	IBK	NB	SMO	LibSVM
AllNominal-K(%)	64.67	**65.55**	56.1	61.72	60.44	56.03	46.25	60.21
AllNominal-MCC(%)	64.94	**65.96**	61.76	60.56	59.59	51.46	47.64	60.25
Dev-K (%)	**50.65**	47.36	44.3	46.91	45.71	38.28	38.26	31.08
Dev-MCC (%)	**51.02**	47.76	44.34	47.73	45.75	47.14	43.57	32.17
App-K (%)	20.95	19.29	31.09	39.61	28.07	**57.25**	15.96	15.68
App-MCC (%)	28.52	26.72	34.32	41.76	31.25	**60.45**	27.76	22.91
Cli-K (%)	5.33	7.6	7.79	20.46	15.22	**21.05**	2.82	11.39
Cli-MCC (%)	5.56	7.7	7.9	21.32	16.47	**32.94**	4.44	11.43
Vis-K (%)	29.49	25.97	31.66	41.73	18.98	41.21	**44.99**	44.06
Vis-MCC (%)	30.84	27.38	32.49	41.92	19.36	50.1	**51.61**	45.85
ML-K (%)	35.56	36.5	33.51	**36.91**	30.49	27.98	18.04	22
ML-MCC (%)	36.91	**37.73**	33.58	37.07	30.47	36.98	21.03	26.68
L-K (%)	23.52	23.02	30.11	32.58	30.3	**57.48**	28.29	11.67
L-MCC (%)	31.61	30.43	33.92	35.14	33.78	**60.63**	36.78	19.63
M-K (%)	15.53	15.32	22.97	28.29	24.41	**59.39**	8.22	20.67
M-MCC (%)	26.26	25	26.41	33.22	27.35	**63.42**	18.78	30.71
SM-K (%)	28.62	28.62	29.87	40.48	41.46	**68.89**	13.97	21.55
SM-MCC(%)	40.83	40.83	36.25	46.49	45.02	**69.47**	27.36	34.76

values using all the classes with project context information). Training with Dev domain, RF achieves the best result (Kappa 50.66%) on Dev testing, but NB outperforms this value for App (57.25%). Besides, the best Kappa for Cli and Vis are achieved by NB and SMO (21.05% and 44.99%, respectively), with values lower than Dev and App. The Wilcoxon test evaluates as significantly greater, with large size effects, the performance of classifiers in the ML domain than in the App and Cli domains with p values and effect size (r) of 0.03906 (0.6436) and 0.003906 (0.003906) respectively, so their null hypotheses H_0^{DA2} and H_0^{DC2} are rejected, while H_0^{DV2} (Vis domain) is not rejected (p =0.125, r =0.4456). In this domain, the performance overcomes the Dev domain for classifiers NB, SMO and LibSVM because they perform poorly on the training domain (Kappa below 40%). On the other hand, we find particularly strange the performance of NB in domains App and Vis because it is often suboptimal in the remaining experiments, so its reliability in this case should be low.

For each of the remaining categories (App, Cli and Vis) we repeated the previous experiment but training the classifiers with domain categories other than Dev. For category App, we trained and tuned the classifiers using 60% and 20% of classes, respectively, and tested using the remaining 20% of the App classes. Afterwards, these classifiers are tested using the Cli, Dev and Vis classes. The process was repeated training with Cli and Vis instead of App, using one and more tools. The results (not included here for brevity) showed that the Kappa values achieved by the classifiers trained with Dev category overcome the Kappa values achieved training with App, Cli or Vis categories. A possible explanation may be that these categories do not include information about some project size categories, so they depend on size as well as domain.

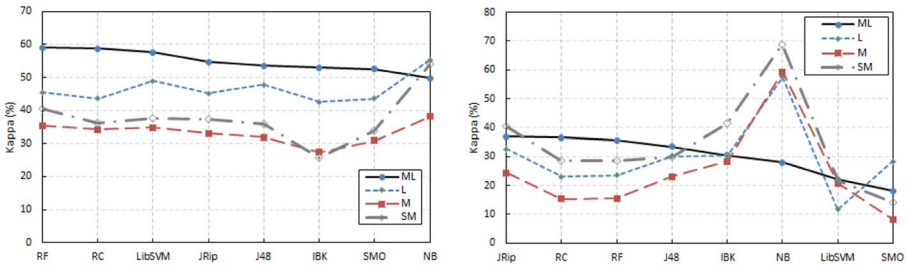


Fig. 7 Left panel: Kappa values achieved by the classifiers testing with ML and the other size categories using one and more tools with SMOTE. Right panel: Kappa values of classifiers with size information using two and more tools using SMOTE. Classifiers are sorted by decreasing Kappa of the ML size category

4.3 Influence of project size

In the experiment of **step 6** we selected the ML size category for the classifier training, while the test was conducted using the ML, SM, M and L categories. We follow the same strategy of step 5 replacing the Dev domain category by the ML size category, because it includes classes from App, Cli, Dev and Vis, so it should not be so influenced as the other size categories by the project domain. We also applied both criteria of detecting the *god class* (one and more tools, and two and more tools). The left panel of Fig. 7 shows the Kappa values of each classifier testing with ML category (black line) and the remaining size categories using one and more detection tools. The black line overcomes the remaining lines, which evidences that the size factor should also be taken into account in *god class* detection. The only exception is classifier NB, which overcomes ML for L and SM size categories. Using one and more tools, the differences between ML and the other size categories are statistically significant, and the Wilcoxon test gives p values (r values) of 0.01562 (0.8416), 0.007812 (0.8911), 0.01562 (0.8416) for L, M and SM lines, respectively, so the corresponding null hypotheses $H_0^{ML,L,1}$, $H_0^{ML,M,1}$ and $H_0^{ML,SM,1}$ are rejected. The third confusion matrix in Table 10 is achieved by RF, which is the best classifier training and testing with ML size category and using one and more tools. The value of Kappa (59.3%) is lower than the one achieved using Dev (first confusion matrix in the same table), which empirically shows that the ML category is not so significant for sizes as Dev category for domains. The rates of false positives and negatives (3.27% and 4.83%, respectively) are only slightly lower than their corresponding Dev values, so the worse behavior is explained by the low number of *god classes* for ML categories compared to Dev domain, and by the low true positive rate (7.09%) compared to Dev (8.5%) in Table 10.

However, in the right panel (with two and more tools) of Fig. 7 the black line only overcomes the other lines for three classifiers, and for the remaining five classifiers (JRip, IBK, NB, LibSVM and SMO) their lines are near or above the black line, so the classifiers achieve equal or better Kappa testing on size categories different to the one used for training. The Kappa values of NB in sizes L, M and SM are specially high compared to ML, refusing our thesis that performance must be higher in the training size category. However, the NB is one of the classifiers which achieves globally the lowest Kappa values (see Subsection 5.3). Therefore, using two and more tools the results do not seem influenced by the project size category. The confusion matrix of JRip, which is the best classifier training and testing with ML size categories and two and more tools, is showed by Table 10 (right part):

the Kappa value (36.91%) is much lower than using Dev domain category (50.66%, second matrix in the same table), and the difference between ML and Dev is much higher using two and more tools (50.66 – 36.91= 13.75) than using one and more tools (62.43 – 59.3 = 3.13). This means that the increase on the classification unbalance caused by requiring two and more tools (from Table 5, *god class* percentages are 15.5% and 4.3% using ≥ 1 and ≥ 2 tools, respectively) affects more the significance of the ML size category than of the Dev domain category.

The specific values of Kappa and MCC training with ML size classes and testing with ML and the remaining sizes are reported by rows 11-18 of Table 11. Again, these values support the comments of the previous paragraph, because using two and more tools the influence of size category it is not so clear as in the domain category case, and we cannot establish that ML is significantly greater than L, M and SM. The p values and effect size (r) are 0.2305 (0.297), 0.09766 (0.4951) and 0.4727 (0.0495), respectively, on a Wilcoxon test, so the corresponding null hypotheses $H_0^{ML,L,2}$, $H_0^{ML,L,2}$ and $H_0^{ML,SM,2}$ cannot be rejected. The reason of this lower influence of the size on the results, compared to domain, may be the ordinal nature of the size category. Besides, the criterion to label a class with a given size category is not so crisp as with domain categories. Analyzing the classifier Kappa and MCC for each size category and two and more tools (lower block of Table 11), the RC achieves the best Kappa (37.73%) testing with ML, but NB is the best for the other size categories with higher Kappa values (57.48%, 59.39% and 68.89% for L, M and SM, respectively), so the *god class* detection works better testing on size categories other than the training size category.

Analogously to the domain case, we trained classifiers with the L, M and SM categories separately, testing them with the same and different size categories and one and more tools. The classifiers trained on these categories exhibited lower performance than those trained with ML size category (the results are not included here for brevity). This arises that results are better when only one factor (in this case, the project size) is taken into account.

5 Replication

In this section a replication of Step 1 of the previous study is presented. The general *RQ* applies, and particularly the specific Step 1 question *RQ*.*S*1 is adapted as *RQ*.*S*1.*rep*.

RQ.*S*1.*rep*: Is the performance of classifiers affected by the use of project context information? using a new dataset in which a human labeling policy has been followed?

We use exactly the same null hypotheses H_0^{Kappa} for Step 1 of the previous study: is Kappa achieved by the classifiers without using project context information greater or equal than using it?

The Machine Learning techniques are the same used in the previous study. The rest of the steps in the methodology do not apply because of the nature of the dataset labeling. The new data collection is described in the next subsection.

5.1 Data collection

The new dataset is generated on the basis of a dataset available in the current literature and previously used in (Pecorelli et al., 2019; Pecorelli et al., 2020).

Table 12 Characteristics of the projects in the replication dataset.

Project name	Domain	Size Category (TLOC)	NOC	(GCs)
ant-rel-1.8.3	L (119256)	Dev	1473	6
argouml-VERSION_0_14	L (199075)	Vis	1373	2
cassandra-cassandra-1.1.0	L (110712)	Cli	699	2
apache-wicket-1.4.11	L (174033)	Dev	1568	4
derby-10.3.3.0	XL (535187)	Cli	1746	24
hadoop-release-0.2.0	M (34662)	Dev	327	2
hsqldb-2.2.0	L (254014)	Cli	590	11
incubator-livy-0.6.0-incubating	L (130696)	Cli	1016	6
nutch-release-0.7	ML (50578)	Cli	532	0
qpid-0.18	L (189271)	Cli	2172	6
xerces-Xerces-J_1_4_2	L (150445)	Dev	489	6
eclipse-R3_4	L (423423)	Dev	5061	25
elasticsearch-v0.19.0	L (315619)	Cli	1395	2
Total			18441	96

Dev Software Development; *Vis* Diagram Generator/Data Visualization; *Cli* Client Server Software; *XL* Very Large; *L* Large; *ML* Medium-Large; *M* Medium

This dataset consists of manually validated code smells instances of 125 releases of 13 projects. The original dataset (Pecorelli et al., 2019) is modified in order to 1) select a release of each project; 2) select *god class* labeling; 3) include the project context information (domain and size category), and 4) include the metrics used in the study described in Section 3. The new dataset is available at the same url mentioned before: https://citius.usc.es/investigacion/datasets/project-nominal-information.

The release of each project was selected using the one with more *god classes*. In our case has no sense to work with different releases of the same project because we are interested in study the influence of the project context and this is the same from one release to other.

Table 12 shows a description of the dataset collected in this replication and the number of *god classes* labeled in each project.

The classification in domains was conducted in the same way the previous study. In this case, no agreement was reach in first round with incubator-livy and hadoop projects. In second round with open discussion was detected a problem with the url describing the incubator-livy project and an agreement was reached after the four authors consult the same project documentation. No agreement was reached in the case of hadoop in second round. Two hadoop experts external to this study. Both indicates the same classification and finally the agreement was reached taking into account the experts opinion. The fifth author, as in the previous study, review, check and agree.

5.2 Results and discussion of the replication

The classifiers used in the previous experiments (see Table 3 in Section 3.2) are applied on the dataset described in Subsection 5.1, including the project context information.

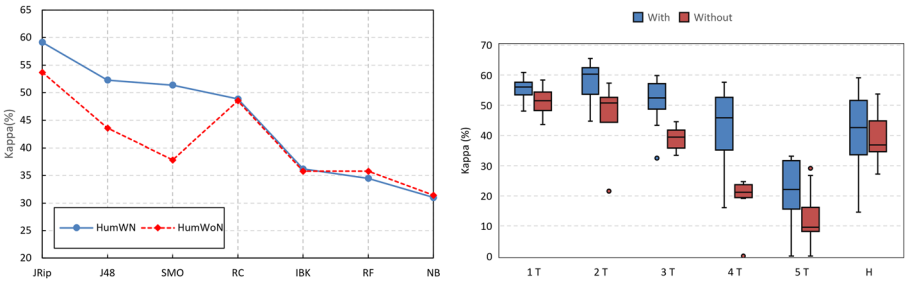


Fig. 8 Left panel: Kappa achieved by classifiers with and without project context information using the *god class* identification of human experts, sorted by decreasing blue values. Right panel: box plot of Kappa for values of classifiers with (blue) and without (red) project context information with labeling polices one and more tools (1T) and so on, and with human labeling (H)

Figure 8 (left panel) shows that the best Kappa values are obtained over the classifiers trained with the project context information, although several of them achieve values below 40%. In this dataset the number of *god classes* labeled by human experts are 96 of a total of 18,441 classes, which represents 0.52% of true positives. This percentage is slightly lower than the one using 4 tools and more and higher than using 5 tools and more in out dataset (see Table 5). The degradation shown in the Kappa achieved by the classifiers with and without project context information in step 4 of our methodology led us to discard this labeling in our final analyses. We also applied the classifiers after a re-sampling with SMOTE, achieving similar results.

Figure 8 (right panel) shows that the average performance decreases with the number of *god classes* marked by the labeling policy (one, two, three, four and five tools and more, and human). In all the policies, the blue box plot (with project context information) is above the red box plot (without project context information). Using the human labeling (right end of the plot), the blue (resp. the red) box plot is similar to (resp. much higher than) using 4 tools and more. The Kappa ranges achieved by the different classifiers (i.e., the box plot height) also change by reducing the number of *god class*. When we have enough examples all the algorithms offer consistently close results (narrow box plots), but when the true examples are reduced the Kappa obtained by the different algorithms are more arbitrary with larger ranges (wider box plots). In addition, the *god classes* are not distributed across all the categories used in previous experiments, so only the initial analysis has been carried out with all the information.

In order to review the question RQ.S1.rep (is the performance of classifiers affected by the use of project context information?) over the human labeled dataset, we conducted a new Wilcoxon test on the Kappa values of the figure. The null hypothesis is that the classifiers trained without project context information achieve Kappa greater than or equal to those trained with project context information. The p value 0.2734 of this test does not allow us to reject the null hypothesis and assume the relevance of the project context information in improving the performance of the classifiers, although the effect size of the test is moderate (r=0.2475). This also happened using 5 tools and more, despite classifiers with better Kappa (excepting RC) achieve higher Kappa trained with project context information (see Fig. 8, left panel). Looking at the right panel of Fig. 8, the Kappa achieved using project context information is always greater than or equal to the one achieved without project context information. This is confirmed by a Wilcoxon test defined by the null hypothesis: the average performance of all the algorithms without using project context information is higher or equal than when using it? This test provides a p value of 0.015 with a large effect size (r=0.78).

Table 13 Kappa median and mean, and Friedman rank achived by each the classifier over all the experiments

Position	1	2	3	4	5	6	7	8
Method	RF	RC	JRip	J48	LibSVM	NB	IBK	SMO
Median	51.0	48.7	46.9	44.3	49.6	48.1	45.7	43.3
Mean	45.5	44.3	45.5	43.3	42.4	42.8	40.6	34.8
Rank	2.6	3.6	3.6	4.1	4.4	5.4	5.5	6.8

5.3 Joint analysis by classifier

Table 13 reports the Kappa median and mean, and Friedman rank (Demšar, 2006) of each classifier, calculated over all the experiments developed in the paper. The first row labels the classifier positions given by their Friedman rank. The RF achieves the best position, which corresponds to the lowest rank (2.6), which means that RF is between positions 2 and 3 in average over all the experiments. Note that RF achieves the best Kappa in most experiments (see e.g. Figs. 3, 4, 5, excepting Four&More and Five tools, left panels of Figs. 6 and 7), and the only exceptions are experiments with severe class unbalance (right panels in Figs. 6 and 7). The RF also achieves the best Kappa median (51.0%) and mean (45.5%), tied with JRip (Table 13). A group of classifiers with ranks between 3.6 and 4.4 includes RC, JRip, J48 and LibSVM. Another group includes NB and IBK (with ranks 5.4-5.5), while SMO achieves the worst rank, with the lowest performance. Please note that the classifiers with the best performance (and lower ranks) are those that support, in the previous experiments, our hypothesis that project context information is important for *god class* detection. Conversely, those classifiers with lower ranks (e.g. NB, IBK and SMO) are the ones which support in a lower degree these hypotheses and exhibit erratic behavior.

6 Threats to validity

This section discusses threats to the validity in the context of the conducted experiments. Construct validity is concerned with the relationship between theory and observation. Internal validity relates to any negative effects on the experiment design, while external validity relates to the significance of the experiment outcomes.

6.1 Construct validity

The selection of the tools and techniques are probably the main threat to the construct validity of the experiment. Firstly, we selected five software tools *DECOR, PMD, JDeodorant, Together* and *iPlasma* which allow *god class* detection, work with Java projects extracted from the SourceForge repository, are available for us and are widely used in the literature. These tools are completely independent because they are developed by different research groups in Software Engineering. Secondly, the metrics are evaluated using *RefactorIT*, a software tool which is independent from the previous ones and is widely used in the literature. We did not use several tools for metric evaluation because, although their metric values and the classifier performance for *god class* detection might differ among tools, the conclusions

of the experiments (e.g., influence of the project context information for *god class* detection) should be similar. Thirdly, we selected a collection of classifiers representing the current state-of-the-art, so it is not expectable that classifiers excluded from this collection exhibit a behavior different to our experiments. Finally, the distribution of projects among domains may seem biased at first glance, given the low number of projects in domains Cli and Vis. However, note that this unbalance reflects the natural distribution of projects across domains, because the selection of projects was random. Therefore, selecting an artificial balanced distribution would not be realistic, biasing the results of the experimentation for sure.

6.2 Internal validity

The main threat to internal validity relates to the lack of consensus in the literature on the correct definition of the *god class design smell*. We managed this threat by using a committee of five different *god class* detection tools (open source and commercial), which are based on different detection strategies, to label each class as *god class* or not. Some of these tools, such as DECOR, include the definition of the anti-pattern *god class* which generates the detection code automatically from a definition of the anti-pattern expressed in a DSL and considering different heuristics.

We used several policies to combine the single labels in order to increase the reliability of the committee label (e.g., to avoid false *god class* labels), and to evaluate how the conclusions of the experiments hold for each policy (step 4 in our methodology). Since the results (e.g., the influence of project context information) are consistent among policies (except requiring ≥ 4 tools, due to the low *god class* rate), it is expectable that these results would also be consistent with a manual *god class* labeling issued by an expert, but such an experimentation falls outside the scope of the current work.

Another threat is related to the low number of *god classes*, which leads to an unbalanced classification problem. We minimized the impact of this unbalance on *god class* detection using a well-known oversampling technique called SMOTE (step 3 in our methodology). The last threat involves the potential relations of dependence and redundancy among class metrics, alongside with the irrelevance of some metrics for *god class* detection, a threat that is already analyzed in step 2. With respect to project context information (domain and size category), whose relevance for *god class* detection is explored in the current work, the threat involves the subjectivity of the project categorization in terms of domain and size, and the bounds in the number of categories. To overcome this threat in relation to the domain categories, we used the same classification as (Fontana et al., 2013), which was initially proposed by (Tempero et al., 2010) for the Qualitas Corpus. As regards the size categories, we considered in the current work the ones defined by (Fontana et al., 2013).

6.3 External validity

Some decisions taken during the experiment construction might constrain the generalization of results. All the software tools used for *god class* detection worked with Java projects, specifically from the SourceForge repository, so we restricted to the open source SourceForge projects due to availability reasons. We randomly selected projects from the repository until there is at least one project of each domain and size category, achieving a collection which represents the distribution of projects in the repository. The experiments involved a high number of classes of each domain and size, although the number of projects is low for some domains and sizes, and this might be another threat to the validity of the conclusions. As a threat to generalization it is

possible that other results arises when experimenting with projects of other domains, age, and not open source. Nevertheless, the exploratory study results are promising. Hence, a large-scale study that avoid this generalization threats should be developed. Finally, the experiments can be hardly extrapolated to other *design smells*, because the selected tools or the metrics used may not be adequate to detect the new *design smells*. Besides, other problems similar to class unbalance might appear for other *design smells*, which would require an specific analysis.

7 Conclusion and future work

In this work, we study the influence of two factors related to the project context information on the automatic detection of *god class design smell*. Specifically, we aimed to investigate whether machine learning classifiers increase their performance for *god class* detection when some project context information, i.e. domain and size category, is included. To this end, we designed an exploratory but broad study to ascertain whether these two project factors are relevant to eight well-known classifiers, and whether they would affect the effectiveness agreement (Kappa) of the classification outcomes. In order to evaluate whether the conclusions depend on the classifier, we selected a collection of classifiers including the support vector machine with Gaussian kernel, the k-nearest neighbor classifier, the C4.5 decision tree, association rules with reduced error pruning, sequential minimal optimization, naive Bayes classifier, random committee and random forest. For the experiments, we created a large dataset consisting of 12,587 classes from 24 Java software projects with different size categories and domains. We selected five commonly used *design smell* detection tools to automatically detect the *god classes* in the dataset, and developed an analysis of the class metrics importance for feature selection. Since the classification problem is unbalanced by nature, we used the synthetic minority over-sampling technique (SMOTE) in order to increase the classifier performance. We also studied the validity of our experiments considering several labeling policies to combine the five software tools for *god class* detection. According to the Kappa values, in our first experiment with the whole dataset and only one labeling policy (one tool and more), the null hypothesis H_0^{Kappa} of that performance without project context information is greater or equal than with it is rejected. This evidences the influence of project context information in this case, giving a positive answer to the research question RQ.S1. When analyzing whether we use unnecessary information (RQ.S2), we discarded collinearity between variables. We also analyzed the variable importance to see if some variable is not useful for *god class* detection, but surprisingly the project context information was identified as of low importance. However, when analyzing their influence through the null hypotheses $H_0^{ALL,IMP+PCI}$, $H_0^{ALL,IMP}$ and $H_0^{IMP,IMP+PCI}$, each comparing the performance using the groups of variables indicated in their names, we found that the selection of the most important features only degrades significantly the results when the project context information is excluded. Thus, all the available information must be considered useful, giving a negative answer to the RQ.S2.

Since the *god class* detection is an unbalanced classification problem, we used SMOTE to balance the training set. Although we can see in Fig. 4 that the performance increases slightly, the null hypotheses H_0^{SMOTE} of that performance without SMOTE is greater or equal to with it cannot be rejected. As a consequence, we cannot obtain empirical evidence of the usefulness of the technique in this case, giving a negative answer to RQ.S3.

In the step 4 of our methodology we reviewed the influence of *god class* labeling policies, specifically of the number of tools required to identify a *god class*, on the classification performance, since the class unbalancing increases when more tools are required.

Through the null hypotheses listed in Table 6 we compare the classifier performances achieved by the best policy and by the others. We found that the same hypothesis are rejected with (H_0^3, H_0^4 and H_0^5) and without (H_0^{3C}, H_0^{4C} and H_0^{5C}) project context information so the RQ.S4.1 (do the labeling policy influences the performance?) can be answered positively because the performance of the classifiers changes strongly from using 3 tools and more regardless of whether the project context information is used or not.

Regarding RQ.S4.2 (does the project context information increase the classifier performance in all the labeling policies?), the answer is also positive, because the null hypotheses that performance without project context information is greater or equal than with project context information is rejected in all the labeling policies (H_0^{iW}, with $i = 1, \ldots, 4$) except with five tools (H_0^{5W}) due to the huge class unbalancing. This analysis is complemented with the study carried out on the replication with the second dataset labeled by human experts. In this case the hypothesis about the influence of the project context information on the performance of the classifiers could not be confirmed. This study also shows that the average performance of classifiers with project context information is better than without project context information. Finally, the average performance of the algorithms and their dispersion is consistent with the degradation observed in Step 4 when the number of *god classes* is reduced.

Finally, when the project domain and size categories are studied separately, the performance of the classifiers is reduced for domain categories that were not present in the classifier training. This effect is weaker for size categories when a labeling policy requiring two and more *god class* detection tools is applied. In both cases, we observe changes in the classifiers' performance, so we cannot rule out the influence of these parameters and questions RQ.S5 (can we discard the influence of project domain on the classifiers performance?) and RQ.S6 (the analogous for project size) are answered negatively. Therefore, we conclude that project domain and size are important for detecting the *design smell* in the context of the classes that are analyzed through machine learning classifiers, and they should be taken into account in future works to obtain a more accurate *god class* detection. However, the project domain exhibits stronger influence compared to project size, whose influence is not so clear using two and more tools for *god class* identification. The weakness of the influence of nominal project size may be affected by also using project size as an ordinal feature. In fact, step 2 points out TLOC as one of the most important characteristics.

We have seen how project context information influences all the experiments: RQ.S1, RQ.S2 and RQ.S4 reflect that performance changes significantly when considering the project context information regardless of the labeling used, except for the most extreme situation (five tools and more labeling policy); besides, RQ.S5 and RQ.S6 also show that influence of domain and size cannot be discarded independently for each factor. Consequently, the research question RQ (does the project context information influence the *god class* detection?) can be answered positively.

Taking these factors (project domain and size category) into account is part of adaptation to the context which should be investigated in a more general sense in future work, including other project context information. Project domain is known before starting the project development but size category is particularly interesting to be researched in future work combined with methods for predicting project size category before starting the project. This can help developers from the very beginning to adequate *god class* detection to the project context.

Considering classifiers, the random forest and random committee achieve the best performance in most experiments, exhibiting stable and consistent behaviors. On the contrary, sequential minimal optimization and naive Bayes report the poorest performances with lack of agreement to the other classifiers.

Regarding other project context factors that can influence on detection there are some possibilities to study such as the software development process, as various authors suggests. Although in (Crespo et al., 2006) the influence of the type of software such as application, framework, library was analyzed and found no evidence of this project characteristic on relative metric's thresholds. Nevertheless, an specific and larger study can be conducted in order to check whether it influences in design smell detection, including SPLs as another type of software construction to analyze. It is also important to study the presence of automatically generated parts of software in some projects and how can it influences in design smell detection.

In addition to the former, our future work will focus on replicating the experiments reported in this work, using different datasets labeled by experts involving professional developers and QAs from the industry in identifying true *god classes*. We also believe that our findings can guide developers to better focus their efforts to identify other *design smells*.

References

Alkharabsheh, K., Crespo, Y., Manso, E., and Taboada, J. (2016). Comparación de herramientas de detección de design smells. In Jornadas de Ingeniera del Software y Bases de Datos, pages 159–172.

Alkharabsheh, K., Crespo, Y., Manso, E., and Taboada, J. (2016). Sobre el grado de acuerdo entre evaluadores en la detección de design smells. In Jornadas de Ingeniera del Software y Bases de Datos, pages 143–157.

Alkharabsheh, K., Crespo, Y., Manso, E., Taboada, J. (2018). Software Design Smell 1356 detection: a systematic mapping study. *Software Quality Journal* http://dx.doi.org/10.1007/s11219-018-9424-8

Alves, T. L., Ypma, C., and Visser, J. (2010). Deriving metric thresholds from benchmark data. In *Proceedings of the 2010 IEEE International Conference on Software Maintenance*, ICSM '10, pages 1–10. IEEE Computer Society.

Azadi, U., Fontana, F. A., Taibi, D. (2019). Architectural smells detected by tools: A catalogue proposal. In *Proceedings of the Second International Conference on Technical Debt*, TechDebt '19, page 88–97. IEEE Press.

Azeem, M. I., Palomba, F., Shi, L., & Whang, Q. (2019). Machine learning techniques for code smell detection: A systematic literature review and meta-analysis. *Information and Software Technology, 108*(4), 115–138.

Bekkar, M., Djemaa, D. K., & Alitouche, D. A. (2013). Evaluation measures for models assessment over imbalanced data sets. *Journal of Information Engineering and Applications, 3*(10).

Blackman, N., & Koval, J. (2000). Interval estimation for Cohen's kappa as a measure of agreement. *Statistics in Medicine, 19*(5), 723–741.

Borland (2008) Together. http://www.borland.com/together

Brown, W. H., Malveau, R. C., McCormick, H. W., & Mowbray, T. J. (1998). *AntiPatterns: refactoring software, architectures, and projects in crisis*. John Wiley & Sons Inc.

Chawla, N. V., Bowyer, K. W., Hall, L. O., & Kegelmeyer, W. P. (2002). SMOTE: synthetic minority over-sampling technique. *Journal of Artificial Intelligence Research, 16,* 321–357.

Choinzon, M., & Ueda, Y. (2006). Detecting defects in object oriented designs using design metrics. In *J. Conf. on Knowledge-Based Software Engineering*, pages 61–72.

Cohen, J. (2013). *Statistical power analysis for the behavioral sciences*. Academic press.

Copeland, T. (2005). *PMD applied*. Centennial Books.

Cortez, P. (2015). *A tutorial on using the rminer r package for data mining tasks*. Technical report: Univ. do Minho. Escola de Engenharia.

Counsell, S., & Mendes, E. (2007) Size and frequency of class change from a refactoring perspective. In *Int. Conf. on Software Evolvability*, pages 23–28.

Crespo, Y., Lopez, C., & Marticorena, R. (2006). Relative thresholds: Case study to incorporate metrics in the detection of bad smells. In *Proceedings of 10th ECOOP Workshop on Quantitative Approaches in Object-Oriented Software Engineering*, pages 109–118. Universita della Svizzera italiana Press.

Demšar, J. (2006). Statistical comparisons of classifiers over multiple data sets. *Journal of Machine Learning Research, 7,* 1–30. http://dl.acm.org/citation.cfm?id=12485413967.1248548

Di Nucci, D., Palomba, F., Tamburri, D. A., Serebrenik, A., & De Lucia, A. (2018). *Detecting code smells using machine learning techniques: are we there yet?* (pp. 612–621). Evolution and Reengineering: In Intl. Conf. on Software Analysis.

P, F., D, N, D., D, R, C,, & D, L, A. (2019). A large empirical assessment on the role of data balancing in machine-learning-based code smell detection - online appendix. https://figshare.com/s/5da162e21b8d54fbfce8

Fernandes, E., Oliveira, J., Vale, G., Paiva, T., & Figueiredo, E. (2016). A review-based comparative study of bad smell detection tools. In *Proc. 20th Intl. Conf. on Evaluation and Assessment in Software Engineering*, page 18.

Fokaefs, M., Tsantalis, N., Chatzigeorgiou, A., Sander, J. (2009). Decomposing object1407 oriented class modules using an agglomerative clustering technique. In: *2009 IEEE International Conference on Software Maintenance*, pp 93–101. https://doi.org/10.1109/ICSM.2009.5306332

Fontana, F. A., Braione, P., & Zanoni, M. (2012a). Automatic detection of bad smells in code: An experimental assessment. *Journal of Object Technology, 11*(2), 5–1.

Fontana, F. A., Braione, P., & Zanoni, M. (2012b). Automatic detection of bad smells in code: An experimental assessment. *Journal of Object Technology*, *11*(2):5:1–38. https://doi.org/10.5381/jot.2012.11.2.a5

Fontana, F. A., Ferme, V., Marino, A., Walter, B., & Martenka, P. (2013). Investigating the impact of code smells on system's quality: An empirical study on systems of different application domains. In *International Conference on Software Maintenance*, pages 260–269.

Fontana, F. A., Ferme, V., Zanoni, M., & Yamashita, A. (2015). Automatic metric thresholds derivation for code smell detection. In *IEEE/ACM 6th International Workshop on Emerging Trends in Software Metrics (WETSoM)*, pages 44–53, Los Alamitos, CA, USA, IEEE Computer Society.

Fontana, F. A., Mäntylä, M. V., Zanoni, M., & Marino, A. (2016). Comparing and experimenting machine learning techniques for code smell detection. *Empirical Software Engineering, 21*(3), 1143–1191.

Fontana, F. A., Mariani, E., Mornioli, A., Sormani, R., & Tonello, A. (2011). *An experience report on using code smells detection tools* (pp. 450–457). Verification and Validation Workshops: In International Conference on Software Testing.

Fontana, F. A., Zanoni, M., Marino, A., & Mantyla, M. V. (2013). Code smell detection: Towards a machine learning-based approach. In *Int. Conf. on Software Maintenance*, pages 396–399.

Fourati, R., Bouassida, N., & Abdallah, H. (2011). A metric-based approach for anti-pattern detection in UML designs. *Computer and Information Science*, pages 17–33.

Fowler, M., & Beck, K. (1999). Refactoring: improving the design of existing code. *Addison-Wesley,*. Professional.

Gronback, R. C. (2003). *Software remodeling: Improving design and implementation quality, using audits, metrics and refactoring in borland together controlcenter*. A Borland White Paper: Technical report.

Guggulothu, T., & Moiz, S. A. (2020). Code smell detection using multi-label classification approach. *Software Quality Journal, 28,* 1063–1086.

Hall, T., Beecham, S., Bowes, D., Gray, D., & Counsell, S. (2011). Developing fault-prediction models: What the research can show industry. *IEEE Software, 28*(6), 96–99.

Hassaine, S., Khomh, F., Guéhéneuc, Y. G., & Hamel, S. (2010). Ids: an immune-inspired approach for the detection of software design smells. In *International Conference Quality of Information and Communications Technology*, pages 343–348.

Herbold, S., Grabowski, J., & Waack, S. (2011). Calculation and optimization of thresholds for sets of software metrics. *Empirical Software Engineering, 16*(6), 812–841.

Hollander, M., Wolfe, D. A., & Chicken, E. (2013). *Nonparametric statistical methods* (Vol. 751). John Wiley & Sons.

Khomh, F., Vaucher, S., Guéhéneuc, Y. G., & Sahraoui, H. (2011). BDTEX: A GQM-based Bayesian approach for the detection of antipatterns. *Journal of Systems and Software, 84*(4), 559–572.

Kreimer, J. (2005). Adaptive detection of design flaws. *Electronic Notes in Theoretical Computer Science, 141*(4), 117–136.

Lanza, M., & Marinescu, R. (2007). *Object-oriented metrics in practice: using software metrics to characterize, evaluate, and improve the design of object-oriented systems*. Springer Science & Business Media.

Li, W., & Shatnawi, R. (2007). An empirical study of the bad smells and class error probability in the post-release object-oriented system evolution. *Journal of Systems and Software, 80*(7), 1120–1128. https://doi.org/10.1016/j.jss.2006.10.018

Linares-Vásquez, M., Klock, S., McMillan, C., Sabané, A., Poshyvanyk, D., & Guéhéneuc, Y. G. (2014). Domain matters: Bringing further evidence of the relationships among anti-patterns, application domains, and quality-related metrics in java mobile apps. In *Proceedings of the 22Nd International Conference on Program Comprehension*, ICPC 2014, pages 232–243. ACM.

Liu, H., Liu, Q., Niu, Z., & Liu, Y. (2016). Dynamic and automatic feedback-based threshold adaptation for code smell detection. *IEEE Transactions on Software Engineering, 42*(6), 544–558.

Lopez Nozal, C. (2012). *Design defects detection based on code metrics (in Spanish)*. PhD thesis, Dpto. Informatica, Universidad de Valladolid.

Maiga, A., Ali, N., Bhattacharya, N., Sabane, A., Gueheneuc, Y. G., & Aimeur, E. (2012). Smurf: A svm-based incremental anti-pattern detection approach. In *International Conference on Reverse engineering*, pages 466–475.

Maiga, A., Ali, N, Bhattacharya, N., Sabané, A., Guéhéneuc, Y. A., Antoniol, G., & Aïmeur, E. (2012). Support vector machines for anti-pattern detection. In *International Conference Automated Software Engineering*, pages 278–281.

Maneerat, N., & Muenchaisri, P. (2011). Bad-smell prediction from software design model using machine learning techniques. In *International Journal of Conference on Computer Science and Software Engineering*, pages 331–336.

Marinescu, C., & Marinescu, R., Mihancea, P. F., & Wettel, R. (2005). iPlasma: An integrated platform for quality assessment of object-oriented design. In *International Conferrence Software Maintenance - Industrial and Tool Volume*, pages 77–80.

Matthews, B. W. (1975). Comparison of the predicted and observed secondary structure of t4 phage lysozyme. *Biochimica et Biophysica Acta (BBA)-Protein Structure, 405*(2):442–451.

Mihancea, P. F., & Marinescu, R. (2005). Towards the optimization of automatic detection of design flaws in object-oriented software systems. In *Ninth European Conference on Software Maintenance and Reengineering*, pages 92–101.

Mistrik, I., Soley, R., Ali, N., Grundy, J., & Tekinerdogan, B. (Eds.). (2015). *Software Quality Assurance*. In Large Scale and Complex Software-intensive: Morgan Kaufmann.

Moha, N. (2007). Detection and correction of design defects in object-oriented designs. In *Conf. on Object-oriented Programming Systems and Applications companion*, pages 949–950.

Moha, N. & Guéhéneuc, Y. G. (2007). DECOR: a tool for the detection of design defects. In *Intl. Conf. on Automated Software Engineering*, pages 527–528.

Moha, N., Gueheneuc, Y. G., Duchien, L., & Le Meur, A. F. (2010). Decor: A method for the specification and detection of code and design smells. *IEEE Transactions on Software Engineering, 36*(1), 20–36.

Morales, R., Soh, Z., Khomh, F., Antoniol, G., & Chicano, F. (2017). On the use of developers context for automatic refactoring of software anti-patterns. *Journal of Systems and Software, 128,* 236–251.

Mori, A., Vale, G., Viggiato, M., Oliveira, J., Figueiredo, E., Cirilo, E., Jamshidi, P., & Kastner, C. (2018). Evaluating domain-specific metric thresholds: An empirical study. In *Proceedings of the 2018 International Conference on Technical Debt*, TechDebt '18, pages 41–50. ACM.

Munro, M. J. (2005). Product metrics for automatic identification of "bad smell" design problems in java source-code. In *International Conference Software Metrics*, pages 15.

Paiva, T., Damasceno, A., Figueiredo, E., & Sant Anna, C. (2017). On the evaluation of code smells and detection tools. *Journal of Software Engineering Research and Development, 5*(1):7.

Palomba, F., Bavota, G., Di Penta, M., Oliveto, R., & De Lucia, A. (2014). Do they really smell bad? a study on developers' perception of bad code smells. In *Intl. Conf. on Software maintenance and evolution*, pages 101–110.

Palomba, F., Bavota, G., Di Penta, M., Oliveto, R., Poshyvanyk, D., & De Lucia, A. (2015). Mining version histories for detecting code smells. *IEEE Transactions on Software Engineering, 41*(5), 462–489.

Palomba, F., Lucia, A. D., Bavota, G., & Oliveto, R. (2014). Anti-pattern detection: Methods, challenges, and open issues. In A. Memon, editor, *Advances in Computers*, volume 95, chapter 4, pages 201–238. Elsevier.

Pecorelli, F., Di Nucci, D., De Roover, C., & De Lucia, A. (2020). A large empirical assessment of the role of data balancing in machine-learning-based code smell detection. *Journal of Systems and Software, 169.*

Pecorelli, F., Palomba, F., Di Nucci, D., & De Lucia, A. (2019). Comparing heuristic and machine learning approaches for metric-based code smell detection. In *Proceedings of the 27th International Conference on Program Comprehension*, ICPC '19, page 93–104. IEEE Press.

Peiris, M., & Hill, J. H. (2014). Towards detecting software performance anti-patterns using classification techniques. *ACM SIGSOFT Software Engineering Notes, 39*(1), 1–4.

Powers, D. (2011). Evaluation: from precision, recall and f-measure to roc, informedness, markedness and correlation. *International Journal of Machine Learning Technologies, 2*(1), 37–63.

Rapu, D., Ducasse, S., Gîrba, T., & Marinescu, R. (2004). Using history information to improve design flaws detection. In *Conference on Software Maintenance and Reengineering*, pages 223–232.

Rasool, G., & Arshad, Z. (2015). A review of code smell mining techniques. *Journal of Software: Evolution and Process, 27*(11), 867–895.

Riel, A. J. (1996). *Object-oriented design heuristics*, volume 335. Addison-Wesley Reading.

Santos, J. A., de Mendonça, M. G., & Silva, C. V. (2013). An exploratory study to investigate the impact of conceptualization in god class detection. In *International Conference on Evaluation and Assessment in Software Engineering*, pages 48–59.

Santos, J. A. M., Rocha-Junior, J. B., Prates, L. C. L., do Nascimento, R. S., Freitas, M. F., & de Mendona, M. G. (2018). A systematic review on the code smell effect. *Journal of Systems and Software*, 144:450 – 477.

Shatnawi, R. (2015). Deriving metrics thresholds using log transformation. *Journal of Software: Evolution and Process, 27*(2), 95–113.

Simons, C., Singer, J., & White, D. R. (2015). Search-based refactoring: Metrics are not enough. In M. Barros and Y. Labiche, editors, *Search-Based Software Engineering*, pages 47–61. Springer International Publishing.

Tahvildar, L., & Kontogiannis, K. (2004). Improving design quality using meta-pattern transformations: a metric-based approach. *Journal of Software: Evolution and Process, 16*(4–5), 331–361.

Tempero, E., Anslow, C., Dietrich, J., Han, T., Li, J., Lumpe, M., Melton, H., & Noble, J. (2010). The qualitas corpus: A curated collection of Java code for empirical studies. In *Asia Pacific Software Engineering Conference*, pages 336–345.

Tsantalis, N., Chaikalis, T., & Chatzigeorgiou, A. (2008). Jdeodorant: Identification and removal of type-checking bad smells. In *Intl. Conf. on Software Maintenance and Reengineering*, pages 329–331.

Tufano, M., Palomba, F., Bavota, G., Oliveto, R., Di Penta, M., De Lucia, A., et al. (2015). When and why your code starts to smell bad. *Intl. Conf. on Software Engineering-Volume,1*, 403–414.

Vale, G., Fernandes, E., & Figueiredo, E. (2019). On the proposal and evaluation of a benchmark-based threshold derivation method. *Software Quality Journal, 27*(1), 275–306.

Witten, I. H., Frank, E., Hall, M. A., & Pal, C. J. (2016). *Data Mining: Practical machine learning tools and techniques*. Morgan Kaufmann.

Yamashita, A., & Moonen, L. (2013). Exploring the impact of inter-smell relations on software maintainability: An empirical study. In *International Conference on Software Engineering*, pages 682–691.

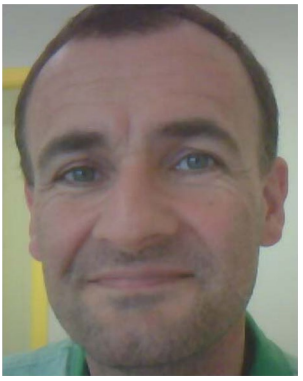
Yamashita, A., Zanoni, M., Fontana, F. A., & Walter, B. (2015). Inter-smell relations in industrial and open source systems: A replication and comparative analysis. In *International Conference on Software Maintenance and Evolution*, pages 121–130.

Zhang, M., Hall, T., & Baddoo, N. (2011). Code bad smells: a review of current knowledge. *Journal of Software: Evolution and Process, 23*(3), 179–202.

Khalid Alkharabsheh is Assistant professor at the Department of Software Engineering of the Al Balqa Applied University (BAU). He is a member of COGRADE research group and trainee of the Centro Singular de Investigación en Tecnoloxías Intelixentes (CITIUS). He received his Master degree (MS) in computer science from Al-Balqa Applied University, Jordan in 2005, and the Ph.D. from Santiago de Compostela University, Spain in 2019. His research interests focused on machine learning, software quality, empirical software engineering, software validation, and verification, Design Smell Detection, Object-Oriented language, and Java.

Yania Crespo is an Associate Professor at the Computer Science Department of the Universidad de Valladolid (UVA). She is a founding member of the GIRO research group of UVA. She obtained a master's degree (1995) and a PhD (2000) in computer science at the University of Havana and University of Valladolid, respectively. Her current research interests are related to software quality, smells detection and refactoring, software product lines, software maintenance and evolution.

Manuel Fernández-Delgado received the B.S. in Physics and the PhD. in Computer Science from the University of Santiago de Compostela (USC) in 1994 and 1999 respectively. He is currently a Lecturer of Computer Science and researcher of the Centro de Investigación en Tecnoloxías Intelixentes da USC (CiTIUS). His research interests include neural computation, machine learning and pattern recognition.



José R.R. Viqueira is Associate Professor at the Universidade de Santiago de Compostela and research staff of COGRADE (Computer Graphics and Data Engineering) at CiTIUS (Centro Singular de Investigación en Tecnoloxías Intelixentes). He obtained a master (1998) and a PhD (2003) in Computer Science at the Universidade de A Coruña. He is author of more than 50 international research papers and he has participated in more than 20 competitive research projects (leading 2 European Projects) and in more than 40 research and development contracts with companies and public administrations. Currently, he is mainly interested in very large scientific data management.

José A. Taboada-González is Associate Professor in the Department of Electronics & Computing at the University of Santiago de Compostela (USC), Spain, and currently member of the Centro Singular de Investigación en Tecnoloxías da Información (CITIUS) at the same University. He is graduated from the electronics program at the Faculty of Physics in 1990 and he received his PhD in Applied Physics in 1996 from the USC. He has been part of several national and European projects and contracts in the fields of Intelligent Systems. Other fields of interest include Big Data.