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How to kill them all: An exploratory study on the impact of code observability on mutation testing

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ABSTRACT

Mutation testing is well-known for its efficacy in assessing test quality, and starting to be applied in the industry. However, what should a developer do when confronted with a low mutation score? Should the test suite be plainly reinforced to increase the mutation score, or should the production code be improved as well, to make the creation of better tests possible? In this paper, we aim to provide a new perspective to developers that enables them to understand and reason about the mutation score in the light of *testability* and *observability*. First, we investigate whether testability and observability metrics are correlated with the mutation score on six open-source Java projects. We observe a correlation between observability metrics and the mutation score, e.g., *test directness*, which measures the extent to which the production code is tested directly, seems to be an essential factor. Based on our insights from the correlation study, we propose a number of "mutation score anti-patterns", enabling software engineers to refactor their existing code or add tests to improve the mutation score. In doing so, we observe that relatively simple refactoring operations enable an improvement or increase in the mutation score.

1. Introduction

Mutation testing has been a very active research field since the 1970s as a technique to evaluate test suite quality in terms of the fault-revealing capability (Jia and Harman, 2011). Recent advances have made it possible for mutation testing to be used in industry (Petrovic et al., 2018). For example, *PIT/PiTest* (Coles, 2019a) has been adopted by several companies, such as *The Ladders* and *British Sky Broadcasting* (Coles, 2019e). Furthermore, *Google* (Petrovic and Ivankovic, 2018) has integrated mutation testing with the code review process for around 6000 software engineers.

As mutation testing gains traction in the industry, a better understanding of the *mutation score* (one outcome of mutation testing) becomes essential. The existing works have mainly linked the mutation score with test quality (Inozemtseva and Holmes, 2014; Li et al., 2009) (i.e., *how good is the test suite at detecting faults in the software?*) and *mutant utility* (Yao et al., 2014; Just et al., 2017) (i.e., *how useful is the mutant?*). However, in our previous study, we have observed that certain mutants could be killed only after refactoring the production code to increase the *observability* of state changes. In such cases, test deficiency is not the only reason for the survival of mutants. Still, some issues in the production code, such as *code observability*, result in difficulties to kill the mutants. Unlike previous works (e.g., Inozemtseva and Holmes, 2014; Li et al., 2009; Yao et al., 2014; Just et al., 2017), our goal is to bring a new perspective to developers that enable them to understand and reason about the mutation score in the light of *testability* and *observability*. Thereby, developers can make a choice when confronting low mutation scores: (1) adding new tests, (2) refactoring the production code to be able to write better tests, or (3) ignoring the surviving mutants.

To this aim, our study consists of two parts: firstly, we investigate the relationship between *testability*/*observability* and mutation testing in order to find the most correlated metrics; secondly, based on what we observe from the correlations, we define anti-patterns or indicators that software engineers can apply to their code to kill the surviving mutants. We start by investigating the relationship between *testability/observability metrics* and the mutation score inspired by the work of Bruntink and van Deursen (2006). *Testability* is defined as the "attributes of software that bear on the effort needed to validate the software product" (ISO, 1991; Bruntink and van Deursen, 2006). Given our context, an important part of testability is *observability*, which is a measure of how well internal states of a system can be inferred, usually through the values of its external outputs (Staats et al., 2011). Whalen et al. (2013) formally defined *observability* as follows: An expression in a program is *observable* in a *test case* if the value of an expression is changed, leaving the rest of the program

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intact, and the output of the system is changed correspondingly. If there is no such value, the expression is not *observable* for that test. Compared to testability that covers various aspects of a project (e.g., inheritance and cohesion), observability specifically addresses the extent to which the value change of expression is observable in a test case.

Our first three research questions steer our investigation in the first part of our study:

RQ1 What is the relation between *testability* metrics and the mutation score?
RQ2 What is the relation between *observability* metrics and the mutation score?
RQ3 What is the relation between the combination of *testability* and *observability* metrics and the mutation score?

After investigating the relationship between *testability*/*observability* and mutation testing, we still lack insight into how these relationships can help developers to take actions when facing survival mutants. That is why, based on the observations from **RQ1–RQ3**, we define anti-patterns or indicators that software engineers can apply to their code/tests to ensure that mutants can be killed. This leads us to the next research question:

RQ4 To what extent does the removal of anti-patterns based on *testability* and *observability* help in improving the mutation score?

In terms of the methodology that we follow in our study, for **RQ1–RQ3**, we use statistical analysis on open-source Java projects to investigate the relationship between testability, observability, and the mutation score. For **RQ4**, we perform a case study with 16 code fragments to investigate whether the removal of anti-patterns increases the mutation score.

2. Background

In this section, we briefly introduce the basic concepts of and related works on mutation testing, testability metrics, and our proposed metrics for quantifying code observability.

2.1. Mutation testing

Mutation testing is defined by Jia and Harman (2011) as a fault-based testing technique that provides a testing criterion called the *mutation adequacy score*. This score can be used to measure the effectiveness of a test suite regarding its ability to detect faults (Jia and Harman, 2011). The principle of mutation testing is to introduce syntactic changes into the original program to generate faulty versions (called *mutants*) according to well-defined rules (mutation operators) (Offutt, 2011). The benefits of mutation testing have been extensively investigated and can be summarised (Zhu et al., 2018b) as (1) having better fault exposing capability compared to other test coverage criteria (Mathur and Wong, 1994; Frankl et al., 1997; Li et al., 2009), (2) being a valid substitute to real faults and providing a good indication of the fault detection ability of a test suite (Andrews et al., 2005; Just et al., 2014).

Researchers have actively investigated mutation testing for decades (as evidenced by the extensive survey Offutt, 2011; Jia and Harman, 2011; Madeyski et al., 2014; Zhu et al., 2018b). Recently, it has started to attract attention from industry (Petrovic et al., 2018). In part, this is due to the growing awareness of the importance of testing in software development (Ammann and Offutt, 2017). *Code coverage*, the most common metric to measure test suite effectiveness, has seen its limitations being reported in numerous studies (e.g. Mathur and Wong, 1994; Frankl et al., 1997; Li et al., 2009; Inozemtseva and Holmes, 2014). Using structural coverage metrics alone might be misleading because, in many cases, statements might be covered, but their consequences might not be asserted (Inozemtseva and Holmes, 2014). Another factor is that well-developed open-source mutation testing tools (e.g., PIT/PiTest Coles, 2019a and Mull GitHub, 2019) have contributed to mutation testing being applied in the industrial environments (Petrovic et al., 2018; Petrovic and Ivankovic, 2018; Coles, 2019e).

However, questions still exist about mutation testing, especially regarding the usefulness of a mutant (Just et al., 2017). The majority of the mutants generated by existing mutation operators are equivalent, trivial, and redundant (Kurtz et al., 2014; Just et al., 2017; Brown et al., 2017; Papadakis et al., 2018; Jimenez et al., 2018), which reduces the efficacy of the mutation score. If a class has a high mutation score while most mutants generated are trivial and redundant, the high mutation score does not promise high test effectiveness. A better understanding of mutation score and mutants is thus important.

To address this knowledge gap, numerous studies have investigated how useful mutants are. Example studies include *mutant subsumption* (Kurtz et al., 2014), *stubborn* mutants (Yao et al., 2014), and *real-fault coupling* (Just et al., 2014; Papadakis et al., 2018). These studies paid attention to the context and types of mutants as well as the impact of the test suite, while the impact of production code quality has rarely been investigated. We have seen how code quality can influence how hard it is to test (Bruntink and van Deursen, 2006) (called software testability Freedman, 1991), and since mutation testing can generally be considered as "testing the tests", production code quality could also impact mutation testing, just like production code quality has been shown to be correlated with the presence of test smells (Spadini et al., 2018). Due to the lack of insights into how code quality affects the efforts needed for mutation testing, especially in how to engineer tests that kill *all* the mutants, we conduct this exploratory study. Our study can help researchers and practitioners deepen their understanding of the mutation score, which is generally related to test suite quality and mutant usefulness.

2.2. Existing object-oriented metrics for testability

The notion of *software testability* dates back to 1991 when Freedman (1991) formally defined *observability* and *controllability* in the software domain. Voas (1992) proposed a dynamic technique coined propagation, infection, and execution (PIE) analysis for statistically estimating the program's *fault sensitivity*. More recently, researchers have aimed to increase our understanding of *testability* by using statistical methods to predict *testability* based on various code metrics. Influential works include that of Bruntink and van Deursen (2006), in which they explored the relationship between nine object-oriented metrics and testability. To explore the relation between *testability* and mutation score (**RQ1**), we first need to collect several existing object-oriented metrics that have been proposed in the literature. In total, we collect 64 code quality metrics, including both class-level and method-level metrics that have been the most widely used. We select those 64 metrics because they measure various aspects of a project, including basic characteristics (e.g., NLOC and NOMT), inheritance (e.g., DIT), coupling (e.g., CBO and FIN), and cohesion (LCOM). A large number of those metrics, such as LCOM and HLTH, have been widely used to explore software testability (Bruntink and van Deursen, 2006; Gao and Shih, 2005) and fault prediction (Arisholm and Briand, 2006; Hall et al., 2011).

We present a brief summary of the 64 metrics in Table 1 (method-level) and Tables 2–3 (class-level). We computed these metrics using a static code analysis tool provided by `JHawk` (JHawk, 2019).

Table 1
Summary of method-level code quality metrics.

Abbr.	Full name	Description
COMP	Cyclomatic Complexity	McCabes cyclomatic complexity for the method
NOA	Number of Arguments	The number of arguments
NOCL	Number of Comments	The number of comments associated with the method
NOC	Number of Comment Lines	The number of comment lines associated with the method
VDEC	Variable Declarations	The number of variables declared in the method
VREF	Variable References	The number of variables referenced in the method
NOS	Number of Java statements	The number of statements in the method
NEXP	Number of expressions	The number of expressions in the method
MDN	Max depth of nesting	The maximum depth of nesting in the method
HLTH	Halstead length	The Halstead length of the metric (one of the Halstead metrics)
HVOC	Halstead vocabulary	The Halstead vocabulary of the method (one of the Halstead metrics)
HVOL	Halstead volume	The Halstead volume of the method (one of the Halstead metrics)
HDIF	Halstead difficulty	The Halstead difficulty of the method (one of the Halstead metrics)
HEFF	Halstead effort	The Halstead effort of the method (one of the Halstead metrics)
HBUG	Halstead bugs	The Halstead prediction of the number of bugs in the method (one of the Halstead metrics)
TDN	Total depth of nesting	The total depth of nesting in the method
CAST	Number of casts	The number of class casts in the method
LOOP	Number of loops	The number of loops (for, while) in the method
NOPR	Number of operators	The total number of operators in the method
NAND	Number of operands	The total number of operands in the method
CREF	Number of classes referenced	The classes referenced in the method
XMET	Number of external methods	The external methods called by the method
LMET	Number of local methods	The number of methods local to this class called by this method
EXCR	Number of exceptions referenced	The number of exceptions referenced by the method
EXCT	Number of exceptions thrown	The number of exceptions thrown by the method
MOD	Number of modifiers	The number of modifiers (public, protected, etc.) in method declaration
NLOC	Lines of Code	The number of lines of code in the method

Table 2
Summary of class-level code quality metrics (1).

Abbr.	Full name	Description
NOMT	Number of methods	The number of methods in the class (WMC — one of the Chidamber and Kemerer metrics)
LCOM	Lack of Cohesion of Methods	The value of the Lack of Cohesion of Methods metric for the class. This uses the LCOM* (or LCOM5) calculation. (one of the Chidamber and Kemerer metrics)
TCC	Total Cyclomatic Complexity	The total McCabes cyclomatic Complexity for the class
AVCC	Average Cyclomatic Complexity	The average McCabes cyclomatic complexity for all of the methods in the class
MAXCC	Maximum Cyclomatic Complexity	The maximum McCabes cyclomatic complexity for all of the methods in the class
NOS	Number of Java statements	The number of statements in the class
HLTH	Cumulative Halstead length	The Halstead length of the code in the class plus the total of all the Halstead lengths of all the methods in the class
HVOL	Cumulative Halstead volume	The Halstead volume of the code in the class plus the total of all the Halstead volumes of all the methods in the class
HEFF	Cumulative Halstead effort	The Halstead effort of the code in the class plus the total of all the Halstead efforts of all the methods in the class
HBUG	Cumulative Halstead bugs	The Halstead prediction of the number of bugs in the code of the class and all of its methods
UWCS	Un Weighted class Size	The Unweighted Class Size of the class
NQU	Number of Queries	The number of methods in the class that are queries (i.e., that return a value)
NCO	Number of Commands	The number of methods in the class that are commands (i.e., that do not return a value)
EXT	External method calls	The number of external methods called by the class and by methods in the class
LMC	Local method calls	The number of methods called by the class and by methods in the class
HIER	Hierarchy method calls	The number of local methods called by the class and by methods in the class that are defined in the hierarchy of the class
INST	Instance Variables	The number of instance variables declared in the class
MOD	Number of Modifiers	The number of modifiers (public, protected, etc.) applied to the declaration of the class
INTR	Number of Interfaces	The number of interfaces implemented by the class

2.3. Code observability

To explore the relation between *observability* and mutation score (**RQ2**), we first need a set of metrics to quantify *code observability*. According to Whalen et al. (2013)'s definition of observability (as mentioned in Section 1), we consider that *code observability* comprises two perspectives: that of production code and that of the test case. To better explain these two perspectives, let us consider the example in Listing 1 from project `jfreechart-1.5.0` showing the method `setSectionPaint` and its corresponding test. This method sets the section paint associated with the specified key for the `PiePlot` object, and sends a `PlotChangeEvent` to all registered listeners. There is one mutant in Line 3 that removes the call to `org/jfree/chart/plot/PiePlot::fireChangeEvent`. This mutant is not killed by `testEquals`. Looking at the *observability* of this mutant from the *production code* perspective, we can see that the `setSectionPaint` method is void; thus, this mutant is hard to detect because there is no return value for the test case to *assert*. From the *test case* perspective, although `testEquals` invokes the method `setSectionPaint` in Line 14 and 17, no

Table 3
Summary of class-level code quality metrics (2).

Abbr.	Full name	Description
PACK	Number of Packages imported	The number of packages imported by the class
RFC	Response for Class	The value of the Response For Class metric for the class. (One of the Chidamber and Kemerer metrics)
MPC	Message passing	The value of the Message passing metric for the class
CBO	Coupling between objects	The value of the Coupling Between Objects metric for the class. (One of the Chidamber and Kemerer metrics)
FIN	Fan In	The value of the Fan In (Afferent coupling (Ca)) metric for the class
FOUT	Fan Out	The value of the Fan Out (Efferent coupling (Ce)) metric for the class
R-R	Reuse Ratio	The value of the Reuse Ratio for the class
S-R	Specialisation Ratio	The value of the Specialisation Ratio for the class
NSUP	Number of Superclasses	The number of superclasses (excluding Object) in the hierarchy of the class
NSUB	Number of Subclasses	The number of subclasses below the class in the hierarchy. (NOC — one of the Chidamber and Kemerer metrics)
MI	Maintainability Index (including comments)	The Maintainability Index for the class, including the adjustment for comments
MINC	Maintainability Index (not including comments)	The Maintainability Index for the class without any adjustment for comments
COH	Cohesion	The value of the Cohesion metric for the class
DIT	Depth of Inheritance Tree	The value of the Depth of Inheritance Tree metric for the class. (One of the Chidamber and Kemerer metrics)
LCOM2	Lack of Cohesion of Methods (variant 2)	The value of the Lack of Cohesion of Methods (2) metric for the class. This uses the LCOM2 calculation. (One of the Chidamber and Kemerer metrics)
CCOM	Number of Comments	The number of comments associated with the class
CCML	Number of Comment Lines	The number of comment Lines associated with the class
cNLOC	Lines of Code	The number of lines of code in the class and its methods

```
public void setSectionPaint(Comparable key, Paint
     paint) {
  this.sectionPaintMap.put(key, paint);
  fireChangeEvent(); // mutant: remove this
       method
}

@Test
public void testEquals() {
...
  PiePlot plot1 = new PiePlot();
  PiePlot plot2 = new PiePlot();
  assertTrue(plot1.equals(plot2));
  assertTrue(plot2.equals(plot1));
  // sectionPaintMap
  plot1.setSectionPaint("A", new GradientPaint
       (1.0f, 2.0f,
    Color.BLUE,3.0f, 4.0f, Color.WHITE));
  assertFalse(plot1.equals(plot2));
  plot2.setSectionPaint("A", new GradientPaint
       (1.0f, 2.0f,
    Color.BLUE,3.0f, 4.0f, Color.WHITE));
  assertTrue(plot1.equals(plot2));
...
}
```

Listing 1: Example of method `org.jfree.chart.plot.PiePlot: setSectionPaint` and its test

proper assertion statements are used to examine the changes of `fireChangeEvent()` (which is used to send an event to listeners).

Starting with two angles of code observability, we come up with a set of the code observability metrics. Since our study is a starting point to design metrics to measure the code observability, we start with the simple and practical metrics, which are easy for practitioners to understand and apply.

First of all, we consider the return type of the method. As discussed in Listing 1, it is hard to observe the changing states inside a void method because there is no return value for test cases to assert. Accordingly, we design two metrics, `is_void` and `non_void_percent` (shown in 1st and 2nd rows in Table 5). The metric `is_void` is to examine whether the return value of the method is void or not. The metric `non_void_percent` addresses the return type at class level which measures the percent of non-void methods in the class. Besides these two, a void method might change the field(s) of the class it belongs to. A workaround to test a *void* method is to invoke getters. So `getter_percentage` (shown in 3rd row in Table 5) is proposed to complement `is_void`.

Secondly, we come up with the access control modifiers. Let us consider the example in Listing 2 from project `commons-lang-LANG_3_7`. The method `getMantissa` in class `NumberUtils` returns the mantissa of the given number. This method has only one mutant: the return value is replaced with "`return if (getMantissa(str, str.length()) != null) null else throw new RuntimeException`".[1] This mutant should be easy to detect given an input of either a legal String object (the return value is not null) or a null string (throw an exception). This "trivial" mutant is not detected because the method `getMantissa` is private. The access control modifier *private* makes it impossible to test the method `getMantissa` *directly*, for this method is only visible to methods from class `NumberUtils`. To test this method, the test case must first invoke a method that calls method `getMantissa`. From this case, we observe that access control modifiers influence the *visibility* of the method, so as to play a significant role in code observability. Thereby, we take access control modifiers into account to quantify code observability, where we design `is_public` and `is_static` (shown in 4th and 5th rows in Table 5).

[1] This mutant is generated by `Return Values Mutator` in PIT (Coles, 2019c). In Listing 2, getMantissa(str, str.length()) returns a `String` object. When the return value of a method is an object, the mutator replaces non-null return values with null and throw a `java.lang.RuntimeException` if the un-mutated method would return null.

```
private static String getMantissa(final String
    str) {
  return getMantissa(str, str.length());
}
```

Listing 2: Example of method `getMantissa` in class `NumberUtils`

```
@Override
public int hashCode() {
  return (getLeft() == null ? 0 : getLeft().
      hashCode()) ^
    (getMiddle() == null ? 0 : getMiddle().
        hashCode()) ^
    (getRight() == null ? 0 : getRight().
        hashCode());
}
```

Listing 3: Example of method `hashCode` in class `Triple`

The third point we raise concerns fault masking. We have observed that mutants generated in *certain locations* are more likely to be *masked* (Gopinath et al., 2017), i.e., the state change cannot propagate to the output of the method. The first observation is that mutants that reside in a nested class. The reasoning is similar to mutants that reside in nested sections of code, namely that a change in intermediate results does not propagate to a point where a test can pick it up. Thus, we come up with `is_nested` (in 6th row in Table 5). Another group of mutants is generated inside nested conditions and loops. These can be problematic because the results of the mutations cannot propagate to the output, and the tests have no way of checking the intermediate results within the method. Accordingly, we define `nested_depth` (shown in 7th row in Table 5) and a set of metrics to quantify the conditions and loops (shown in 8th through 13 rows in Table 5). The last observation is related to mutants that are inside a long method (the reason is similar to the mutants inside nested conditions and loops), thus, we design `method_length` (shown in 14th row in Table 5).

The next aspect we consider is test directness. Before we dig into test directness, we take Listing 3 as an instance. Listing 3 shows the class `Triple` from project `commons-lang-LANG_3_7`, which is an abstract implementation defining the basic functions of the object, and that consists of three elements. It refers to the elements as "left", "middle" and "right". The method `hashCode` returns the hash code of the object. Six mutants are generated for the method `hashCode` in class `Triple`. Table 4 summarises all the mutants from Listing 3. Of those six mutants, only Mutant 1 is killed, and the other mutants are not equivalent. Through further investigation of method `hashCode` and its test class, we found that although this method has 100% coverage by the test suite, there is no *direct* test for this method. A *direct* test would mean that the test method directly invoking the method (production code) (Athanasiou et al., 2014). The direct test is useful because it allows to control the input data directly and to assert the output of a method directly. This example shows that test directness can influence the outcome of mutation testing, which denotes the test case angle of *code observability*. Previous works such as Huo and Clause (2016) also addressed the significance of test directness in mutation testing. Therefore, we design two metrics, `direct_test_no.` and `test_distance` (shown in 15th and 16th row in Table 5), to quantify test directness. Those two metrics represent the *test case* perspective of code observability.

Last but not least, we take assertions into considerations. As discussed in Listing 1, we have observed that mutants without appropriate assertions in place (throwing exceptions is also under consideration) cannot be killed, as a prerequisite to killing a mutant is to have the tests fail in the mutated program. Schuler and Zeller (2013) and Zhang and Mesbah (2015) also drew a similar conclusion to ours. Accordingly, we come up with three metrics to quantify assertions in the method, `assertion_no.`, `assertion-McCabe_Ratio` and `assertion_density` (shown in 17th-19th rows in Table 5). The `assertion-McCabe_Ratio` metric (Athanasiou et al., 2014) is originally proposed to measure *test completeness* by indicating the ratio between the number of the actual points of testing in the test code and the number of decision points in the production code (i.e., how many decision points are tested). For example, a method has a McCabe complexity of 4, then in the ideal case, we would expect 4 different assertions to test those linear independent paths (in this case this ration would be 1), but if the ratio is lower than 1, it could be an indication that either not all paths are tested, or that not all paths are tested in a direct way. The `assertion_density` metric (Kudrjavets et al., 2006) aims at measuring the ability of the test code to detect defects in the parts of the production code that it covers. We include those two metrics here as a way to measure the quality of assertions. These three metrics are proposed based on the *test case* perspective of code observability.

To sum up, Table 5 presents all the code observability metrics we propose, where we display the name, the definition of each metric, and the category.

3. Experimental setup

To examine our conjectures, we conduct an experiment using six open-source projects. We recall the research questions we have proposed in Section 1:

- **RQ1**: *What is the relation between* testability *metrics and the mutation score?*
- **RQ2:** *What is the relation between* observability *metrics and the mutation score?*
- **RQ3:** *What is the relation between the combination of* testability *and* observability *metrics and the mutation score?*
- **RQ4:** *To what extent does removal of anti-patterns based on testability and observability help in improving the mutation score?*

3.1. Mutation testing

We adopt PIT (Version 1.4.0) (Coles, 2019a) to apply mutation testing in our experiments. The mutation operators we adopt are the *default* mutation operators provided by PIT (Coles, 2019c): `Conditionals Boundary Mutator`, `Increments Mutator`, `Invert Negatives Mutator`, `Math Mutator`, `Negate Conditionals Mutator`, `Return Values Mutator`, and `Void Method Calls Mutator`. We did not adopt the extended set of mutation operators provided PIT, as the operators in the default version are largely designed to be stable (i.e., not be too easy to detect) and minimise the number of equivalent mutations that they generate (Coles, 2019c).

3.2. Subject systems

We use six systems publicly available on GitHub in this experiment. Table 6 summarises the main characteristics of the selected projects, which include the lines of code (LOC), the number of tests (#Test), the total number of methods (#Total Methods), the number of selected methods used in our experiment (#Selected), the total number of mutants (#Total Mutants), and the killed mutants (#Killed). In our experiment, we remove the methods with no generated mutant by PIT, thus resulting in the number of

Table 4
Summary of mutants from Listing 3.

ID	Line no.	Mutator	Results
1	3	Negated conditional	Killed
2	3	Replaced return of integer sized value with (x == 0 ? 1 : 0)	Survived
3	3	Replaced XOR with AND	Survived
4	4	Negated conditional	Survived
5	4	Replaced XOR with AND	Survived
6	5	Negated conditional	Survived

Table 5
Summary of code observability metrics.

#	Name	Definition	Category
1	is_void	Whether the return value of the method is void or not	Return type
2	non_void_percent (class-level)	The percent of non-void methods in the class	
3	getter_percentage	The percentage of getter methods in the class[a]	
4	is_public	Whether the method is public or not	Access control modifiers
5	is_static	Whether the method is static or not	
6	is_nested (class-level)	Whether the method is located in a nested class or not	Fault masking
7	nested_depth	The maximum number of nested depth (`MDN` from Section 2.2)	
8	(cond)	The number of conditions (`if`, `if-else` and `switch`) in the method	
9	(cond(cond))	The number of nested conditions (e.g.,`if{if{}}`) in the method	
10	(cond(loop))	The number of nested condition-loops (e.g.,`if{for{}}`) in the method	
11	(loop)	The number of loops (`for`, `while` and `do-while`) in the method (`LOOP` from Section 2.2)	
12	(loop(cond))	The number of nested loop-conditions (e.g.,`for{if{}}`) in the method.	
13	(loop(loop))	The number of nested loop-conditions (e.g.,`for{for{}}`) in the method.	
14	method_length	The number of lines of code in the method (`NLOC` from Section 2.2)	
15	direct_test_no.	The number of test methods directly invoking the method under test (production code)[b]	Test directness
16	test_distance	The shortest method call sequence required to invoke the method (production code) by test methods[c]	
17	assertion_no.	The number of assertions in direct tests	Assertion
18	assertion-McCabe_Ratio	The ratio between the total number of assertions in direct tests and the McCabe Cyclomatic complexity	
19	assertion_density	The ratio between the total number of assertions in direct tests and the lines of code in direct tests	

[a]A getter method must follow three patterns (Zhang and Mesbah, 2015): (1) must be public; (2) has no arguments and its return type must be something other than void. (3) have naming conventions: the name of a getter method begins with "get" followed by an uppercase letter.
[b]If the method is not directly tested, then its direct_test_no. is 0.
[c]If the method is directly tested, then its test_distance is 0. The maximum test_distance is set `Integer.MAX_VALUE` in Java which means there is no method call sequence that can reach the method from test methods.

Table 6
Subject systems.

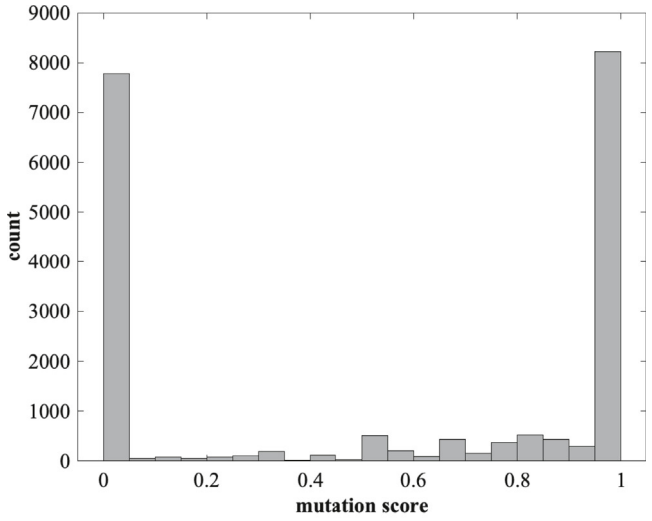
PID	Project	LOC	#Tests	#Methods		#Mutants	
				#Total	#Selected	#Total	#Killed
1	Bukkit-1.7.9-R0.2	32 373	432	7 325	2 385	7 325	947
2	commons-lang-LANG_3_7	77 224	4 068	13 052	2 740	13 052	11 284
3	commons-math-MATH_3_6_1	208 959	6 523	48 524	6 663	48 524	38 016
4	java-apns-apns-0.2.3	3 418	91	429	150	429	247
5	jfreechart-1.5.0	134 117	2 175	34 488	7 133	34 488	11 527
6	pysonar2-2.1	10 926	269	3 070	719	3 074	836
Overall		467 017	13 558	106 888	19 790	106 892	62 857

selected methods (#Selected). These systems are selected because they have been widely used in the research domain (e.g., Schuler and Zeller, 2013; Zhang and Mesbah, 2015; Huo and Clause, 2016; Zhu et al., 2018a; Zhang et al., 2018). All systems are written in Java, and tested by means of JUnit. The granularity of our analysis is at the method-level.

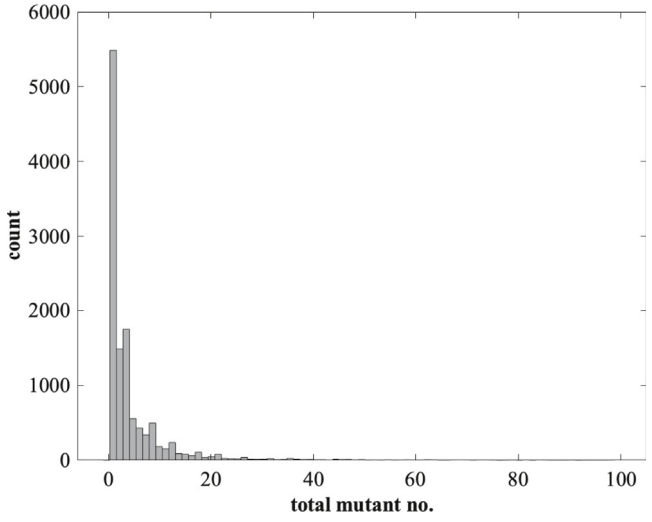
The results of the mutants that are killable for all of the subjects are shown in Columns 7–8 of Table 6. Fig. 1a shows the distribution of mutation scores among selected methods. The majority of the mutation scores are either 0 or 1. Together with Fig. 1b, we can see that the massive number of 0s and 1s are due to the low number of mutants per method. Most methods show less than 10 mutants, which is mainly due to most methods being short methods (`NOS` < 2 as shown in Fig. 2). Writing short methods is a preferred strategy in practice, for a long method is a well-known code smell (Beck et al., 1999). Besides, PIT adopts several optimisation mechanisms (Coles, 2019d) to reduce the number of mutants. Thus, the number of mutants (#Total Mutants) shown in Table 6 is fewer than the actual number of generated mutants. The large number of methods with low mutant number is an unavoidable bias in our experiment.

3.3. Tool implementation

To evaluate the *code observability metrics* that we have proposed, we implemented a prototype tool (coined MUTATION OBSERVER) to capture all the necessary information from both the program under test and the mutation testing process. This tool is openly available on GitHub (Zhu, 2019).



(a) Distribution of mutation score per method



(b) Distribution of total mutant no. per method

Fig. 1. Distribution of mutation score and mutant no.

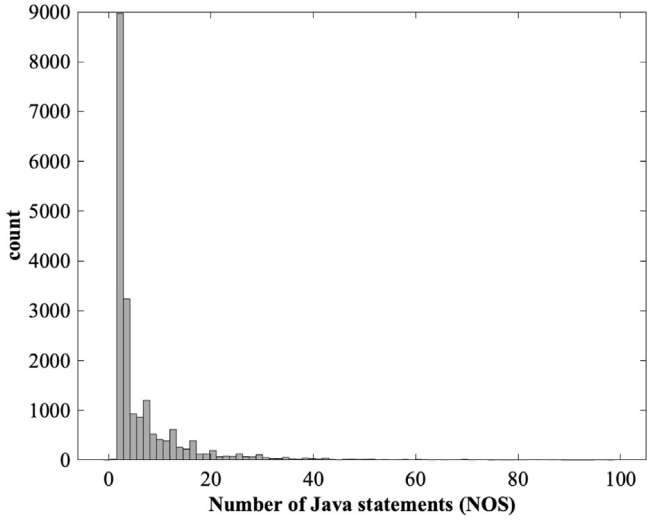


Fig. 2. Distribution of Number of Java statements (NOS) per method.

Our tool extracts information from three parts of the system under test (in Java): source code, bytecode, and tests. Firstly, Antlr (2019) parses the source code to obtain the basic code features, e.g., **is public**, **is static**, and **(cond)**. Secondly, we adopt Apache Commons BCEL (Apache, 2019) to parse the bytecode. Then, `java-callgraph` (java-callgraph, 2019) generates the pairs of method calls between the source code and tests, which we later use to calculate **direct test no.** and other test call-related metrics. The last part is related to the mutation testing process, for which we adopt PIT (Version 1.4.0) (Coles, 2019a) to obtain the killable mutant results. An overview of the architecture of MUTATION OBSERVER can be seen in Fig. 3.

3.4. Design of experiment

3.4.1. *RQ1–RQ3*

Our investigation of the relationships between testability/observability metrics and the mutation score (**RQ1**–**RQ3**) is two-fold: in the first part, we adopt Spearman's rank-order correlation to measure the *pairwise correlations* statistically between each metric (both testability and observability metrics) and the mutation score; in the second part, we turn the correlation problem into a binary classification problem (where we adopt Random Forest as the classification algorithm) to investigate how those metrics *interact* with one another.

Pairwise correlations. To answer **RQ1**, **RQ2**, and **RQ3**, we first adopt Spearman's rank-order correlation to statistically measure the correlation between each metric (both testability and observability metrics) and the mutation score of the corresponding methods or classes. Spearman's correlation test checks whether there exists a monotonic relationship (linear or not) between two data samples. It is a *non-parametric* test and, therefore, it does not make any assumption about the distribution of the data being tested. The resulting coefficient ρ takes values in the interval $[-1; +1]$; the higher the correlation in either direction (positive or negative), the stronger the monotonic relationship between the two data samples under analysis. The strength of the correlation can be established by classifying into "negligible" ($|\rho| < 0.1$), "small" ($0.1 \leq |\rho| < 0.3$), "medium" ($0.3 \leq |\rho| < 0.5$), and "large" ($|\rho| \geq 0.5$) (Hinkle et al., 1988). Positive ρ values indicate that one distribution increases when the other increases as well; negative ρ values indicate that one distribution decreases when the other increases. To measure the statistical significance of Spearman's correlation test, we look at *p-values* that measure the probability of an observed (or more extreme) result assuming that the null hypothesis is true. Any test size larger than the *p*-value leads to rejection, whereas using a test size smaller than the *p*-value fails to reject the null hypothesis (Hung et al., 1997). Here we consider the test size of 5% as the cutoff for statistical significance.

The mutation score[2] is calculated by Eq. (1) (method-level).

$$\textit{mutation score}\,(A) = \frac{\#\ \textit{killed mutants in method } A}{\#\ \textit{total mutants in method } A} \tag{1}$$

We adopt `Matlab` (MATLAB, 2019) to calculate the Spearman's rank-order correlation coefficient between each metric and the mutation score. In particular, we used the statistical analysis

[2] In the original equation for mutation score, the divisor is the number of non-equivalent. In our study, our main focus is the relation between testability/observability metrics and mutation score, rather than mutation score itself. In our previous literature review (Zhu et al., 2018b), we have found that treating all mutants as non-equivalent is a common method when the mutation score is used as a relative comparison. Therefore, we do not manually analyse the equivalent mutants, and treated all mutants as non-equivalent.

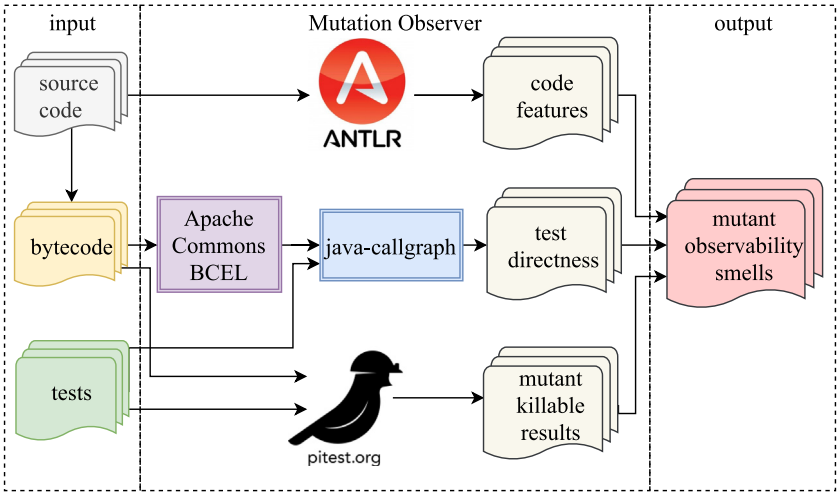


Fig. 3. Overview of MUTATION OBSERVER architecture.

(`corr` function with the option of "`Spearman`" in Matlab's default package[3]).

Interactions. Except for the pairwise correlations between metrics and mutation score, we are also interested in how those metrics interact with one another. First, we try regression models to predict mutation scores based on the metrics. However, all the regression models incur extremely high cross-validation errors, i.e., Root Relative Squared Errors (RRSEs) are $> 70\%$ (e.g., RRSE of linear regression is 76.62%). Therefore, we turn the correlation problem into a classification problem for better performance. For simplicity, we use 0.5 as the cutoff between `HIGH` and `LOW` mutation core because 0.5 is widely used as a cutoff in classification problems whose independent variable ranges in [0,1] (e.g., defect prediction (Zhang et al., 2016; Tosun and Bener, 2009)). We consider all the metrics to predicate whether the method belongs to classes with `HIGH` or `LOW` mutation score. One thing to notice here is that building a perfect prediction model is not our primary goal. Our interest is to see which metrics and/or combinations of the metrics contribute to the LOW mutation score by building the prediction models. Therefore, deciding different threshold values is outside the scope of this paper.

For prediction, we adopt *Random Forest* (Breiman, 2001) as the classification algorithm, where we use `WEKA` (Frank et al., 2016) to build the prediction model. Random Forest is an ensemble method based on a collection of decision tree classifiers, where the individual decision trees are generated using a random selection of attributes at each node to determine the split (Han et al., 2011). Besides, Random Forest is more accurate than one decision tree, and it is not affected by the overfitting problem (Han et al., 2011).

As our investigation includes testability and observability metrics, for each project, we compare three types of classification models: (1) a model based on merely existing testability metrics, (2) a model based on merely code observability metrics, and (3) a model based on the combination of existing and our observability metrics (overlapping metrics, e.g., `method_length` to `NLOC`, are only considered once). In particular, we include the model based on the combination of the two aspects for further comparison: to see whether the combination of the two aspects can work better than each aspect itself. To examine the effectiveness of Random Forest in our dataset, we also consider `ZeroR`, which classifies all the instances to the majority and ignores all predictors, as the baseline. It might be that our data is not balanced, as in that one project has over 90% methods with a `HIGH` mutation score. This could entail that the classification model achieving 90% accuracy is not necessarily an effective model. In this situation, `ZeroR` could also achieve over 90% accuracy in that scenario. Our Random Forest model must thus perform better than `ZeroR`; otherwise, the Random Forest model is not suitable for our dataset.

In total, we consider four classification models: (1) `ZeroR` (i.e., the constant classifier), (2) Random Forest based on existing metrics, (3) Random Forest based on code observability metrics, and (4) Random Forest based on the combination of existing metrics and code observability metrics. To build Random Forest, `WEKA` (Frank et al., 2016) adopts bagging in tandem with random attribute selection. We use WEKA's default parameters to train the Random Forest model, i.e., "-P 100 -I 100 -num-slots 1 -K 0 -M 1.0 -V 0.001 -S 1". To evaluate the performance of the classifier model (e.g., precision and recall), we use K-fold cross-validation with $K = 10$ (Kohavi et al., 1995).

In terms of feature importance, we apply `scikit-learn` (Pedregosa et al., 2011) to conduct the analysis. To determine the feature importance, `scikit-learn` (Pedregosa et al., 2011) implements "*Gini Importance*" or "*Mean Decrease Impurity*" (Breiman, 2017). The importance of each feature is computed by the probability of reaching that node (which is approximated by the proportion of samples reaching that node) averaged over total tree ensembles (Breiman, 2017). We use the method of `feature_importances_` in `sklearn.ensemble.RandomForestRegressor` (scikit-learn, 2019) package to analyse the feature importance.

3.4.2. *RQ4*

To answer **RQ4**, we first need to establish the anti-patterns (or smells) based on these metrics. An example of an anti-pattern rule generated from the metrics is `method_length` > 20 and `test_distance` > 2. In this case, it is highly likely that the method has a low mutation score. To obtain the anti-pattern rules, we adopt J48 to build a decision tree (Quinlan, 1993; Frank et al., 2016). We consider J48 because of its advantage in interpretation over Random Forest. After building the decision tree, we rank all leaves (or paths) according to instances falling into each leaf and accuracy. We select the leaves with the highest instances and accuracy ≥ 0.8 for further manual analysis, to understand to what extent refactoring of the anti-patterns can help improve the mutation score.

[3] https://www.mathworks.com/help/stats/corr.html.

3.5. Evaluation metrics

For **RQ1**, **RQ2**, and **RQ3**, to ease the comparisons of the four classification models, we consider four metrics widely used in classification problems: precision, recall, AUC, and the mean absolute error.

In our case, we cannot decide which class is positive or not, or in other words, we cannot say HIGH mutation score is what we expect. We use a prediction model to investigate the interactions between those metrics or how they interact with each other. So we adopt *weighted* precision and recall, which also take the number of instances in each class into consideration.

Weighted precision. The precision is the fraction of true positive instances in the instances that are predicted to be positive: TP/(TP+FP). The higher the precision, the fewer false positives. The *weighted precision* is computed as follows, where p_{c1} and p_{c2} are the precisions for class 1 and class 2, and $|c1|$ and $|c2|$ are the number of instances in class 1 and class 2, respectively:

$$weighted\ precision\ = \frac{p_{c1} \times |c1| + p_{c2} \times |c2|}{|c1| + |c2|} \quad (2)$$

Weighted recall. The recall is the fraction of true positive instances in the instances that are actual positives: TP/(TP+FN). The higher the recall, the fewer false-negative errors there are. The *weighted recall* is computed as follows, where r_{c1} and r_{c2} are the recalls for class 1 and class 2, and $|c1|$ and $|c2|$ are the number of instances in class 1 and class 2:

$$weighted\ recall\ = \frac{r_{c1} \times |c1| + r_{c2} \times |c2|}{|c1| + |c2|} \quad (3)$$

AUC. The area under ROC curve, which measures the overall discrimination ability of a classifier. An area of 1 represents a perfect test; an area of 0.5 represents a worthless test.

Mean absolute error. The mean of overall differences between the predicted values and actual values.

4. RQ1–RQ3 testability versus observability versus combination

We opt to discuss the three research questions, **RQ1**, **RQ2**, and **RQ3**, together, because it gives us the opportunity to compare testability, observability, and their combination in detail.

4.1. Spearman's rank order correlation

4.1.1. Testability

Findings. Table 7 presents the overall results of Spearman's rank-order correlation analysis for existing code metrics. The columns of "rho" represent the pairwise correlation coefficient between each code metric and the mutation score. The *p-values* columns denote the strength of evidence for testing the hypothesis of no correlation against the alternative hypothesis of a non-zero correlation using Spearman's rank-order. Here we used 0.05 as the cutoff for significance. From Table 7, we can see that except for `NOS`, `NLOC`, `MOD`, `EXCR`, `INST(class)`, `NSUB(class)`, `COH(class)` and `S-R(class)` (which, for convenience, we highlighted by underlining the value), the correlation results for the metrics are all statistically significant.

Overall, the pairwise correlation between each source code metric and the mutation score is not strong ($|rho| < 0.27$). We speculate the reason behind the weak correlations to be the collinearity of these code metrics. More specifically, Spearman's rank-order correlation analysis only evaluates the correlation between individual code metric and mutation score. Some code metrics could interact with one another. For example, a long method does not necessarily have a low mutation score. Alternatively, another example: if there are more than four loops in a long method, then the method is very likely to have a low mutation score. That is also an example of collinearity, i.e., the number of loops and the method length are highly correlated.

From Table 7, we can see that the highest *rho*[4] is −0.2634 for both `NSUP (class)` standing for Number of Superclasses, and `DIT(class)`, or Depth of Inheritance Tree. Followed by `R-R(class)`, for Reuse Ratio, and `HIER(class)`, for Hierarchy method calls. At first glance, the top 4 metrics are all class-level metrics. However, we cannot infer that class-level metrics are more impactful on the mutation score than method-level ones. In particular, it can be related to the fact that we have considered more class-level metrics than method-level ones in the experiment.

Additionally, we expected that the metrics related to McCabe's Cyclomatic Complexity, i.e., `COMP`, `TCC`, `AVCC` and `MAXCC` would show stronger correlation to the mutation score. In fact, McCabe's Cyclomatic Complexity has been widely considered a powerful measure to quantify the complexity of a software program, and it is used to provide a lower bound to the number of tests that should be written (Woodward et al., 1979; Gill and Kemerer, 1991; Fenton and Ohlsson, 2000). Based on our results without further investigation, we could only speculate that McCabe's Cyclomatic Complexity might not directly influence the mutation score.

Summary. *We found that the pair-wise correlations between the 64 existing source code metrics and the mutation score to be not so strong ($|rho| < 0.27$). The top 4 metrics with the strongest correlation coefficients are `NSUP(class)`, `DIT(class)`, `R-R(class)` and `HIER(class)`.*

4.1.2. Observability

Findings. Table 8 shows the overall results of Spearman's rank-order correlation analysis for code observability metrics. From Table 8, we can see that except for `method_length` and `(cond(loop))`, whose *p-value* is greater than 0.05, the results of the other observability metrics are statistically significant. The overall correlation between code observability metrics and mutation score is still not strong (<0.5), but significantly better than existing code metrics (<0.27). The top five metrics are `test_distance`, `direct_test_no.`, `assertion-density`, `assertion-McCabe`, and `assertion_no`. The metrics related to *test directness*, i.e., `test_distance` (−0.4923) and `direct_test_no` (0.4177) are ranked first in terms of *rho* among all metrics that we consider (including existing code metrics in Section 2.2). This observation corresponds to our hypothesis in Section 2.3 that the methods with no direct tests are more challenging to kill mutants. In terms of *rho* values, the assertion related metrics are ranked after test directness related metrics; this confirms both our conjectures in Section 2.3 and what has been reported in the related literature (Schuler and Zeller, 2013; Zhang and Mesbah, 2015) that the quality of assertions can influence the outcome of mutation testing.

Summary. *The correlations between code observability metrics and mutation score are not very strong (<0.5); however, they are significantly better than the correlations for existing code metrics. Test directness (`test_distance` and `direct_test_no.`) takes the first place of `NSUP(class)` in |rho| among all metrics (including existing ones in Section 2.2), followed by assertion-based metrics (`assertion-density`, `assertion-McCabe` and `assertion_no`).*

[4] In terms of absolute value.

Table 7
Spearman results of existing code metrics for testability.

Metric	rho	p-value	Metric	rho	p-value	Metric	rho	p-value
COMP	0.0398	2.16E−08	NOC	0.1908	1.254E−161	R-R(class)	**−0.2524**	3.721E−285
NOCL	0.1047	2.32E−49	NOA	0.0423	2.723E−09	NSUB(class)	−0.0048	0.5009
NOS	−0.0139	0.05024	CAST	−0.0162	0.02302	NSUP(class)	**−0.2634**	0
HLTH	0.0518	2.927E−13	HDIF	0.1334	2.691E−79	NCO(class)	−0.0751	3.602E−26
HVOC	0.0485	8.831E−12	NEXP	0.0288	5.135E−05	FOUT(class)	−0.1073	9.482E−52
HEFF	0.0856	1.595E−33	NOMT(class)	0.0981	1.564E−43	DIT(class)	**−0.2634**	0
HBUG	0.0518	3.163E−13	LCOM(class)	0.0564	2.125E−15	CCOM(class)	0.1695	1.589E−127
CREF	0.0193	0.00653	AVCC(class)	0.0405	1.206E−08	COH(class)	0.0001	0.9852
XMET	0.0465	5.743E−11	NOS(class)	0.0793	5.416E−29	S-R(class)	0.0016	0.8184
LMET	−0.0221	0.00191	HBUG(class)	0.0824	3.826E−31	MINC(class)	−0.0255	0.0003272
NLOC	−0.0004	0.95	HEFF(class)	0.0982	1.213E−43	EXT(class)	−0.0636	3.314E−19
VDEC	0.0281	7.702E−05	UWCS(class)	0.0929	3.708E−39	INTR(class)	−0.0571	9.413E−16
TDN	0.0408	9.634E−09	INST(class)	0.0045	0.5238	MPC(class)	−0.0636	3.314E−19
NAND	0.0357	5.191E−07	PACK(class)	−0.1029	9.956E−48	HVOL(class)	0.0823	4.344E−31
LOOP	0.0685	5.116E−22	RFC(class)	0.095	6.38E−41	HIER(class)	**−0.212**	6.066E−200
MOD	0.0103	0.1482	CBO(class)	−0.0157	0.0274	HLTH(class)	0.0911	9.53E−38
NOPR	0.067	3.801E−21	MI(class)	0.0482	1.144E−11	SIX(class)	−0.197	2.388E−172
EXCT	0.1125	9.723E−57	CCML(class)	0.1559	6.998E−108	TCC(class)	0.0897	1.203E−36
MDN	0.053	8.3E−14	NLOC(class)	0.0756	1.692E−26	NQU(class)	0.1489	1.568E−98
EXCR	−0.0067	0.3473	RVF(class)	−0.033	3.498E−06	F-IN(class)	0.0875	6.031E−35
HVOL	0.0512	5.719E−13	LCOM2(class)	−0.0486	7.691E−12	MOD(class)	0.0516	3.738E−13
VREF	0.0446	3.42E−10	MAXCC(class)	−0.0178	0.01245	LMC(class)	0.1034	3.68E−48

Table 8
Spearman results of code observability metrics.

Metric	rho	pvlaue	Metric	rho	pvlaue
is_public	−0.0639	2.35E−19	(cond(cond))	−0.0415	5.4E−09
is_static	0.1137	6.29E−58	(cond(loop))	0.0073	0.302
is_void	−0.1427	1.42E−90	(loop)	0.0685	5.12E−22
is_nested	0.0466	5.38E−11	(loop(cond))	0.0216	0.00242
method_length	−0.0004	0.95	(loop(loop))	0.0428	1.65E−09
nested_depth	0.053	8.3E−14	non_void_percent	0.2424	1.24E−262
direct_test_no	**0.4177**	0	getter_percent	−0.153	6.23E−104
test_distance	**−0.4921**	0	assertion-McCabe	**0.3956**	0
assertion_no	**0.3858**	0	assertion-density	**0.4096**	0
(cond)	0.023	0.00124			

Table 9
Random forest results of code observability metrics vs. Existing metrics.

pid	ZeroR				Existing				Code observability				Combined			
	Prec.	Recall	AUC	Err.	Prec.	Recall	AUC	Err.	Prec.	Recall	AUC	Err.	Prec.	Recall	AUC	Err.
pid	Prec.	Recall	AUC	Err.	Prec.	Recall	AUC	Err.	Prec.	Recall	AUC	Err.	Prec.	Recall	AUC	Err.
1	–	0.856	0.497	0.2465	0.927	0.93	0.961	0.1014	0.940	0.942	0.960	0.0786	**0.946**	**0.948**	**0.976**	**0.0741**
2	–	0.913	0.498	0.1595	0.947	0.951	0.932	0.0775	**0.960**	**0.962**	0.946	**0.063**	0.957	0.959	**0.951**	0.067
3	–	0.815	0.499	0.3015	0.848	0.861	0.836	0.2039	0.866	0.864	0.871	0.1727	**0.887**	**0.893**	**0.909**	**0.167**
4	–	0.507	0.468	0.5001	0.667	0.667	0.733	0.3831	**0.861**	**0.860**	**0.909**	**0.2044**	0.827	0.827	0.887	0.2626
5	–	0.62	0.5	0.4712	0.842	0.843	0.908	0.2347	0.868	0.869	0.931	0.1801	**0.901**	**0.901**	**0.955**	**0.168**
6	–	0.726	0.493	0.3982	0.73	0.743	0.804	0.2948	0.708	0.716	0.779	0.2976	**0.742**	**0.755**	**0.802**	**0.2946**
all	–	0.569	0.5	0.4905	0.862	0.862	0.928	0.2133	0.864	0.864	0.937	0.1846	**0.905**	**0.905**	**0.963**	**0.1625**
dir.	–	0.853	0.499	0.2513	0.945	0.946	0.949	0.0915	0.941	0.943	0.955	0.0933	**0.950**	**0.951**	**0.962**	**0.0886**
non.	–	0.593	0.5	0.4829	0.853	0.853	0.923	0.2329	0.813	0.814	0.893	0.2371	**0.878**	**0.879**	**0.941**	**0.2075**

4.2. Random forest

Classification effectiveness. As discussed in Section 3.4, we compare the four models in terms of both our code observability metrics and the existing metrics, namely:

1. `ZeroR`: model using ZeroR approach
2. `existing`: Random Forest model based on existing code metrics
3. `code observability`: Random Forest model based on code observability metrics
4. `combined`: Random Forest model based on the combination of existing metrics and code observability metrics

Table 9 shows the results of the comparison of the four models. To make clear which model performs better than the others, we highlighted the values of the model achieving the best performance among the four in **bold**, that of second best in underline. For precision, recall, and AUC, the model with the best performance is the one with the highest value, while for the mean absolute error, the best scoring model exhibits the lowest value. For the ZeroR model, because this model classifies all the instances to the majority (i.e., one class), the precision of the minority is not valid due to 0/0. Thus, in Table 9, we mark the precisions by "–".

From Table 9, we can see that the Random Forest models are better than the baseline `ZeroR` which only relies on the majority. This is the *prerequisite* for further comparison. `Combined` achieves the best performance (in 5 out of 6 projects) compared to the existing code metrics and code observability metrics in terms of AUC; this observation is as expected since `combined` considered both the existing and our metrics during training, which provides the classification model with more information.

Table 10
Feature importance of classification model (1).

1		2		3		4		5	
Metric	Imp.	Metric	Imp.	Metric	Imp.	Metric	Imp.	Metric	Imp.
test_distance	0.35	test_distance	0.15	test_distance	0.13	test_distance	0.48	test_distance	0.23
NLOC(class)	0.15	HIER(class)	0.12	NOCL	0.05	method_length	0.03	is_void	0.1
NOCL	0.03	CCML(class)	0.05	HDIF	0.03	COMP	0.03	EXCT	0.04
CREF	0.03	NLOC(class)	0.05	MI(class)	0.03	NOCL	0.03	NOCL	0.03
MINC(class)	0.03	NOCL	0.04	is_static	0.02	CAST	0.03	NOS	0.03
non_void_percent	0.02	MI(class)	0.04	non_void_percent	0.02	HDIF	0.03	S-R(class)	0.03
HDIF	0.02	assertion-density	0.03	HVOC	0.02	(Cond)	0.02	is_public	0.02
NOS(class)	0.02	CREF	0.03	HEFF	0.02	VREF	0.02	nested_depth	0.02
PACK(class)	0.02	HDIF	0.03	CREF	0.02	is_void	0.01	direct_test_no	0.02
TCC(class)	0.02	PACK(class)	0.03	VREF	0.02	direct_test_no	0.01	assertion_no	0.02
LMC(class)	0.02	method_length	0.02	NEXP	0.02	assertion_no	0.01	CREF	0.02
HLTH	0.01	HVOC	0.02	HEFF(class)	0.02	non_void_percent	0.01	HDIF	0.02
HVOC	0.01	HEFF	0.02	PACK(class)	0.02	assertion-density	0.01	PACK(class)	0.02
HEFF	0.01	LMET	0.02	CBO(class)	0.02	HLTH	0.01	F-IN(class)	0.02
XMET	0.01	NOA	0.02	CCML(class)	0.02	HVOC	0.01	method_length	0.01

Table 11
Feature importances of classification model (2).

6		all		dir.		non-dir.	
Metric	Imp.	Metric	Imp.	Metric	Imp.	Metric	Imp.
CBO(class)	0.09	test_distance	0.29	is_void	0.22	test_distance	0.16
HDIF	0.07	PACK(class)	0.06	PACK(class)	0.13	NOCL	0.09
NQU(class)	0.06	NOCL	0.05	HDIF	0.05	non_void_percent	0.04
test_distance	0.04	is_void	0.03	NOS	0.04	EXCT	0.04
non_void_percent	0.03	EXCT	0.03	assertion-density	0.03	HDIF	0.03
HVOC	0.03	non_void_percent	0.02	NEXP	0.03	PACK(class)	0.03
HEFF	0.03	CREF	0.02	direct_test_no	0.02	MI(class)	0.03
CREF	0.03	HDIF	0.02	assertion_no	0.02	CREF	0.02
XMET	0.03	MI(class)	0.02	assertion-McCabe	0.02	CBO(class)	0.02
NAND	0.03	is_public	0.01	NOCL	0.02	MINC(class)	0.02
VREF	0.03	is_nested	0.01	CREF	0.02	HIER(class)	0.02
NOA	0.03	method_length	0.01	NOA	0.02	F-IN(class)	0.02
NEXP	0.03	nested_depth	0.01	MINC(class)	0.02	MOD(class)	0.02
method_length	0.02	assertion_no	0.01	method_length	0.01	is_public	0.01
NOCL	0.02	getter_percent	0.01	nested_depth	0.01	is_static	0.01

The only exception is `java-apns-apns-0.2.3` (pid = 4). We conjecture that the number of instances (selected methods) in this project might be too small (only 150 methods) to develop a sound prediction model. In second place comes the model based on code observability metrics, edging out the model based on existing metrics.

For the overall dataset (the 7th row marked with "all" in Table 9), `combined` takes the first place in all evaluation metrics. In second place comes the `code observability`, slightly better than `existing`. Another interesting angle investigate further is the *test directness*. If we only consider the methods that are directly tested (the second to last row in Table 9), `combined` again comes in first, followed by the existing code metrics model. The same observation holds for the methods that are not directly tested (the last row in Table 9). It is easy to understand that when the dataset only considers methods that are directly tested (or not), the test directness features in our model become irrelevant. However, we can see that the difference between existing metrics and ours are quite tiny (<3.4%).

Feature importance analysis. Tables 10 and 11 show the top 15 features per project (and overall) in descending order. We can see that for five out of the six projects (including the overall dataset), `test_distance` ranks first. This again supports our previous findings that *test directness* plays a significant role in mutation testing. The remaining features in the top 14 vary per project; this is not surprising, as the task and context of these projects vary greatly. For example, `Apache Commons Lang` (Column "2" in Table 10) is a utility library that provides a host of helper methods for the `java.lang API`. Therefore, most methods in `Apache Commons Lang` are public and static; thus, `is_public` and `is_static` are not among the top 15 features for `Apache Commons Lang`. A totally different context is provided by the `JFreeChart` project (Column "5" in Table 10). `JFreeChart` is a Java chart library, whose class encapsulation and inheritance hierarchy are well-designed, so `is_public` appears among the top 15 features.

Looking at the overall dataset (Column "all" in Table 11), there are eight metrics from our proposed code observability metrics among the top 15 features. The importance of `test_distance` is much higher than the other features (>4.83X). In second place comes `PACK(class)`, or the number of packages imported. This observation is easy to understand since `PACK(class)` denotes the complexity of dependency, and dependency could influence the difficulty of testing, especially when making use of mocking objects. Thereby, dependency affects the mutation score. Clearly, more investigations are required to draw further conclusions. The third place in the feature importance analysis is taken by `NOCL`, which stands for the Number of Comments. This observation is quite interesting since `NOCL` is related to how hard it is to understand the code (*code readability*). This implies that code readability might have an impact on mutation testing.

As for the methods with direct tests (Column "dir." in Table 11), `is_void` takes the first position, which indicates that it is more difficult to achieve a high mutation score for void methods. Considering the methods without direct tests (Column "non-dir." in Table 11), `test_distance` again ranks first.

Another observation stems from the comparison of the performance of assertion related metrics in the feature importance analysis and the Spearman rank order correlation results (in Section 4.1). For Spearman's rank order correlation, we can see that

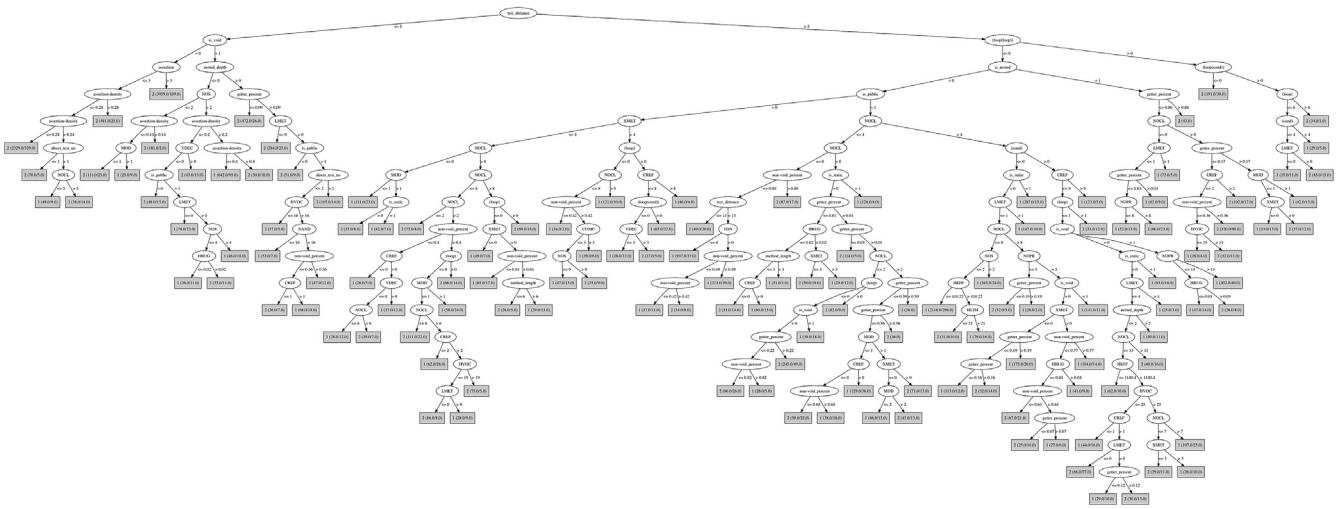


Fig. 4. Overview of J48 decision tree.

assertion related metrics are the second significant category right after test directness (in Table 8 in Section 4.1). While in the feature importance analysis, assertion related metrics mostly rank after the top 5 (shown in Tables 10 and 11) To further investigate the reason behind the dramatic changes of ranks for assertion related metrics, we analyse the correlations between test directness (i.e., `direct_test_no` and `test_distance`) and assertion related metrics (i.e., `assertion_no`, `assertion-McCabe` and `assertion_distance`). Looking at the correlation results between test directness and assertion related metrics in Table 12, the major reason is that test directness and assertion related metrics are almost *collinear* in the prediction model (where $|rho| > 0.87$). To put simply, there are almost no tests without assertions for the six subjects. If the method has a direct test, then the corresponding assertion no. is always greater than 1. Therefore, the ranks of assertion related metrics are not as high as we had initially expected in the feature importance analysis.

Moreover, we would like to put our observations into perspective by comparing our results with the work of Zhang et al. (2018), where they have constructed a similar Random Forest model to predict the result of killable mutant based on a series of features related to mutants and tests. The metrics that are common to their model and ours are Cyclomatic Complexity (COMP), Depth of Inheritance Tree (DIT), `nested_depth`, Number of Subclasses (NSUB), and `method_length`. Only two metrics in their study, i.e., `method_length` (in 6th place) and `nested_depth` (in 10th place) appear in our top 15 (Column "all" in Table 11). Especially `COMP` which ranks nine in their results is not in our top 15. There are multiple reasons for the difference in results: (i) we do consider a much larger range of metrics, which provide a better explanatory power (statistically speaking) than the one in their paper; (ii) our goal is to determine patterns in production and test code that may prevent killing some mutants while Zhang et al. (2018) predict if a mutant is killable (aka different prediction target and different granularity level). Besides, as we see later (next section), we can use our model to determine common anti-patterns with proper statistical methods. (iii) the subjects used in our experiment are different from theirs. For example, in project `java-apns-apns-0.2.3` (Column "4" in Table 11), `COMP` appears among the top 15.

Summary. *Overall, Random Forest based on the combination of existing code metrics and code observability metrics perform best, followed by that on code observability metrics. The analysis of feature importances shows that* test directness *ranks highest, remarkably higher than the other metrics.*

5. RQ4 code refactoring

Our goal is to investigate whether we can refactor away the observability issue that we expect to hinder tests from killing mutants and thus to affect the mutation score. In an in-depth case study, we manually analysed 16 code fragments to understand better the interaction between testability/observability metrics that we have been investigating, and the possibilities for refactoring.

Our analysis starts from the `combined` model, which as Table 9 shows, takes the leading position among the models. We then apply *Principal Component Analysis (PCA)* (Wold et al., 1987) to perform feature selection, which, as Table 13 shows, leaves us with 36 features (or metrics). Then, as discussed in Section 3, we build a decision tree based on those 36 metrics using J48 (shown in Fig. 4), and select the top 6 leaves (also called end nodes) in the decision tree for further *manual* analysis as potential refactoring guidelines. We present the top six anti-patterns in Table 14.

Here, we take a partial decision tree to demonstrate how we generate rules (shown in Fig. 5). In Fig. 5, we can see that there are three attributes (marked as an ellipse) and four end nodes or leaves (marked as a rectangle) in the decision tree. Since we would like to investigate how code refactoring increases mutation score (**RQ4**), we only consider the end nodes labelled with "LOW" denoting mutation score < 0.5. By combining the conditions along the paths of the decision tree, we obtain the two rules for "LOW" end nodes (as shown in the first column of the table in Fig. 5). For every end node, there are two values attached to the class: the first is the number of instances that correctly fall into the node, the other is the instances that incorrectly fall into the node. The accuracy in the table is computed by the number of correct instances divided by that of total instances. As mentioned earlier, we select the top 6 end nodes from the decision tree, where the end nodes are ranked by the number of correct instances under the condition accuracy ≥ 0.8.

After selecting the rules, the first author of this paper has conducted the main task of the manual analysis. If there were any questions during the manual analysis, the attempts of refactoring or adding tests are discussed among all the authors to reach an agreement. In our *actual* case study, we manually analysed 16 cases in total. Due to space limitations, we only highlight six cases in this paper (all details are available on GitHub (Zhu, 2019)). We will discuss our findings in code refactoring case by case.

Table 12
Spearman results of test directness vs. assertions in terms of *rho*.

(rho)	assertion_no	assertion-McCabe	assertion_distance
direct_test_no	0.9604	0.9472	0.9334
test_distance	−0.8707	−0.8707	−0.8707

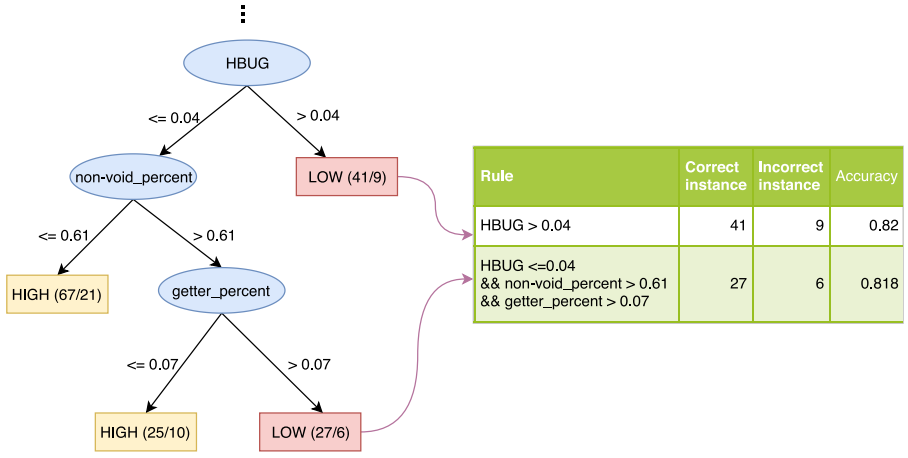


Fig. 5. Demo of rule generation.

```
/**                                                          1139
 * Draws the value label just below the center               1140
      of the dial.
 *                                                           1141
 * @param g2 the graphics device.                           1142
 * @param area the plot area.                               1143
 */                                                          1144
protected void drawValueLabel(Graphics2D g2,                 1145
    Rectangle2D area) {
  g2.setFont(this.valueFont);                                1146
  g2.setPaint(this.valuePaint);                              1147
  String valueStr = "No value";                              1148
  if (this.dataset != null) {                                1149
    Number n = this.dataset.getValue();                      1150
    if (n != null) {                                         1151
      valueStr = this.tickLabelFormat.format(n               1152
          .doubleValue()) + " "
          + this.units;                                      1153
    }                                                        1154
  }                                                          1155
  float x = (float) area.getCenterX();                       1156
  float y = (float) area.getCenterY() +                      1157
      DEFAULT_CIRCLE_SIZE;
  TextUtils.drawAlignedString(valueStr, g2, x,               1158
       y,TextAnchor.TOP_CENTER);
}                                                            1159
```

Listing 4: `plot.MeterPlot::drawValueLabel` (Case 1)

5.1. Case 1: `plot.MeterPlot::drawValueLabel` from JFree Chart

This case (shown in Listing 4) is under anti-pattern **Rule 1**: `test_distance > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 0 && XMET > 4 && (loop) ≤ 0 && NOCL ≤ 9 && non-void_percent ≤ 0.42`. In total, there are 5 mutants generated from this method (shown in Table 15). All 5 mutants survive the test suite.

Code changes. We start with `test_distance > 5` which means there is no direct test for this method. Accordingly, we add one direct test (shown in Listing 5).

However, Mutant 4 and 5 cannot be killed by adding the above direct test. Upon inspection, we found that Mutant 4 and 5 cannot

```
@Test                                                        1
public void testDrawValueLabel(){                           2
  MeterPlot p1 = new MeterPlot(new                           3
      DefaultValueDataset(1.23));
  BufferedImage image = new BufferedImage(3, 4,              4
       BufferedImage.TYPE_INT_ARGB);
  Graphics2D g2 = image.createGraphics();                    5
  Rectangle2D area = new Rectangle(0, 0, 1, 1);              6
  p1.drawValueLabel(g2,area);                                7
  assertTrue(g2.getFont() == p1.getValueFont())              8
      ;
  assertTrue(g2.getPaint() == p1.getValuePaint               9
      ());
}                                                            10
```

Listing 5: Direct test for Listing 4 (Case 1)

be killed because the `DrawValueLabel(...)` method is void. In particular, this means that the changes in the state caused by the `TextUtils.drawAlignedString()` method (line 1158) cannot be assessed. This is indicated by `non-void_percent ≤ 0.42` in **Rule 1**. We then refactor the method to have it return `Rectangle2D` (shown in Listing 6). Also, we improve the direct test for this method in Listing 5 by adding a new test method (shown in Listing 7) to avoid the *assertion roulette* test smell (Moonen et al., 2008; Palomba et al., 2016). By refactoring the method to non-void and adding a direct test, all previously surviving mutants are now successfully killed.

5.2. Case 2: `axis.SymbolAxis::drawGridBands` from JFree Chart

This case (shown in Listing 8) is under **Rule 2**: `test_distance > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 0 && XMET > 4 && (loop) ≤ 0 && NOCL > 9`. In total, 4 mutants are generated from this method (see Table 16). None of the mutants are killed.

Code changes. It is clear that this method is private, thus, it is impossible to call this method from outside the class directly. We first refactor this method from `private` to `public`. This is revealed by `is_public = 0` in **Rule 2**.

Table 13
Selected feature by PCA.

is_public	(cond)	assertion-density	XMET
is_static	(cond(cond))	COMP	LMET
is_void	(cond(loop))	NOCL	NLOC
is_nested	(loop)	NOS	VDEC
method_length	(loop(cond))	HLTH	TDN
nested_depth	(loop(loop))	HVOC	NAND
direct_test_no	non-void_percent	HEFF	LOOP
test_distance	getter_percent	HBUG	MOD
assertion_no	assertion-McCabe	CREF	NOPR

Table 14
Top six anti-patterns from J48 decision tree 4.

Rule no.	Details
1	test_distance > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 0 && XMET > 4 && (loop) ≤ 0 && NOCL ≤ 9 && non-void_percent ≤ 0.42
2	test_distance > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 0 && XMET > 4 && (loop) ≤ 0 && NOCL > 9
3	test_distance > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 1 && NOCL ≤ 4 && NOCL > 0 && is_static = 0 && getter_percent ≤ 0.01 && HBUG ≤ 0.02 && method_length > 3
4	test_distance > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 1 && NOCL > 4 && (cond) ≤ 0 && is_static = 0 && LMET ≤ 1 && NOCL > 8 && NOPR > 5 && is_void = 1
5	test_distance ≤ 5 && is_void = 1 && nested_depth ≤ 0 && NOS ≤ 2 && assertion-density ≤ 0.14 && MOD > 1
6	test_distance ≤ 5 && is_void = 1 && nested_depth ≤ 0 && NOS > 2 && assertion-density ≤ 0.22 && CREF > 1 && XMET > 0

Table 15
Summary of mutants from Listing 4 (Case 1).

ID	Line no.	Mutator	Results
1	1146	Removed call to java/awt/Graphics2D::setFont	SURVIVED
2	1147	Removed call to java/awt/Graphics2D::setPaint	SURVIVED
3	1149	Negated conditional	SURVIVED
4	1151	Negated conditional	SURVIVED
5	1157	Replaced float addition with subtraction	SURVIVED

```
protected Rectangle2D drawValueLabel(Graphics2D g2, Rectangle2D area) {                1145
  g2.setFont(this.valueFont);                                                          1146
  g2.setPaint(this.valuePaint);                                                        1147
  String valueStr = "No value";                                                        1148
  if (this.dataset != null) {                                                          1149
    Number n = this.dataset.getValue();                                                1150
    if (n != null) {                                                                   1151
      valueStr = this.tickLabelFormat.format(n.doubleValue()) + " "                    1152
          + this.units;                                                                1153
    }                                                                                  1154
  }                                                                                    1155
  float x = (float) area.getCenterX();                                                 1156
  float y = (float) area.getCenterY() + DEFAULT_CIRCLE_SIZE;                           1157
  return TextUtils.drawAlignedString(valueStr, g2, x, y,TextAnchor.TOP_CENTER);        1158
}                                                                                      1159
```

Listing 6: Refactoring of Listing 4 (Case 1)

Then, guided by `test_distance` > 5 from **Rule 2**, we add a direct test for this method to kill all mutants (see Listing 10).

5.3. Case 3: `builder.IDKey::hashCode` *from Apache Commons Lang*

This case (shown in Listing 11) is under **Rule 3**: `test_distance` > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 1 && NOCL ≤ 4 && NOCL > 0 && is_static = 0 && getter_percent ≤ 0.01 && HBUG ≤ 0.02 && method_length > 3. Only one mutant is generated for this method: a mutant that replaces the return value with (x == 0 ? 1 : 0). This mutant survives.

Code changes. Starting with `test_distance` > 5, we add a direct test for this method (shown in Listing 12), which works perfectly to kill the mutant.

5.4. Case 4: `AbstractCategoryItemRenderer::drawOutline` *from JFreeChart*

This case (shown in Listing 13) is under **Rule 4**: `test_distance` > 5 && (loop(loop)) ≤ 0 && is_nested = 0 && is_public = 1 && NOCL > 4 && (cond) ≤ 0 && is_static = 0 && LMET ≤ 1 && NOCL > 8 && NOPR > 5 && is_void = 1. Also in this case, only 1 mutant is generated for this method. The

```
@Test
public void testDrawValueLabelArea() {
  MeterPlot p1 = new MeterPlot(new
      DefaultValueDataset(1.23));
  BufferedImage image = new BufferedImage(3, 4,
      BufferedImage.TYPE_INT_ARGB);
  Graphics2D g2 = image.createGraphics();
  Rectangle2D area = new Rectangle(0, 0, 1, 1);
  Rectangle2D drawArea = p1.drawValueLabel(g2,area);
  assertEquals(0.5,drawArea.getCenterX(),0.01);
  assertEquals(18.8671875,drawArea.getCenterY(),0.01);
  assertEquals(15.0,drawArea.getHeight(),0.01);
  assertEquals(64.0,drawArea.getWidth(),0.01);
}
```

Listing 7: Improved direct test for Listing 4 (Case 1)

```
/**
 * Similar to {@link Color#darker()}.
 * <p>
 * The essential difference is that this method
 * maintains the alpha-channel unchanged<br>
 *
 * @param paint a {@code Color}
 *
 * @return a darker version of the {@code Color
     }
 */
private static Color darker(Color paint) {
  return new Color(
      (int)(paint.getRed () * FACTOR),
      (int)(paint.getGreen() * FACTOR),
      (int)(paint.getBlue () * FACTOR), paint.
          getAlpha());
}
```

Listing 8: `axis.SymbolAxis::drawGridBands` (Case 2)

```
public static Color darker(Color paint) {
  return new Color(
      (int)(paint.getRed () * FACTOR),
      (int)(paint.getGreen() * FACTOR),
      (int)(paint.getBlue () * FACTOR), paint.
          getAlpha());
}
```

Listing 9: Refactoring of Listing 8 (Case 2)

```
@Test
public void testDarker(){
  Color paint = new Color(10,20,30);
  Color darker = PaintAlpha.darker(paint);
  assertEquals(7,darker.getRed());
  assertEquals(14,darker.getGreen());
  assertEquals(21,darker.getBlue());
}
```

Listing 10: Direct test for Listing 8 (Case 2)

particular change applied is the removal of the call to `AbstractCategoryPlot::drawOutline`. The original test suite did not kill the mutant.

Code changes. Based on `test_distance` > 5, we add one direct test (as shown in Listing 14) for this method to kill the surviving mutant.

```
  /**
   * returns hash code - i.e., the system
       identity hashcode.
   * @return the hashcode
   */
  @Override
  public int hashCode() {
    return id;
  }
```

Listing 11: `builder.IDKey::hashCode` (Case 3)

```
@Test
public void testHashCode(){
  IDKey idKey = new IDKey(new Integer(123));
  assertEquals(989794870,idKey.hashCode());
}
```

Listing 12: Direct test for Listing 11 (Case 3)

```
/**
 * Draws an outline for the data area. The
     default implementation just
 * gets the plot to draw the outline, but some
     renderers will override this
 * behaviour.
 *
 * @param g2 the graphics device.
 * @param plot the plot.
 * @param dataArea the data area.
 */
@Override
public void drawOutline(Graphics2D g2,
    CategoryPlot plot,
    Rectangle2D dataArea) {
  plot.drawOutline(g2, dataArea);
}
```

Listing 13: `AbstractCategoryItemRenderer::drawOutline` (Case 4)

```
@Test
public void testDrawOutline(){
  AbstractCategoryItemRenderer r = new
      LineAndShapeRenderer();
  BufferedImage image = new BufferedImage(200 ,
      100,
      BufferedImage.TYPE_INT_RGB);
  Graphics2D g2 = image.createGraphics();
  CategoryPlot plot = new CategoryPlot();
  Rectangle2D dataArea = new Rectangle2D.Double
      ();
  r.drawOutline(g2,plot,dataArea);
  assertTrue(g2.getStroke()==plot.
      getOutlineStroke());
}
```

Listing 14: Direct test for Listing 13 (Case 4)

5.5. *Case 5:* `builder.ToStringStyle::setUseShortClassName` *from Apache Commons Lang*

This case (shown in Listing 15) is under **Rule 5**: `test_distance` ≤ 5 && `is_void` = 1 && `nested_depth` ≤ 0 && `NOS` ≤ 2 && `assertion-density` ≤ 0.14 && `MOD` > 1. In this case, a single (surviving) mutant is generated that removes the call to `builder.ToStringStyle::setUseShortClassName`.

Code changes. We can see that **Rule 5** is different from the previous rule in that `test_distance` is less than 5, while in

```
/**                                                          81
 * <p>Sets whether to output short or long                   82
      class names.</p>
 *                                                           83
 * @param useShortClassName the new                         84
      useShortClassName flag
 * @since 2.0                                               85
 */                                                          86
@Override                                                    87
public void setUseShortClassName(final boolean              88
     useShortClassName) { // NOPMD as this is
     implementing the abstract class
  super.setUseShortClassName(useShortClassName)              89
       ;
}                                                            90
```

Listing 15: `builder.ToStringStyle::setUseShortClassName` (Case 5)

```
@Test                                                        1
public void testSetUseShortClassName(){                     2
  assertTrue(STYLE.isUseShortClassName());                   3
  STYLE.setUseShortClassName(false);                         4
  assertFalse(STYLE.isUseShortClassName());                  5
  STYLE.setUseShortClassName(true);                          6
  assertTrue(STYLE.isUseShortClassName());                   7
}                                                            8
```

Listing 16: Additional assertions for Listing 15 (Case 5)

```
/**                                                          30
 * Construct the exception.                                 31
 *                                                          32
 * @param max Maximum number of evaluations.                33
 */                                                          34
public TooManyEvaluationsException(Number max)               35
    {
  super(max);                                                36
  getContext().addMessage(LocalizedFormats.                  37
      EVALUATIONS);
}                                                            38
```

Listing 17: `exception.TooManyEvaluationsException::<init>` (Case 6)

Rule 4 `test_distance` > 5. A more in-depth analysis reveals that the method in Listing 15 is already directly invoked by the original test suite. The surviving mutant is due to the fact that there are no assertions that examine the changes after the `setUseShortClassName` method call. This situation is reflected by `assertion-density` ≤ 0.14 in **Rule 5**. Therefore, we add assertions to assess the changes (seen in Listing 16), which leads to the mutant being killed.

5.6. Case 6: `exception.TooManyEvaluationsException::<init>` from Apache Commons Math

This case (shown in Listing 17) is under **Rule 6**: `test_distance` ≤ 5 && `is_void` = 1 && `nested_depth` ≤ 0 && `NOS` > 2 && `assertion-density` ≤ 0.22 && `CREF` > 1 && `XMET` > 0 && `VDEC` ≤ 0 && `NOCL` ≤ 12. A single mutant is generated: a removal of the call to `exception.util.ExceptionContext::addMessage`. This mutant is surviving the test suite.

Code changes. We found that the mutant in Line 37 cannot be killed because the function `addMessage` changes the field `List<Localizable> msgPatterns`. This field is private in the class `ExceptionContext` and there is no other way to access it.

```
public List<Localizable> getMsgPatterns(){                   1
  return msgPatterns;                                        2
}                                                            3
```

Listing 18: Refactoring of Listing 17 (Case 6)

```
@Test                                                        1
public void testMsgPatterns() {                              2
  final int max = 12345;                                     3
  final TooManyEvaluationsException e = new                  4
      TooManyEvaluationsException(max);
  final String msg = e.getLocalizedMessage();                5
  Assert.assertTrue(e.getContext().getMsgPatterns()          6
      .contains(LocalizedFormats.EVALUATIONS));              7
}                                                            8
```

Listing 19: Additional assertion for Listing 17 (Case 6)

As such, our first step is to add a getter for `msgPatterns` (shown in Listing 18). In **Rule 6**, we can see that `is_void` = 1 is the underlying cause since void methods could be difficult to test if no getters for private fields exist.

To kill the surviving mutant, we add one extra assertion (in a new test method) to examine the changes in `msgPatterns` (in Listing 19). This action is also partly evidenced by `assertion-density` ≤ 0.22 in **Rule 6**. As assertion-density denotes the ratio between the total number of assertions in direct tests and the lines of code in direct tests, low assertion-density is a sign of insufficient assertions in the direct tests to detect the mutant.

5.7. RQ4 summary

Based on all 16 cases that we analysed (available in our GitHub repository Zhu, 2019), we found that our code observability metrics can lead to simple refactorings that enable to kill mutants that were previously not being killed. Ultimately, this leads to an increase of the mutation score:

- Most cases can be easily fixed by adding direct tests if `test_distance`>5.
- Most cases can be easily fixed by adding assertions if `test_distance` ≤ 5.
- Private methods must be refactored to protected/public for testing (indicated by `is_public`=0).
- Three void methods had to be refactored to be non-void (indicated by `is_void`=1 and `non-void_percent` ≤ 0.42).
- One void method needed an additional getter because a private field was changed (indicated by `is_void`=1).

5.8. Discussion

From the findings of **RQ4**, we can see that some code refactorings break OO design principles (Booch, 2006). For instance, we suggest to change the access modifier from *private* to *protected/public* to kill the mutants; this violates the idea of Encapsulation, the ability to protect some components of the object from external entities (Booch, 2006). A new *hypothesis* emerges from our study: the trade-off between OO design principles and testing and hence software testability (Suri and Singhani, 2015). The main concepts of OO design are centred around the features of Data abstraction, Encapsulation, Inheritance, Polymorphism, and Dynamic binding. However, some factors such as Encapsulation and Inheritance could increase the complexity of OO systems and

Table 16
Summary of mutants from Listing 8 (Case 2).

ID	Line no.	Mutator	Results
1	165	Mutated return of Object value for org/jfree/chart/util/PaintAlpha::darker to (if (x ! = null) null else throw new RuntimeException)	NO_COVERAGE
2	166	Replaced double multiplication with division	NO_COVERAGE
3	167	Replaced double multiplication with division	NO_COVERAGE
4	168	Replaced double multiplication with division	NO_COVERAGE

hence hinder testing and testability (Suri and Singhani, 2015). Existing literature (Mouchawrab et al., 2005; Singh and Saha, 2010; Zhou et al., 2012; Nazir et al., 2010) has already addressed this dilemma. Mouchawrab et al. (2005) pointed out that increasing the size of the inheritance hierarchy could increase the cost of testing due to dynamic dependencies. Singh and Saha's work (Singh and Saha, 2010) has shown that Inheritance and Polymorphism increase testing effort and lower software testability. All the works above indicate that there is a trade-off between OO design features and software testability. Currently, it is up to practitioners to balance the two perspectives themselves, depending on the requirements of software and their preferences.

In the context of mutation testing, a similar trade-off between OO design features and the ease of killing mutants exists. In this study, we relate the ease of killing mutants to the testability and observability. In Section 5.7, we found that a simple strategy to kill *all* the mutants is to write additional direct tests and/or assertions. However, some OO design features related to Encapsulation, such as the *private* access modifier (see Listing 8), increase the difficulty to add a direct test. Also, the void return type prevents killing the mutants generated from the immediate states that cannot propagate to the output (see Listing 4). As such, a very important note here is that our refactoring recommendations listed in Section 5.7 are centred around the anti-patterns based on the testability and observability; they *do not* take OO design principles into consideration. The recommendations attempt to help developers in understanding the cause of the low mutation score considering testability and observability, but not all surviving mutants are due to test quality.

Take Listing 8 for instance. The developer found the mutation score of this method is low, and our tool shows the low mutation score is mainly due to *private* access control modifier. Then, the developer can decide to ignore the surviving mutants if he cannot break Encapsulation based on the requirement. Or if this method is critical and must be well-tested according to the document, he may alter the access control modifier from *private* to *protected/public* to kill the mutants. Whether the developers make use of these testability and observability recommendations depends on their choices with regard to either (1) adding test cases (Beller et al., 2015b,a, 2019) (2) refactoring the production code to kill the mutants, or (3) ignoring the surviving mutants.

6. Threats to validity

External validity. Our results are based on mutants generated by the operators implemented in PIT. While PIT is a frequently used mutation testing tool, our results might be different when using other mutation tools (Kurtz et al., 2016). Concerning the subject systems selection, we choose six open-source projects from GitHub; the selected projects differ in size, the number of test cases, and application domain. Besides, as mentioned in Section 3, the large number of methods with low number of mutants is an unavoidable bias in our experiment. The reason is partly due to the optimisation mechanism of PIT (Coles, 2019d) and partly due to a large number of short methods in those projects. Nevertheless, we do acknowledge that a broad replication of our study would mitigate any generalisability concerns even further.

Internal validity. The main threat to internal validity for our study is the implementation of the MUTATION OBSERVER tool for the experiment. To reduce internal threats to a large extent, we rely on existing tools that have been widely used, e.g., `WEKA`, `MATLAB`, and PIT. Moreover, we carefully reviewed and tested all code for our study to eliminate potential faults in our implementation. Another threat to internal validity is the disregard of equivalent mutants in our experiment. However, this threat is unavoidable and shared by other studies on mutation testing that attempt to detect equivalent mutants or not (Grün et al., 2009; Mirshokraie et al., 2013). Moreover, we consider equivalent mutants as a potential weakness in the software (reported by Coles Coles, 2019b, slide 44–52); thereby, we did not manually detect equivalent mutants in this paper.

Construct validity. The main threat to construct validity is the measurement we used to evaluate our methods. We minimise this risk by adopting evaluation metrics that are widely used in research (such as recall, precision, and AUC), as well as a sound statistical analysis to assess the significance (Spearman's rank-order correlation).

7. Related work

The notion of *software testability* dates back to 1991 when Freedman (1991) formally defined *observability* and *controllability* in the domain of software. Voas (1992) proposed a dynamic technique coined propagation, infection, and execution (PIE) analysis for statistically estimating the program's *fault sensitivity*. More recently, researchers have aimed to increase our collective understanding of *testability* by using statistical methods to predict *testability* based on various code metrics. A prime example is the work of Bruntink and van Deursen (2006), who have explored the relationship between nine class-level object-oriented metrics and testability. To the best of our knowledge, no study uses statistical or machine learning methods to investigate the relationship between *testability*/*observability* metrics and the mutation score.

Mutation testing was initially introduced as a fault-based testing method which was regarded as significantly better at detecting errors than the *covering measure* approach (Budd et al., 1979). Since then, mutation testing has been actively investigated and studied, thereby resulting in remarkable advances in its concepts, theory, technology, and empirical evidence. For more literature on mutation testing, we refer to the existing surveys of DeMillo (1989), Offutt and Untch (2001), Jia and Harman (2011), Offutt (2011) and Zhu et al. (2018b). Here we mainly address the studies that concern *mutant utility* (Just et al., 2017), the efficacy of mutation testing. Yao et al. (2014) have reported on the causes and prevalence of equivalent mutants and their relationship to stubborn mutants based on a manual analysis of 1230 mutants. Visser (2016) has conducted an exhaustive analysis of all possible test inputs to determine how hard it is to kill a mutant considering three common mutation operators (i.e., relational, integer constants and arithmetic operators). His results show that *mutant reachability, mutation operators, and oracle sensitivity* are the key contributors to determining how hard it is to kill a mutant. Just et al. (2017) have shown a strong correlation

between mutant utility and context information from the program in which the mutant is embedded. Brown et al. (2017) have developed a method for creating potential faults that are more closely coupled with changes made by actual programmers where they named "wild-caught mutants". Chekam et al. (2018) have investigated the problem of selecting the fault revealing mutants. They put forward a machine learning approach (decision trees) that learns to select fault revealing mutants from a set of static program features. Jimenez et al. (2018) investigated the use of natural language modelling techniques in mutation testing. All studies above have enriched the understanding of mutation testing, especially its efficacy. However, the aim of our work is different from those studies, as we would like to gain insights into how code quality in terms of testability and observability affects the efforts needed for mutation testing, especially in how to engineer tests to kill *more* the mutants.

Similar to our study, there have been a few recent studies also investigating the relationships between assertions and test directness with mutation testing. Schuler and Zeller (2013) introduced *checked coverage* – the ratio of statements that contribute to the computation of values that are later checked by the test suite – as an indicator for oracle quality. In their experiment, they compared checked coverage with the mutation score, where they found that checked coverage is more sensitive than mutation testing in evaluating oracle quality. Huo and Clause (2016) proposed *direct coverage and indirect coverage* by leveraging the concepts of *test directness* with conventional statement coverage. They used the mutants as an indicator of the test suite effectiveness, and they found faults in indirectly covered code are significantly less likely to be detected than those in directly covered code. Zhang and Mesbah (2015) evaluated the relationship between test suite effectiveness (in terms of the mutation score) and the (1) number of assertions, (2) assertion coverage, and (3) different types of assertions. They found test assertion quantity and assertion coverage are strongly correlated with the mutation score, and assertion types could also influence test suite effectiveness. Compared to our studies, those works only addressed one or two aspect(s) of *code observability* in our study. We provide a complete view of the relationships between code observability and mutation testing.

The study most related to ours is that of Zhang et al. (2018)'s *predictive mutation testing*, where they have constructed a classification model to predict killable mutant result based on a series of features related to mutants and tests. In their discussion, they compared source code related features and test code related features in the prediction model for the mutation score. They found that test code features are more important than source code ones. But from their results, we cannot draw clear conclusions on the impact of production code on mutation testing as their goal is to predict exact killable mutant results. Another interesting work close to our study is Vera-Pérez et al. (2017)'s *pseudo-tested methods*. Pseudo-tested methods denote those methods that are covered by the test suite, but for which no test case fails even if the entire method body is completely stripped. They rely on the idea of "extreme mutation", which completely strips out the body of a method. The difference between Vera-Pérez et al. (2017)'s study and ours is that we pay attention to *conventional* mutation operators rather than "extreme mutation".

8. Conclusion & future work

This paper aims to bring a new perspective to software developers helping them to understand and reason about the mutation score in the light of *testability* and *observability*. This should enable developers to make decisions on the possible actions to take when confronted with low mutation scores. To achieve this goal, we firstly investigate the relationship between *testability* and *observability* metrics and the mutation score. More specifically, we have collected 64 existing source code quality metrics for testability, and have proposed a set of metrics that specifically target *observability*. The results from our empirical study involving 6 open-source projects show that the 64 existing code quality metrics are not strongly correlated with the mutation score ($|rho| < 0.27$). In contrast, the 19 newly proposed *code observability metrics*, that are defined in terms of both production code and test cases, do show a stronger correlation with the mutation score ($|rho| < 0.5$). In particular, *test directness*, `test_distance`, and `direct_test_no` stand out.

To better understand the causality of our insights, we continue our investigation with a manual analysis of 16 methods that scored particularly bad in terms of mutation score, i.e., a number of mutants were not killed by the existing tests. In particular, we have refactored these methods and/or added tests according to the anti-patterns that we established in terms of the code observability metrics. Our aim here is to establish whether the removal of the observability anti-patterns would lead to an increase in the mutation score. We found that these anti-patterns can indeed provide insights in order to kill the mutants by indicating whether the production code or the test suite needs improvements. For instance, we found that private methods (expressed as `is_public=0` in our schema) are prime candidates to potentially refactor to increase their observability, e.g., by making them public or protected for testing purposes.

However, some refactoring recommendations could violate OO design principles. For example, by changing *private* to *protected/public* we increase observability, but we also break the idea of encapsulation. Therefore, we suggest developers make a choice between – (1) adding test cases, (2) refactoring the production code to kill the mutants, or (3) ignoring the surviving mutants – by considering the trade-off between OO design features and testability/observability.

To sum up, our paper makes the following contributions:

1. 19 newly proposed *code observability* metrics
2. A detailed investigation of the relationship between *testability*/*observability* metrics and the mutation score (**RQ1**–**RQ3**)
3. A case study with 16 code fragments to investigate whether removal of the anti-patterns increases the mutation score (**RQ4**)
4. A guideline for developers to make choices when confronting low mutation scores
5. A prototype tool coined Mutation Observer (openly available on GitHub (Zhu, 2019)) that automatically calculates code observability metrics

Future work. With our tool, and since the results are encouraging, we envision the following future work: (1) conduct additional empirical studies on more subject systems; (2) evaluate the usability of our code observability metrics by involving practitioners; (3) investigate the relations between more code metrics (e.g., code readability) and mutation score.

CRediT authorship contribution statement

Qianqian Zhu: Methodology, Software, Data curation, Visualization, Investigation, Validation, Writing - original draft, Resources. **Andy Zaidman:** Conceptualization, Methodology, Supervision, Project administration, Funding acquisition, Writing - review & editing. **Annibale Panichella:** Methodology, Supervision, Writing - review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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