Do Automatically Generated Test Cases Make Debugging Easier? An Experimental Assessment of Debugging Effectiveness and Efficiency

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Several techniques and tools have been proposed for the automatic generation of test cases. Usually, these tools are evaluated in terms of fault revealing or coverage capability, but their impact on the manual debugging activity is not considered. The question is whether automatically generated test cases are equally effective in supporting debugging as manually written tests.

We conducted a family of three experiments (five replications) with humans (in total, 55 subjects), to assess whether the features of automatically generated test cases, which make them less readable and understandable (e.g., unclear test scenarios, meaningless identifiers), have an impact on the effectiveness and efficiency of debugging. The first two experiments compare different test case generation tools (Randoop vs. EvoSuite). The third experiment investigates the role of code identifiers in test cases (obfuscated vs. original identifiers), since a major difference between manual and automatically generated test cases is that the latter ones contain meaningless (obfuscated) identifiers.

We show that automatically generated test cases are as useful for debugging as manual test cases. Furthermore, we find that for less experienced developers, automatic tests are more useful on average due to their lower static and dynamic complexity.

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1. INTRODUCTION

Automated test case generation has been a major area of investigation in the last decade [Pacheco and Ernst 2007] [Fraser and Arcuri 2011] [Tillmann and Halleux 2008] [Fraser and Zeller 2010]. Tools for automated test case generation typically aim at maximizing branch or path coverage. Randoop [Pacheco and Ernst 2007] produces Java test cases automatically as random sequences of method invocations with random parameter values. EvoSuite [Fraser and Arcuri 2011] and eToc [Tonella 2004] use a genetic algorithm to maximize branch coverage for Java classes under test. Other tools target path coverage by means of dynamic symbolic execution [Godefroid et al. 2005] [Sen et al. 2005], including DART [Godefroid et al. 2005], CUTE [Sen et al. 2005], EXE [Cadar et al. 2008] and Crest [Burnim and Sen 2008] for C code; PEX [Tillmann and Halleux 2008] for C# code; and, SAGE [Godefroid et al. 2008] for binary code.

While substantial effort has been devoted to empirically study the effectiveness of these solutions in revealing faults [Pacheco and Ernst 2007] [Beckman et al. 2010] [Tillmann and Halleux 2008] [Godefroid et al. 2005] [Fraser and Zeller 2010], the problem of the actual usability of the automatically generated test cases for the developers who rely on them for debugging is a neglected topic. Automatically generated test cases (autogen, for brevity) are, in fact, less understandable and meaningful than manually designed test cases. Indeed, autogen tests do not exercise any meaningful test scenario, from the end user's viewpoint; they do not address explicitly any high-level requirement or functionality; they include meaningless identifiers and data values. Hence, interpretation of a failing execution might be substantially more difficult with autogen test cases than with manual tests. This might impact negatively the debugging activities, in comparison to debugging when supported by manually written tests.

The problem of the actual usability and effectiveness of autogen test cases is a key problem in the area of automated software testing. So far, no empirical evidence has been collected to show if there is any negative impact on debugging and what is the extent of such an impact. When test case generation technologies are considered for industrial adoption, estimating all the associated costs, including indirect costs such as an additional cost during debugging, is critical for informed decision-making.

This paper presents the first family of experiments that investigates the impact of automated test case generation on debugging. We have considered two test case generators (Randoop [Pacheco and Ernst 2007] and EvoSuite [Fraser and Arcuri 2011]) in two separate experiments, each replicated twice, to measure empirically the effectiveness and efficiency of debugging when autogen test cases are used, as compared to manual test cases available for the applications under test. Since autogen test cases include meaningless identifiers (e.g., `x0`, `x1`) that are generated automatically by tools, we also investigated, in a separate experiment, whether the obfuscation of identifiers in the test cases has any negative impact on debugging. This third experiment aims at assessing the role of meaningful identifiers in test cases during debugging, regardless of any other feature of autogen tests, to understand if the generation of meaningless identifiers alone has a negative impact on debugging. In total, we involved 55 human subjects in all replications of our three experiments, with a wide range of expertise and capabilities (from BSc and MSc students to PhD/Post-docs and researchers/professors).

This paper extends our previous work [Ceccato et al. 2012] with the following key contributions:

— In addition to the Randoop random test case generator, we consider test cases generated by another tool, EvoSuite, which generates test cases using genetic algorithms. A new experiment, replicated twice, was designed and conducted to assess the impact of EvoSuite test cases on debugging.
— We investigate the role of meaningful and understandable code identifiers used in test cases in a new experiment, where identifiers of manual test cases are deliberately obfuscated.
— We extend the discussion of our findings to account for the results obtained from the new studies, which allows us to provide a detailed and wide spectrum analysis of the impact of autogen tests on debugging.

— We analyze the representativeness of the two applications under test chosen for the empirical studies, JTopas and XML-Security, by comparing their features with those of other open source Java projects.

The paper is organized as follows. After commenting related work in Section 2, Section 3 describes the design underlying the family of three experiments reported in this paper. Sections 4, 5 and 6 report the results of the three experiments. A discussion of the experimental results is provided in Section 7. Section 8 discusses the threats to the validity of our experiments. We draw our final conclusions in Section 9.

2. RELATED WORK

Test cases are one of the most relevant sources of information to locate faults. Several papers [Artzi et al. 2010; Jones et al. 2002; Zeller and Hildebrandt 2002] propose approaches and techniques to analyze test cases and their execution with the aim of detecting and locating faults. The underlying idea of test-based debugging is that the information collected during test case execution (e.g., code coverage and application state) can be useful to identify the set of "suspicious" code statements (i.e., those where the fault may be located). Automatically generated test cases have been found to be quite effective in detecting faults [Andrews et al. 2008; Ciupa et al. 2007; Duran 1984]. Several automated test case generation tools have been proposed. Concrete and symbolic execution are mixed in tools such as PEX [Tillmann and Halleux 2008], Cute [Sen et al. 2005] and DART [Godefroid et al. 2005]. Randoop [Pacheco and Ernst 2007] creates test cases based on randomly generated sequences of method invocations enriched with assertions automatically inferred through dynamic analysis. EvoSuite [Fraser and Arcuri 2011] takes advantage of genetic algorithms to generate test cases for Java classes, with the goal of maximizing the level of branch coverage. Fraser et al. [Fraser and Zeller 2010] apply mutation analysis to automatically generate test cases. In our experiment, we compared manually defined test cases to the ones obtained with Randoop and EvoSuite. This does not cover the full range of possible test case generators, but we think that Randoop and EvoSuite are representative of two important families of tools (relying on random testing and search-based testing). Moreover, the experiment with obfuscated identifiers provides key insights that hold for all test case generators, since they all generate meaningless (obfuscated) identifiers.

Empirical studies on automated test case generators [Andrews et al. 2008; Ciupa et al. 2007; Duran 1984] are focused on the fault finding capabilities of tools. However, no study is available about the impact of automatically generated test cases on debugging. In this paper, we report a family of experiments that, for the first time, measures the effectiveness of automatically generated test cases when they are used to carry out debugging tasks.

Most of the literature containing empirical studies related to testing and debugging describes experiments that do not involve human subjects. For instance, Frankl and Weiss [Frankl and Weiss 1991] compared the capability of revealing faults when test cases satisfy different coverage criteria. However, their work did not consider the intensive manual activity necessary to locate a fault, after a test case has revealed it. A few empirical studies on software testing and debugging involved human subjects, but they considered directions different from the one investigated in this paper. For instance, the studies by Ricca et al [Ricca et al. 2009] and Huang et al [Huang and Holcombe 2009] focus on the testing process and strategy, evaluating the impact of the "test first" strategy either on the accuracy of change tasks or on the quality of the final code.

A few attempts have been made to investigate the relationship between testing and debugging. Fry et al. [Fry and Weimer 2010] presented an observational study on the accuracy of human subjects in locating faults. They discovered that certain types of faults are difficult to locate for humans. For instance, "extra statement" faults seem to be easier to detect than "missing statement" faults. However, the authors did not investigate the role of test cases. They also observed that, independently from the faults, certain code contexts are difficult to debug for humans. For instance, the array abstraction is easier than the tree abstraction. Weiser et al. [Weiser and Lyle 1986] empirically evaluated the impact of a slicing tool on debugging. They did not observe any improvement when developers use a slicing tool to debug small, faulty programs. Parnin and Orso [Parnin and Orso 2011] performed an experiment to investigate how developers use debugging tools and whether these improve performance. Tools are shown to help complete debugging tasks faster. Still, Parnin and Orso's study does not consider the role of test cases.

Other empirical studies on software testing, which are to some extent related to the present paper, include: (1) Itkonen et al. [Itkonen et al. 2009] present an observational study in which they report the manual (functional) testing practices in four software companies. They noticed that many of the applied techniques are based on similar ideas as traditional test case design techniques, even if they rely on experience rather than a formalized technique, as available in scientific literature. (2) Ricca et al. [Ricca et al. 2009] report a series of studies that investigate the role of acceptance tests when facing requirement changes. (3) Yu et al. [Yu et al. 2008] performed an experiment to evaluate the impact of test-suite reduction on the effectiveness of fault-localization techniques. They also show that fault-localization effectiveness strongly varies depending on the test-suite reduction strategy considered. (4) Huang et al. [Huang and Holcombe 2009] report an experiment in which they compared the effectiveness of Test First and Test Last strategies. Although their results are not confirmed statistically, they found that the quality of the produced software increases with an increased time spent on application testing. (5) Ruthruff et al. [Ruthruff et al. 2005] report an experiment in which they studied the impact of two factors, i.e., information base and mapping, in fault localization. They showed that such factors impact to a major extent the effectiveness of fault localization activities.

To the best of our knowledge, our work is the first empirical study with human subjects that investigates the effectiveness and efficiency of debugging when autogen and manually written test cases are used.

3. EXPERIMENT DEFINITION AND PLANNING

This section describes the definition, the design, and the settings of the experiments in a structured way, following the template and guidelines by Wohlin et al. [Wohlin et al. 2012].

The *goal* of the study is to investigate the differences between manually written and autogen test cases, with the purpose of evaluating how well they support debugging tasks. The *quality focus* regards how manually written and autogen test cases affect the capability of developers to correctly and efficiently debug the code. The results of the experiments are interpreted regarding two *perspectives*: (1) a researcher interested in empirically validating autogen test cases and (2) a quality manager who wants to understand whether the time spent in writing test cases pays off when facing actual debugging tasks.

The *context* of the studies is defined as follows: the *subjects* of the study are developers facing debugging tasks, while the *objects* are applications that contain the faults to be fixed. We collected empirical data from three experiments:

(1) **Manual vs. Randoop [MvR]**: This experiment compares manual test cases to test cases produced automatically by a random test case generator, *Randoop* [Pacheco and Ernst 2007].
(2) **Manual vs. EvoSuite [MvE]**: This experiment compares manual test cases to test cases produced automatically by an evolutionary test case generator, *EvoSuite* [Fraser and Arcuri 2011].
(3) **Manual vs. Obfuscated [MvO]**: This experiment investigates the impact on debugging of a specific aspect of autogen test cases: the artificial identifiers that appear in their implementation. To evaluate the impact of artificial identifiers we compare manual test cases with original identifiers to manual test cases with identifiers obfuscated as in the tests produced by test generation tools.

While several tools are available for automated test case generation [Pacheco and Ernst 2007; Fraser and Arcuri 2011; Tillmann and Halleux 2008; Tonella 2004; Godefroid et al. 2005; Sen et al. 2005; Cadar et al. 2008; Burnim and Sen 2008], we restricted our choice to those that support the Java programming language, because the students involved in the experiment are mostly familiar with Java. Moreover, we wanted to choose tools that have a reasonable usability and support from their developers and that have reached a good maturity level. Under such constraints, the only two tools that we could find as freely available are Randoop and EvoSuite.

Experiments MvR and MvE do not distinguish two different aspects that affect autogen test cases: (1) their structure and (2) the identifiers they use. Usually, autogen test cases have a simpler structure than manual test cases and, differently from manual test cases, they use meaningless, automatically generated identifiers. The aim of experiment MvO is to factor out one of these two aspects from the other. Namely, MvO investigates the extent to which the presence of meaningless identifiers in autogen test cases is detrimental to the debugging activity. Since the simpler structure of autogen tests is potentially beneficial to debugging, by combining the outcome of MvO with that of MvR and MvE we can understand whether one of the conflicting aspects, simple structure versus meaningless identifiers, compensates for or prevails on the other.

Table I. Overview of the family of experiments.

Experiment	Treatments		Replication	University	Subjects
MvR	Manual tests	Randoop tests	I	Univestity of Trento	7 MSc
			II	Univestity of Milano Bicocca	14 BSc + 8 MSc
MvE	Manual tests	EvoSuite tests	I	Univestity of Milano Bicocca	6 Researchers
			II	Fondazione Bruno Kessler	9 Researchers
MvO	Original identifiers	Obfuscated identifiers	I	Univestity of Milano Bicocca	11 MSc studentes

The overview of the experiments is summarized in Table I. Experiment MvR was conducted in two replications: the first one involved 7 MSc students of the University of Trento, attending the "Software Analysis and Testing" course. The second replication involved 14 BSc students of the University of Milano-Bicocca, attending the "Software Analysis and Testing" course, and 8 MSc students of the University of Milano-Bicocca, attending the "Software Quality Control" course. Experiment MvE involved 6 professors/post-docs from University of Milano-Bicocca and 9 researchers/post-docs from Fondazione Bruno Kessler. MvE differs from MvR on both the type of autogen test cases used in the experiment and the skills of the subjects (mostly PhD students and researchers instead of MSc and BSc students). The involvement of skilled subjects allows the assessment of the role of experience and ability on debugging. Experiment MvO was conducted at the University of Milano-Bicocca and involved 11 MSc students, attending the "Software Development Process" course. MSc students from Trento and from Milan share a similar background in computer programming and software engineering, having attended courses with similar content (e.g., Java programming; fundamentals of software engineering; etc.) in previous years at the two universities. The initial results obtained from MvR have been used to tune the design of the experiments MvE and MvO. We report the differences of the empirical setup when applicable. All the subjects involved in the studies have basic skills in Java programming, debugging, and use of the Eclipse IDE[1].

The applications used in the experiment are *JTopas* and *XML-Security*, further characterized in Appendix A. Both applications are available with manually written tests. Moreover, some of their faults are documented in the SIR[2] repository [Do et al. 2005].

JTopas is a customizable tokenizer that tokenizes input text files. It allows users to customize the grammar of the input files by specifying the structure of keywords, compounds and comments, and the case sensitivity. JTopas consists of 15 classes and 4,482 NCLoCs (Non-Comment Lines of Code). *XML-Security* is a library that provides functionalities to sign and verify signatures in XML

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[1] http://www.eclipse.org

[2] http://sir.unl.edu/portal/index.php. Software-artifact Infrastructure Repository.

documents. It supports many mature digital signature and encryption algorithms on standard XML formats, such as XHTML and SOAP. It consists of 228 classes, for a total of 29,255 NCLoCs.

3.1. Hypotheses Formulation

Based on the study definition reported above, we formulate the following null hypotheses to be tested:

> H_{01}. There is no difference in the **effectiveness** of debugging, when debugging is supported by different kinds of test cases.
> H_{02}. There is no difference in the **efficiency** of debugging, when debugging is supported by different kinds of test cases.

However, since different kinds of test cases are considered in different experiments (see Table I), these hypotheses can be broken down as follows:

- Experiment MvR
  - H_{01R}. There is no difference in the **effectiveness** of debugging, when debugging is supported either by manually written or *Randoop test cases*.
  - H_{02R}. There is no difference in the **efficiency** of debugging, when debugging is supported either by manually written or *Randoop test cases*.
- Experiment MvE
  - H_{01E}. There is no difference in the **effectiveness** of debugging, when debugging is supported either by manually written or *EvoSuite test cases*.
  - H_{02E}. There is no difference in the **efficiency** of debugging, when debugging is supported either by manually written or *EvoSuite test cases*.
- Experiment MvO
  - H_{01O}. There is no difference in the **effectiveness** of debugging, when debugging is supported either by manually written or *Obfuscated test cases*.
  - H_{02O}. There is no difference in the **efficiency** of debugging, when debugging is supported either by manually written or *Obfuscated test cases*.

These null hypotheses are *two-tailed* because there is no a-priori knowledge on the expected trend that should favor either manually written or autogen test cases. On the one hand, manual test cases are meaningful for a developer who is determining the position of a fault, while automatic tests may contain meaningless statements and code identifiers that may confuse developers. On the other hand, manual tests could be difficult to understand because they may require understanding of complex parts of the application logic, while automatically generated tests may be simpler, since they are generated without a clear knowledge of the application business logic.

The null hypotheses indicate that we have two *dependent variables*: *debugging effectiveness* and *debugging efficiency*. In experiments MvR, MvE and MvO, we asked subjects to fix eight faults in total (four faults for each subject application).

During their fault fixing activities, developers typically resort to a regression test suite to check whether the code changes introduced to fix the bug have broken any pre-existing functionality. In MvE and MvO, we have provided the developers with the regression test suites available for the object applications, XML-Security and JTopas. To avoid any interference between regression testing and bug localization and correction, we made sure that the regression test suite cannot reveal the faults to be fixed in the experiments.

To determine whether manual and autogen test cases differ according to the identifier understandability and code complexity metrics, we introduce two additional derived null hypotheses:

> DH_{03}. There is no difference in the number of **artificial / user-defined identifiers** of the manually written and autogen test cases used in the experiment.
> DH_{04}. There is no difference in the **static / dynamic complexity** of the manually written and autogen test cases used in the experiment.

Experimental support for the alternative hypotheses associated with DH_{03} and DH_{04} provides useful information for the interpretation of the results about the two dependent variables considered in H_{01} and H_{02}.

We have collected the participants opinions on the performed debugging tasks through survey questionnaires. Answers are on a Likert scale, whose extremes are *strongly agree/disagree* and whose middle point is *uncertain*. We analyze the answers to the survey questionnaires by formulating the following null-hypotheses:

HQx_{05}. Participants are *uncertain* about what stated in question Qx.

HQx_{06}. There is no difference in the answer to question Qx between participants who were supported by different kinds of test cases during debugging.

3.2. Variable Selection

In our experiments, the *effectiveness* of debugging is measured as the number of correctly fixed faults. We evaluated the correctness of the fixes by running a predefined set of test cases that cover the faults in the subject programs (these test cases have not been provided to subjects). If all the test cases passed, we further manually inspected the fixed code to verify whether the fix was correct. The *efficiency* of debugging is evaluated as the number of correct tasks (i.e., the number of correctly fixed faults) divided by the total amount of time spent for these tasks (measured in minutes): $eff = \frac{\sum Corr_i}{\sum Time_i}$; where $Corr_i$ is equal to one if the i-th task is performed correctly, zero otherwise, while $Time_i$ is the time spent to perform the i-th task. In other words, efficiency is measured as the number of correctly performed tasks per minute.

The *independent variables* (the main factors of the experiments) are the treatments applied during the execution of the debugging tasks. The two alternative treatments in the three experiments are: (1) manually written test cases: those distributed as unit tests for the object applications (obtained from the SIR repository); and, (2) either test cases automatically generated by Randoop [Pacheco and Ernst 2007] (MvR experiment), or test cases automatically generated by EvoSuite [Fraser and Arcuri 2011] (MvE experiment), or obfuscated test cases (MvO experiment).

The *understandability* of the test cases that reveal the faults might affect the debugging performance and may vary substantially between manual and autogen test cases. Since we cannot control this factor in the experiments, because it depends on how manual test cases have been defined and how the test case generation algorithms work, we measure this factor in our experimental setting. The test case understandability might, in fact, represent one of the key features in which manual and autogen test cases differ, which could possibly explain some of the observed performance differences.

Unfortunately, there is no easy, widely-accepted way of measuring the understandability of test cases. We approximate such measurement by considering two specific factors of understandability, namely identifier meaningfulness and complexity of the test code. For the former we manually classify each identifier in a test case as either *Artificial* (automatically generated) or *UserDef* (user-defined) and count the respective numbers. In order to measure the test case complexity, we consider both static metrics (*MeLOC* and *McCabe*) and dynamic metrics (*Exec. methods* and *Exec. LOCs*), which provide an approximation of how complex a test case is from the developer's perspective[3]. Metrics are computed using the Eclipse plugin Metrics (http://metrics.sourceforge.net). As static metrics we measure:

- *MeLOC*, number of non-blank and non-comment lines of code inside each method body;
- *McCabe*, cyclomatic complexity of each test method.

As dynamic metrics, we consider the amount of application code exercised by each test case. We count it at two granularity levels:

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[3] Although some of the used metrics are actually size metrics, we regard them as test case complexity indicators, since they reflect the perceived complexity associated with the usage of the test cases during debugging.

— *Exec. methods*, the number of methods executed by a test case;
— *Exec. LOCs*, the number of statements executed by a test case.

3.3. Other factors

We measured the following other factors that could influence the dependent variables:

(1) The subjects' ability;
(2) The subjects' experience;
(3) The object system;
(4) The experiment session; and
(5) The fault to be fixed.

(1) *Subjects' ability:* the ability of subjects in performing debugging tasks was measured using a pre-test questionnaire with questions about programming and debugging ability, experience with the development of large applications, and scores in academic courses related to development and testing. In the case of MvR, where subjects include BSc students, we also exploited the results of a training session where subjects were asked to answer some code-understanding questions and to fix faults in each of the two object applications. According to the answers given to the pre-questionnaire and the results of the training lab, we classified the subjects who participated in MvR into three categories. *High ability* subjects are those who had experience with the development of large applications, they have an academic score of at least 27/30[4] and completed correctly at least 50% of the tasks in the training lab. *Medium ability* subjects are those who either had experience with the development of large applications or have an academic score of at least 27/30, and correctly completed at least 25% of the tasks in the training lab. The rest of the subjects are classified as *low ability* subjects. In MvO, since subjects did not participate to a training session, we use these same definitions of the ability levels, except for the part about the number of correctly completed tasks. Finally, since all the participants to MvE have both experience with large applications and valuable CVs, we do not consider ability levels in MvE (i.e., all subjects involved in MvE are high ability subjects).
(2) *Subjects' experience:* we classified subjects according to four levels of experience: BSc students, MSc students, PhD/Post-docs, and researchers/professors. In MvR we used BSc and MSc students, in MvE we used PhD/Post-docs and researchers/professors, and in MvO we used MSc students only. Thus, this factor is investigated in MvR and MvE, but not in MvO. In addition to the subjects' experience, for PhD/Post-docs and researchers/professors we also considered the subjects' research *field* as a factor.
(3) *Object system (i.e., application):* since we adopted a balanced design with two systems (see Section 3.4), subjects could show different performance on different systems. Hence the system is also a factor.
(4) *Experiment session (i.e., Lab):* We measured whether any learning effect occurred between the two labs (see Section 3.4 for a description of the counter-balanced design that we adopted).
(5) *Fault to be fixed:* since faults are all different (a detailed description of the faults used in our experiments is provided in Appendix A), the specific features of the fault to be fixed may interact with the main factor.

For each factor, we test if there is any effect on the debugging effectiveness and debugging efficiency and we check its interaction with the main factor. We formulate the following null hypotheses on the other factors:

> H_{0c_i}. the factor i, $i = \overline{1..5}$, does not significantly influence effectiveness and efficiency in performing debugging tasks.

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[4] In the Italian academic grade system, a score of 27/30 corresponds to a B in the ECTS grade system and to an A- in the US system.

These null hypotheses are also two-tailed, because we do not have any a-priori knowledge about the direction in which a factor could influence effectiveness and efficiency. Whenever a statistically significant influence exists, we also test the interaction of the factor with the main factor.

3.4. Experimental Design

We adopted a counter-balanced design: each replication of the experiments consists of two experimental sessions (*Lab 1* and *Lab 2*), with 2-hours allocated for each lab. Subjects have been split into four groups, balancing the level of ability and experience in each group. This design ensures that each subject works on the two applications (*JTopas* and *XML-Security*) and with the two different treatments (manual vs autogen test cases), as shown in Table II. Moreover, this design allows us to study the effect of all the factors, using statistical tests.

Table II. Experimental design. A = autogen test cases, M = manually written test cases.

	Group1	Group2	Group3	Group4
Lab 1	JTopas M	XML-Security M	JTopas A	XML-Security A
Lab 2	XML-Security A	JTopas A	XML-Security M	JTopas M

3.5. Experimental Procedure and Material

Before each experiment, we asked the subjects to fill a pre-questionnaire in which we collected information about their ability and experience in programming and testing. BSc students have also been trained with lectures on testing and debugging, and participated in a training laboratory where they were asked to cope with debugging tasks very similar to the experimental tasks. This made us confident that all the subjects, including BSc students, were quite familiar with the development environment and the debugging process. In the case of BSc students, their effectiveness in the training tasks has been used to assess the subjects' level of ability.

To perform the experiment, subjects used a personal computer with the Eclipse development environment equipped with a standard Java debugger. We distributed the following material:

— The application code: depending on the group, either JTopas or XML-Security. The code contains four faults in every repetition of MvR, MvE, and MvO; subjects are said that there are four faults to be debugged and that one fault revealing test case is available per fault;
— Fault revealing test cases: either manually written or autogen, depending on the group the subjects belong to, as shown in Table II. Each test case reveals exactly one fault; faults are supposed to be addressed in order and they are sorted according to their difficulty, from easier to harder to fix. The difficulty of the tasks has been established in a testing session by the authors of this paper. A regression test suite is provided in MvE and MvO, to allow subjects to check for regressions once they have completed the fault fixing task;
— Printed instructions describing the experimental procedure.

Before the experiment, we gave subjects a description of the experimental procedure, but no reference was made to the study hypotheses. The experiment has been carried out according to the following instructions:

(1) Import the application code into Eclipse;
(2) For each fault revealing test case, (i) mark the start time; (ii) run the test case and use it to debug the application and fix the fault; (iii) mark the stop time;
(3) Create an archive containing the modified source code and send it to the experimenter by email;
(4) Fill a post-experiment survey questionnaire.

During the experiment, teaching assistants were present in the laboratory to prevent collaboration among subjects and to verify that the experimental procedure was respected – in particular that faults were addressed in the right order and that time was correctly marked.

After the experiment, subjects have been asked to fill a post-experiment survey questionnaire on the subjects' behavior during the experiment, so as to find justification for the quantitative observations. The questionnaire we used for MvR consists of 17 questions related to:

Q1:. Adequacy of the time given to complete the tasks;
Q2:. Clarity of tasks;
Q3-4:. Difficulties experienced in understanding the source code of the application and the source code of the test cases;
Q5:. Difficulties in understanding the features under test;
Q6:. Difficulties in identifying the portion of code to change;
Q7-8:. Use and usefulness of the Eclipse debugging environment;
Q9:. Number of executions of the test case;
Q10-11:. Percentage of total time spent looking at the code of the test cases and of the application;
Q12:. Difficulties in using the test cases for debugging;
Q13:. Fixes of bugs achieved without fully understanding the bugs, relying just on test cases;
Q14:. Need for inspecting the application code to understand bugs;
Q15:. Perceived level of redundancy in test cases;
Q16:. Usefulness of local variables in test cases to understand the test;
Q17:. Test cases being misleading (they initially drove the subject to wrong paths in locating faults).

Answers are given on the following five-level Likert scale [Oppenheim 1992]: *strongly agree, agree, uncertain, disagree, strongly disagree*.

After the experiment MvR, we recognized that several questions have not been useful, especially considering that the answers were formulated in a Likert scale. For the studies MvE and MvO, we thus formulated a new post-questionnaire that includes open questions, to address the issues with the Likert scale. The new questionnaire still includes questions Q1, Q2, Q3, Q4, Q7 and Q8 from the previous questionnaire, but it also includes two open questions:

OQ1:. Requesting a description of the main challenges faced during debugging;
OQ2:. Requesting a description of the debugging process that has been followed.

In the case of MvE, since subjects consist of PhD students, post-docs, researchers and professors, we also added two questions aimed at determining whether the subjects had already some experience with the object applications (question *SQ1*) and with the SIR faults (question *SQ2*).

3.6. Seeded Faults

We seeded faults that satisfy the following requirements. First, faults are located in different parts of the object applications. Second, each fault is revealed by a manual and an autogen test case. Third, faults do not interact with each other, i.e., each test case reveals a single fault. Finally, faults are based on the bugs available in the Software-artifact Infrastructure Repository (SIR)[5].

In MvR, eight faults have been seeded into the two applications for debugging. In order to meet the four requirements, since the autogen test cases are not able to reveal some of the SIR faults, we slightly changed these faults so that they can be detected. However, such changes do not modify the nature of the faults. An example of such changes, together with a thorough description and characterisation of the faults used in the empirical study, can be found in Appendix A.2.

In MvE, among the eight seeded faults, six have been generated by the mutation tool Jester[6], while two are in common with MvR (the other six faults used in MvR could not be revealed by any EvoSuite test, so we have replaced them). A characterisation and detailed description of the six mutants used in experiment MvE can be found in Appendix A.2.

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[5] http://sir.unl.edu
[6] http://jester.sourceforge.net.

3.7. Analysis Method

General linear model (GLM) incorporates a number of different statistical models: ANOVA, ANCOVA, MANOVA, MANCOVA, ordinary linear regression, t-test and F-test. To test the effectiveness and efficiency of subjects in performing debugging tasks (H_{01} and H_{02}) we used a general linear model. This consists of fitting a model of the *dependent* output variables (effectiveness and efficiency of debugging) as a function of the *independent* input variables (all the factors, including the main factor, i.e., the kind of test cases). A general linear model allows to test the statistical significance of the influence of all factors on the effectiveness and efficiency of debugging (H_{01}, H_{02}, H_{0c_1}, H_{0c_2}, H_{0c_3}). We assume significance at 95% confidence level (α=0.05), so we reject the null-hypotheses having p-value<0.05. In case of relevant factors, the interpretations are formulated by visualizing interaction plots.

In case we can not reject the null hypothesis, we risk to commit a type-II error, i.e., accepting a null hypothesis that is actually false. We can estimate the probability of committing a type-II error as $1 - Power$, where $Power$ is the statistical power of the adopted general linear model.

We used a non-parametric statistical test, the Wilcoxon two-tailed paired test [Wohlin et al. 2012], to address the derived null hypotheses DH_{03} and DH_{04}. The use of non-parametric tests does not require any assumption on the normal distribution of the population. Such a test checks whether differences between different types of test cases in terms of (i) the number of artificial/user-defined identifiers and (ii) difference in the static/dynamic complexity are statistically significant.

In order to understand whether the test case complexity is a property that characterizes the main treatments (manual vs. autogen test cases), we measured the goodness of the test case complexity metrics as predictors of the treatment. Specifically, we computed the confusion matrix where each test case complexity metric is a possible predictor and the binary classification between manual and autogen test cases is the predicted factor. Standard classification metrics (number of true/false positives/negatives) and derived metrics (precision, recall, accuracy, and F-measure) are then used to assess the degree to which manual and autogen test cases can be separated using the test case complexity as the distinguishing feature. Specifically, correct classifications are indicated as TP (true positives, i.e., correctly classified as autogen) and TN (true negatives, i.e., correctly classified as non-autogen), while errors are of two types: FP (false positives, i.e., manual test cases classified as autogen) and FN (false negatives, i.e., autogen test cases classified as non-autogen). The four derived metrics [van Rijsbergen 1979] are defined as follows: *precision* = TP / (TP + FP); *recall* = TP / (TP + FN); *accuracy* = (TP + TN) / (TP + FP + TN + FN); *F-measure* = 2 *precision* * *recall* / (*precision* + *recall*).

Regarding the analysis of the survey questionnaires, we evaluate the questions related to time availability, general difficulties found by subjects, and the use of the debugging environment (Q1-Q8) by verifying whether the answers are either “Strongly agree” (2) or “Agree” (1). We test medians, using a two-tailedMann-Whitney test for the null hypothesis HQx_{05}, i.e., $\widetilde{Qx} = 0$, where zero corresponds to “Uncertain”, and $\widetilde{Qx}$ is the median for question Qx. The same test is also performed for questions SQ1 and SQ2.

Among these questions, for those specific to test cases (Q4-Q8), the answers of the subjects using manually written tests are compared to the answers of the subjects using autogen tests. In this case a two-tailed Mann-Whitney test is used for the null hypothesis HQx_{06}, i.e., $\widetilde{Q}_{autogen} = \widetilde{Q}_{Manual}$. The same comparison is also performed for the questions Q9-Q17.

To analyze the answers to the open questions, we adopted the following process. We summarized the answers given by the subjects who worked with different treatments, and we compared them to look for commonalities and differences. We grouped similar answers into common concepts and we discarded non-confirmed observations, that is those that were reported by just one subject. Eventually we compared the concepts emerged from answers formulated by subjects who worked with different treatments.

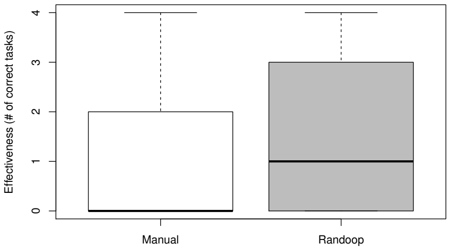
4. RESULTS OF MANUAL VS. RANDOOP [MVR]

This experiment compares manual and Randoop test cases [Pacheco and Ernst 2007]. It was replicated twice, the first time with 7 master students from University of Trento, and the second with 8 master and 14 bachelor students from University of Milano-Bicocca.

The data used in this section have been already reported in our previous conference paper [Ceccato et al. 2012]. They are provided also in this paper for completeness and to facilitate the comparison with the results of the experiments presented in the next two sections. Moreover, in this section we carry out a different and more detailed analysis as compared to the conference paper.

4.1. Debugging Effectiveness

Figure 1 (top) shows the boxplot of the effectiveness in fault fixing. The figure compares the number of correct answers given by the subjects when the faults are debugged using either manually written or randomly generated test cases. Figure 1 (bottom) shows descriptive statistics (mean, median and standard deviation) of effectiveness for the two distinct factors.



	Mean	Median	Sd
Manual tests	0.89	0.00	1.31
Randoop tests	1.65	1.00	1.50

Fig. 1. Boxplots and descriptive statistics for Effectiveness (Manual vs. Randoop): debugging was significantly more effective when Randoop test cases are used.

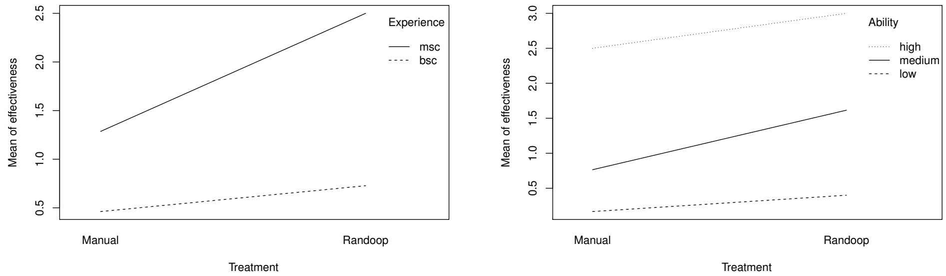
Table III. GLM analysis of Effectiveness (Manual vs. Randoop); p-values in bold face indicate a statistically significant influence on Effectiveness.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	-0.8222	0.2448	-3.36	0.0016
Treatment	0.1679	0.0795	2.11	**0.0405**
System	-0.0325	0.0799	-0.41	0.6860
Lab	0.1075	0.0819	1.31	0.1962
Experience	0.2246	0.0837	2.68	**0.0102**
Ability	0.2258	0.0676	3.34	**0.0017**

Table III reports the analysis of effectiveness of debugging with GLM. The model takes into account not only the effect of the main treatment (manual or autogen test cases) but all the factors that we considered in our experimental design, i.e., the system, the lab, the experience and ability of participants. Statistically significant cases are in boldface. We can observe that subjects who used autogen tests showed better effectiveness (i.e., correctly fixed more faults) than subjects who used manually written tests. Data confirm the trend with significance at 95% confidence level ($\alpha = 0.05$).

Thus we reject H_{01R} and conclude that *The debugging effectiveness is higher when debugging is supported by random tests than manually written tests*.

From Table III we can also understand the role of the other factors in influencing the main factor. Let us first consider *Ability* (*high*, *medium* or *low*) and *Experience* (*BSc* or *MSc* student). We can notice that both Ability and Experience have a significant effect. From the interaction plots reported in Figure 2, we can notice that the high ability and high experience subjects are associated with a line substantially higher than the line for the low ability/experience subjects. For what concerns experience (see Figure 2 (a)), we can notice that lines are not parallel and tend to diverge, instead. This indicates some interaction between experience and main treatments. Indeed, high experience subjects improve their performance when using autogen tests much more than lower experience subjects do. In other words, subjects with high experience are better at taking advantage of the higher effectiveness provided by autogen tests.



(a) Experience

(b) Ability

Fig. 2. Interaction plot of Effectiveness between Treatment (Manual vs. Randoop) & Experience/Ability; diverging/converging lines indicate potential interactions. High experience subjects exhibit higher effectiveness increase when using Randoop test cases as compared to low experience subjects.

In Table III, we can notice that *System* and *Lab* are not significant factors, thus there is no effect of the system and no learning effect between the two experimental sessions.

Finally, we analyze the role of *Fault* as a factor, to see if the faults influenced the result or interacted with the main factor (manual vs. autogen tests) to influence the result. We cannot study the impact of this factor on effectiveness, as the latter is a metrics over all faults, while we are interested in each fault individually. So we resort to $Corr_i$, which measures the correctness of the fix for each i-th fault.

The analysis is performed separately for the two systems (JTopas and XML-Security) because faults are different. Results of the analysis with GLM by Treatment and Fault on *Correctness* do not reveal any statistically significant influence of the faults on the effectiveness of debugging (see Table IV).

Table IV. GLM analysis of Correctness by Treatment and Fault (Manual vs. Randoop); p-values in bold face indicate a statistically significant influence on Correctness

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.4699	0.1549	3.03	0.0029
Treatment	0.0675	0.0830	0.81	0.4172
Fault	-0.0617	0.0383	-1.61	0.1089

(a) Jtopas

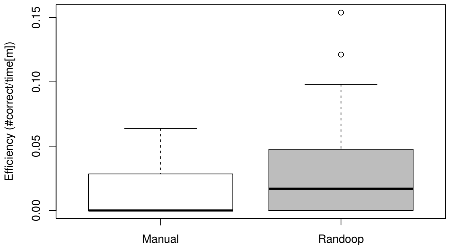
	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	-0.0247	0.1450	-0.17	0.8650
Treatment	0.2710	0.0812	3.34	**0.0011**
Fault	0.0052	0.0379	0.14	0.8915

(b) Xml-Security

4.2. Debugging Efficiency

The same procedure used with effectiveness was also applied to efficiency. Figure 3 (top) shows the boxplot for efficiency with the two treatments. It compares the efficiency of the subjects when the faults are debugged using either manually written or randomly generated test cases. The corresponding descriptive statistics are reported in Figure 3 (bottom).

Table V reports the analysis with GLM. The trend shown for effectiveness is confirmed here: the efficiency of subjects working with autogen tests is higher than when working with manually written tests. Thus we reject H_{02R} and conclude that *The efficiency of debugging is higher when debugging is supported by random tests than manually written tests*.



	Mean	Median	Sd
Manual tests	0.01	0.00	0.02
Randoop tests	0.03	0.02	0.04

Fig. 3. Boxplots and descriptive statistics for Efficiency (Manual vs. Randoop): debugging was significantly more efficient when Randoop test cases are used.

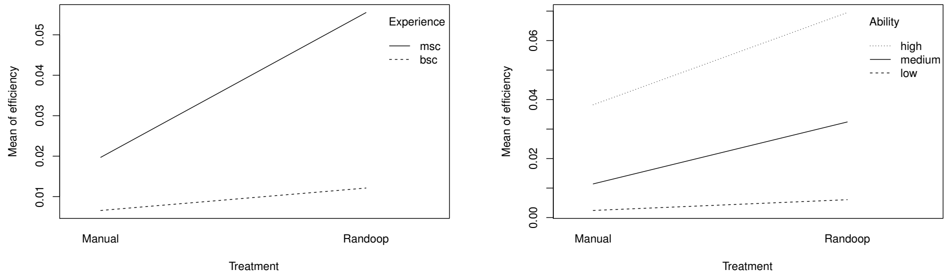
Table V. GLM analysis of Efficiency (Manual vs. Randoop); p-values in bold face indicate a statistically significant influence on Efficiency.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	-0.0924	0.0240	-3.85	0.0004
Treatment	0.0200	0.0078	2.57	**0.0135**
System	0.0046	0.0078	0.59	0.5587
Lab	0.0100	0.0080	1.24	0.2200
Experience	0.0195	0.0082	2.38	**0.0219**
Ability	0.0182	0.0066	2.74	**0.0087**

From Table V, we can also understand the role of the other factors. Both Ability and Experience have a significant effect and interact with the main treatment. The interaction plots in Figure 4 indicate a similar effect as for the effectiveness: higher ability/experience subjects are particularly good at taking advantage of the higher efficiency associated with the use of autogen tests.

Factors *System* and *Lab* do not have a significant influence on efficiency of debugging.

Since we cannot study the impact of the *Fault* factor on efficiency, as the latter is a metrics over all faults, while we are interested in each fault individually, we resort to $Time_i$, which is the time taken to produce the fix for the i-th fault. The analysis is performed separately for the two systems (JTopas and XML-Security) because faults are different. Results of the analysis with GLM by Treatment and Fault on *Time* are significant for XML-Security, while they are close to significance, but not significant (at level 0.05), for JTopas (see Table VI). Faults influence efficiency and, as apparent from the interaction plot in Figure 5, they also interact with the main factor to



(a) Experience

(b) Ability

Fig. 4. Interaction plot of Efficiency between Treatment (Manual vs. Randoop) & Experience/Ability; diverging/converging lines indicate potential interactions. High experience subjects exhibit higher efficiency increase when using Randoop test cases as compared to low experience subjects.

influence Time. In fact, fault 1 is quite different from faults 2-4, since with fault 1 manual and random tests are equally (in-)efficient, resulting in the highest mean fixing time, while for the other faults random tests support a more efficient debugging activity. We conclude that for faults (as fault 1) that are particularly difficult to debug, the choice between manual and random tests does not affect efficiency, while for normal faults (the majority of XML-Security faults, i.e., faults 2-4), random tests are preferable.

Table VI. GLM analysis of Time by Treatment (Manual vs. Randoop) and Fault; p-values in bold face indicate a statistically significant influence on Time.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	36.8888	6.6480	5.55	0.0000
Treatment	-5.3017	3.6000	-1.47	0.1431
Fault	-3.0500	1.6777	-1.82	0.0712

(a) Jtopas

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	50.6453	6.3140	8.02	0.0000
Treatment	-6.3437	3.4764	-1.82	0.0704
Fault	-9.1720	1.6638	-5.51	**0.0000**

(b) Xml-Security

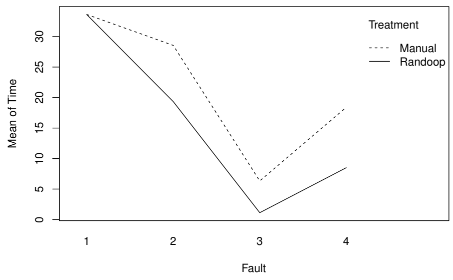


Fig. 5. Interaction plot of Time between Treatment (Manual vs. Randoop) & Fault for Xml-Security. Fault 1 requires a similar amount of fixing time for both treatments, while for the other faults manual tests require more time than Randoop.

4.3. Test Case Understandability

Table VII shows the occurrence of artificially generated and user-defined identifiers in autogen and in manual test cases. User-defined identifiers are of course present also in autogen test cases, for instance due to names of classes instantiated or methods called in the test cases. Artificial identifiers may be present in manual test cases as well, for instance when code generation tools (e.g., tools for parser generation from grammars) are used. This never happens in our two case studies.

Each random test case has on average 28 artificial identifiers and the difference with manual test cases (having no artificial identifiers) is statistically significant according to the Wilcoxon paired test. Random tests have on average 15 user-defined identifiers less than the corresponding manual tests. This difference is not statistically significant (it would be significant at level 0.10).

In summary, the number of artificial (meaningless) identifiers in random test cases is substantially higher than in manual test cases and the number of user-defined (meaningful) identifiers substantially smaller. The difference in the identifiers does not explain the observed difference in effectiveness and efficiency, which goes in the opposite direction: random tests yield superior performance.

Table VII. Occurrences of artificial/user-defined identifiers in the test cases (Manual vs. Randoop)

	Random tests		Manual tests	
	Artificial IDs	UserDef IDs	Artificial IDs	UserDef IDs
		JTopas		
T1	20	4	0	36
T2	18	9	0	59
T3	19	8	0	26
T4	61	22	0	16
		XML-Security		
T1	7	3	0	9
T2	63	27	0	18
T3	13	5	0	20
T4	23	7	0	21

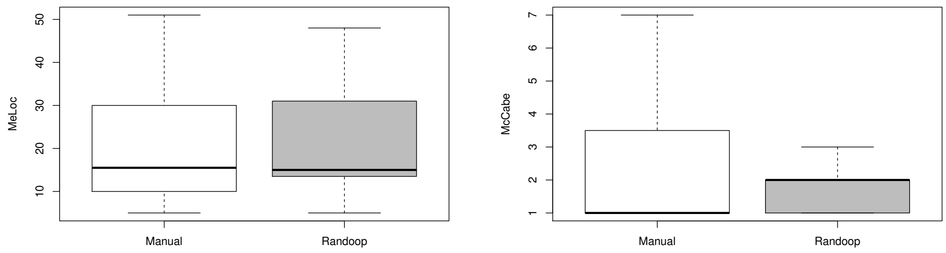
Table VIII. Descriptive statistics and paired analysis (Wilcoxon's test) of static (top) and dynamic (bottom) test case metrics (Manual vs. Randoop): Manual tests are substantially more complex than Random tests according to the dynamic metrics Methods and LOCs.

Metric	N	Randoop.mean	Randoop.sd	Manual.mean	Manual.sd	diff.mean	diff.median	diff.sd	p.value
MeLoc	8	21.50	14.98	20.88	15.42	0.62	0.50	21.12	1.00
McCabe	8	1.75	0.71	2.38	2.20	-0.62	0.00	2.07	0.59
Methods	8	28.38	15.50	119.75	116.97	-91.38	-65.00	120.77	**0.04**
LOCs	8	117.00	65.39	676.38	707.53	-559.38	-416.50	725.33	**0.04**

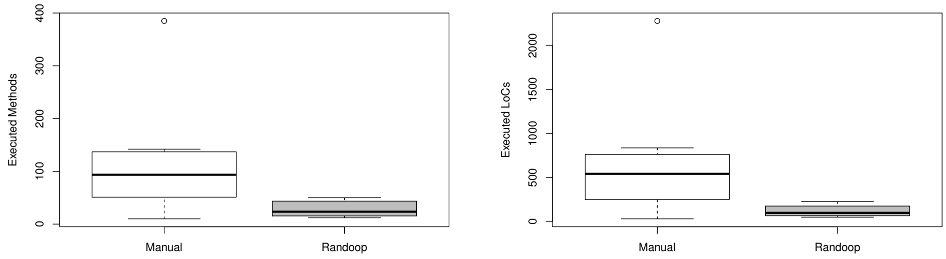
Figure 6 shows the boxplots of the four complexity metrics, for manual and autogen test cases. While the two types of tests are quite similar with respect to *MeLoc* complexity, manual tests have slightly higher *McCabe* complexity than random tests. However, none of these two metrics differ by a statistically significant amount. The difference between test cases is more pronounced when considering dynamic complexity metrics. Manual test cases are more complex than random tests both in *Executed Methods* and *Executed LoCs*.

The mean number of methods (119.75) and LOCs (676.38) executed by manual test cases (see Table VIII) is substantially higher than the number of methods (28.38) and LOCs (117.00) executed by random tests. The ratio is the order of two for JTopas, while it is even higher for XML-Security (reaching an order of magnitude when LOCs are considered). The difference between manual and random test cases is statistically significant at 95% confidence level, so we can reject the null hypothesis DH_{04} (with respect to dynamic metrics).

We also computed the confusion matrix (see Table IX) associated with a nearest neighbor classifier that predicts the test case type based on one of the two dynamic complexity metrics (either the



(a) MeLoc (b) McCabe



(c) Executed Methods (d) Executed LoCs

Fig. 6. Boxplots of test case size and complexity (Manual vs. Randoop). The figures suggest that Manual and Randoop are comparable in terms of MeLoc and McCabe. With respect to executed methods and LoCs, Manual is more complex.

Table IX. Nearest neighbor classifier prediction of the test case type (Manual vs. Randoop) based on executed methods or LOCs.

Metric	Randoop		Manual	
	TP	FN	FP	TN
Exec. Methods	6	2	2	6
Exec. LOCs	8	0	2	6

executed methods or LOCs). The predictor classifies a new test case by determining the available test case having the closest dynamic complexity metrics value and assigning it to the class (autogen or manual) of such closest test case [Cover and Hart 1967]. The confusion matrix was obtained by applying 1-fold cross validation.

Table X. Prediction performance metrics for the Nearest Neighbor classifier using executed methods or LOCs; the test case type is Manual or Randoop.

Metric	Precision	Recall	Accuracy	F.measure
Exec. Methods	0.75	0.75	0.75	0.75
Exec. LOCs	0.80	1.00	0.88	0.89

Executed LOCs is a better predictor than executed methods. The values reported in Table X (bottom) for this predictor are quite close to one, showing that in our experiment it is possible to predict the type of a test case from its dynamic complexity metrics (specifically, executed LOCs) with high accuracy. This means that the autogen and manual test cases used in the experiment can be characterized with a high accuracy respectively as *low dynamic complexity* and *high dynamic complexity* test cases.

In summary, manual test cases are dynamically more complex than random test cases. This might explain the observed performance degradation exhibited by subjects working with manual test cases, despite the presence of more meaningful identifiers in these test cases.

4.4. Analysis of Post Questionnaire

We used the answers to the questions from Q1 to Q8 to gain insights on the subjects' activity. Results are summarized in Table XI. Considering data over all the replications, answers to questions Q2, Q4, Q7 and Q8 produced statistically significant results (p-value < 0.05), while answers to the other questions are not statistically significant. Subjects found the tasks to be clear ($\widetilde{Q2}$ = 1, i.e., "agree", with p-value $<$0.01) and, overall, they had no difficulty in understanding the source code of the test cases (Q4). The debugger was used (Q7) only by high ability subjects, although it was judged useful (Q8) by all.

Table XI. Analysis of post questions Q1-Q8. Mann-Whitney test for the null hypothesis $median(Qx) = 0$ (Manual vs. Randoop); p-values in bold face indicate that the result is statistically significant at level 0.05.

Question	Low ability		High ability		All	
	median	p.value	median	p.value	median	p.value
Q1: Enough time	not certain	0.59	agree	**0.01**	not certain	0.10
Q2: Tasks clear	not certain	0.05	strongly agree	**0.01**	agree	<**0.01**
Q3: No difficulty in understand application code	not certain	0.37	not certain	0.23	not certain	0.98
Q4: No difficulty in understand test code	not certain	1.00	not certain	1.00	agree	**0.03**
Q5: Easily understand the feature under test	not certain	0.85	not certain	0.37	not certain	0.30
Q6: No difficulty in identifying where to fix	not certain	0.19	not certain	0.07	not certain	0.32
Q7: Used the Eclipse debugger	not certain	0.30	strongly agree	**0.01**	agree	**0.01**
Q8: Debugger was useful	agree	0.41	agree	**0.01**	agree	<**0.01**

Then, we compared the answers for the questions specific to test cases (Q9 to Q17), to understand if any statistical difference can be observed between subjects who worked with manually written test cases and those who used autogen ones. The unpaired Mann-Whitney's test never reported statistical significance, so we omit the table (it can be found in the technical report [Ceccato et al. 2013]).

Let us now analyze the differences between the answers given by the low and by the high ability subjects (see Table XI, columns 2-3, 4-5). According to the post questionnaire, there is a remarkable difference in the use of the Eclipse debugger (Q7) between the low and high ability subjects, in that only the latter declare to have used it extensively. This might be part of the explanation for the gap between manual and random test cases observed in both effectiveness and efficiency (see Figures 1 and 3). Without the debugger, low ability subjects could take advantage only of simple test cases (i.e., those generated by Randoop), while they could not manage the complexity of most manual test cases, resulting in lower performance in the latter case. On the contrary, high ability subjects, who used the debugger more extensively, were able to take advantage also of the complex test scenarios. Of course, they also had better performance with the simpler, random tests.

Differently from the low ability subjects, high ability subjects considered the debugger useful (Q8), which is consistent with the extensive use of the Eclipse debugger (Q7), reported only by the high ability subjects. Another difference is that time was regarded as sufficient to complete the debugging task (Q1) and tasks were regarded as clear (Q2) by high ability subjects, while this was not the case for low ability subjects.

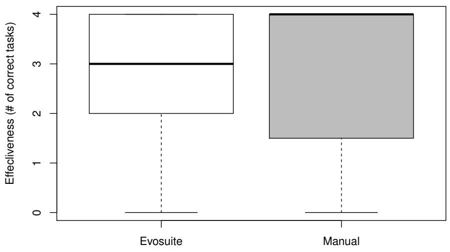
5. RESULTS OF MANUAL VS. EVOSUITE [MVE]

This experiment compares manual and autogen test cases, but considers a different test case generation algorithm (i.e., EvoSuite [Fraser and Arcuri 2011]) and subjects with higher experience. It was also replicated twice, the first time with six professors/post-docs from University of Milano-Bicocca and the second time with nine researchers/post-docs from Fondazione Bruno Kessler. Participants were asked to locate and fix faults, supported by (i) manually written test cases, or (ii) test cases generated by EvoSuite.

5.1. Debugging Effectiveness

Figure 7 shows the boxplots of the effectiveness in fault fixing. The median of the effectiveness when debugging with EvoSuite tests is lower than when manual tests are used, but the overall distribution of effectiveness is very similar. Figure 7 (bottom) reports the corresponding descriptive statistics.

Table XII reports the analysis with GLM to study the influence of the main factor (i.e. the treatment) and of the other factors on the effectiveness of debugging. The statistical test reports a p-value>0.05. Thus, we cannot reject the null hypothesis H_{01E}, stating that *There is no difference in the effectiveness of debugging when debugging is supported either by manually written or EvoSuite test cases*. The probability of a type II error (accepting a false null hypothesis), obtained from GLM power analysis, is 1%.



	Mean	Median	Sd
Manual tests	2.73	4.00	1.58
EvoSuite tests	2.73	3.00	1.28

Fig. 7. Boxplots and descriptive statistics for Effectiveness (Manual vs. EvoSuite): debugging was equally effective with Manual and EvoSuite test cases.

Table XII. GLM analysis of Effectiveness (Manual vs. EvoSuite); p-values in bold face indicate a statistically significant influence on Effectiveness.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.4103	0.3845	3.67	0.0012
Treatment	-0.0264	0.1128	-0.23	0.8167
Experience	-0.1538	0.1279	-1.20	0.2408
Field	0.0962	0.1279	0.75	0.4595
System	-0.4014	0.1128	-3.56	**0.0016**
Lab	0.0048	0.1128	0.04	0.9664

Table XII also reveals the role of the factors in influencing the dependent variable, i.e., the debugging effectiveness.

As first factor we studied if the *Experience* of participants (PhD student/post-doc or researchers/professors) influenced the results. Differently from the MvR experiment, we can notice that experience did not influence the effectiveness of debugging. Then, we considered if the research *Field* of participants (software testing or a different field) influenced the results. Also in this case, the large p-values bring us to accept the null hypothesis (non-influence of the research field).

We considered if the particular *System* used in the experimental sessions (JTopas or XML-Security) influenced the result. Differently from the MvR experiment, we can notice that the subject system had a significant effect in influencing the effectiveness (p-value$<$0.05). Figure 8 shows the interaction plot of Effectiveness between Treatment and System. We can notice that when working on *XML-Security* participants had, on average, a lower effectiveness than when working on *JTopas*, both with manual tests and with EvoSuite tests. Moreover, such gap is amplified when manual tests are used, hence indicating a level of interaction with the main factor. XML-Security was more difficult to debug and the associated difficulty was further increased when manual tests were available for the debugging task.

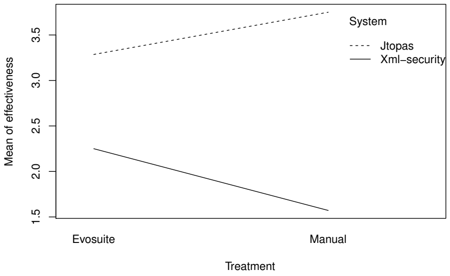


Fig. 8. Interaction plot of Effectiveness between Treatment (Manual vs. EvoSuite) & System; diverging/converging lines indicate potential interactions. XML-Security is more difficult to debug than Jtopas and it becomes even more difficult when Manual test cases are used.

We analyzed the learning effect by studying the *Lab* factor. We can notice (see Table XII) that the lab did not influence the effectiveness. The last factor that we consider is the *Fault*. We analyze whether the faults influenced the result and whether they interacted with the main factor to influence the result. We adopt the same analysis procedure that was applied for the MvR experiment (i.e., GLM for $Corr_i$ for each i-th fault).

Table XIII reports the results of GLM for Correctness by Treatment and Fault. There is a statistically significant influence of Fault only for XML-Security.

Table XIII. GLM analysis of Correctness by Treatment and Fault (Manual vs. EvoSuite); p-values in bold face indicate a statistically significant influence on Correctness

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.7887	0.1632	4.83	0.0000
Treatment	0.1161	0.0832	1.39	0.1686
Fault	-0.0333	0.0371	-0.90	0.3733

(a) Jtopas

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.0155	0.2412	4.21	0.0001
Treatment	-0.1696	0.1264	-1.34	0.1847
Fault	-0.1133	0.0564	-2.01	**0.0492**

(b) Xml-Security

By looking at the interaction plot shown in Figure 9, we can notice that overall, the use of autogen tests increases the proportion of correctly executed debugging tasks, as compared to the use of manual tests, but such improvement is fault specific. For Fault 1 the improvement is marginal; for Faults 2-3 there is a remarkable improvement; Fault 4 is a case where the difference between autogen and manual tests is substantial. On Fault 4 the mean correctness of debugging is more than doubled when autogen tests are used.

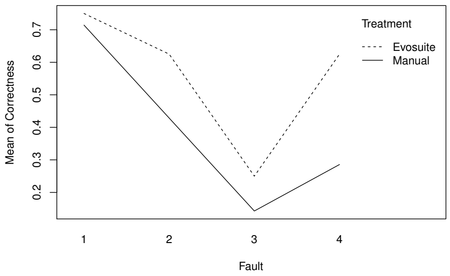


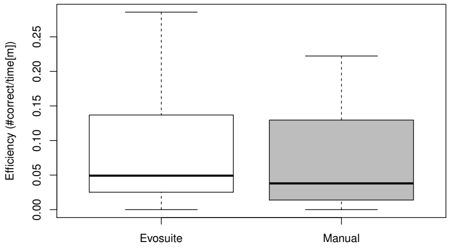
Fig. 9. Interaction plot of Correctness between Treatment (Manual vs. EvoSuite) & Fault on XML-Security; diverging/-converging lines indicate potential interactions.

5.2. Debugging Efficiency

A similar procedure was used to study the efficiency of debugging. Figure 10 shows the boxplot of the efficiency with the two alternative treatments. The picture reveals no clear trend. The efficiency with EvoSuite is on average higher than with manual tests, but the distribution is similar.

Table XIV shows the result of the analysis with GLM. Statistical significance is not reached (p-value>0.05), so we cannot reject the null hypothesis H_{02E}, stating that *There is no difference in the efficiency of debugging when debugging is supported either by manual or EvoSuite test cases*. The probability of a type II error (accepting a false null hypothesis), obtained from GLM power analysis, is 1%.

In summary, EvoSuite test cases are as good as manual test cases in supporting debugging in terms of both effectiveness and efficiency.



	Mean	Median	Sd
Manual tests	0.07	0.04	0.07
EvoSuite tests	0.10	0.05	0.10

Fig. 10. Boxplots and descriptive statistics for Efficiency (Manual vs. EvoSuite): debugging was equally efficient with Manual and EvoSuite test cases

Table XIV. GLM analysis of Efficiency (Manual vs. EvoSuite); p-values in bold face indicate a statistically significant influence on Efficiency.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.2978	0.0810	3.68	0.0012
Treatment	-0.0294	0.0238	-1.24	0.2273
Experience	-0.0025	0.0269	-0.09	0.9269
Field	-0.0096	0.0269	-0.36	0.7252
System	-0.1208	0.0238	-5.08	**0.0000**
Lab	0.0213	0.0238	0.90	0.3789

From Table XIV, we can see that factors *Experience*, *Field* and *Lab* did not influence the results. As with effectiveness, we can notice that the factor *System* has a significant effect in influencing the efficiency (p-value<0.05). Figure 11 shows the interaction plot of Efficiency between Treatment and System. Similarly to the interaction plot for the effectiveness (see Figure 8), when working on *XML-Security* participants had, on average, a lower efficiency than when working on *JTopas*, both with manual tests and with EvoSuite tests. Moreover, the availability of manual tests further reduces the debugging efficiency on both Jtopas and XML-Security, hence revealing some interaction with the main treatment.

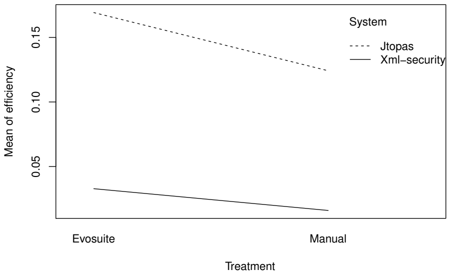


Fig. 11. Interaction plot of Efficiency between Treatment (Manual vs. EvoSuite) & System; diverging/converging lines indicate potential interactions. XML-Security is more difficult to debug than Jtopas; both become more difficult to debug when Manual test cases are used.

For the factor *Fault*, we apply GLM to estimate $Time_i$ for each i-th fault. Table XV reports the results of GLM for Time by Treatment and Fault. No statistically significant influence of Fault on Efficiency was reported by the statistical test.

Table XV. GLM analysis of Time by Treatment (Manual vs. EvoSuite) and Fault; p-values in bold face indicate a statistically significant influence on Time.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	6.7574	5.5761	1.21	0.2306
Treatment	1.8973	2.8439	0.67	0.5074
Fault	-0.3333	1.2690	-0.26	0.7937

(a) Jtopas

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	17.3869	6.4545	2.69	0.0093
Treatment	7.2589	3.3810	2.15	**0.0361**
Fault	-2.3333	1.5087	-1.55	0.1275

(b) Xml-Security

5.3. Test Case Understandability

Table XVI reports the number of artificial/user defined identifiers in EvoSuite test cases (second and third columns) and in manual test cases (fourth and fifth columns). This table differs from Table VII because Randoop and EvoSuite reveal different faults and correspondingly different manual test cases are used in experiments MvR and MvE. No artificial identifiers are present in manual test cases, while EvoSuite tests contain both artificial and user-defined identifiers.

Table XVII reports the results of the Wilcoxon test on the identifiers. EvoSuite test cases have on average 3 artificial identifiers and manual tests have on average 12 user-defined identifiers more than EvoSuite tests. This difference is statistically significant according to the results of the Wilcoxon test (p-value<0.05). So we can reject the null-hypothesis DH_{03E} and we can formulate the alternative hypothesis that *the number of meaningless (artificial) identifiers in EvoSuite tests is higher than in manual tests and the number of meaningful (user-defined) identifiers is smaller*.

Table XVI. Occurrences of artificial/user-defined identifiers in the test cases (Manual vs. EvoSuite)

	EvoSuite tests		Manual tests	
	Artificial ID	UserDef ID	Artificial ID	UserDef ID
		JTopas		
T1	2	5	0	27
T2	3	7	0	18
T3	3	6	0	15
T4	2	5	0	18
		XML-Security		
T1	4	10	0	26
T2	3	8	0	27
T3	3	9	0	9
T4	1	6	0	12

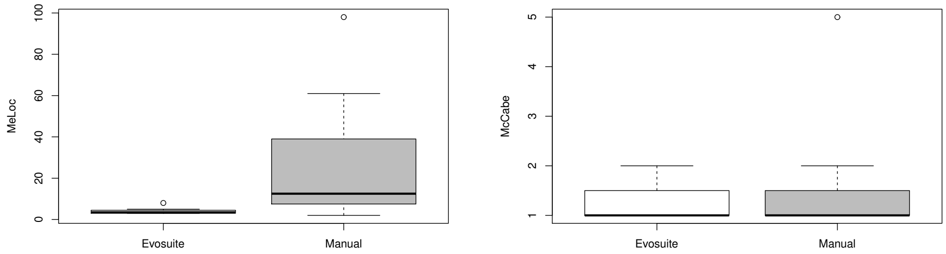
Table XVII. Paired (Manual vs. EvoSuite) analysis of artificial/user-defined identifiers (Wilcoxon's test); p-values in bold face indicate a statistically significant difference between Manual and EvoSuite tests.

ID type	N	Man.mean	Man.sd	Evo.mean	Evo.sd	diff.mean	diff.median	diff.sd	p.value
Artificial ID	8	0.00	0.00	2.62	0.92	-2.62	-3.00	0.92	**0.01**
UserDef ID	8	19.00	7.01	7.00	1.85	12.00	12.00	7.13	**0.02**

Figure 12 compares the complexity metrics computed on EvoSuite and manual test cases. While the two types of tests are very similar with respect to *McCabe* complexity, manual tests have higher *MeLoc*. Manual tests have also higher dynamic complexity than EvoSuite tests, in terms of both *Executed Methods* and *Executed LoCs*.

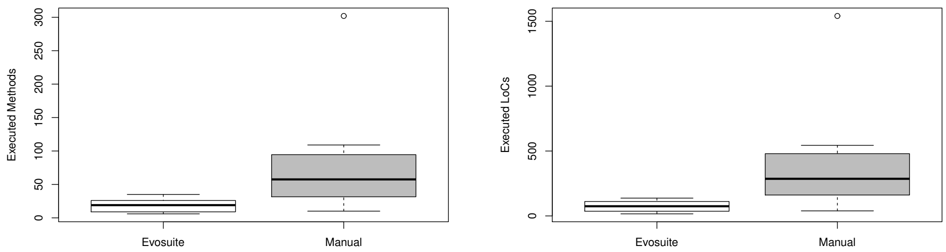
These trends are confirmed by the results of the Wilcoxon test reported in Table XVIII. While there is no statistically significant difference on *McCabe* complexity, differences in all the other (static and dynamic metrics) are statistically significant. So we can reject the null-hypothesis DH_{04E} and formulate the alternative hypothesis that *static (MeLoc) and dynamic complexity of manually written test cases is significantly higher than in EvoSuite test cases.*

In summary, as observed in the case of Randoop test cases, manual tests on the one hand contain a higher number of meaningful identifiers than EvoSuite tests. On the other hand, manual tests are more complex than EvoSuite test cases. Differently from the MvR experiment, in this experiment the lower complexity of EvoSuite test cases is not associated with a higher effectiveness or efficiency of debugging. At the same time, the reduced understandability of EvoSuite tests does not make them less effective than manual test cases during debugging, in terms of effectiveness and efficiency.



(a) MeLoc

(b) McCabe



(c) Executed Methods

(d) Executed LoCs

Fig. 12. Boxplots of test case size and complexity (Manual vs. EvoSuite). The two types of tests are comparable with respect to McCabe complexity. Manual tests have higher MeLoc, Executed Methods, and Executed LoCs.

Table XVIII. Descriptive statistics and paired analysis (Wilcoxon's test) of static (top) and dynamic (bottom) test case metrics (Manual vs. EvoSuite): Manual tests are substantially more complex than EvoSuite tests according to the static metrics MeLoc and the dynamic metrics Methods and LOCs.

Metric	N	Man.mean	Man.sd	Evo.mean	Evo.sd	diff.mean	diff.median	diff.sd	p.value
MeLoc	8	27.25	34.03	4.12	1.73	23.12	7.00	34.46	**0.02**
McCabe	8	1.62	1.41	1.25	0.46	0.38	0.00	1.60	0.85
Methods	8	84.88	93.07	18.62	10.70	66.25	27.00	91.14	**0.01**
LoCs	8	429.12	477.50	74.50	43.94	354.62	161.50	473.89	**0.01**

5.4. Analysis of Post Questionnaire

Results of the Mann-Whitney test for questions Q1-Q4 are shown in Table XIX. Statistical significance is observed for Questions Q1, Q2 and Q4, but not for Q3. Participants *strongly agree* that they had enough time to complete the tasks and that tasks were clear. While they *agree* that understanding the application is required to complete the tasks, they are *not certain* that understanding the test cases is also required.

Questions Q7 and Q8 deal with the used (Q7) and most used (Q8) features of the IDE. All the features mentioned in the questionnaire have been used, with debugger and code navigation reported as the most used features.

Participants *never* used either JTopas or XML-Security before the experiment (question SQ1), and *never* used the faults from the SIR repository related to these two applications (SQ2).

Table XIX. Analysis of post questions Q1-Q4. Mann-Whitney test for the null hypothesis $median(Qx) = 0$ (Manual vs. EvoSuite); p-values in bold face indicate that the result is statistically significant at level 0.05.

Question	Median	P-value
Q1: Enough time	strongly agree	$<$**0.01**
Q2: Tasks clear	strongly agree	$<$**0.01**
Q3: Test case understanding required	not certain	0.68
Q4: Application understanding required	agree	$<$**0.01**

Questions OQ1 and OQ2 deal with the main challenges faced during debugging and with the followed process, respectively. The most frequently reported challenge during debugging (OQ1) is understanding the application code both when using manual and EvoSuite tests. Only 2 participants indicated understanding the test cases as a challenge, which corroborates our interpretation of the answers to Q3.

The strategy adopted to fix faults (OQ2) does not show relevant differences when using manual or EvoSuite test cases. Interestingly, understanding the test cases is not among the actions taken by participants to locate and fix the faults (still, in line with Q3).

According to the post questionnaire, the code of the test cases is not to be necessarily understood, in the opinion of the expert subjects involved in MvE, while it is supposed to be deeply understood according to the less experienced subjects involved in MvR. This could explain the different results obtained in the two studies. The analysis of the manual tests, which are more complex than Randoop and EvoSuite tests, is hard and takes time. The less experienced subjects performed better with Randoop tests because these tests are simpler to understand as compared to the manual tests. The expert subjects involved in MvE, who did not spend much time understanding the tests, performed equally well with EvoSuite and manual tests, despite the higher complexity of the latter tests. On the other hand, deep understanding of the application logic was reported as the major challenge by many subjects, when using either random and EvoSuite tests. Another remarkable difference is in the use of the debugger and of the code navigation functionalities offered by the IDE. Differently from experiment MvR, the expert subjects involved in MvE made extensive use of the IDE, in particular debugging and code navigation functionalities.

6. RESULTS OF MANUAL VS. OBFUSCATED [MVO]

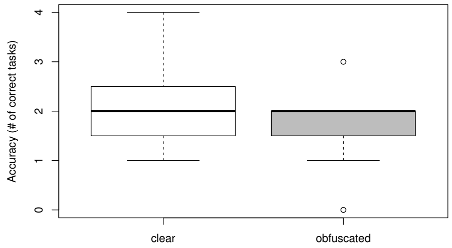
Since MvR and MvE indicate that the presence of meaningful identifiers and the fact that tests are manually created with a specific intent in mind are not relevant for debugging, we designed this experiment to specifically investigate the impact of the identifiers on the debugging activity. This study was conducted involving 11 MSc students from the University of Milano-Bicocca. Participants have been asked to locate and fix faults, supported by (i) manually written test cases, or (ii) manually written test cases with obfuscated identifiers.

We produced the obfuscated test cases from the manually written test cases by changing the name of every local variable and method parameter into `x` followed by an incremental number, and the name of every class attribute into `y` followed by an incremental number. In this way no variable or parameter has a name that describes the semantics of the value it stores.

6.1. Debugging Effectiveness

Figure 13 shows the boxplot of the effectiveness in fault fixing. The figure compares the number of correctly fixed faults when participants worked with the original, manually written, test cases (indicated as *clear* in the boxplot) to the number of correct fixes when working with obfuscated test cases (indicated as *obfuscated* in the boxplot). The effectiveness of clear tests is sometimes higher than the effectiveness of obfuscated tests. However, the two distributions have the same median, suggesting that the type of the identifier (clear or obfuscated) has no impact on the effectiveness of debugging tasks.

Table XX shows the GLM for effectiveness when clear and obfuscated test cases are used. Statistical significance is not reached (p-value is not <0.05), that is we cannot reject the null hypothesis: *there is no difference in the effectiveness of debugging, when debugging is supported by manually written test cases, either with clear or obfuscated identifiers*. The probability of a type II error in this claim is 23%; this relatively high value is due to the low number of subjects (11) and the high dispersion of data.



	Mean	Median	Sd
Manual tests	2.18	2.00	1.08
Obfuscated tests	1.75	2.00	0.89

Fig. 13. Boxplots and descriptive statistics for Effectiveness (Manual vs. Obfuscated): debugging was equally effective with Manual and Obfuscated test cases.

Table XX. GLM analysis of Effectiveness (Manual vs. Obfuscated); no factor has a statistically significant (p-value < 0.05) influence on Effectiveness.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.6641	0.4319	1.54	0.1464
Treatment	-0.0348	0.1202	-0.29	0.7763
Ability	-0.0472	0.0994	-0.48	0.6420
System	-0.1598	0.1202	-1.33	0.2049
Lab	0.1371	0.1274	1.08	0.3002

For what concerns the other factors, we can notice that none of them had a statistically significant influence on effectiveness (see Table XX), with the exception of faults for XML-Security (see Table XXI).

By looking at the interaction plot shown in Figure 14, we can notice that some faults are fixed equally well on clear and on obfuscated code (Faults 3-4), while on other faults (1-2) having either clear or obfuscated code is slightly preferable.

Table XXI. GLM analysis of Correctness by Treatment (Manual vs. Obfuscated) and Fault; p-values in bold face indicate a statistically significant influence on Correctness

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.9743	0.3043	3.20	0.0028
Treatment	-0.2171	0.1730	-1.25	0.2173
Fault	-0.0160	0.0782	-0.20	0.8388

(a) Jtopas

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.0911	0.3292	3.31	0.0029
Treatment	-0.0050	0.1664	-0.03	0.9764
Fault	-0.2877	0.0837	-3.44	**0.0021**

(b) Xml-Security

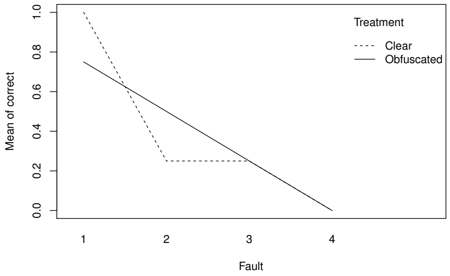
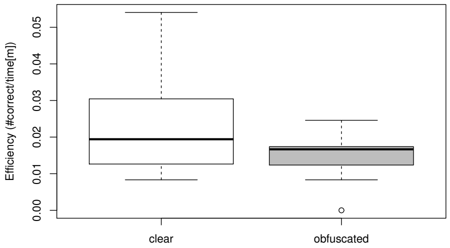


Fig. 14. Interaction plot of Correctness between Treatment (Manual vs. EvoSuite) & Fault on XML-Security; diverging/-converging lines indicate potential interactions.

6.2. Debugging Efficiency

Figure 15 shows the boxplots of efficiency when the faults are debugged using manually written test cases either with clear or obfuscated identifiers. Also in this case the efficiency of clear tests is sometimes higher than the efficiency of obfuscated tests. However, the two distributions have similar medians, suggesting that the type of the identifier (clear vs. obfuscated) has no impact on the efficiency of debugging.

Table XXII reports the GLM for efficiency. The test did not reach statistical significance, so also for efficiency we cannot reject the null hypothesis: *there is no difference in the efficiency of debugging, when debugging is supported by manually written test cases, either with clear or obfuscated identifiers*. The probability of a type II error is 13%.



	Mean	Median	Sd
Manual tests	0.02	0.02	0.01
EvoSuite tests	0.01	0.02	0.01

Fig. 15. Boxplots and descriptive statistics for Efficiency (Manual vs. Obfuscated): debugging was equally efficient with Manual and Obfuscated test cases.

For what concerns the other factors, we can notice that none of them had a statistically significant influence on effectiveness, including the faults (see Tables XXII and XXIII).

6.3. Test Case Understandability

In Sections 4, 5, we have found no significant difference in the effectiveness or efficiency of debugging when comparing Randoop/EvoSuite against manual tests. In Section 6, identifier names

Table XXII. GLM analysis of Efficiency (Manual vs. Obfuscated); no factor has a statistically significant (p-value $<$ 0.05) influence on Efficiency.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.0224	0.0207	1.08	0.2988
Treatment	-0.0041	0.0058	-0.70	0.4931
Ability	-0.0018	0.0048	-0.38	0.7088
System	-0.0053	0.0058	-0.91	0.3769
Lab	0.0094	0.0061	1.54	0.1470

Table XXIII. GLM analysis of Time by Treatment (Manual vs. Obfuscated) and Fault; no factor has a statistically significant (p-value $<$ 0.05) influence on Time.

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	32.3684	10.2913	3.15	0.0032
Treatment	1.8230	5.8527	0.31	0.7571
Fault	-2.5308	2.6453	-0.96	0.3448

(a) Jtopas

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	18.1722	15.0818	1.20	0.2400
Treatment	7.8179	7.6261	1.03	0.3155
Fault	1.9106	3.8342	0.50	0.6228

(b) Xml-Security

alone (clear vs. obfuscated) have been found to be insufficient to explain the lack of apparent difference between autogen and manual tests. We now investigate whether the understandability of the obfuscated and the clear tests that we used in the study was indeed significantly different.

Even if we renamed the identifiers in the tests, references to external entities, such as library or application methods called from the test cases, are not changed, so meaningful identifiers are still present in the obfuscated test cases. Table XXIV reports the number of obfuscated and user-defined identifiers for obfuscated test cases in the second and third columns, and the number of obfuscated and user-defined identifiers for clear test cases in the fourth and fifth columns. Table XXV reports the results of the Wilcoxon test on identifiers. The results confirm the impact of obfuscation: the difference in the number of obfuscated identifiers (first row) and of user defined identifiers (second row) between obfuscated and clear test cases is statistically significant (p-value = 0.01). We can thus reject DH_{03O}.

Since the obfuscation process does not alter the static/dynamic metrics, DH_{04O} is not applicable in this study.

In summary, the use of test cases with an understandability that is statistically significantly lower than the understandability of manually written test cases did not result in any major difference in the effectiveness and efficiency of debugging.

Table XXIV. Occurrences of obfuscated/user-defined identifiers in the test cases (Manual vs. Obfuscated)

	Obfuscated tests		Manual tests	
	Obfuscated IDs	UserDef IDs	Obfuscated IDs	UserDef IDs
		JTopas		
T1	9	35	0	44
T2	12	31	0	43
T3	5	13	0	18
T4	6	18	0	24
		XML-Security		
T1	3	15	0	18
T2	8	18	0	26
T3	6	25	0	31
T4	5	12	0	16

Table XXV. Paired analysis (Wilcoxon's test) of static (top) and dynamic (bottom) test case metrics (Manual vs. Obfuscated vs. User-Defined)

Id type	N	diff.mean	diff.median	diff.sd	P-value
Obfuscated	8	6.75	6.00	2.82	**0.01**
UserDef	8	-6.62	-6.00	2.92	**0.01**

6.4. Analysis of Post Questionnaire

Results of Questions Q1-Q4 are summarized in Table XXVI. Questions Q1-Q2 do not have statistical significance, while Questions Q3-Q4 do (p-value $<$0.05). Participants (MSc students) *agree* that test case understanding and application code understanding are required to complete the debugging tasks.

We compared the answers of participants who worked with clear test cases with those of participants who worked with obfuscated test cases. No question shows any statistically significant difference.

Table XXVI. Analysis of post questions Q1-Q4. Mann-Whitney test for the null hypothesis $median(Qx) = 0$ (Manual vs. Obfuscated); p-values in bold face indicate that the result is statistically significant at level 0.05.

Question	Median	P-value
Q1: Enough time	not certain	0.85
Q2: Tasks clear	agree	0.10
Q3: Test case understanding required	agree	**0.04**
Q4: Application understanding required	agree	$<$**0.01**

Questions Q7 and Q8 investigate what features of the IDE have been used to complete the debugging task and which one of these features was the most useful. The debugger and code navigation facilities resulted to be the most used features. For each feature, we used the Fisher's exact test to compare the answers given when using clear tests with the answers given when using obfuscated tests. No statistically significant difference is observed.

Questions OQ1 and OQ2 are open questions, meant to let participants formulate free comments on how they faced the debugging tasks. The main challenges (OQ1) for the subjects who worked with both clear and obfuscated tests are the comprehension of the application code and domain, the lack of comments, and the lack of a general overview of the application architecture. It is interesting to notice that the presence of meaningless identifiers in obfuscated test cases are not mentioned as a challenge. The steps followed by participants to fix the faulty code (OQ2) are quite standard and they are the same with either clear or obfuscated identifiers.

Summarizing the results of the post questionnaire, participants agree that code (both test code and application code) understanding was very important to successfully complete the debugging tasks. This is consistent with the results reported for MvR, where the subjects (MSc and BSc students) indicated test case understanding as a relevant factor. However, obfuscation of the identifiers does not seem to be a barrier to the usability of the test cases in debugging. This is also in agreement with the observation (see experiments MvR and MvE) that static and, even more, dynamic complexity metrics better reflect the difficulty of debugging, as compared to the understandability of the tests. The answers to the open questions of the post questionnaire support this finding: the presence of meaningless identifiers was not reported as a challenge, while comprehension of code, domain and overall architecture has been reported as the major difficulty encountered while executing the debugging tasks.

7. DISCUSSION

In this section we report the findings (*Find*) that we derived from our experiments. Each finding is summarized with one sentence, followed by a summary of the piece of evidence that supports the finding. We discuss how we interpreted the piece of evidence and a list of the practical implications generated by the finding.

Find1. *Meaningfulness of test case identifiers does not affect debugging effectiveness and efficiency*.

Pieces of Evidence.

- Test cases generated with EvoSuite and Randoop include meaningless identifiers (see the analysis reported in Sections 4.3 and 5.3).
- The presence of meaningless identifiers did not negatively affect effectiveness and efficiency (see the analysis about effectiveness reported in Sections 4.1, 5.1, and 6.1, and the analysis about efficiency reported in Sections 4.2, 5.2, and 6.2).
- Experienced subjects did not spend time understanding the purpose of the test cases, either manual or autogen (see the analysis of the post-questionnaire reported in Section 5.4)

Interpretation. Manual test cases are implemented with a specific intent in mind, so as to exercise a meaningful and representative test scenario. Moreover, (in our experiments) manual tests include mostly user-defined identifiers. On the contrary, autogen test cases, generated by Randoop or EvoSuite, do not explicitly cover any meaningful testing scenario and include a large number of artificially generated identifiers. Obfuscated test cases are still associated with a meaningful test scenario, because they are derived from manual test cases, but they include a substantial number of artificial, meaningless identifiers. All these differences do not result in superior debugging performance of subjects using manual test cases. We conjecture that the presence of meaningless identifiers in autogen tests is not an influential factor because such identifiers appear only when debugging the top-level methods in a test execution (i.e., the test methods). Below such top level, identifiers are perfectly understandable and meaningful. Moreover, any difficulty of interpretation of a test method due to its identifiers does not matter as long as the test reveals a fault. Subjects (in particular, subjects with higher experience, as PhD/Post-docs and researchers/professors) did not even attempt to attribute any intent to autogen tests. They did not spend any time understanding the purpose of the test cases, while they focused on understanding the bug and the application code.

Practical Implications.

Imp1.1. Since lacking of meaningful identifiers does not affect debugging performance, while the simplicity of autogen tests can ease debugging, developers should consider testing components with autogen tests first, to quickly rule out the faults that can be addressed with automatic tools, and then design the manual tests, to reveal the other faults that cannot be addressed with autogen tests.

Find2. *Test case complexity affects debugging effectiveness and efficiency of less experienced subjects*.

Pieces of Evidence.

- Autogen test cases are simpler than manual test cases (see static and dynamic test case complexity in Sections 4.3 and 5.3)
- Less experienced subjects performed better with autogen test cases than manual test cases (see analysis of efficiency and effectiveness in Sections 4.1 and 4.2)
- Experienced subjects performed almost equally well with autogen and with manual test cases (see analysis of effectiveness and efficiency in Sections 5.1 and 5.2)
- Less experienced subjects tried to understand the purpose of the analyzed tests (see answers to the post questionnaire in Section 4.4)

- Test case complexity is not reported as a meaningful factor for debugging (see answers of the post questionnaire in Section 5.4)

Interpretation. Manual test cases exercise complex, long execution scenarios that would require substantial effort to be fully understood. Autogen test cases are simple, short linear sequences of method invocations. In experiment MvR, involving less experienced subjects (BSc and MSc students), this difference provides an explanation for the superior performance of autogen test cases. In fact, the (less experienced) subjects involved in this experiment report a substantial effort devoted to test case understanding, which is a major obstacle with manual test cases. On the contrary, the experienced subjects involved in experiment MvE (PhD/Postdocs and researchers/professors) performed equally well with manual and autogen test cases, showing that for experienced testers the complexity of the test cases is not a relevant obstacle. This is confirmed by their answers to the post questionnaires, in which test case complexity is never mentioned as a major factor affecting the debugging process.

Practical Implications.

Imp2.1. Less experienced developers should debug the failures produced by autogen tests and simple manual tests before being allocated to the debugging of complex manual test cases.

Find3*. Ability and experience are key factors affecting the debugging performance.*

Pieces of Evidence.

- The low ability students had hard-time with both the debugging process and the debugging tools (see analysis of the post questionnaire Section 4.4), and experienced difficulties in fixing faults regardless of the type of test cases used (see analysis of interactions between treatment and experience in Sections 4.1 and 4.2).
- The high ability students knew the debugging process and the debugging tools (see post questionnaire in Sections 4.4, 6.4) and performed significantly better with autogen tests than manual tests (see analysis of interaction between treatment and experience in Sections 4.1, 4.2).
- The experienced subjects spent more time on the bug than on the test code (see analysis of post questionnaire in Section 5.4) and performed well with both autogen and manual test cases (see analysis of efficiency and effectiveness in Sections 5.1, 5.2).

Interpretation. We considered four levels of experience (BSc students, MSc students, PhD/Post-docs and researchers/professors) and we further analyzed the actual ability of BSc/MSc students through questionnaire and debugging exercises. The performance of subjects is distributed along a spectrum that nicely follows their levels of ability and experience. Low ability/BSc students have a quite poor performance regardless of the type of test cases used. They had a hard time fixing the faults and they encountered difficulties on the whole debugging process, including the use of tools and environments. High ability/MSc students show a good performance when using autogen test cases. For them, availability of focused, simple test cases empowers their fault finding capabilities. These subjects have enough ability and skills to take advantage of the simplicity of the fault revealing test cases generated automatically by tools, while they face more difficulties when provided with complex, manually written test cases. They know the debugging process and the associated tools relatively well, but they still work much better if simple test cases are provided. At the end of the spectrum are experienced subjects (PhD/Post-docs and researchers/professors), who have good debugging performance with any kind of test case (manual or autogen). As long as a test case reveals the fault, these subjects can perform debugging accurately and efficiently. They do not spend much time on the test case itself and they rather focus on the understanding of the bug and of the application code.

Practical Implications.

Imp3.1. Low ability developers should not be allocated to debugging at all.

Imp3.2. Debugging of complex manual tests should be allocated to senior developers.

Find4*. The debugging performance depends on the complexity of the system.*

Pieces of Evidence.

- The subject system has a significant effect in influencing the efficiency for experienced subjects (see analysis in Sections 5.1, 5.2).
- The control-flow of failing tests is more complicated in XML-Security than JTopas (see analysis of dynamic complexity in Section A.2)
- Understanding the application code has been reported as a significant factor in all the experiments (see analysis of post questionnaires in all experiments, reported in Sections 4.4, 5.4, and 6.4)

Interpretation. Experienced subjects had a similar performance with both manual and autogen tests, but demonstrated a different effectiveness when working with JTopas rather than XML-Security, although the faults themselves consist in either case of similar unit-level defects. Actually, in the failing executions, XML-Security follows a control-flow that is more complicated to understand and analyze than JTopas. This is expected to be associated with a higher application code understanding effort. Since such an effort has been reported by all subjects as a major factor affecting their debugging performance, we conclude that the complexity of the system when exercised under the failing scenario is a key factor affecting the capability of accurately and efficiently fixing the bug.

Practical Implications.

Imp4.1. It is important to take into consideration the complexity and the nature of the system, not only the nature of the tests, when allocating debugging tasks to developers.

Find5*. Usage of advanced debugging environments is fundamental with complex test scenarios.*

Pieces of Evidence.

- Only the subjects who took advantage of the Eclipse debugger mastered the most complex manual test cases (see analysis of feedback questionnaire for MvR, reported in Section 4.4, and the post questionnaire for MvE, reported in Section 5.4).

Interpretation. Only subjects being able to effectively use the Eclipse debugger could take advantage of the more complex manual test cases to fix faults. When the complexity of a test case becomes high, automation of the debugging activities is required in order for the tester to be able to effectively and efficiently investigate the execution, and locate and fix the fault. Experienced subjects used extensively the debugging functionalities offered by Eclipse and were not impacted by the complexity of the test scenarios.

Practical Implications.

Imp5.1. The use of proper automation tools is fundamental for success when the system and the faults are not trivial.

Imp5.2. Training junior developers on the use of debugging tools is an investment with a very high potential return, to be seen during the execution of actual debugging tasks.

To summarize, we can highlight two key findings. First, the efficiency and the effectiveness of debugging are not affected by autogen test cases; rather, autogen test cases, compared to manually written tests, are easier to debug for the least experienced developers. Second, the understandability of the test cases (e.g., the presence of meaningful identifiers and the existence of a meaningful test scenario) does not affect debugging, while the static and, more importantly, the dynamic complexity of the tests strongly impacts the effectiveness and the efficiency of debugging.

We have listed a number of implications derived from these findings. For what concerns the use of tools for automated test case generation, we observe that the debugging capabilities of testers, especially the less experienced ones, can be amplified by providing them with focused and simple autogen test cases that reveal the faults to be fixed. Such a benefit is not compromised by the use of meaningless identifiers in the test cases. Hence, whenever the same fault can be revealed by complex, manual test cases, but also by simple, automated tests, the latter are preferable because they can be usually generated faster than manually written tests and, for the less experienced people, they even maximize the debugging performance. The faults that can be revealed by both autogen and manual test cases are typically the ones that cause failures that can be detected without exploiting any specific knowledge of the application, such as crashes, hangs, exceptions and assertion violations.

Based on the results obtained in our experiments, we reconsider the whole testing process and the potential room for automated test case generation. We think that our results suggest the following strategy: (1) first, generate automated test cases and fix any bug possibly revealed by them; (2) write/consider manual test cases only later. Compared to autogen tests, manual tests are more expensive to be implemented and to use by less experienced subjects. As a result, less experienced developers should consider autogen tests first in debugging. Besides, autogen and manual test cases are equally effective for experienced developers, so the choice between them is not critical for these subjects.

When using autogen test cases, developers might occasionally experience false positives. For instance, an autogen test case might fail because it violates an implicit method precondition. However, this is a general problem of automated test case generation and is out of the scope of the present investigation. Under the assumption of a reasonably low false positive rate, according to the results of our empirical study, increasing the number of faults that are debugged using failing autogen test cases, and decreasing the ones that are debugged using manual test cases, can significantly improve the debugging effectiveness and efficiency.

8. THREATS TO VALIDITY

The main threats to the validity of this experiment belong to the conclusion, internal, construct and external validity threat categories.

Conclusion validity threats concern the relationship between treatment and outcome. We used statistical tests (General Linear Models and Wilcoxon) to draw our conclusions. Inability to reject the null hypothesis exposes us to type-II errors (incorrectly accepting a false null hypothesis), when we claim that no statistically significant difference was observed in the experiments. However, this probability was computed and reported, and it was always fairly low. We have further mitigated this threat by replicating the experiment MvE, in which the null hypothesis could not be rejected, two times, so as to increase the number of participants. In fact, the probability of a type II error can be reduced by increasing the sample size. In MvO the probability of a type II error is relatively high (23%) on effectiveness, but it is acceptable (13%) on efficiency.

Since we used GLM to determine the statistical significance of our results, we have applied the ShapiroWilk test to check the normality of the residuals. The only case of deviation from normality is on the first experiment (i.e., MvR), but the inspection of the corresponding Q-Q plot did not reveal major problems. The survey questionnaire was designed using standard scales and improved after experiment MvR to better detect issues and opinions.

Internal validity threats concern external factors that may affect the independent variable. Subjects were not aware of the experimental hypotheses. Subjects were not rewarded for the participation in the experiment and they were not evaluated on their performance in doing the experiment.

Construct validity threats concern the relationship between theory and observation. They are mainly due to how we measure the effectiveness of debugging. We relied on previously defined test cases to objectively evaluate whether the fixes were correct. The order in which subjects face tasks might affect the results. To control this factor we pre-ordered tasks (by difficulty). The ability of students was estimated according to their development background and using their exam scores. For the BSc students involved in MvR we also used the result of the training lab.

External validity concerns the generalization of the findings. In our experiments we considered test cases generated by Randoop and EvoSuite. Although other generators could be used, the results obtained with two generators, working according to different principles, already support well our interpretations.

Our studies exploited two different real-world systems from different domains and with different complexity. In principle different results could be obtained for different systems. To mitigate this issue we identified the domain of validity of the reported results by characterizing the two applications, their tests and their faults, within the domain of open source software. The characterization reported in Appendix A could be used by other researchers to compare their results with ours.

The study was performed in an academic environment, which may differ substantially from an industrial setup. However, we mitigate this threat by using subjects with different levels of experience and ability, including highly experienced PhD/Postdocs and researchers/professors, some of which with experience in professional software development. Moreover, we considered ability and experience as factors to detect any influence on the results.

9. CONCLUSION

We conducted a family of three experiments having a common goal: understanding the impact of automatically generated test cases on the effectiveness and efficiency of debugging. The first two experiments are based on test cases produced by two different test case generators, Randoop and EvoSuite. The third experiment used manually written test cases with obfuscated identifiers. It investigated the impact of identifier obfuscation alone, since all test case generators produce "obfuscated" (meaningless) identifiers. Experiments were conducted on two applications, JTopas and XML-Security, which have been found empirically to include test cases that are representative of medium-complex test suites available with open source projects and faults that are representative of real faults. In total, we involved 55 human subjects in the experiments, with a wide range of experience and ability, from MSc and BSc students to PhD/Post-docs and researchers/professors.

The key findings obtained from our experiments are that while *meaningfulness* of the identifiers that appear in test cases is not significantly detrimental to debugging, *complexity* of the test cases is a major factor affecting both effectiveness and efficiency of debugging. Although autogen test cases contain meaningless identifiers, they usually consist of very simple, linear statement sequences. They have been found to be equally effective as manual test cases for debugging, in general. Indeed, they are even more effective than manual test cases when they are used by subjects with intermediate testing experience and ability (such as MSc students with high ability), thanks to their low dynamic complexity. Experienced subjects (PhD/Post-docs and researchers/professors) are affected more by the complexity of the system under test than the test cases themselves. According to the answers they gave to the post-questionnaire, their understanding effort was focused mostly on application code, not on test case code, during debugging, while for less experienced subjects the understanding of the test code was also quite important.

The overall result of our family of experiments indicates that *automatically generated test cases* are not a major factor that affects the performance of testers while debugging. Other factors – test case dynamic complexity, system complexity, developers' experience, use of tools – have been found to play a much more prominent role. Hence, in the testing process, automated test case generation has the potential to give a key contribution, by inexpensively providing evidence of faults and by supporting debugging of such faults as effectively as manually defined test cases. We recommend to run automatically-generated test cases first and to use them in debugging the detected faults. Developers can take advantage of the fault revealing capability of automated tools without any major negative impact on the debugging effectiveness and efficiency.

As with any empirical research, our study is open to further validation and refinement. By replicating our study in different configurations (with alternative systems, faults, test case generation tools and subjects) we will be able to accumulate a body of knowledge on the impact of automated test case generation on debugging, which is a key issue when tools are to be adopted in production software development environments. We provide all the material and data of our study to facilitate and support future replications[7].

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[7] Replication package available at http://selab.fbk.eu/ceccato/replication_packages/debugging_replication_package.tgz.

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A. CHARACTERIZATION OF TEST CASES AND FAULTS

In this appendix, we analyze the complexity of the test cases and of the mutation faults used in our experiments. The test case complexity analysis is useful to position our test cases in the larger spectrum of the test cases that can be found in existing (open source) software projects. Since we used mutants, in addition to SIR faults, we analyze the complexity of the considered mutants as compared to SIR faults known for the programs used in our experiments. Unfortunately, there is no established metrics and procedure for such an assessment, so we had to resort to a set of custom complexity metrics, which all together are expected to provide a reasonable characterization of test cases and of mutation faults.

A.1. Complexity of Manual Test Cases

In our empirical studies we used JTopas and XML-Security as subject systems. Here we characterize the complexity of the manual test cases provided with the distributions of JTopas and XML-Security, as compared to a sample of applications available as open source code and released with a manual test suite. The aim is to understand whether the manual test suites of the two subject applications can be classified as representative of the simple/average/complex test suites available in a sampled population of open source software systems, thus obtaining deeper insights about the generality and the scope of the conclusions reported in the paper.

The applications used in the characterization are shown in Table XXVII. They are Java applications from several different domains for a total of 784,086 NCLoCs (Non-Comment Lines of Code). All the applications include a full suite of test cases for a total of 372,664 Test NCLoCs (Non-Comment Lines of Test Code). They have been randomly sampled from open source code repositories (such as *GitHub*[8], *Sourceforge*[9], and *Google code*[10]). The selection criteria are three folds: (1) *diversity*: the selected applications must be of different domains and sizes; (2) *test availability*: a test suite should accompany the source code; and (3) *maturity*: the selected applications must be well tested in terms of the proportion of test code vs source code, and they must be popularly used (received at least 1000 downloads).

Table XXVII. Applications used in the characterization of the test case complexity

Name	Domain	NCLoCs	Test NCLoCs
JTopas	parsing	4,482	4,547
Xml-Security	cryptography	29,255	11,438
H2database	database engine	111,080	37,788
Gwt-dev	Google web toolkit	108,972	18,823
Gwt-user	Google web toolkit	168,405	85,873
Jason-marshaller	JSON marshalling library for Java	2,773	4,448
Java2word	conversion utility	2,567	1,733
Jcloud-core	cloud computing library	19,058	12,310
Jcloud-blobstore	cloud computing library	5,783	4,148
Jcloud-compute	cloud computing library	12,711	3,735
Jfreechart	chart utility	94,550	48,554
Jgap	genetic programming library	43,501	19,771
Jodatime	date and time library	27,213	51,679
Openmrs-api	medical record system	78,381	29,541
Openmrs-web	medical record system	31,318	4,388
Pmd	source code analyzer	60,572	14,513
Xstream	XML utility	17,202	19,375
TOT		**784,086**	**372,664**

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[8] https://github.com

[9] http://sourceforge.net

[10] https://code.google.com

We are interested in characterizing the sampled applications in terms of static and dynamic complexity metrics (*MeLOC* and *McCabe*; *Exec. methods* and *Exec. LOCs*; see Section 3), by analyzing the distribution of the metrics values to check if any difference holds among the applications' test suites and if applications can be grouped based on the test suite complexity level. Our null hypothesis is that *there is no significant difference in the metric distribution between the test suite of application A and that of application B*. To test this null hypothesis we perform pairwise comparisons among all the pairs of test suites using an unpaired non-parametric statistical test, the Mann-Whitney two-tailed test. We assume a significance level of 95% ($\alpha = 0.05$), that is we reject the null hypothesis if $p\text{-}value < 0.05$. When multiple comparisons are performed, the number of hypotheses in a test increases, and so does the likelihood of witnessing a rare event. Hence, the chance to reject a true null hypotheses may also increase (type I error). To control this problem, we adopt the *Bonferroni* correction and the *Holm* correction [Holm 1979].

Let us first consider the static complexity metrics. The boxplot in Figure 16 shows the distribution of MeLOC (method lines of code) across all applications, sorted by their medians. Data for JTopas and XML-Security are highlighted in green. As we can notice, XML-Security is positioned in the medium of the spectrum, while JTopas is in the middle of the right half of the spectrum.

Based on the pairwise comparisons, we computed the clusters of applications that provide MeLOC values statistically similar to our two subject applications. The two clusters which contain JTopas and XML-Security are reported in Table XXVIII (column 2) and are represented in Figure 16 as different fill textures. XML-Security has values of MeLOC that are similar to three applications in the middle of the spectrum (*Gwt-dev*, *Java2world* and *Gwt-user*). JTopas is similar to almost all the applications in the right part of the spectrum. Depending on the correction method, such similar applications are either 6 (Bonferroni correction) or 5 (Holm correction; the excluded one, Jgap, is marked by an asterisk).

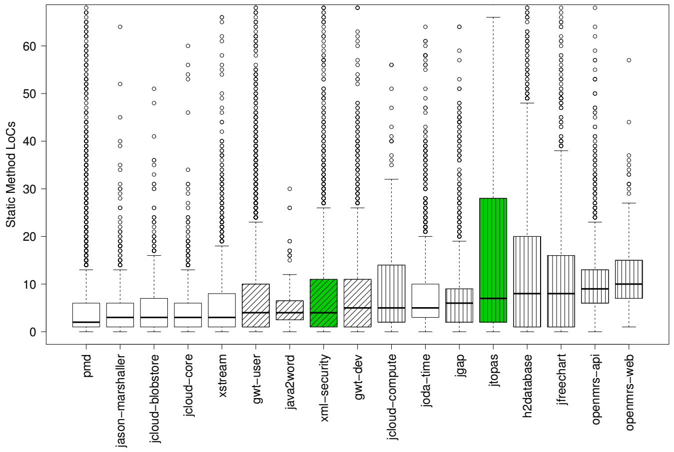

Fig. 16. Boxplots of static Method-LOCs. XML-Security is positioned in the middle of the spectrum, while JTopas is in the middle of the right half of the spectrum.

In the case of the McCabe complexity, all the applications have median equal to 1, so all the applications can be considered equivalent with respect to this metric. This result indicates that developers tend to write test cases with a very simple control flow.

Table XXVIII. Similarity clusters – asterisk means not included by Holm correction

Application	**MeLOC**	**Exec. Methods**	**Exec. LOCs**
XML-Security	Gwt-dev Java2world Gwt-user	JTopas Pmd	Pmd* Xstream JTopas
JTopas	Jcloud-compute H2database Jfreechart Openmrs-api Openmrs-web Jgap*	Openmrs-api* Pmd XML-Security	Pmd Xstream* XML-Security

Switching to dynamic metrics[11], the boxplots of *Exec. methods* and *Exec. LOCs* are shown respectively in Figure 17 and Figure 18. In both cases, JTopas and XML-Security are positioned in the middle-right part of the spectrum and they belong to the same similarity clusters. Clusters are reported in Table XXVIII (columns 3 and 4). For *Exec. methods* the cluster includes *Openmrs-api* and *Pmd*, while the similarity cluster for *Exec. LOCs* includes *Pmd* and *Xstream*.

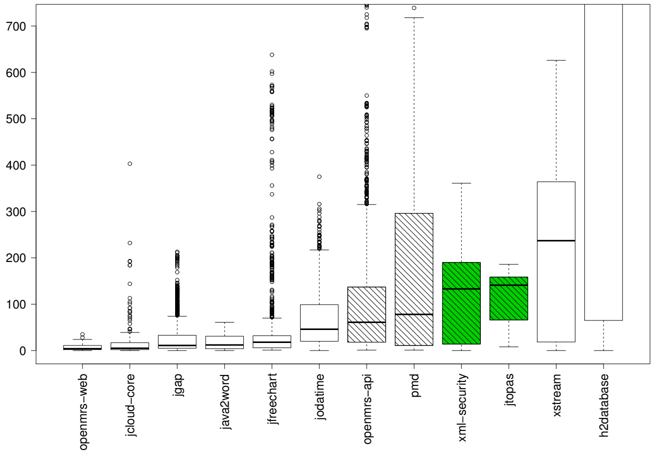

Fig. 17. Boxplots of dynamic Executed Methods. JTopas and XML-Security are positioned in the middle-right part of the spectrum.

In summary, we can conclude that the test suites of JTopas and XML-Security are representative of medium/complex open source test suites. In particular, in terms of static metrics, XML-Security is in the middle range, while JTopas is in the upper range for the LOC metrics. All applications are indistinguishable in terms of McCabe metrics. For what concerns the dynamic metrics, both subject applications belong to the medium/high complexity range. Hence, they can be regarded as representative of open source systems provided with medium/high complexity test suites. *This defines the context in which our empirical results can be interpreted.*

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[11] When considering dynamic metrics, we had to exclude some applications, because their execution figures were incomplete due to special test environment requirements that we could not satisfy.

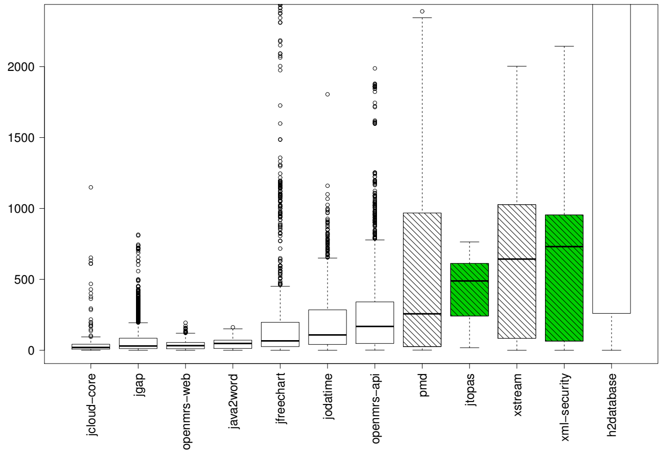
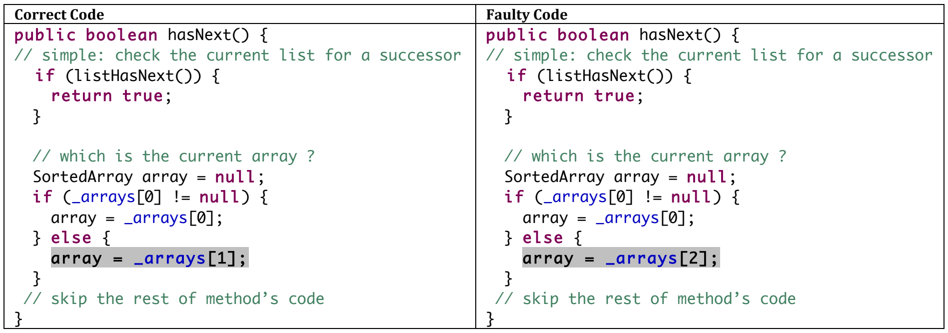


Fig. 18. Boxplots of dynamic Executed LOCs. JTopas and XML-Security are positioned in the middle-right part of the spectrum.

A.2. Seeded Faults

In MvR, seeded faults have been obtained from SIR. However, since some of the faults available in SIR for JTopas and XML-Security are not revealed by any of the test cases generated by Randoop, we have slightly modified them so as to make them detectable by the tool, without altering their nature.

For instance, according to the SIR repository, Fault #3 of JTopas is injected into the application by removing the `getMessage(..)` method from the `ExtIndexOutOfBoundsException` class. Since Fault #3 is not detected by autogen test cases, we changed the location of Fault #3 to the `TokenException` class, so that it can be revealed by both autogen and manual tests.



Correct Code

```
public boolean hasNext() {
// simple: check the current list for a successor
  if (listHasNext()) {
    return true;
  }

  // which is the current array ?
  SortedArray array = null;
  if (_arrays[0] != null) {
    array = _arrays[0];
  } else {
    array = _arrays[1];
  }
 // skip the rest of method's code
}
```

Faulty Code

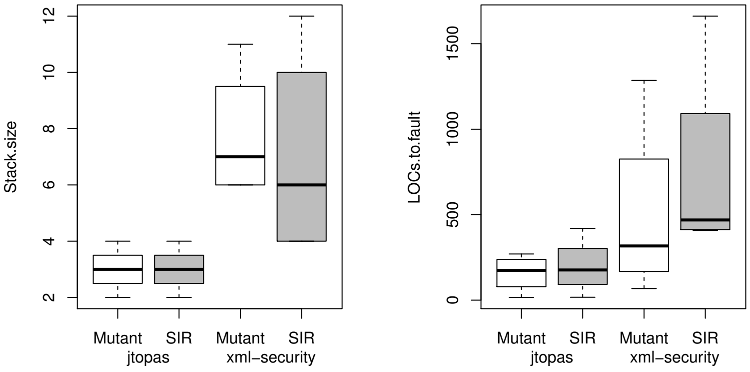
```
public boolean hasNext() {
// simple: check the current list for a successor
  if (listHasNext()) {
    return true;
  }

  // which is the current array ?
  SortedArray array = null;
  if (_arrays[0] != null) {
    array = _arrays[0];
  } else {
    array = _arrays[2];
  }
 // skip the rest of method's code
}
```

Fig. 19. An example of a fault seeded into JTopas. The correct code is on the left listing, while the faulty one is on the right. The fault is in the highlighted line.

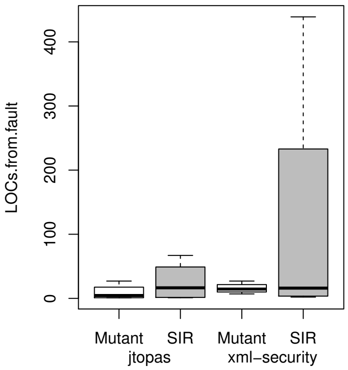
Figure 19 shows one of the the faults used in our experiments. The fault was injected in method $hasNext()$ of a class of JTopas. The correct index 1 of variable $\_array$ was replaced by a faulty index 2, causing an *ArrayIndexOutOfBoundsException* exception. It is worth noticing that the fault looks rather simple when it is isolated, but when it is put in a context with layers of code and within a program that performs several different functionalities, looking for the fault is non-trivial.

When running the corresponding JUnit test case that triggers the fault, the developer will receive the exception together with a stack of methods called prior to the exception. Such a call stack is particularly useful in debugging because it helps the developer narrow down the area (classes, methods, code blocks) where the fault is hidden. The developer, then, can put breakpoints into the code and use a debugger to step back and forth to hunt and fix the fault.



(a) Failure stack size

(b) LOCs to failure



(c) LOCs between fault and failure

Fig. 20. Boxplots comparing mutants and SIR bugs on Stack Size, LOCs to Failure and LOCs between Fault and Failure

Randoop and EvoSuite reveal two different sets of faults, that are not completely overlapping. This forced us to replace six of the eight faults used with Randoop when executing experiment MvE. We generated six new faults with the Jester mutation tool. Existing literature [Andrews et al. 2005] indicates that artificially generated bugs (mutations) can be reliably used in testing experiments. However, we have also verified that the faults actually seeded by Jester in our case are indeed similar to the faults available in the SIR repository. The latter are regression faults manually re-inserted into the code by experienced programmers. The metrics we used to characterize the faults produced by the mutation tool Jester as compared to the ones in the SIR are:

- **Stack size at fault location**: We stop the execution of the test case when it executes a faulty statement (fault location) and we measure the call stack size. The call stack size depends on how many nested method invocations have occurred since the beginning of the program up to the fault location. A large size of the call stack indicates that the developer may have to analyze a lot of call relationships before reaching the fault, while on a small stack only a few methods are to be inspected.
- **LOCs to the failure**: This metric counts how many statements a test case executes since the beginning of the program, before executing the statement that results in a program failure. This metric is relevant to characterize the fault, because it indicates how many lines a programmer may have to inspect before encountering the one that produces the failure.
- **LOCs between fault and failure**: This metric measures how many statements are executed between the fault (i.e., the incorrect program statement) and the fault manifestation (i.e., the failure). This metric is relevant because it quantifies the number of steps that a programmer may have to backtrack to link the program failure to the statement that caused the problem.

Figure 20 shows the boxplots for JTopas and XML-Security. In particular they compare the faults injected by the mutation tool (*mutant*) with the faults documented in the SIR repository (*SIR*), on a total of 16 data points. As we can see in Figure 20(a), there is no big difference between the stack size for SIR and mutant faults, even if there is some difference on the stack size between the two applications. Probably, XML-Security is more complex than JTopas, so faults involve a larger invocation stack on the former application. However, the size is consistent across different faults.

Table XXIX. ANOVA test to compare faults by category (mutant vs. SIR bug) and by application; p-values in bold face indicate a statistically significant difference.

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
F.category	1	0.56	0.56	0.10	0.7523
Application	1	76.56	76.56	14.19	**0.0027**
F.category:Application	1	0.56	0.56	0.10	0.7523
Residuals	12	64.75	5.40		

(a) Failure stack size

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
F.category	1	86289.06	86289.06	0.49	0.4963
Application	1	796110.06	796110.06	4.54	0.0544
F.category:Application	1	46764.06	46764.06	0.27	0.6149
Residuals	12	2103265.25	175272.10		

(b) LOCs to failure

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
F.category	1	14042.25	14042.25	1.19	0.2960
Application	1	9900.25	9900.25	0.84	0.3769
F.category:Application	1	7482.25	7482.25	0.64	0.4406
Residuals	12	141115.00	11759.58		

(c) LOCs from fault to failure

In Figure 20(b) we can see that the number of statements executed before reaching the failure point is quite constant on JTopas for SIR faults vs. mutations. It is slightly higher for SIR faults on XML-Security. In Figure 20(c) we can see that the median number of statements from the faulty statement to the fault manifestation does not change much across faults and applications.

To test if the differences observed in the graphs are statistically significant, we used two-way Analysis Of Variance (ANOVA) [Wohlin et al. 2012]. Table XXIX reports the three ANOVA tables by Fault Category and Application, to study the variance of the metrics due to the kind of fault (mutant or SIR), to the specific application (JTopas and XML-Security) and to the interaction of these two factors. We regard as statistically significant cases those with p-value$<$0.05 (shown in boldface). There is only one statistically significant case, related to the influence of the specific application on the failure stack size. We observe no significant influence of the kind of fault on the considered metrics, so we cannot reject the following null hypotheses:

- There is no difference between faults seeded by the mutation tool and faults from the SIR repository, with respect to the failure stack size;
- There is no difference between faults seeded by the mutation tool and faults from the SIR repository, with respect to the number of statements executed from the beginning of the program to the statement resulting in a program failure;
- There is no difference between faults seeded by the mutation tool and faults from the SIR repository, with respect to the number of statements executed between the faulty statement and the failure.

Based on these statistical results, we deem *the six mutants used in the experiment with EvoSuite as comparable to the faults obtained from the SIR repository*.