Accounting for Defect Characteristics in Evaluations of Testing Techniques

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As new software-testing techniques are developed, and before they can achieve widespread acceptance, their effectiveness at detecting defects must be evaluated. The most common way to evaluate testing techniques is with empirical studies, in which one or more techniques are tried out on software with known defects. However, the defects used can affect the performance of the techniques. To complicate matters, it is not even clear how to effectively describe or characterize defects. To address these problems, this work describes an experiment architecture for empirically evaluating testing techniques, which takes both defect and test-suite characteristics into account. As proof of concept, an experiment on GUI-testing techniques is conducted. It provides evidence that the defect characteristics proposed do help explain defect detection, at least for GUI testing, and it explores the relationship between the coverage of defective code and the detection of defects.

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1. INTRODUCTION

Software-testing techniques need to be good at detecting defects in software. Researchers *evaluate* testing techniques to determine if they are good—relative to other techniques, within some domain of software and defects, by some measurable definition of "good" that considers the resources used and the defects detected.

Anyone who has dealt with software defects knows that some defects are more susceptible to detection than others. Yet, for decades, evaluations of testing techniques have not been able to take this into account very well. This work offers a remedy: an experiment architecture for empirical evaluations that accounts for the impact that defect characteristics have on evaluation results.

As motivation, consider the well-known experiment on data-flow- and control-flow-based testing techniques by Hutchins et al. [1994]. The experiment compared test

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suites covering all def-use pairs, suites satisfying predicate ("edge") coverage, and random suites. Test suites were created and run for seven C programs with a total of 130 hand-seeded *faults*—mistakes in the source code. In order to compare the techniques, each fault was classified according to which kinds of test suites (i.e., techniques) were most likely to detect it. It turned out that very few faults were equally likely to be detected by all techniques; instead, most faults clearly lent themselves to detection by just one or two of the techniques. This is a tantalizing conclusion, one that could potentially help testers to choose a testing technique based on the kinds of faults they hope or expect to detect. Unfortunately, the authors "were not able to discern any characteristics of the faults, either syntactic or semantic, that seem to correlate with higher detection by either method."

At least one other empirical study, by Basili and Selby [1987], has shown that certain defects can be "harder" to detect with one testing technique and "easier" with another. Based on one's experience testing software, one might also suspect that some faults are "harder" or "easier" than others in a more general sense. Offutt and Hayes [1996] formalized this notion, and they and others have observed it empirically [Andrews et al. 2006; Hutchins et al. 1994; Rothermel et al. 2004].

Thus, the defects against which testing techniques are evaluated can make the techniques look better or worse—both absolutely, in defect-detection rates, and relative to other techniques. Without understanding defects, it is difficult to integrate results from different experiments [Basili et al. 1999], and it is usually impossible to explain why one technique outperforms another.

To understand fully how evaluations of testing techniques may depend on the defects used, one must be familiar with the typical procedure for such evaluations. Although some analytical approaches have been proposed [Frankl and Weyuker 1993], by far the most common and practical way to evaluate testing techniques continues to be empirical studies. Typically, these studies investigate hypotheses like "Technique A detects more defects than technique B" or "A detects more than zero defects more often than B" [Juristo et al. 2004].

Empirical studies, by their very nature as sample-based evaluations, always face the risk that different samples might lead to different results. An empirical study must select a sample of software to test, a sample of the test suites that can be generated (or recognized) by each technique for the software, and a sample of the defects— typically faults—that may arise in the software. Because different studies usually use different samples, one study might report that technique A detects more faults than B, while another would report just the opposite. Increasingly, published empirical studies of software testing are acknowledging this as a threat to external validity [Andrews et al. 2006; Graves et al. 2001; Rothermel et al. 2004].

This threat to external validity can be mitigated by replicating the study with different samples of test suites and fault-ridden software. But, while replicated studies are necessary for scientific progress [Basili et al. 1999; Zelkowitz and Wallace 1997], they are not sufficient. It is not enough to *observe* differing results in replicated studies; it is necessary to *explain* and *predict* them, for several reasons. First, explanation and prediction of phenomena are goals of any science, including the science of software testing. Second, the ability to predict situations in which a software-testing technique might behave differently than in a studied situation would aid researchers by pointing to interesting situations to study in the future [Basili et al. 1999]. Third, it would aid practitioners by alerting them if a testing technique may not behave as expected for their project.

If evaluators of testing techniques are to explain and predict the techniques' performance outside the evaluation, then they must identify and account for all the characteristics of the studied samples of software, test suite, and faults that can significantly

affect the evaluation's results. Some previous work has identified and accounted for characteristics of the software (e.g., size) and the test suites (e.g., granularity) [Elbaum et al. 2001; Morgan et al. 1997; Rothermel et al. 2004; Xie and Memon 2006]. Characteristics of faults, however, have resisted scrutiny. Few characteristics of faults have been identified, even fewer have been practical and objective to measure, and none of those have been demonstrated to help explain testing techniques' behavior [Strecker and Memon 2007]. In the words of Harrold et al. [1997], "Although there have been studies of fault categories... there is no established correlation between categories of faults and testing techniques that expose those faults."

This work proposes a new set of fault characteristics and shows how they—or any set of fault characteristics—can be accounted for in empirical studies of testing techniques. The challenge here is that, for a given piece of software under test, fault characteristics *and* test-suite characteristics may both affect fault detection. To account for both kinds of characteristics, this work presents an *experiment architecture*, or high-level view of experiment design. The experiment architecture shows how a study's inputs (test suites and faulty software) can be assembled and analyzed to discover how well different kinds of test suites cover different parts of the software and detect different kinds of faults.

Finally, this work presents an experiment that instantiates the architecture. The experiment provides a proof of concept for the architecture. Furthermore, it shows that each of the fault characteristics proposed in this work helps explain differences in faults' susceptibility to detection in at least one domain of testing (GUI testing).

In summary, the major contributions of this work are:

— to propose and empirically validate a simple, practical fault characterization for software-testing studies and
— to present an experiment in the domain of GUI testing that:
  — demonstrates one way to account for test-suite and fault characteristics in evaluations of testing techniques, building on preliminary work [Strecker and Memon 2008];
  — explores the relationship between execution of faulty code and detection of faults.

The next section provides necessary background information and surveys related work. Section 3 describes the experiment architecture (Figure 3). Then, Section 4 gives the fault characterization (Table I) and specifics of the experiment procedure. Experiment results are presented in Section 5 and discussed in Section 6. Sections 7 and 8 offer conclusions and future work.

2. BACKGROUND AND RELATED WORK

This work presents a new way of conducting software-testing studies—which takes into account characteristics of the faults and test suites used in the study—and, as proof of concept, a study in the domain of GUI testing. This section provides necessary background information on GUI testing and on characteristics of faults, test suites, and (for completeness) other influences on test effectiveness. It goes on to describe models of failure propagation that inspired this work's treatment of faulty-code coverage and fault detection.

2.1. GUI testing

Experiments in software testing have often focused on a particular domain of software (e.g., UNIX utilities) and of testing (e.g., JUnit test cases). This work focuses on GUI-intensive applications and model-based GUI testing [Memon 2007; Strecker and Memon 2009], a form of system testing. GUI-intensive applications make up a large portion of today's software, so it is important to include them as subjects of empiri-

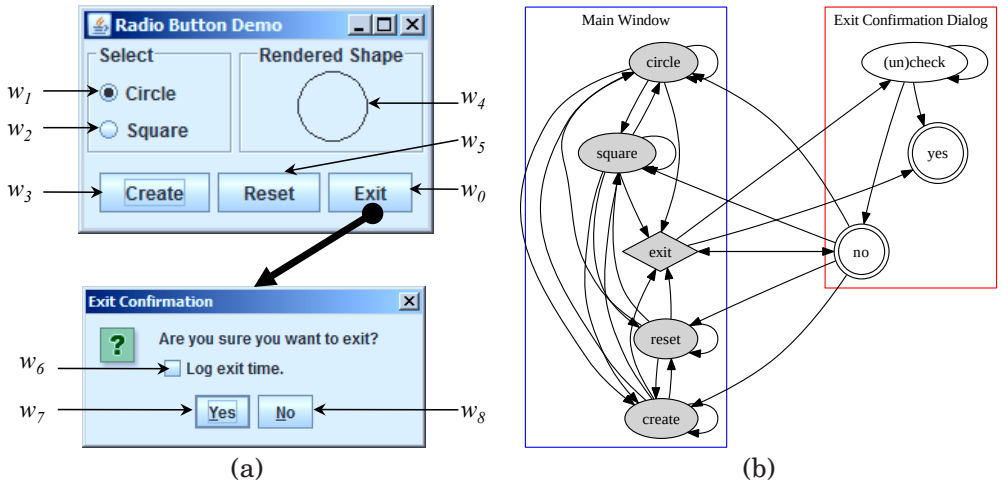


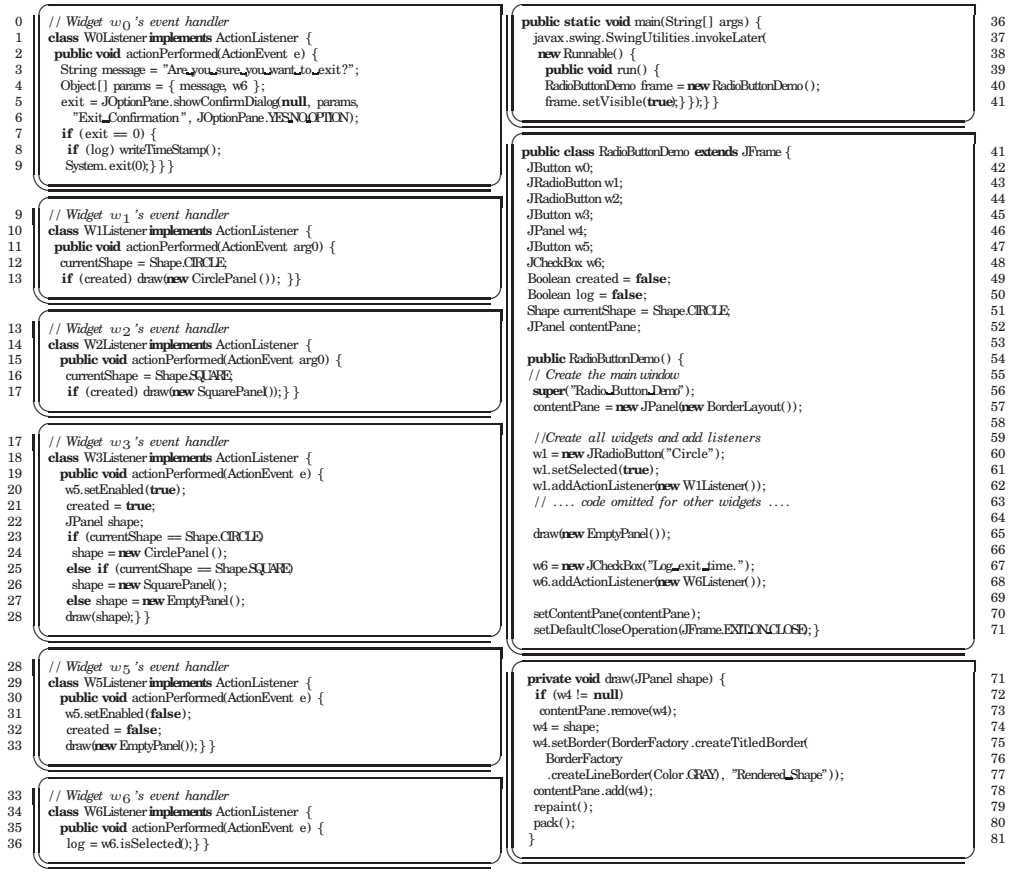
Fig. 1. (a) A Simple GUI and (b) its Event-Flow Graph

cal studies. Conveniently, model-based GUI testing lends itself to experimentation because test cases can be generated and executed automatically, enabling experimenters to create large samples of test cases.

The basic unit of interaction with a GUI is an *event*. Some examples of events in the GUI of Figure 1(a) are clicking on *widget* w_3, i.e., the *Create* button and checking/unchecking the check-box labeled w_6. In this GUI, each widget has exactly one event associated with it. The complete list of $(widget, event)$ pairs is: { (w_0, ***Exit***), (w_1, ***Circle***), (w_2, ***Square***), (w_3, ***Create***), (w_5, ***Reset***), (w_6, ***(un)check***), (w_7, ***Yes***), (w_8, ***No***) }; there is no event associated with w_4. Another common example of an event, not seen in this simple GUI, is entering a text string in a text-box. As this example suggests, some events are parameterized—e.g., text entry in a text box is parameterized by the text string entered. In actual GUI testing, only a finite set of parameter values can be tested for each event. This work uses just one parameter value for each event, thus eliminating, in effect, the distinction between unparameterized and parameterized events.

The portion of the application code that executes in response to a GUI event is called the *event handler*. The event handlers for our running example are shown in Figure 2. The code in the left column shows six event handlers; the right column shows the `main()` method, *initialization code* that executes only when the application starts, and a `draw()` method called by several event handlers. When the application starts, the `main()` method creates a new `RadioButtonDemo` object, which creates all the widgets, adds them to the main window frame, and associates `ActionListeners` with each widget. The application then waits for user events.

The dynamic behavior of a GUI can be modeled by an *event-flow graph (EFG)*, in which each node represents a GUI event. A directed edge to node n_2 from node n_1 means that the corresponding event e_2 can be executed *immediately after* event e_1. Henceforth, in this paper, the term *event* will be used to mean both an actual event and a node representing an event. The EFG for the GUI of Figure 1(a) is shown in Figure 1(b). In this EFG, there is an edge from *circle* to *circle* because a user can execute *circle* in succession; however, there is no edge from *circle* to *yes*. The EFG has a set of *initial events*, shaded in Figure 1(b), which can be executed in the GUI's initial state. We show *exit*, a window-opening event, using a diamond; *yes* and *no*, both window-termination events, using the double-circle shape; and the remaining system-interaction events using ovals.



```
0  // Widget w0's event handler
1  class W0Listener implements ActionListener {
2   public void actionPerformed(ActionEvent e) {
3    String message = "Are you sure you want to exit?";
4    Object[] params = { message, w6 };
5    exit = JOptionPane.showConfirmDialog(null, params,
6      "Exit Confirmation", JOptionPane.YES_NO_OPTION);
7    if (exit == 0) {
8     if (log) writeTimeStamp();
9     System.exit(0);}}}

9  // Widget w1's event handler
10 class W1Listener implements ActionListener {
11  public void actionPerformed(ActionEvent arg0) {
12   currentShape = Shape.CIRCLE;
13   if (created) draw(new CirclePanel()); }}

13 // Widget w2's event handler
14 class W2Listener implements ActionListener {
15   public void actionPerformed(ActionEvent arg0) {
16    currentShape = Shape.SQUARE;
17    if (created) draw(new SquarePanel());}}

17 // Widget w3's event handler
18 class W3Listener implements ActionListener {
19   public void actionPerformed(ActionEvent e) {
20    w5.setEnabled(true);
21    created = true;
22    JPanel shape;
23    if (currentShape == Shape.CIRCLE)
24     shape = new CirclePanel();
25    else if (currentShape == Shape.SQUARE)
26     shape = new SquarePanel();
27    else shape = new EmptyPanel();
28    draw(shape);}}

28 // Widget w5's event handler
29 class W5Listener implements ActionListener {
30   public void actionPerformed(ActionEvent e) {
31    w5.setEnabled(false);
32    created = false;
33    draw(new EmptyPanel());}}

33 // Widget w6's event handler
34 class W6Listener implements ActionListener {
35   public void actionPerformed(ActionEvent e) {
36    log = w6.isSelected();}}

public static void main(String[] args) {                          36
  javax.swing.SwingUtilities.invokeLater(                         37
   new Runnable() {                                               38
    public void run() {                                           39
     RadioButtonDemo frame = new RadioButtonDemo();               40
     frame.setVisible(true);}});}}                                41

public class RadioButtonDemo extends JFrame {                     41
 JButton w0;                                                      42
 JRadioButton w1;                                                 43
 JRadioButton w2;                                                 44
 JButton w3;                                                      45
 JPanel w4;                                                       46
 JButton w5;                                                      47
 JCheckBox w6;                                                    48
 Boolean created = false;                                         49
 Boolean log = false;                                             50
 Shape currentShape = Shape.CIRCLE;                               51
 JPanel contentPane;                                              52
                                                                  53
 public RadioButtonDemo() {                                       54
 // Create the main window                                        55
  super("Radio Button Demo");                                     56
  contentPane = new JPanel(new BorderLayout());                   57
                                                                  58
  //Create all widgets and add listeners                          59
  w1 = new JRadioButton("Circle");                                60
  w1.setSelected(true);                                           61
  w1.addActionListener(new W1Listener());                         62
  // .... code omitted for other widgets ....                     63
                                                                  64
  draw(new EmptyPanel());                                         65
                                                                  66
  w6 = new JCheckBox("Log exit time.");                           67
  w6.addActionListener(new W6Listener());                         68
                                                                  69
  setContentPane(contentPane);                                    70
  setDefaultCloseOperation(JFrame.EXIT_ON_CLOSE);}                71

private void draw(JPanel shape) {                                 71
 if (w4 != null)                                                  72
  contentPane.remove(w4);                                         73
 w4 = shape;                                                      74
 w4.setBorder(BorderFactory.createTitledBorder(                   75
    BorderFactory                                                 76
    .createLineBorder(Color.GRAY), "Rendered Shape"));            77
 contentPane.add(w4);                                             78
 repaint();                                                       79
 pack();                                                          80
}                                                                 81
```

Fig. 2. Source Code Snippets of the Running Example.

A *length-l test case* consists of any path through l events in the EFG, starting at an initial event. The *depth* of an event in the EFG is the length of the shortest test case containing the event, counting the event itself; initial events have a depth of 1. Event *circle*, *square*, *exit*, *reset*, and *create* have a depth of 1; *(un)check*, *yes* and *no* have a depth of 2. For usability reasons, most GUI applications are organized as a hierarchy of windows [Memon 2009]; these hierarchies are not very deep (most applications are 5–10 windows deep); consequently the EFG depths are also small.

In model-based GUI testing, the *oracle information* used to determine whether a test case passes or fails consists of a set of observed properties (e.g., title, background color) of all windows and widgets in the GUI. The *oracle procedure* compares these properties to their expected values after each event in the test case is executed. Such oracles are relevant for many GUI-based applications, that we call *GUI-intensive* applications, which expose much of their state, via visual cues, during execution.

The main steps in GUI testing—including reverse-engineering an EFG from a GUI, generating and executing test cases, and applying the oracle—have been automated in the GUI Testing Framework (GUITAR) [Xie and Memon 2006]. In summary, GUITAR uses graph traversal algorithms to generate test cases from a GUI's EFG. To date, our experiments have shown that test suites that contain test cases of a specific length that

cover all GUI events are most effective at detecting faults. Such test suites ensure that all events in the GUI are executed, and that each event has been tested in a number of *contexts* [Yuan et al. 2010]. We will use such test cases in the experiment procedure described in Section 4.

2.2. Characterizing faults

How should faults in software-testing studies be characterized? This is an open question. One or more of three approaches to characterizing faults are usually taken:

(1) Characterize faults by their origin (natural, hand-seeded, or mutation). Often, all faults in a study share a common origin, but some studies [Andrews et al. 2006; Do and Rothermel 2006] have compared results for faults of different origins.
(2) Describe each fault and report results individually for each one [Myers 1978]. This is only practical if few faults are used.
(3) Calculate the "difficulty" or "semantic size" of each fault relative to the test cases used in the study and compare results for "easier" and "harder" faults [Hutchins et al. 1994; Offutt and Hayes 1996; Rothermel et al. 2004].

The third approach comes closest to characterizing faults to help explain and predict results from other studies and real situations. But the "difficulty" of a fault can only be calculated relative to a set of test cases. Two different sets of test cases—e.g., a huge test pool in an empirical study and an early version of a test suite in practice, or test sets generated from two different operational profiles—would assign different "difficulty" values, and possibly different "difficulty" rankings, to a set of faults.

A fourth approach has occasionally been used:

(4) Characterize faults by some measure intrinsic to them, such as the type of programming error (omissive or commissive; initialization, control, data, computation, interface, or cosmetic) [Basili and Selby 1987] or the fault's effect on the program dependence graph [Harrold et al. 1997].

Basili and Selby [1987] and Harrold et al. [1997] each compare the ability of different testing techniques to detect faults, reporting the number of faults of each category detected by each technique. The fault characterization schema used by Basili and Selby [1987] proves to be relevant to the testing and inspecting techniques studied—certain techniques better detect certain kinds of faults—but the characterization is labor-intensive and not entirely objective. Conversely, the schema used by Harrold et al. [1997] is objective, allowing faults to be seeded automatically, but has not been shown to help explain why some faults were more likely to be detected than others. (Unfortunately, this result could not be re-evaluated in this work because the necessary tools were not available for Java software.)

In summary, fault characterization remains an open problem, but, for software-testing studies, objective and quick-to-measure characteristics are best. When described by such characteristics, faults can be grouped into types or clusters that retain their meaning across studies and situations. Section 4.1 identifies several such characteristics, and the experiment of Section 4 evaluates their ability to explain why some faults are more likely to be detected than others.

2.3. Characterizing test suites

Test-suite characteristics and their effects on fault detection have been studied much more intensively than fault characteristics. Probably the most studied characteristic of test suites is the *technique* used to generate or recognize them. In many studies, a sample of test suites from a technique has been used to evaluate the technique empirically against other testing or validation techniques. Techniques that have been

compared in this way include code reading, functional testing, and structural testing [Basili and Selby 1987]; data-flow- and control-flow-based techniques [Hutchins et al. 1994]; regression test selection techniques [Graves et al. 2001]; and variations of mutation testing [Offutt et al. 1996].

Often, the technique used to create a test suite is closely tied to the proportion of the software it covers, which in turn may affect the proportion of faults it detects. A study by Morgan et al. [1997] finds that the *proportion of coverage* (measured in blocks, decisions, and variable uses) and, to a lesser extent, the *test-suite size* influence fault detection. A study of regression testing by Elbaum et al. [2001] finds that, of several test-suite characteristics studied, two related to coverage—the *mean percentage of functions executed per test case* and the *percentage of test cases that reach a changed function*—best explain the observed variance in fault detection. In the domain of GUI testing, McMaster and Memon [2008] show that some coverage criteria (call-stack and event-pair coverage) are more effective than others (event, line, and method coverage) at preserving test suites' fault-detecting abilities under test-suite reduction. In addition, certain faults are detected more consistently by some coverage criteria than by others.

Another important way in which test suites can differ is in their *granularity*—the amount of input given by each test case. Rothermel et al. [2004] show that granularity (defined by them as a partition on a set of test inputs into a test suite containing test cases of a given size) significantly affects (sometimes increasing, sometimes decreasing) the number of faults detected by several regression-testing techniques. For GUI testing, Xie and Memon [2006] have found that more faults are detected by test suites with more test cases, while different faults are detected by suites whose test cases have a different granularity (length). They posit that longer test cases are required to detect faults in more complex event handlers.

In summary, several studies concur that the coverage, size, and granularity of test suites can affect their ability to detect faults. The current work bolsters the empirical evidence about these test-suite characteristics and, for the first time, looks for interaction effects between them and fault characteristics.

2.4. Characterizing other influences on testing

Although this work focuses on test-suite and fault characteristics, these are not the only factors that can influence fault detection in testing. Other factors include characteristics of the oracle used and the software under test.

Characteristics of the oracle information and the oracle procedure can affect fault detection. For example, using more thorough oracle information, or using an oracle procedure that checks the software's state more often, can improve fault detection [Xie and Memon 2007].

Characteristics of the software product under test, as well as the process used to develop it, can also affect fault detection. From a product perspective, it has been found that several measures of the size and complexity of software may help explain the number of faults detected. The studies by Elbaum et al. [2001] and Morgan et al. [1997], mentioned above for their results on test-suite characteristics, also consider software characteristics. Of the software characteristics studied by Elbaum et al. [2001], the *mean function fan-out* and the *number of functions changed* together explain the most variance in fault detection. Morgan et al. [1997] find that *software size*—measured in lines, blocks, decisions, or all-uses counts—contributes substantially to the variance in fault detection.

From a process perspective, one would expect fewer faults, and perhaps a different distribution of faults, to exist at the time of testing if other defect-removal or defect-prevention techniques had been applied prior to testing.

2.5. Models of failure propagation

Part of this work is concerned with understanding the conditional probability that a test suite detects a fault, given that the test suite covers the code containing the fault. As Section 4 explains, this conditional probability separates the concerns of fault detection and fault coverage. It shows how susceptible a fault is to detection, regardless of whether the code it lies in is frequently or rarely executed.

The relationship between fault detection and coverage has previously been viewed from the perspective of the RELAY model and PIE analysis. The RELAY model of Richardson and Thompson [1993] traces the steps by which a fault in source code leads to a failure in execution: from the incorrect evaluation of an expression to unexpected internal states to unexpected output. RELAY is the basis for propagation, infection, and execution (PIE) analysis of program testability proposed by Voas [1992]. PIE uses the probability that a given program element is executed and the probability that a fault in that element is detected.

Like RELAY, the current work is concerned with the relationship between faults and failures. This work, however, ignores internal program state. In contrast to this work's empirical approach, Richardson and Thompson use RELAY to compare test adequacy criteria analytically.

PIE differs from the current work because it estimates execution probabilities with respect to some fixed input distribution and infection probabilities with respect to some fixed distribution of faults. In contrast, the current work studies how variations in the input distribution (test suite) and the type of fault affect the test suite's likelihood of executing and detecting the fault.

3. EXPERIMENT ARCHITECTURE

We now provide a way to design empirical studies of software testing, particularly evaluations of testing techniques. Because it is more general than an experiment design, we call it an *experiment architecture*. (An experiment architecture is to an experiment design as a software architecture is to a software design; it is a high-level approach that can be instantiated to plan a specific experiment.) Our architecture enables experimenters to compare the effectiveness of different testing techniques and, at the same time, to measure the influence of other factors on the results.

In a given software product with a given oracle, two kinds of factors can influence a testing technique's ability to detect a fault: characteristics of the test suite used (other than testing technique) and characteristics of the fault. Thus, the architecture cannot account for *just* fault characteristics, even though they are the focus of this work. *It must simultaneously account for both test-suite and fault characteristics.* In other words, it must be able to show that certain kinds of test suites generally detect more faults, and certain kinds of faults are generally more susceptible to detection, and certain kinds of test suites are better at detecting certain kinds of faults.

The key observation leading to the architecture is that fault characteristics and test-suite characteristics, including the testing technique, can be accounted for simultaneously with the right kind of multivariate statistical analysis. The experiment in this work uses logistic-regression analysis (a decision that is justified in Section 4.3). Hence, the architecture must satisfy the requirements of logistic regression.

As Section 4.3 will explain, the input to logistic regression is a data set in which each data point consists of a vector of some independent variables and a binomial dependent variable. The output is a vector of coefficients, which estimate the strength of each independent variable's effect on the dependent variable. If each data point consists of a vector of characteristics of a $\langle$*test suite, fault*$\rangle$ pair and a value indicating whether the test suite detects the fault, then the output is just what we want: an estimate of each

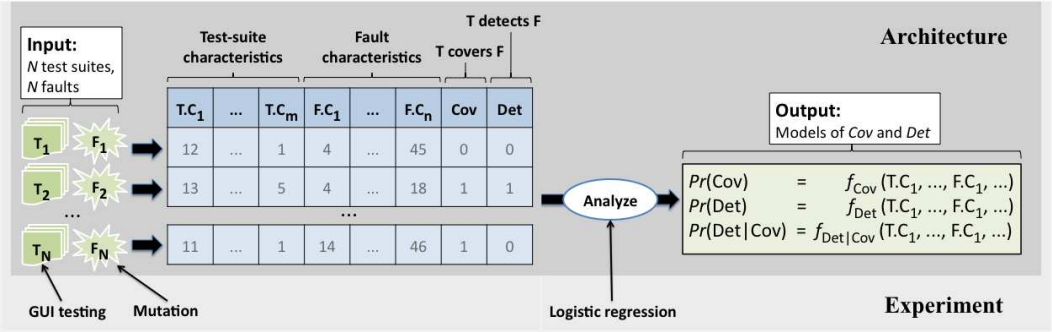


Fig. 3. Architecture and experiment procedure

characteristic's effect on the likelihood that a given test suite detects a given fault. (For example, in the special case where the independent variables are just the testing technique and one fault characteristic, logistic regression would show how likely each technique would be to detect a hypothetical fault having any given value for the fault characteristic.)

Logistic-regression analysis is a flexible technique, able to model different kinds of relationships between the independent variables and the dependent variable. The main requirement is that *the data points must be chosen by independent sampling*[1]. If each data point consists of characteristics of a ⟨*test suite, fault*⟩ pair, then the same test suite or the same fault cannot be used in more than one data point. This leads to a somewhat unusual (for software-testing studies) experiment design, in which each test suite is paired with a single fault.

Now that some motivation for the architecture has been given, Figure 3 illustrates the architecture (top darker gray box) and its instantiation in the experiment to be presented (bottom lighter gray box). For now, let us focus on the architecture itself, moving from left to right through the figure. The objects of study are a sample of N test suites ($T_1, \ldots, T_N$) for a software application and a sample of N faults ($F_1, \ldots, F_N$) in that application. Each test suite is paired with exactly one fault (T_1 with F_1, T_2 with F_2, etc.) to form a ⟨*test suite, fault*⟩ pair.

Each test suite in a pair is run to see whether it (1) executes (covers) the piece of code containing the fault in the pair and (2) if so, whether it detects the fault. These facts are recorded in the dependent variables, Cov (which is 1 if the suite covers the fault, 0 otherwise) and Det (which is 1 if the suite detects the fault, 0 otherwise). In addition, certain characteristics of each fault ($F.C_1, \ldots, F.C_n$) and each test suite ($T.C_1, \ldots, T.C_m$) are recorded. Determined by the experimenters, these characteristics may include the main variable of interest (e.g., testing technique) as well as factors that the experimenters need to control for. All of these characteristics together comprise the independent variables. As the next part of Figure 3 shows, the data collected for the independent and dependent variables form a table structure. For each of the N ⟨*test suite, fault*⟩ pairs, there is one data point (row in the table) consisting of a vector of values for the independent and dependent variables.

The data points are analyzed, as the right half of Figure 3 shows, to build one or more statistical models of the relationship between the independent variables and the dependent variables. The models estimate the probability that a given test suite

[1]Independent sampling means that the error terms of any two data points are statistically independent [Garson 2006]. That is, any random factor (including but not limited to errors in measurement) that affects one data point's value affects either none or all of the other data points' values.

(i.e., a given vector of values for $T.C_1, \ldots, T.C_m$) covers or detects a given fault (i.e., a given vector of values for $F.C_1, \ldots, F.C_n$) as a function of the test-suite and fault characteristics. Additionally, if only data points with $\mathsf{Cov} = 1$ are considered, then models of Det can be built from them to estimate the conditional probability of fault detection given fault coverage ($\Pr(\mathsf{Det}|\mathsf{Cov})$).

The architecture offers experimenters some choices: in the test-suite and fault characteristics to use as independent variables and in the way the samples of test suites and faults are provided. For this experiment in this work, the choice of independent variables is explained in Section 4.1. The test suites were generated randomly using a GUI-testing technique (Section 4.2.2), and the faults were generated by mutation of single lines of source code (Section 4.2.3). Because all faults in the experiment were confined to one line, a fault was considered to be covered by a test suite if the line containing it was covered.

No single experiment architecture is right for everyone. To use this architecture, the context of an experiment must satisfy the following preconditions:

— The experimenters must be able to obtain an adequate sample size of ⟨*test suite, fault*⟩ pairs for their purposes. The sample size depends on how many test-suite and fault characteristics are studied and how balanced the data are. (The experiment in Section 4 studied 20 characteristics, so we strove for a very large sample size.)
— For the type of faults studied, there must be some notion of "coverage". For faults generated by mutation and located inside of methods (as in our experiment), it is obvious whether a test suite covers the faulty code. But for more complex faults, such as faults of omission, it may not be obvious [Frankl et al. 1998]. Experimenters must either define "coverage" of faults or forgo calculating $\Pr(\mathsf{Cov})$ and $\Pr(\mathsf{Det}|\mathsf{Cov})$.
— The experimenters must be able to measure coverage without affecting the behavior of the software. This could be an issue for real-time software, for example, where instrumentation might affect response time.

The architecture could be trivially extended to consider a broader class of defect characteristics, including the characteristics of the failures caused by a fault. However, the preceding description emphasized fault characteristics because they are the focus of the experiment in the next section.

4. EXPERIMENT PROCEDURE

This experiment applies the architecture described in the previous section, as shown in Figure 3. It serves multiple purposes:

— as a stand-alone experiment, testing hypotheses about the influence of test-suite and fault characteristics on fault detection;
— as a concrete example for potential users of the experiment architecture to follow; and
— as a validation of the fault characterization described in Section 4.1, showing empirically that the fault characteristics chosen *can* affect faults' susceptibility to detection.

The data and artifacts from the experiment have been made available to other researchers as a software-testing benchmark (Section 4.2).

This experiment significantly extends, and resolves major problems with, a preliminary study of several test-suite characteristics and just two fault characteristics [Strecker and Memon 2008]. The results of the preliminary study raised intriguing questions about the relationship between the execution (coverage) of faulty code and the detection of faults: Are certain kinds of faults more likely to be detected just because the faulty code is more likely to be covered during testing? Or are these faults

harder to detect even if the code is covered? This work pursues those questions by studying not just the likelihood of detecting faults, but the likelihood of detecting them *given that* the faulty code has been covered. This perspective echoes existing models of failure propagation (Section 2.5), but its use to study faults empirically is unprecedented; our novel experiment architecture makes it possible.

For each fault characteristic c studied in the experiment, the experiment tests the following null hypotheses:

— H1: The fault characteristic c does not affect a fault's likelihood of being detected by a test suite.
— H2: No interaction effect between the fault characteristic c and a test-suite characteristic affects a fault's likelihood of being detected by a test suite. (Certain kinds of faults are not more likely to be detected by certain kinds of test suites.)
— H3: The fault characteristic c does not affect a fault's likelihood of being detected by a test suite, given that the test suite covers the faulty code.
— H4: No interaction effect between the fault characteristic c and a test-suite characteristic affects a fault's likelihood of being detected by a test suite, given that the test suite covers the faulty code.

Hypotheses analogous to H1 and H3 are tested for each test-suite characteristic:

— H5: The test-suite characteristic does not affect a fault's likelihood of being detected by a test suite.
— H6: The test-suite characteristic does not affect a fault's likelihood of being detected by a test suite, given that the test suite covers the faulty code.

Hypotheses H2 and H4 also apply to test-suite characteristics. The main concern of the experiment, however, is the fault characteristics because they need to be evaluated to determine whether they really are relevant to software testing.

Like any experiment, this one restricts itself to a limited domain of applications, testing techniques, and faults. In choosing this domain, important factors were the cost and replicability of the experiment. Since we knew of no existing data set that fit the requirements of this experiment, and since creating a large sample of test suites and faults by hand is expensive and hard to replicate, we decided to generate test suites and faults automatically. For faults, this led us to choose the domain of mutation faults. For test suites (and consequently applications), the authors' experience with automated GUI testing made this domain an obvious choice. As Section 2.1 explained, GUI testing is a form of system testing in which test cases are generated by traversing an event-flow-graph (EFG) model of a GUI. Considering that many computer users today use GUIs exclusively and have encountered GUI-related failures, research on GUIs and GUI testing is timely and relevant.

4.1. Test-suite and fault characterization

The independent variables in this experiment are characteristics of faults and test suites hypothesized to affect the probability of fault detection. Since there is currently no standard way to choose these characteristics, the selection was necessarily somewhat improvised but was driven by earlier research (Section 2). Although the literature does not directly suggest viable fault characteristics, it does clearly point to certain test-suite characteristics. Because the test suites and faults in this experiment were generated automatically, characteristics related to human factors did not need to be considered. To make this experiment practical to perform, replicate, and apply in practice, only characteristics that can be measured objectively and automatically were considered.

Truly objective measures of faults—measures of intrinsic properties, independent of any test set—would be derived from static analysis. In this experiment, however, some characteristic measures were derived from execution data from the *test pool* (the set of all test cases used in the experiment), not from static analysis. This is not entirely objective because a different test pool would result in different measurements. However, measures that are closely correlated to the specific test pool (e.g., those averaged across the test cases in the pool) were avoided. We deferred the use of static analysis to future work because the bias introduced by the execution data was expected to be acceptably small, and because static analysis tools accounting for the event-oriented structure of GUI-based applications were still under development [Staiger 2007].

In this work, a *characteristic* is an informally defined property of a fault or test suite, such as the degrees of freedom in execution of faulty code or the proportion of the application covered by the test suite. Each characteristic can be measured by one or more *metrics*, which are more precisely defined. When one of several metrics might be used to measure a characteristic, it is not clear a priori which one best predicts a fault's likelihood of being covered or detected. The rest of this section lists the fault and test-suite metrics explored in this experiment, organized by characteristic. The metrics are summarized in Table I.

4.1.1. Fault characteristics. One fault characteristic studied is the **method of creation**, which for this experiment is some form of mutation. The mutation operators fall into two categories: class-level that are added to class code not specific to any particular method (e.g., changing `Shape.CIRCLE` to `Shape.SQUARE` on Line 51 in Figure 2) and method-level that are within the scope of a single method (e.g., changing $!=$ to $==$ on Line 72 in Figure 2). Class-level and method-level mutations were previously studied in Strecker and Memon [2008]; the results were inconclusive but suggested that class-level and method-level faults may be differently susceptible to detection. The label for the metric of mutation type is F.MutType. To provide finer-grained analysis, in future work, we intend to model mutation operators as characteristics of faults.

Another fault characteristic is the **distance of faulty code from the initial state**. Faults residing in code that is "closer", in some sense, to the beginning of the program are probably easier to cover and may be easier to detect. For example, Line 57 in Figure 2 is executed before Line 78. One metric measuring this is the minimum number of source-code lines that must be covered before the faulty line is executed for the first time (F.CovBef). This can be estimated by running the test pool with program instrumentation to collect coverage data.

Another way to measure the distance of a line of code from the program's initial state is to compute the minimum EFG depth, discussed in Section 2.1, of the events associated with a faulty line. Events can be associated with particular lines by collecting coverage data for each event in each test case of the test pool. The label for this metric is F.Depth.

The **repetitions in which the faulty code is executed** may affect fault detection. Faults that lie in code that, when executed, tends to be executed multiple times by iteration, recursion, or multiple invocations may be easier to detect. The exact number of times a line is executed varies by test case; two binomial metrics are studied instead. One is whether the line is *ever* executed more than once by an event handler (F.SomeRep). The other is whether the line is *always* executed more than once by an event handler (F.AllRep).

Another fault characteristic that may affect fault detection is the **degree of freedom in execution of the faulty code**. In GUI-based applications, an event handler can typically be executed just before or after any of several other event handlers. In our example of Figure 1, event $(un)check$ can be executed only after itself and $exit$; whereas

Table I. Test-suite and fault characteristics studied

Characteristic		Metric	Definition
Fault	Method of creation	F.MutType	1 if a method-level mutant, 0 if a class-level mutant
	Distance from initial state	F.CovBef	Est. minimum lines covered before first execution of faulty line, normalized by total lines
		F.Depth	Est. minimum EFG depth of first event executing faulty line in each test case, normalized by EFG depth
	Repetitions	F.SomeRep	1 if est. minimum executions of faulty line by each executing event is > 0, 0 otherwise
		F.AllRep	1 if est. maximum executions of faulty line by each executing event is > 0, 0 otherwise
	Degrees of freedom	F.MinPred	Est. minimum EFG predecessors of events executing faulty line, normalized by total events in EFG
		F.MaxPred	Est. maximum EFG predecessors of events executing faulty line, normalized by total events in EFG
		F.MinSucc	Est. minimum EFG successors of events executing faulty line, normalized by total events in EFG
		F.MaxSucc	Est. maximum EFG successors of events executing faulty line, normalized by total events in EFG
		F.Events	Est. number of distinct events executing faulty line, normalized by total events in EFG
	Size of event handlers	F.MinWith	Est. minimum lines covered in same event as faulty line, normalized by total lines
		F.MaxWith	Est. maximum lines covered in same event as faulty line, normalized by total lines
Test suite	Granularity	T.Len	Length (number of events) of each test case
	Size	T.Events	Number of events, normalized by total events in EFG
	Proportion of coverage	T.Class	Percent of classes covered
		T.Meth	Percent of methods covered
		T.Block	Percent of blocks covered
		T.Line	Percent of lines covered
		T.Pairs	Percent of event pairs in EFG covered
		T.Triples	Percent of event triples in EFG covered

circle can be executed after *circle*, *square*, *reset*, *create*, and *no*. Faulty code executed by an event that can be preceded or succeeded by many other events may be easier to cover, and it is not clear whether it would be more or less susceptible to detection. The minimum or maximum number of event predecessors or successors associated with a faulty line (F.MinPred, F.MaxPred, F.MinSucc, F.MaxSucc) can be estimated by associating coverage data from the test pool with the EFG. Faulty code executed by more events (e.g., code in the `draw()` method, invoked by several events, in Figure 2) may also be easier to cover and either more or less susceptible to detection. The number of events executing the faulty code (F.Events), too, can be estimated with coverage data from the test pool.

Morgan et al. [1997] report that program size affects fault detection in testing, so the **size of the event handler(s) that execute a faulty line** may similarly have an effect. Event-handler size can be measured as the minimum or maximum number of lines covered by each event handler that executes the faulty line (F.MinWith, F.MaxWith).

4.1.2. Test-suite characteristics. For test suites, one interesting characteristic is the **granularity of test cases**—the amount of input provided by each test case. In GUI testing, granularity can easily be measured by the length (number of events) of a test case (T.Len). In this experiment, the length of the test cases in a test suite could be measured either by taking the average of different-length test cases in a suite or by constructing each suite such that its test cases have a uniform length. The latter approach is chosen because it has a precedent in previous work [Rothermel et al. 2004; Xie and Memon 2006]. As mentioned in Section 2.1, uniform-length test suites ensure that all events in the GUI are covered, and that each event has been tested in a number of *contexts* [Yuan et al. 2010].

Clearly, the characteristic of **test-suite size** can affect fault detection: larger test suites are likely to cover and detect more faults. An important question studied in this experiment is whether they do so when other factors, such as the suite's coverage level, are controlled for. In some studies, test-suite size is measured as the number of test cases. But for this experiment, since different suites have different test-case lengths, a more meaningful metric is the total number of events in the suite, which is the product of the test-case length and the number of test cases (T.Events).

Another test-suite characteristic that can affect fault detection is the **proportion of the application covered**. Obviously, the more of an application's code a test suite covers, the more likely it is to cover a specific line, faulty or not. It may also be likely to detect more faults [Morgan et al. 1997]. The proportion of coverage may be measured by any of the myriad coverage metrics proposed over the years. This experiment considers several structural metrics—class (T.Class), method (T.Meth), block (T.Block), and line coverage (T.Line)—because of their popularity and availability of tool support. (Block and line coverage are very similar, but both are studied because it is not clear a priori which better predicts fault detection.) For GUI-based applications, additional coverage metrics based on the event-flow graph (EFG) are available. Event coverage (coverage of nodes in the EFG) turns out *not* to be a useful metric for this experiment because each suite is made to cover all events. However, coverage of event pairs (EFG edges or length-2 event sequences; T.Pairs) and event triples (length-3 event sequences; T.Triples) is considered. (Longer event sequences could have been considered as well, but length 3 seemed a reasonable stopping point for this experiment. We note that a similar notion of "length 3 sequences," albeit in the context of state machines, has been developed earlier; it has been called edge-pair, tranistion-pair, or two-trip coverage [Ammann and Offutt 2008]. Because our experiment does show coverage of

Table II. Applications under test

	Lines	Classes	Events	EFG edges	EFG depth	Data points
CrosswordSage 0.3.5	2171	36	98	950	6	2230
FreeMind 0.7.1	9382	211	224	30146	3	970

length-2 and length-3 sequences to be influential variables, future experiments can study coverage of longer sequences.)

4.2. Data collection

The first stage of the experiment involves building and collecting data from a sample of test suites and a sample of faults. One of the contributions of this work is to make data and artifacts from this experiment—products of thousands of computation-hours and hundreds of person-hours—available to other researchers as a software-testing benchmark[2]. These data and artifacts, which include the samples of test suites and faults, can help other researchers replicate this work and perform other studies.

4.2.1. Applications under test. Two medium-sized, open-source applications were studied: CrosswordSage 0.3.5[3], a crossword-design tool; and FreeMind 0.7.1[4], a tool for creating "mind maps". Both are implemented in Java and rely heavily on GUI-based interactions. Table II gives each application's size as measured by executable lines of code, classes, and GUI events modeled in testing; the depth and number of edges of its EFG; and the number of data points ($\langle$*test suite, fault*$\rangle$ pairs) generated for it.

GUI testing of the applications was performed with tools in the GUITAR suite [Xie and Memon 2006]. To make the applications more amenable to these tools, a few modifications were made to the applications' source code and configuration files (e.g., to make file choosers open to a certain directory and to disable automatic saves). The modified applications are referred to as the *clean-uninstrumented* versions. Each application was made to have the same configuration throughout the experiment. A simple input file was created for each application; it could be opened by performing the correct sequence of events on the GUI. Using GUITAR, an EFG was created for each application. GUI events that could interfere with the experiment (e.g., events involved in printing) were removed from the EFG. The applications, configuration files, input files, and EFG are provided in the software-testing benchmark described at the beginning of Section 4.2.

To collect coverage data, each clean-uninstrumented application was instrumented with Instr[5] and Emma[6]. The instrumented applications are referred to as the *clean versions*. Instr reports how many times each source line was executed, while Emma reports (among other information) the proportion of classes, methods, blocks, and lines covered. Coverage reports from Instr were collected after each event in a test case; a report from Emma was collected at the end of the test case.

To identify lines in *initialization code*—code executed before any events are performed—an "empty" test case (with no GUI events) was run on each application and coverage reports were collected. The initialization code was treated as an initial event in the EFG having depth 0, no in-edges, and out-edges extending to all depth-1 events.

[2]http://www.cs.umd.edu/~atif/Benchmarks/UMD2008a.html
[3]http://crosswordsage.sourceforge.net
[4]http://freemind.sourceforge.net
[5]http://www.glenmccl.com/instr/index.htm
[6]http://emma.sourceforge.net

4.2.2. Test suites. The sample of test suites used in this experiment should be large[7]; and the results of the experiment should be replicable, i.e., not influenced by the skill of the tester. These criteria suggest an automated testing technique. For this experiment, a form of automated GUI testing (Section 2.1) is chosen.

Because of our choice of logistic regression analysis (discussed in Section 3), not only should the sample of test suites be large and replicable, but it should also be an independent sample. As mentioned earlier, this imposes the requirement that test suites in different ⟨*test suite, fault*⟩ pairs should not be "related" to one another, i.e., the same test suite or the same fault cannot be used in more than one data point. This ensures that our data points are independently sampled, i.e., the error terms of any two data points are statistically independent [Garson 2006]. For this reason, a unique set of tests was generated, using the process described below, for each ⟨*test suite, fault*⟩ pair. (The alternative would be to form each test suite by selecting, with replacement, a subset of a large pool of test cases, as was done in a preliminary study [Strecker and Memon 2008].)

Each test suite satisfies two requirements. First, it covers every event in the application's EFG at least once. This is to ensure that faults in code only executed by one event have a chance of being covered or detected. Second, its test cases are all the same length. This is so that test-case length can be studied as an independent variable. The length must be greater than or equal to the depth of the EFG to ensure that all events can be covered.

Model-based GUI testing has the advantage of being automated, but this is tempered by the fact that existing tools for generating and executing GUI test cases are immature; they are still under development and may contain bugs. Also, the EFG is only an approximation of actual GUI behavior; because of enabling/disabling of events and other complex behavior in the actual GUI, not every test case generated from the EFG model is executable [Yuan and Memon 2007]. For these reasons, each test suite must be generated carefully to ensure that every test case runs properly.

Each test suite was generated in two stages.

— *Stage 1:* First, a test-case length L between the EFG depth and 20 (inclusive) is randomly chosen. The choice of 20 is based on our earlier experience with GUI testing [Xie and Memon 2006]; because of enabling/disabling state-based relationships between events, very long test cases become unexecutable because some event in the test case may be disabled. The list E of events that remain to be covered is initialized to include all events in the EFG. A length-L test case is generated to cover a randomly-selected event $e \in E$. Then the test case is run on the application. If it runs successfully, then e and all other events it covers are removed from E; otherwise, it is discarded and a new test case is generated. Test cases continue to be generated until E is empty.
— *Stage 2:* The mean and variance of the total number of events in the test suites generated in Stage 1 scales with test-case length. This is an undesirable feature for this experiment because we want the number of events and the test-case length to be independent (explained in Section 3). Stage 2 adds random test cases to the suite to make test-suite size and test-case length independent. In preparation for the experiment, 100 test suites of each test-case length were generated for each application using the procedure in Stage 1. The number of events per suite was observed to be approximately normally distributed for each length; a mean and variance for each normal distribution was estimated from these test suites. During the experiment, Stage 2 for each test suite begins by calculating the quantile on the normal distri-

[7]Section 6.6 discusses sample size.

bution for length L corresponding to the suite's number of events after Stage 1. The number of events corresponding to the same quantile on the normal distribution for length 20 is then found; this becomes the target number of events for the suite. Test cases are generated by randomly traversing the EFG and are added to the suite to reach the target number.

4.2.3. Faults. An important consideration in any empirical study of fault detection is whether to use natural, manually-seeded, or automatically-seeded faults [Andrews et al. 2006; Do and Rothermel 2006]. To obtain this experiment's large sample size (Table II) with the resources available, using automatically-seeded faults was the only feasible option. Even apart from resource considerations, automatically-seeded faults offer some advantages for experimentation: unlike natural or hand-seeded faults, automatically-seeded faults are not influenced by the person (accidentally or intentionally) seeding the fault. The tool MuJava[8] was used to seed mutation faults (syntactically-small changes to the source code, such as replacing one operator or identifier with another; a full list of the mutation types is available at the referenced URL). Although the use of mutation faults is a threat to external validity, it should be noted that in at least some cases mutation faults turn out to be about equally difficult to detect as natural faults [Andrews et al. 2006; Do and Rothermel 2006], and a fault's syntactic size has little to do with its difficulty of detection [Offutt and Hayes 1996].

Using MuJava, all possible faults within MuJava's parameters were generated for each application. Of those, faults that spanned multiple lines and faults in application classes corresponding to events deliberately omitted from the application's EFG (e.g., `crosswordsage.PrintUtilities`; see Section 4.2.1) were omitted. Faults not inside methods (i.e., in class-variable declarations and initialization) were also omitted because their coverage is not tracked by Emma or Instr and because most extra-method faults turned out to be either trivially detectable (e.g., removing a necessary variable initialization) or not faults at all (e.g., removing an unnecessary variable initialization). For CrosswordSage, all 2230 of the remaining faults were used. For FreeMind—which requires much more time to generate and run each test suite because of its larger GUI—1000 of the 5729 single-line faults in acceptable classes were initially selected at random, and of those the 970 faults located inside methods were used. Equivalent mutants were not accounted for in this experiment because it would have been infeasible to examine every mutant to see if it could lead to a failure.

4.2.4. Test execution and characteristic measurement. A test suite was generated for each fault in the sample. For each ⟨*test suite, fault*⟩ pair, each test case was executed on the clean version of the application and, if it covered the line containing the fault, on the faulty version. Test cases were executed by GUITAR on a cluster of Linux machines. Most of the computation time for the experiment was spent running test cases. With the parameters set for GUITAR, a test case of length L took at least $5 + 2.5L$ seconds to run on the clean version. For CrosswordSage, test suites consisted of 18 to 101 test cases (306 to 680 events); for FreeMind, 45 to 343 test cases (770 to 1178 events); we attribute the wide variance to the randomness built into the two-stage test-suite creation process.

To determine whether a test suite covered a faulty line (Cov), the coverage report from Instr was examined. To determine whether a test suite that covered a faulty line also detected the fault (Det), the oracle information collected by GUITAR for the clean and faulty versions was compared.

When the experiment was run, some false reports of fault detection were anticipated. Because of timing problems in the current version of the test-case-replayer component

[8] `http://cs.gmu.edu/~offut/mujava`

of GUITAR (an issue in other GUI-testing tools as well [Testbeds 2009]), test cases sometimes fail to run to completion, making it appear as if a fault has been detected when really it has not been. In addition, GUITAR by default detects even trivial differences in oracle information, such as in the coordinates of GUI components, which may not actually indicate fault detection.

To avoid false reports of fault detection, usually-trivial differences (e.g., any changes to the size or absolute position of GUI components) were ignored[9]. The *test-step number* (e.g., 4 for the 4th event in the test case) of the first non-trivial difference was determined. To find and fix false reports of fault detection, the test-step number at which fault detection was reported was checked against coverage reports to make sure that no fault was incorrectly detected before its line had been covered. For CrosswordSage, only one test case supposedly detected a fault before covering it (but this did not affect Det for the suite), and no false reports of fault detection were found in a manual inspection of the first 100 $\langle$*test suite, fault*$\rangle$ pairs, so no further checking was done. For FreeMind, there were more false reports of fault detection, so all $\langle$*test suite, fault*$\rangle$ pairs with $\mathsf{Det} = 1$ were manually inspected and corrected.

Some metrics of the faults and test suites could be measured before test execution. For faults, the mutant type was apparent from MuJava's output. The test-case length, size, event-pair coverage, and event-triple coverage of test suites were also known prior to execution. The remaining metrics were calculated from the coverage reports generated by Instr and Emma during execution. To allow comparison between results for CrosswordSage and FreeMind, metrics that vary with application size (e.g., number of events in a test suite, number of lines covered before a faulty line) were normalized (e.g., by number of events in the EFG, by number of executable lines in the application).

4.3. Data analysis

The goal of this experiment is to evaluate the strength and significance of the effects of test-suite and fault characteristics on coverage (Cov) and detection (Det) of faults. For data of this structure, logistic regression [Agresti 2007; Garson 2006; Rosner 2000] is the most popular analysis technique (although other techniques, such as Bayesian belief networks or decision trees, may be explored in future work). We chose logistic regression in large part because of its popularity, even canonicality, among statisticians: several recent introductory statistics textbooks cover logistic regression but not its alternatives [Agresti 2007; Garson 2006; Rosner 2000]. Logistic regression is commonly used to evaluate the effects of several explanatory variables on a binomial response variable (e.g., the effects of race and medication usage on the presence of AIDS symptoms [Agresti 2007]). It has occasionally been used in software-testing research [Briand et al. 2002; Frankl and Weiss 1991], although never to study test-suite and fault characteristics simultaneously. Given a data set, logistic-regression analysis finds the function—out of a certain class of functions—that best describes the relationship between the explanatory (independent) variables and the probability of the response (dependent) variable for that data set. The class of functions is versatile, encompassing functions that approximate linear, quadratic, and more complex relationships [Agresti 2007].

Logistic regression is so named because it uses the *logit* function,

$$\mathrm{logit}(x) = \log\left(\frac{x}{1-x}\right),$$

[9]A few real faults may also have been ignored, but it was not feasible to check all results by hand. One consequence is discussed in Section 6.5.

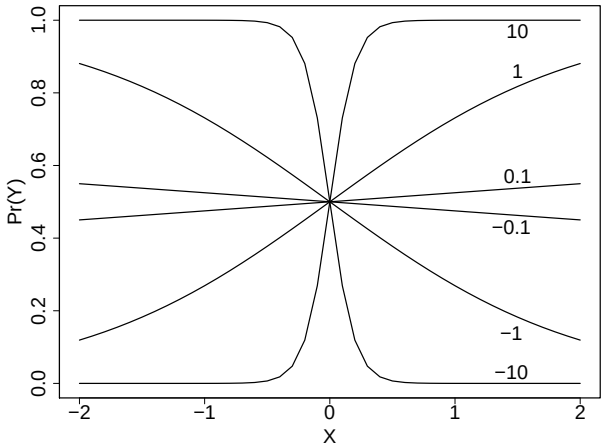


Fig. 4. Predicted probabilities for example logistic-regression models

to map probabilities—values between 0 and 1—onto the entire range of real numbers. For a dependent variable Y and a vector of independent variables $\vec{X}$, the logistic-regression model has the form

$$\text{logit}(\Pr(Y)) = \alpha + \vec{\beta} \cdot \vec{X}. \tag{1}$$

The intercept term α is related to the overall probability of Y, and, as explained below, the coefficients $\vec{\beta}$ show the strength of relationship between each element of $\vec{X}$ and Y. Note that $\text{logit}(\Pr(Y))$ equals the log of the odds of Y. Rewritten, Equation 1 expresses the probability of the dependent variable as a function of the independents:

$$\Pr(Y) = \frac{\exp(\alpha + \vec{\beta} \cdot \vec{X})}{1 + \exp(\alpha + \vec{\beta} \cdot \vec{X})}. \tag{2}$$

Figure 4 plots this function for $\vec{X} = X$, $\alpha = 0$, and $\vec{\beta} = \beta \in \{-10, -1, -0.1, 0.1, 1, 10\}$.

In logistic-regression analysis, a data set consists of a set of data points, each a vector of values for $\vec{X}$ (here, test-suite and fault characteristics) paired with a value for Y (here, Cov or Det). The goal of logistic regression is to find values for the intercept α and coefficients $\vec{\beta}$ that maximize the likelihood that the set of observed values of Y in the data set would have occurred given α, $\vec{\beta}$, and the observed values of $\vec{X}$. The process of choosing values for α and $\vec{\beta}$ is called *model fitting* and is accomplished by a maximum-likelihood-estimation algorithm.

Coefficients in a logistic-regression model indicate the magnitude and direction of each independent variable's relationship to the log of the odds of the dependent variable. If $\beta_i = 0$, then there is no relationship between X_i and Y; if $\beta_i < 0$, then the odds and probability of Y decrease as X_i increases; if $\beta_i > 0$, then the odds and probability of Y increase as X_i increases. However, the increase or decrease in the odds of Y is multiplicative, not additive; it is a factor not of β_i but of $\exp(\beta_i)$. More precisely, the ratio of the odds of Y when $X_i = x_i + \Delta$ to the odds of Y when $X_i = x_i$, when all other $X_j \in \vec{X}$ are held constant, is estimated to be

$$OR = \exp(\beta_i \Delta). \tag{3}$$

Table III. Data sets

Data set	Description
ALLFAULTS	All ⟨*test suite, fault*⟩ pairs All test-suite metrics Fault metrics not requiring execution data
POOLCOVFAULTS	Pairs where test pool covers fault All test-suite metrics All fault metrics
SUITECOVFAULTS	Pairs where test suite covers fault All test-suite metrics All fault metrics

Associated with each coefficient is a p-value for the chi-square test of deviance, a statistical test of whether the independent variable is actually related to the dependent variable, or whether the apparent relationship (non-zero coefficient) is merely due to chance. A p-value ≤ 0.05, indicating that there is only a 5% chance of seeing a fitted coefficient value that extreme under the null hypothesis, is typically considered to be statistically significant. For exploratory analysis (as in this experiment), p-values ≤ 0.10 may also be considered [Garson 2006].

4.3.1. Data sets. This study tests hypotheses about the effects of test-suite and fault characteristics on (1) the likelihood of a test suite detecting a fault and (2) the likelihood of a test suite detecting a fault, given that the test suite covers the fault. The hypotheses can be grouped according to these two dependent variables.

To explore the second group of hypotheses, a data set consisting of all ⟨*test suite, fault*⟩ pairs where the test suite covers the fault was constructed. This data set is called SUITECOVFAULTS, and its properties are described in Table III.

To explore the first group of hypotheses, a data set consisting of ⟨*test suite, fault*⟩ pairs where the test suite does not necessarily cover the fault was needed. Because of the way fault metrics are measured in this study, two data sets were actually constructed. Measurement of most of the fault metrics requires some information collected from executing the faulty line of code. But many of the faulty lines were not executed by any test case in the test pool, so no information was available for them. The ⟨*test suite, fault*⟩ pairs for which information was available were collected into a data set called POOLCOVFAULTS. All ⟨*test suite, fault*⟩ pairs were collected into a data set called ALLFAULTS, but the only fault metric in ALLFAULTS was the one that could be measured without execution information (F.MutType). Table III also lists the properties of POOLCOVFAULTS and ALLFAULTS.

4.3.2. Model fitting. For each data set, two kinds of logistic-regression models were fitted: univariate and multivariate. Each univariate model, which assesses the effect of an independent variable X_i by itself on the dependent variable Y, has the form

$$\text{logit}(\Pr(Y)) = \alpha + \beta_i X_i.$$

Each multivariate model, which assesses the contribution of each independent variable toward explaining the dependent variable in the context of the other independent variables, has the form in Equation 1, with $\vec{X}$ consisting of some subset of the independent variables for the data set. For the ALLFAULTS and POOLCOVFAULTS data sets, Y can be either Cov or Det; for SUITECOVFAULTS, Y can only be Det since Cov is always 1. Before model-fitting, all non-categorical data was centered[10]—the mean was

[10]In models with interaction terms, centering is "strongly recommended to reduce multicollinearity and aid interpretation of coefficients" [Garson 2006].

subtracted. Model-fitting and other statistical calculations were performed with the R software environment[11] [Ripley 2001].

A potential problem in fitting multivariate logistic-regression models—for which other studies of fault detection have been criticized [Fenton and Neil 1999]—is that strongly correlated ("multicolinear") independent variables can result in models with misleading coefficients and significance tests. If two or more multicolinear variables are included as parameters in a multivariate model, then none may appear to be statistically significant even if each is significant in its respective univariate model. Also, the fitted coefficients may be very different from those in the univariate models, even changing signs. Thus, the models may be overfitted to the data; they would not fit other data well. Although some correlation among model parameters is acceptable—indeed, the intention of multivariate analysis is to control for such correlation—for the purposes of this work it is desirable to avoid serious multicolinearity.

To avoid multicolinearity, as well as to provide multivariate models that are small enough to comprehend, a subset of the metrics in Table I may need to be selected for each model. There is no standard way to do this; to some extent, it is a process of trial and error [Slud 2008]. Only metrics that are close to being statistically significant in their univariate models ($p <= 0.10$) are considered. Correlations among these metrics turn out to be complex. Not surprisingly, groups of metrics measuring the same characteristic tend to be strongly correlated. However, for the test-suite characteristic of coverage level, the code-coverage metrics (T.Class, T.Meth, T.Block, and T.Line) and the event-coverage metrics (T.Pairs and T.Triples) are strongly correlated amongst themselves but not as strongly correlated between the two groups. Between metrics measuring different characteristics, some correlations also arise—for example, between T.Pairs and T.Len—and vary in strength across data sets. To re-group the data according to correlation, we tried principal-component analysis, but it proved unhelpful: many metrics did not fall neatly into one component or another. For each data set, then, the problem of selecting the subset of metrics that form the best-fitting multivariate model for the dependent variable, while not being too strongly correlated to each other, was an optimization problem.

We reduced the problem size and then applied a brute-force solution. To reduce the problem size, metrics were grouped according to the characteristic they measured, with the exception that the test-suite coverage metrics were split into code-coverage and event-coverage groups. While only a heuristic, this grouping makes sense because, as stated above, metrics within each group tend to be strongly correlated. To find the best-fitting multivariate model, a program was written in R to fit logistic-regression models made up of every combination of metrics that could be formed by choosing one metric from each group. (This brute-force solution was feasible because there were few enough groups.) The model with the lowest AIC (Section 4.3.3) and without severe multicolinearity[12] was chosen. This is the *main-effects* model.

Each main-effects model was expanded by adding interaction effects of interest—namely, those between a test-suite metric and a fault metric. The models were then reduced by stepwise regression based on AIC to eliminate independent variables and interactions whose contribution toward explaining the dependent variable is negligible. The result is the *interaction-effects* model. This is the multivariate model presented in the next section.

[11] `http://www.r-project.org/`

[12] In this experiment, the only criterion for severe multicolinearity is unexpected coefficient signs. For a more stringent definition of multicolinearity, some maximum allowable correlation value would have to be chosen arbitrarily.

Table IV. Data summary: ALLFAULTS

	CrosswordSage			FreeMind 0.7.1		
	Min.	Mean	Max.	Min.	Mean	Max.
F.MutType	0	0.474	1	0	0.705	1
T.Len	6	13.0	20	3	11.6	20
T.Events	3.12	5.00	6.94	3.44	4.35	5.26
T.Class	0.556	0.697	0.750	0.749	0.795	0.815
T.Meth	0.328	0.454	0.490	0.521	0.569	0.595
T.Block	0.361	0.509	0.552	0.419	0.485	0.524
T.Line	0.345	0.484	0.525	0.423	0.488	0.525
T.Pairs	0.192	0.272	0.338	0.0141	0.0230	0.0286
T.Triples	0.0198	0.0337	0.0499	0.0000554	0.0001540	0.0002132

4.3.3. Goodness of fit. To evaluate the multivariate models' goodness of fit to the data, several measures were used. One was deviance [Agresti 2007; Garson 2006], which indicates how much of the variance in the dependent variable fails to be explained by the model[13]; lower deviance means better fit. (R^2, a similar measure for linear-regression models, is not applicable to logistic-regression models). The deviance should be interpreted relative to the null deviance, which is the deviance of a model consisting of just a constant term (intercept). Some statistical tests of goodness of fit involve comparing the deviance to the null deviance. Another goodness-of-fit measure used was Akaike's "an information criterion" (AIC) [Agresti 2007; Garson 2006], a function of the deviance and the number of independent variables in the model that balances model fit with parsimony (fewer variables); lower AIC is better.

Other measures of goodness of fit used, which may be more familiar outside the statistics community, were sensitivity and specificity [Agresti 2007; Garson 2006]. These have to do with the number of correct classifications ("predictions") made by the model on the data to which it was fit. Although probabilities predicted by logistic-regression models may fall anywhere between 0 and 1, they can be classified as 0 ("negative") or 1 ("positive") using the sample mean of the dependent variable as the cutoff. For example, for the ALLFAULTS data set for CrosswordSage, 27.0% of Det values are 1, so model predictions < 0.270 are considered to be 0 and those > 0.270 are considered to be 1. Sensitivity is the proportion of actual positives (e.g., $\mathsf{Det} = 1$) correctly classified as such. Specificity is the proportion of actual negatives correctly classified as such.

5. RESULTS

Tables IV, V, and VI summarize the three data sets to be analyzed, which were described in Table III. These tables list the minimum, mean, and maximum of each independent variable in each data set. Understanding the range of the data will help to interpret the logistic-regression models presented in this section. Some key observations about the data sets can also be made.

The first observation is that the mean values of the test-suite metrics are nearly the same for the ALLFAULTS and POOLCOVFAULTS data sets. For example, the mean value of T.Class is 0.697 in ALLFAULTS and 0.694 in POOLCOVFAULTS for CrosswordSage, and 0.795 in both data sets for FreeMind. The second observation is that, for the mean value of the one fault metric shared between ALLFAULTS and POOLCOVFAULTS,

[13]More precisely, deviance is a function of "the probability that the observed values of the dependent may be predicted by the independents" [Garson 2006]. This probability is called the *likelihood*. The deviance of a model is actually $-2(L_M - L_S)$, where L_M is the log of the likelihood for the model and L_S is the log of the likelihood for a perfectly-fitting model.

Table V. Data summary: POOLCOVFAULTS

	CrosswordSage			FreeMind 0.7.1		
	Min.	Mean	Max.	Min.	Mean	Max.
F.MutType	0	0.448	1	0	0.759	1
F.CovBef	0.0000	0.0736	0.2165	0.000	0.138	0.292
F.Depth	0.000	0.291	0.833	0.000	0.397	1.000
F.SomeRep	0	0.323	1	0	0.745	1
F.AllRep	0	0.304	1	0	0.176	1
F.MinPred	0.0000	0.0293	0.2041	0.00000	0.02450	0.77679
F.MaxPred	0.0000	0.1560	0.2041	0.00000	0.67410	0.77679
F.MinSucc	0.0102	0.0592	0.2041	0.00446	0.14032	0.75893
F.MaxSucc	0.0204	0.1660	0.2041	0.01786	0.66597	0.75893
F.Events	0.0102	0.0698	0.4388	0.00446	0.54611	0.96429
F.MinWith	0.000461	0.060423	0.171350	0.000107	0.041343	0.143253
F.MaxWith	0.00322	0.12423	0.17181	0.0576	0.1552	0.1768
T.Len	6	12.9	20	3	11.6	20
T.Events	3.12	5.02	6.89	3.44	4.34	5.22
T.Class	0.556	0.694	0.750	0.749	0.795	0.815
T.Meth	0.328	0.453	0.487	0.521	0.568	0.595
T.Block	0.361	0.508	0.552	0.420	0.484	0.524
T.Line	0.345	0.483	0.525	0.424	0.487	0.525
T.Pairs	0.192	0.272	0.336	0.0141	0.0230	0.0286
T.Triples	0.0198	0.0337	0.0460	0.0000554	0.0001545	0.0002132

Table VI. Data summary: SUITECOVFAULTS

	CrosswordSage			FreeMind 0.7.1		
	Min.	Mean	Max.	Min.	Mean	Max.
F.MutType	0	0.429	1	0	0.711	1
F.CovBef	0.0000	0.0716	0.2165	0.000	0.116	0.238
F.Depth	0.000	0.283	0.833	0.000	0.330	0.667
F.SomeRep	0	0.319	1	0	0.731	1
F.AllRep	0	0.306	1	0	0.166	1
F.MinPred	0.0000	0.0277	0.2041	0.00000	0.00931	0.02679
F.MaxPred	0.0000	0.1631	0.2041	0.00000	0.65850	0.77679
F.MinSucc	0.0102	0.0589	0.2041	0.00446	0.12396	0.75893
F.MaxSucc	0.0204	0.1718	0.2041	0.01786	0.65450	0.75890
F.Events	0.0102	0.0729	0.4388	0.00446	0.59915	0.96429
F.MinWith	0.000461	0.057481	0.171350	0.000107	0.038661	0.137178
F.MaxWith	0.00322	0.12132	0.17181	0.0666	0.1596	0.1768
T.Len	6	12.9	20	3	11.5	20
T.Events	3.12	5.02	6.89	3.44	4.34	5.22
T.Class	0.556	0.703	0.750	0.763	0.796	0.815
T.Meth	0.328	0.457	0.487	0.521	0.569	0.595
T.Block	0.361	0.514	0.552	0.420	0.486	0.524
T.Line	0.345	0.489	0.525	0.424	0.489	0.525
T.Pairs	0.192	0.272	0.336	0.0141	0.0230	0.0286
T.Triples	0.0198	0.0337	0.0460	0.0000553	0.0001539	0.0002132

Table VII. Data summary: Cov and Det

	CrosswordSage						
	Cov			Det			Total
	0	1	Mean	0	1	Mean	
ALLFAULTS	1147	1083	0.486	1629	601	0.270	2230
POOLCOVFAULTS	101	1083	0.915	583	601	0.508	1184
SUITECOVFAULTS	0	1083	1.000	482	601	0.555	1083
	FreeMind 0.7.1						
	Cov			Det			Total
	0	1	Mean	0	1	Mean	
ALLFAULTS	506	464	0.478	800	170	0.175	970
POOLCOVFAULTS	137	464	0.772	431	170	0.283	601
SUITECOVFAULTS	0	464	1.000	294	170	0.366	464

F.MutType, the difference between data sets is greater—0.474 vs. 0.448 for CrosswordSage, 0.705 vs. 0.759 for FreeMind. Since ALLFAULTS includes all ⟨*test suite, fault*⟩ pairs, while POOLCOVFAULTS includes just the ⟨*test suite, fault*⟩ pairs where the test pool covers the fault—a factor dependent on the fault but not on the test suite—it makes sense for the test-suite characteristics to stay the same but the fault characteristics to differ.

The third observation is that the mean values of many of the fault characteristics differ noticeably between the POOLCOVFAULTS and the SUITECOVFAULTS data sets. For example, the mean value of F.CovBef is 0.0736 in POOLCOVFAULTS and 0.0716 in SUITECOVFAULTS for CrosswordSage, and 0.138 in POOLCOVFAULTS and 0.116 in SUITECOVFAULTS for FreeMind. The fourth observation is that the mean values of the test-suite characteristics do not seem to differ much, but they sometimes differ a little more between POOLCOVFAULTS and SUITECOVFAULTS than between ALLFAULTS and POOLCOVFAULTS. For example, the mean value of T.Line for CrosswordSage is 0.484 in ALLFAULTS, 0.483 in POOLCOVFAULTS, and 0.489 in SUITECOVFAULTS. Since SUITECOVFAULTS includes just the ⟨*test suite, fault*⟩ pairs where the test suite covers the fault—a factor dependent on both the fault and the test suite—it would make sense for both fault characteristics and test-suite characteristics to differ between SUITECOVFAULTS and the other data sets.

These observations concern only the apparent differences between the data sets. Whether those differences are statistically meaningful, and whether they are in the expected direction, will be examined later in this section.

A final observation is that the levels of coverage in this experiment are rather low, by research standards if not by industrial standards. Even though each test suite executes each event in the EFG an average of 3 to 7 times (T.Events), class coverage (T.Class) falls at less than 82% and line coverage (T.Line) at less than 53%. With a few tweaks in test-case generation, coverage could have been improved (e.g., by providing more input files or forcing more test cases to open input files), but only at the expense of the randomness and replicability of the test suites. Since fault detection seems to increase super-linearly with coverage [Hutchins et al. 1994], different results might be obtained with greater coverage levels.

Table VII summarizes the dependent variables, Cov and Det, for each data set, giving the frequency of each value (0 or 1), the mean (proportion of 1 values), and the total number of data points. With each successive data set, from ALLFAULTS to POOLCOVFAULTS to SUITECOVFAULTS, more data points with $\mathsf{Cov} = 0$ (and therefore $\mathsf{Det} = 0$) are eliminated, so the proportion of Cov and Det grows.

In the rest of this section, the three data sets are used to test the hypotheses listed in Section 4 by fitting logistic-regression models to them. These hypotheses ask questions about whether any test-suite or fault characteristics affect fault detection. Sections 5.2 and 5.3 directly answer those questions. But first, as a stepping stone toward those answers, Section 5.1 explores whether test-suite and fault characteristics affect fault coverage. In addition, Section 5.1 explains how to interpret the logistic-regression models.

5.1. What characteristics affect whether a test suite covers a faulty line?

For this work, understanding the fault and test-suite characteristics that affect coverage of faulty lines is not an end in itself. Rather, it assists in understanding the characteristics that affect detection of faults.

Because the study of coverage plays only a supporting role in this work, the experiment set-up is not ideal for drawing conclusions about coverage. Only a limited sample of lines of source code is studied—those where faults happened to be seeded—and lines with more opportunities for fault seeding are more likely to be in the sample. This is a threat to the validity of conclusions drawn about the hypotheses below, but it is not a threat to the validity of the main experiment (i.e., Hypotheses H1–H6 in Section 4).

Hypotheses: This section examines the following null hypotheses for each fault characteristic and each test-suite characteristic:

— H7: The fault characteristic does not affect a fault's likelihood of being covered by a test suite.
— H8: The test-suite characteristic does not affect a fault's likelihood of being covered by a test suite.
— H9: No interaction effect between a fault characteristic and a test-suite characteristic affects a fault's likelihood of being covered by a test suite.

(Hypothesis H8 also applies to test-suite characteristics.) These hypotheses correspond to Hypotheses H1, H2, and H5 from Section 4, except that the dependent variable is fault coverage instead of fault detection.

Data sets: Two of the data sets can be used to test these hypotheses: ALLFAULTS and POOLCOVFAULTS. Each addresses the hypotheses from a different perspective. The POOLCOVFAULTS data set shows what happens only for faults covered by at least one test case in the test pool. The ALLFAULTS data set shows what happens for faults regardless of their coverage, but, as a consequence, only considers the one fault characteristic that can be measured without coverage data (fault's method of creation). For both data sets, the dependent variable is Cov. The SUITECOVFAULTS data set cannot be used because it only contains faults where Cov is 1.

To show empirically that a test-suite or fault characteristic affects the likelihood that a test suite covers a fault, at least one metric measuring that characteristic must have a statistically significant relationship to coverage in at least one data set. Consistent results across metrics for a characteristic and across applications bolster the evidence.

For some metrics, it is obvious even without empirical evidence that they affect coverage of faulty lines. The probability of Cov is bound to increase, on average, as T.Class, T.Meth, T.Block, and T.Line increase, and it cannot decrease as T.Events, T.Pairs, and T.Triples increase. Certainly F.CovBef, F.Depth, F.MinPred, and F.MaxPred affect coverage as well: all faults with a value of 0 for these variables are guaranteed to be covered, since they lie in initialization code that is executed by every test case. For other metrics, like those measuring the size of event handlers (F.MinWith and F.MaxWith), their relationship to coverage can only be discovered by analyzing the data.

Table VIII. Univariate models: ALLFAULTS, Cov

		CrosswordSage			FreeMind 0.7.1		
		Int.	Coef.	Sig.	Int.	Coef.	Sig.
Meth. of creation	F.MutType	0.108	-0.349	***	-0.126	0.0558	
Granularity	T.Len	-0.0575	-0.0131		-0.0867	-0.00588	
Size	T.Events	-0.0574	0.0901		-0.0867	-0.107	
Proportion of coverage	T.Class	-0.0581	2.16	***	-0.0874	12.3	*
	T.Meth	-0.0582	4.28	***	-0.0868	4.85	
	T.Block	-0.0582	3.28	***	-0.0869	4.27	
	T.Line	-0.0582	3.54	***	-0.0869	4.28	
	T.Pairs	-0.0574	1.15		-0.0867	0.265	
	T.Triples	-0.0574	2.45		-0.0867	-76.4	

Table IX. Univariate models: POOLCOVFAULTS, Cov

		CrosswordSage			FreeMind 0.7.1		
		Int.	Coef.	Sig.	Int.	Coef.	Sig.
Meth. of creation	F.MutType	2.84	-0.875	***	2.50	-1.54	***
Distance from initial state	F.CovBef	2.48	-14.1	***	2.93	-43.3	***
	F.Depth	2.53	-4.34	***	1.91	-6.70	***
Repetitions	F.SomeRep	2.46	-0.255		1.50	-0.361	
	F.AllRep	2.35	0.0888		1.28	-0.30	
Degrees of freedom	F.MinPred	2.41	-6.34	***	-1.11	-197	***
	F.MaxPred	2.81	13.1	***	1.25	-1.25	**
	F.MinSucc	2.37	-1.09		1.23	-0.874	**
	F.MaxSucc	2.95	21.7	***	1.24	-1.09	*
	F.Events	2.90	27.8	***	1.34	1.99	***
Size of event handlers	F.MinWith	2.62	-18.4	***	1.24	-6.79	**
	F.MaxWith	2.62	-14.7	***	1.33	24.3	***
Granularity	T.Len	2.37	-0.0164		1.22	-0.0205	
Size	T.Events	2.37	-0.0314		1.22	-0.00992	
Proportion of coverage	T.Class	3.37	19.6	***	1.25	28.7	***
	T.Meth	3.28	36.5	***	1.23	14.8	*
	T.Block	3.31	28.1	***	1.24	12.5	**
	T.Line	3.30	30.2	***	1.24	12.7	**
	T.Pairs	2.37	0.86		1.22	-22.3	
	T.Triples	2.37	-3.83		1.22	-2150	

5.1.1. Univariate models. Univariate logistic-regression models (Section 4.3) were fit to the ALLFAULTS and POOLCOVFAULTS data sets, with Cov as the dependent variable. Tables VIII and IX summarize the univariate models, giving intercepts, coefficients, and significance levels. For example, the first row of Table VIII for CrosswordSage represents the model

$$\text{logit}(\Pr(\mathsf{Cov})) = 0.108 + -0.349\mathsf{F.MutType}.$$

Metrics with significance levels of "$\circ$", "*", "**", and "***" have p-values less than or equal to 0.1, 0.05, 0.01, and 0.001, respectively. The smaller the p-value, the more likely it is that the theoretical (true) coefficient value is non-zero and has the same sign as the estimated coefficient. If the p-value is greater than 0.10, the coefficient value is considered to be statistically meaningless—it is too likely to be non-zero due to random variation—and therefore it is ignored in the analysis.

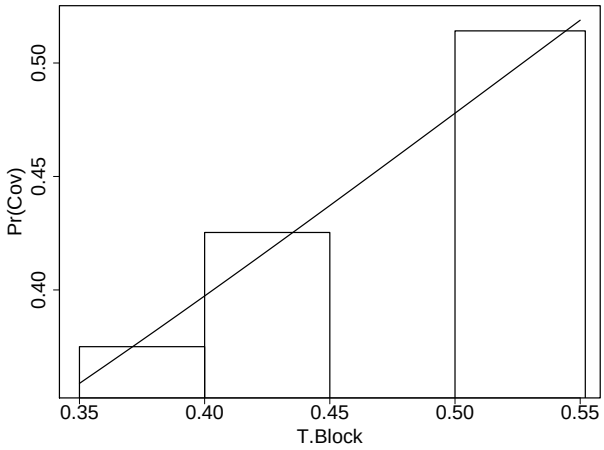


Fig. 5. Predicted probabilities for CrosswordSage T.Block model in Table VIII

This section alternates between a tutorial on interpreting logistic-regression models and a description of major results, with sub-sections labeled accordingly. Readers familiar with logistic-regression analysis may skip the tutorial.

Tutorial. Recall from Section 4.3 that a univariate model shows how much an independent variable (fault or test-suite metric) affects the likelihood of the dependent variable (Cov), without controlling for any other independent variables.

To understand how to interpret the coefficients in these models, consider T.Block for CrosswordSage in Table VIII. The coefficient, 3.28, means that the odds of Cov are expected to increase by a factor of $\exp(3.28)^{\Delta}$ when T.Block increases by Δ (Equation 3). For example, if T.Block increases from 0.4 to 0.5, the odds of covering a given faulty line are expected to increase by a factor of $\exp(3.28)^{0.5-0.4} \approx 1.39$. Figure 5 shows how this translates to probabilities. The plotted curve represents the predicted probability of Cov across the range of T.Block (similar to Figure 4). (The near-linear shape of the curve demonstrates the ability of logistic-regression functions to mimic other functions, such as linear functions.) For comparison, the curve is superimposed on a bar plot of the sample probabilities of Cov at different levels of T.Block in the ALLFAULTS data set. (No T.Block values fall between 0.45 and 0.5, so no bar is drawn.) The predicted probability for a particular value of T.Block is found by subtracting the mean of T.Block for the data set (0.509)—since the models are fit to centered data—to get X, and then plugging X, the intercept (-0.0582), and the coefficient (3.28) into Equation 2. For instance, for a T.Block value of 0.540, X is $0.540 - 0.509 = 0.031$, and the predicted probability of Cov is $\exp(-0.0582 + 3.28 * 0.031)/(1 + \exp(-0.0582 + 3.28 * 0.031)) = 0.511$.

For certain metrics—F.MutType, F.SomeRep, and F.AllRep—X can only be 0 or 1. In Table IX for FreeMind, for example, the predicted probability of Cov for method-level mutation faults (encoded as 1) is $\exp(2.50 + -1.54 * 1)/(1 + \exp(2.50 + -1.54 * 1)) = 0.724$, whereas for class-level mutation faults (encoded as 0) it is $\exp(2.50 + -1.54 * 0)/(1 + \exp(2.50 + -1.54 * 0)) = 0.924$.

While it is important to understand how to interpret the magnitude of model coefficients—or at least to understand that they do not denote a linear relationship with the probability of the dependent variable—the analysis to follow focuses on the significance levels, the signs, and occasionally the relative magnitudes of coefficients.

Major results. The univariate models can address Hypotheses H7 and H8, which asked whether any fault characteristic or test-suite characteristic affects a fault's like-

Table X. Multivariate models: ALLFAULTS, Cov

	CrosswordSage		FreeMind 0.7.1	
	Coef.	Sig.	Coef.	Sig.
Intercept	0.105		-0.0874	
F.MutType	-0.346	***		
T.Class			12.3	*
T.Block	3.25	***		
Null deviance	3089.6		1342.9	
Deviance	3056.8		1338.6	
AIC	3062.8		1342.6	
Sensitivity	$444/1083 = 0.410$		$395/464 = 0.851$	
Specificity	$785/1147 = 0.684$		$99/506 = 0.196$	

lihood of being covered by a test suite. (The multivariate models will address Hypothesis H9, which concerns interaction effects.) In the univariate models (Tables VIII and IX), most of the fault characteristics do affect coverage: the fault's method of creation (F.MutType, except for FreeMind in FreeMind's ALLFAULTS model), its distance from the initial state (F.CovBef and F.Depth), its degrees of freedom in execution (F.MinPred, F.MaxPred, F.MinSucc for FreeMind, F.MaxSucc, and F.Events), and the size of its enclosing event handlers (F.MinWith and F.MaxWith). One fault characteristic does not affect Cov: the repetitions in which the faulty code is executed (F.SomeRep and F.AllRep). This makes sense because a faulty line only has to be executed once to be covered.

In the univariate models, just one test-suite characteristic affects the likelihood of covering a fault: the proportion of the application that the test suite covers (T.Class for all models; T.Meth, T.Block, and T.Line for all but FreeMind's ALLFAULTS model). The event-coverage metrics (T.Pairs and T.Triples), the length of test cases (T.Len), and the size of the test suite (T.Events) do not significantly affect fault coverage. For the event-coverage metrics, the small range of metric values studied (Tables IV and V) may not be sufficient to show effects on fault coverage.

In addition to identifying the characteristics that affect fault coverage, it is worth observing the direction of the effect, indicated by the model coefficients. For the one significant test-suite characteristic—proportion of coverage—fault coverage increases with a test suite's proportion of coverage, as expected. For the fault characteristic of distance from the initial state, coefficients are negative for both metrics and both applications—meaning that, as expected, faulty lines lying farther from the initial state are less likely to be covered. For the fault characteristics of degrees of freedom and size of event handlers, the signs of coefficients are inconsistent across metrics and across applications. For the fault characteristic of method of creation, as measured by F.MutType, the results are the same in three of four models but surprising: class-level mutation faults are more likely to be covered than method-level mutation faults. Or, put another way, class-level mutation faults were more likely than method-level mutation faults to be seeded in code covered by the test pool.

5.1.2. Multivariate models. Multivariate models (Section 4.3.2) were fit to the ALLFAULTS and POOLCOVFAULTS data sets, again with Cov as the dependent variable. Tables X and XI summarize the multivariate models for the two data sets. For example, the model for CrosswordSage in Table X is

$$\text{logit}(\Pr(\text{Cov})) = 0.105 + -0.346\text{F.MutType} + 3.25\text{T.Block}.$$

The lower portion of each table lists several measures of goodness of fit (Section 4.3.3).

Table XI. Multivariate models: POOLCOVFAULTS, Cov

	CrosswordSage		FreeMind 0.7.1	
	Coef.	Sig.	Coef.	Sig.
Intercept	10.6		3.19	
F.MutType			-1.16	***
F.CovBef	-142	***	-50.9	***
F.MinPred			-106	***
F.MaxPred	67.2	***		
F.MinWith			-13.7	*
F.MaxWith	-113	***		
T.Class			110	***
T.Block	-39.6	***		
T.Class × F.CovBef			-1150	***
T.Block × F.CovBef	2350			
T.Block × F.MaxPred	-1940	***		
T.Block × F.MaxWith	3260	***		
Null deviance	690.36		645.22	
Deviance	58.81		308.82	
AIC	74.81		322.82	
Sensitivity	$1059/1083 = 0.978$		$376/464 = 0.810$	
Specificity	$100/101 = 0.990$		$122/137 = 0.891$	

Recall from Section 4.3.2 that each multivariate model uses the subset of all metrics that provides the best balance of model fit and parsimony (i.e., the lowest AIC) without severe symptoms of multicolinearity. For each "best" multivariate model, often several similar models are nearly as good; for example, for a model that includes T.Line, models with T.Block, T.Meth, or T.Class in its place often have similar AIC values.

Tutorial. Recall from Section 4.3.2 that a multivariate model can be used to assess each independent variable's contribution to explaining the dependent variable in the context of the other independent variables. An independent variable's coefficient in a multivariate model shows how much it affects the dependent variable, relative to the other metrics in the model, when the other metrics are held constant.

As with the univariate models, predicted probabilities of the dependent variable, Cov, can be calculated by plugging values for the independent variables into Equation 2. Coefficients should be interpreted relative to other coefficients. For example, in the model for FreeMind in Table XI, the coefficient of F.MinPred (-106) is almost 8 times that of F.MinWith (-13.7). Thus, the model estimates that a change of Δ in F.MinPred would increase or decrease the odds of Cov by roughly the same factor that a change of 8Δ in F.MinWith would, if all other metrics were held constant. For instance, when F.MinPred increases from 0.01 to 0.02, the odds of Cov are predicted to decrease by a factor of $\exp(-106)^{0.01} = 0.346$. When F.MinWith increases from 0.01 to 0.09, the odds of Cov are predicted to decrease by roughly the same factor: $\exp(-13.7)^{0.08} = 0.334$.

Interaction effects in a model can be understood by looking at the signs of their coefficients or by calculating predicted probabilities. Consider the interaction between T.Class and F.CovBef (T.Class × F.CovBef) for FreeMind in Table XI. The positive coefficient of T.Class and the negative coefficient of F.CovBef imply that the probability of Cov generally increases as T.Class increases or F.CovBef decreases. But when these metrics have opposite signs in the centered data (i.e., one is above its mean and the other is below its mean in the original data), their product is negative, and this multiplied by the negative coefficient of T.Class × F.CovBef is positive—boosting the probability of Cov. Conversely, if T.Class and F.CovBef have the same sign in the centered data,

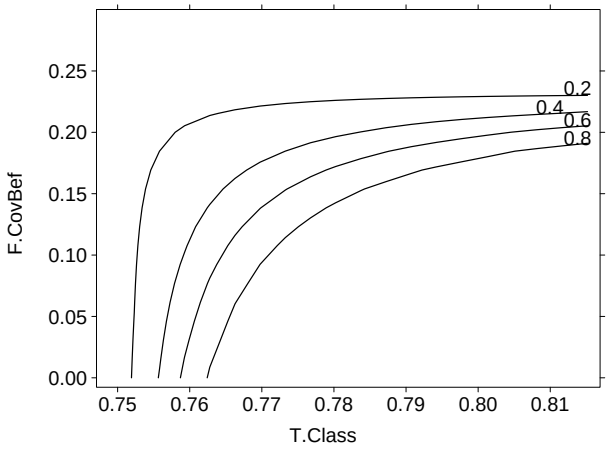


Fig. 6. Predicted probabilities for FreeMind model in Table XI

the interaction effect diminishes the probability of Cov. To illustrate, the contour plot[14] in Figure 6 shows the predicted probabilities at various levels of T.Class and F.CovBef when F.MutType is held constant at 0 and F.MinPred and F.MinWith are held constant at their respective means. The basic idea is that test suites with high class coverage (T.Class) are especially good at covering lines at a small distance from the initial state (F.CovBef), and test suites with low class coverage are especially bad at covering lines at a great distance from the initial state.

Major results. The multivariate models can again address Hypotheses H7 and H8—whether any fault characteristic or test-suite characteristic affects a fault's likelihood of being covered by a test suite. For both applications, the multivariate models (Tables X and XI) show that, when all other characteristics are held constant, a fault's distance from the initial state, a fault's degrees of freedom, and a test suite's proportion of coverage all affect fault coverage. A fault's method of creation also affects fault coverage in CrosswordSage's ALLFAULTS model and FreeMind's POOLCOVFAULTS model. The set of metrics chosen to represent each characteristic in the model is only consistent across applications for a fault's distance from the initial state (F.CovBef).

The multivariate models can also address Hypothesis H9—whether any interaction effects between a fault characteristic and a test-suite characteristic affect a fault's likelihood of being covered by a test suite. For FreeMind, the interaction between a test suite's coverage and a fault's distance from the initial state (T.Class $\times$ F.CovBef) significantly affects coverage. The negative coefficient indicates that test suites that cover many classes are especially good at covering faults lying closer to the initial state, while test suites that cover few classes are especially bad at covering such faults (Figure 6). For CrosswordSage, the interaction between a test suite's coverage and a fault's degrees of freedom (T.Block $\times$ F.MaxPred, T.Block $\times$ F.MaxWith) significantly affects fault coverage, but the direction of the effect depends on the metric.

CrosswordSage's ALLFAULTS model contains just one characteristic (test suite's proportion of coverage), since it was the only one significant in the univariate models. But

[14]Each line of the contour plot represents all values of T.Class and F.CovBef for which the predicted probability of Cov equals the value on the line label. For example, for each point on the uppermost line, the predicted probability of Cov is 0.20.

Table XII. Univariate models: ALLFAULTS, Det

		CrosswordSage			FreeMind 0.7.1		
		Int.	Coef.	Sig.	Int.	Coef.	Sig.
Meth. of creation	F.MutType	-1.10	0.211	*	-1.16	-0.577	**
Granularity	T.Len	-0.997	-0.00597		-1.55	0.0120	
Size	T.Events	-1.00	0.230	**	-1.55	0.362	
Proportion of coverage	T.Class	-0.999	1.07	○	-1.55	-4.10	
	T.Meth	-0.999	2.41	*	-1.55	-0.128	
	T.Block	-0.999	1.85	*	-1.55	0.177	
	T.Line	-0.999	2.01	*	-1.55	0.154	
	T.Pairs	-1.00	5.78	**	-1.55	39.9	
	T.Triples	-0.999	23.2	**	-1.55	2520	

Table XIII. Univariate models: POOLCOVFAULTS, Det

		CrosswordSage			FreeMind 0.7.1		
		Int.	Coef.	Sig.	Int.	Coef.	Sig.
Meth. of creation	F.MutType	-0.209	0.536	***	-0.124	-1.12	***
Distance from initial state	F.CovBef	0.0296	-6.56	***	-1.10	-12.1	***
	F.Depth	0.0305	-1.05	**	-1.12	-3.79	***
Repetitions	F.SomeRep	-0.112	0.444	***	-0.606	-0.445	*
	F.AllRep	-0.131	0.537	***	-1.03	0.532	*
Degrees of freedom	F.MinPred	0.0293	-3.87	**	-3.07	-144	***
	F.MaxPred	0.0292	3.76	***	-0.971	-2.00	***
	F.MinSucc	0.0304	0.744		-0.935	-0.531	
	F.MaxSucc	0.0202	10.3	***	-0.972	-2.25	***
	F.Events	0.0409	7.89	***	-0.953	-0.984	***
Size of event handlers	F.MinWith	0.0293	-8.83	***	-0.965	10.9	***
	F.MaxWith	0.0304	0.699		-0.930	-0.209	
Granularity	T.Len	0.0304	0.00198		-0.931	0.012	
Size	T.Events	0.0305	0.246	*	-0.936	0.504	○
Proportion of coverage	T.Class	0.0303	2.30	**	-0.931	-3.16	
	T.Meth	0.0302	4.89	***	-0.930	1.43	
	T.Block	0.0302	3.70	***	-0.930	1.41	
	T.Line	0.0302	4.01	***	-0.930	1.45	
	T.Pairs	0.0305	7.35	**	-0.934	41.1	
	T.Triples	0.0305	31.0	*	-0.932	2450	

for the rest of the models, it is a combination of test-suite and fault characteristics that best explains fault coverage.

The models for the POOLCOVFAULTS data set fit the data well. For CrosswordSage, the fit is nearly perfect, with sensitivity and specificity at 97.8% and 99.0%. For FreeMind, the fit is not quite as good, with sensitivity and specificity at 81.0% and 89.1%. The models for the ALLFAULTS data set, having fewer fault metrics to work with, do not fit nearly as well.

5.2. What characteristics affect whether a test suite detects a fault?

Considering the widespread use of code coverage as a predictor of fault detection, one might assume that the same characteristics that affect whether a test suite covers faulty code would also affect whether a test suite detects a fault. Our results show that this is not necessarily the case.

Hypotheses: This section examines hypotheses H1, H2, and H5 from Section 4. These hypotheses state that no fault characteristic, test-suite characteristic, or interaction effect between the two affects a fault's likelihood of being detected by a test suite.

Data sets: To test these hypotheses, the same data sets as in the previous section can be used: ALLFAULTS and POOLCOVFAULTS. The dependent variable now is Det instead of Cov.

5.2.1. Univariate models. Tables XII and XIII summarize the univariate models of Det for the two data sets.

In all of these models, all of the fault characteristics affect fault detection. This includes all of the fault characteristics that affected fault coverage (Tables VIII and IX), as well as repetitions in execution (F.SomeRep and F.AllRep). For FreeMind's ALLFAULTS model, a fault's method of creation affects its likelihood of detection but not its likelihood of coverage.

For CrosswordSage, two of the three test-suite characteristics affect fault detection: size and proportion of coverage. Size and the event-coverage metrics for proportion of coverage (T.Pairs and T.Triples) both affect fault detection without affecting fault coverage. For FreeMind, the only characteristic that is near significant is proportion of coverage (T.Events in the POOLCOVFAULTS model). For neither application does granularity affect fault detection; the same was true for fault coverage.

For most test-suite and fault characteristics that affected fault coverage as well as fault detection—fault's distance from the initial state, fault's degrees of freedom, fault's size of event handlers, test suite's proportion of coverage—the direction of coefficients is the same as before: either consistent with expectations (e.g., fault's distance from the initial state) or inconsistent among metrics and applications (e.g., fault's degrees of freedom). For the fault characteristic of repetitions in execution, the coefficient direction could be expected to go either way—faulty code inside a loop could be executed in more states (higher detection), but it could fail only in a few states (lower detection). In three of the four cases here, it leads to higher detection, while in the other case (F.SomeRep in FreeMind's POOLCOVFAULTS model) it leads to lower detection. Interestingly, for CrosswordSage, the fault characteristic of method of creation has opposing effects on fault coverage and fault detection: class-level mutation faults are more likely to be covered but less likely to be detected.

To see whether metrics have a stronger effect on fault detection or fault coverage, the confidence intervals of *significant* coefficients in the models of fault detection (Tables XII and XIII) can be compared to those in the models of fault coverage (Tables VIII and IX).[15] For CrosswordSage's ALLFAULTS models, only the difference in coefficients for F.MutType (opposite signs) is meaningful. For FreeMind's ALLFAULTS models, no metrics were significant in both. For both applications' POOLCOVFAULTS models, most of the differences in coefficients are statistically meaningful. (Exceptions are F.MinPred for CrosswordSage and F.MutType, F.MinPred, and F.MaxPred for FreeMind.) Thus, for example, a fault's distance from the initial state, as measured by either F.CovBef or F.Depth, has a stronger effect on fault coverage than on fault detection.

5.2.2. Multivariate models. Tables XIV and XV summarize the multivariate models of Det for the two data sets.

[15]Differences in coefficient magnitudes are meaningful only if both coefficients are statistically significant and their confidence intervals do not overlap. (A sufficient condition is that the coefficients have opposite signs.) The 95% Wald confidence interval for a coefficient β_i is $\beta_i \pm 1.96SE$, where SE is the standard error for β_i. [Agresti 2007]

Table XIV. Multivariate models: ALLFAULTS, Det

	CrosswordSage		FreeMind 0.7.1	
	Coef.	Sig.	Coef.	Sig.
Intercept	-1.11		-1.16	
F.MutType	0.213	*	-0.577	**
T.Line	0.363	*		
T.Pairs	5.82	**		
T.Line × F.MutType	3.22			
Null deviance	2599.1		900.40	
Deviance	2580.2		889.91	
AIC	2590.2		893.91	
Sensitivity	$296/601 = 0.493$		$68/170 = 0.400$	
Specificity	$974/1629 = 0.598$		$582/800 = 0.728$	

Table XV. Multivariate models: POOLCOVFAULTS, Det

	CrosswordSage		FreeMind 0.7.1	
	Coef.	Sig.	Coef.	Sig.
Intercept	-0.561		-1.24	
F.MutType	0.910	***	-0.711	***
F.CovBef	-15.4	***	-7.68	***
F.AllRep	0.387	***		
F.MinPred			-49.9	***
F.MaxSucc	11.7	***		
F.MinWith	-8.94	***		
T.Events			0.506	○
T.Line	5.17	***		
T.Pairs	9.02	**		
T.Line × F.MutType	4.93	**		
T.Line × F.CovBef	95.2	*		
T.Line × F.MaxSucc	-109	***		
T.Line × F.MinWith	150	***		
T.Pairs × F.CovBef	235	**		
Null deviance	1641.1		715.95	
Deviance	1398.1		588.07	
AIC	1424.1		598.07	
Sensitivity	$428/601 = 0.712$		$102/170 = 0.600$	
Specificity	$399/583 = 0.684$		$352/431 = 0.817$	

The multivariate models show that, when all other characteristics are held constant, all of the fault characteristics studied can affect fault detection. So can a test suite's proportion of coverage (CrosswordSage only) and perhaps size (FreeMind's POOLCOVFAULTS only). Test-suite size is significant in CrosswordSage's univariate models but not chosen for its multivariate models, indicating that it does not explain any fault detection that is not already explained by a test suite's proportion of coverage.

Significant interaction effects appear only in CrosswordSage's POOLCOVFAULTS model. As in CrosswordSage's POOLCOVFAULTS model of fault coverage (Table XI), interactions between a test suite's proportion of coverage and a fault's size of event handlers, and between proportion of coverage and a fault's degrees of freedom, affect fault detection. So do interactions between proportion of coverage and a fault's method

of creation, and between proportion of coverage and a fault's distance from the initial state.

Once again, for most of the models and data sets, it is a combination of test-suite and fault characteristics that best explains fault detection.

As with fault coverage (Tables X and XI), the models of fault detection for the POOLCOVFAULTS data set fit the data better than the models for the ALLFAULTS data set, since more fault metrics are considered. But, whereas the models of fault coverage for POOLCOVFAULTS had sensitivity and specificity of 0.810–0.990, the models of fault detection for POOLCOVFAULTS do not fit as well. Thus, while the set of characteristics in POOLCOVFAULTS is nearly sufficient to explain coverage of faulty lines, it is not sufficient to explain detection of faults.

5.3. What characteristics affect whether a test suite detects a fault, given that the test suite covers the faulty line?

The previous section asked: What characteristics affect whether a test suite detects a fault? Some of those characteristics might affect the probability that a fault is detected *primarily because* they affect the probability that the faulty code is covered. For example, some faults may be harder to detect simply because they lie in a part of the program that is harder to cover. But other faults may be harder to detect even if code containing them is covered, which is why it is necessary to ask the question for this section.

Hypotheses: This section examines hypotheses H3, H4, and H6 from Section 4. These hypotheses state that no fault characteristic, test-suite characteristic, or interaction effect between the two affects a fault's likelihood of being detected by a test suite, given that the fault is covered by the test suite.

Data sets: To test these hypotheses, we use the SUITECOVFAULTS data set. This data set consists of the POOLCOVFAULTS data set minus any data points where the test suite does not cover the fault (Cov $= 0$). Thus, SUITECOVFAULTS is ideal for understanding the conditional probability that a test suite detects a fault given that it covers the faulty line. The dependent variable of interest is Det.

5.3.1. Univariate models. Table XVI summarizes the univariate models of Det given Cov.

The set of characteristics that significantly affects fault detection given coverage for at least one application is the same as the set that affected fault detection (Tables XII and XIII) and a superset of the set that affected fault coverage (Tables VIII and X). Specifically, every fault characteristic and the test-suite characteristics of size and proportion of coverage affect fault detection given coverage. For CrosswordSage, the code-coverage metrics for proportion of coverage behave differently than the event-coverage metrics (T.Pairs and T.Triples): while the code-coverage metrics affect fault detection primarily because they affect fault coverage (as shown by their absence in CrosswordSage's SUITECOVFAULTS model), the event-coverage metrics affect fault detection yet do not affect fault coverage.

For all statistically significant metrics, the sign of their coefficients is the same as in the models of fault detection (Tables XII and XIII). To see whether metrics more strongly affect fault detection or fault detection given coverage, the confidence intervals surrounding model coefficients can again be compared, as in Section 5.2. The only metrics for which the difference is meaningful turn out to be F.MaxSucc for CrosswordSage and F.CovBef and F.Depth for FreeMind (which have a stronger effect on Det) and F.Events for FreeMind (which has a stronger effect on Det given Cov).

5.3.2. Multivariate models. Table XVII summarizes the multivariate models.

Table XVI. Univariate models: SUITECOVFAULTS, Det

		CrosswordSage			FreeMind 0.7.1		
		Int.	Coef.	Sig.	Int.	Coef.	Sig.
Meth. of creation	F.MutType	-0.104	0.778	***	0.0299	-0.834	***
Distance from initial state	F.CovBef	0.221	-3.94	*	-0.596	-9.03	***
	F.Depth	0.221	-0.251		-0.607	-2.74	***
Repetitions	F.SomeRep	0.0488	0.554	***	-0.274	-0.380	○
	F.AllRep	0.0479	0.582	***	-0.682	0.760	**
Degrees of freedom	F.MinPred	0.221	-2.70	○	-0.602	-116	***
	F.MaxPred	0.221	1.25		-0.563	-1.90	***
	F.MinSucc	0.221	1.08		-0.548	-0.256	
	F.MaxSucc	0.223	6.72	***	-0.564	-2.30	***
	F.Events	0.229	5.54	***	-0.586	-2.05	***
Size of event handlers	F.MinWith	0.223	-6.16	***	-0.568	15.1	***
	F.MaxWith	0.222	2.69	**	-0.555	-10.3	**
Granularity	T.Len	0.221	0.00537		-0.549	0.021	
Size	T.Events	0.222	0.273	*	-0.553	0.583	*
Proportion of coverage	T.Class	0.221	-0.814		-0.551	-17.4	○
	T.Meth	0.221	-1.12		-0.548	-4.29	
	T.Block	0.221	-0.846		-0.548	-3.40	
	T.Line	0.221	-0.891		-0.548	-3.44	
	T.Pairs	0.222	7.92	**	-0.552	54.3	○
	T.Triples	0.222	34.8	*	-0.55	3540	

Table XVII. Multivariate models: SUITECOVFAULTS, Det

	CrosswordSage		FreeMind 0.7.1	
	Coef.	Sig.	Coef.	Sig.
Intercept	-0.310		-0.395	
F.MutType	0.940	***	-0.472	***
F.CovBef	-11.062	***		
F.Depth			-1.552	
F.AllRep	0.503	**	0.669	***
F.MinPred			-53.810	***
F.MaxSucc	5.328	***		
F.MinWith	-7.275	***		
F.MaxWith			-15.226	***
T.Class			-20.190	○
T.Pairs	8.806	**	90.017	*
T.Pairs × F.CovBef	173.314	○		
Null deviance	1488.3		609.7	
Deviance	1365.9		526.4	
AIC	1381.9		542.4	
Sensitivity	$393/601 = 0.654$		$105/170 = 0.618$	
Specificity	$312/482 = 0.647$		$222/294 = 0.755$	

The multivariate models show that, when all other characteristics are held constant, every fault characteristic studied can still affect fault detection given coverage. Of the test-suite characteristics, proportion of coverage affects fault detection given coverage, but size does not. (As in the models of fault detection, test-suite size does not give any information about fault detection given coverage that is not already provided by proportion of coverage.)

The only interaction effect in these models is between a test suite's proportion of coverage and a fault's distance from the initial state for CrosswordSage.

CrosswordSage's model of fault detection given coverage fits the data slightly worse than its model of fault detection for POOLCOVFAULTS (Table XV), based on a comparison of sensitivity and specificity. FreeMind's model of fault detection given coverage has better sensitivity than its model of fault detection for POOLCOVFAULTS, but the specificity and the total percentage of data points correctly classified are lower (70.5% vs. 75.5%). Thus, FreeMind's model of fault detection given coverage also fits the data slightly worse than its model of fault detection for POOLCOVFAULTS.

6. DISCUSSION

Sections 6.1, 6.2, and 6.3 sum up the effects of faults characteristics, test-suite characteristics, and interactions between them on coverage and fault detection. In Section 6.4, implications of the fit of the logistic-regression models are considered. Section 6.5 discusses differences in the results for CrosswordSage and FreeMind. Finally, Section 6.6 considers threats to validity.

6.1. Fault characteristics

Every fault characteristic, and every fault metric but F.MinSucc, turned out to significantly affect the likelihood of fault detection in at least one logistic-regression model. Furthermore, there were some cases where the likelihood of fault detection depended *only* on fault characteristics, not on test-suite characteristics (i.e., the models for FreeMind in Tables XII and XIV). These facts suggest that the kinds of faults used to evaluate the effectiveness of testing techniques can significantly impact the percentage of faults they detect.

The characteristics provide a fairly orthogonal classification of faults for studies of fault detection, as most or all fault characteristics were represented in each multivariate model of fault detection. The rest of this section considers each characteristic in turn.

6.1.1. Method of creation. F.MutType had a different effect on fault detection for the two applications. For CrosswordSage, method-level mutation faults were easier to detect, while for FreeMind, class-level mutation faults were easier. Strangely, for both applications class-level mutation faults were easier to cover. The hypothesis from preliminary work [Strecker and Memon 2008] was only that class-level mutation faults in class-variable declarations/initializations would be easier to detect, and such faults were omitted from this experiment. That F.MutType makes a difference in Cov and Det seems to be a quirk of the way CrosswordSage and FreeMind are structured—in providing opportunities to seed mutation faults—not an inherent difference between class- and method-level mutation faults. But it is something for experimenters to be aware of.

6.1.2. Distance from initial state. F.CovBef and F.Depth behaved just as expected: faults lying "closer" to the initial state were easier to cover and detect. Interestingly, these faults were easier to detect even given that they had been covered.

6.1.3. Frequency of execution. F.AllRep and F.SomeRep significantly affected fault detection, although the direction of that effect was mixed. Faulty lines that, when executed by an event, were only sometimes executed more than once (F.SomeRep $= 1$) were easier to detect for CrosswordSage but harder for FreeMind. Faulty lines that, when executed, were always executed more than once (F.AllRep $= 1$) were easier to detect for both applications. Since F.AllRep was chosen over F.SomeRep for the multivariate models, its effect on fault detection was not only more consistent but also more important.

6.1.4. Degrees of freedom. It was not clear at the outset which measures of degrees of freedom in execution of a faulty line—F.MinPred, F.MaxPred, F.MinSucc, F.MaxSucc, or F.Events—would be most closely associated with fault detection. Nor was it clear whether faults in code with more degrees of freedom—code that could be executed in many different contexts—would be easier or harder to cover and detect. For FreeMind, the minimum number of EFG predecessors of a faulty event (F.MinPred) turned out to be the most influential of these metrics, having been selected for each multivariate model of Det and of Det given Cov (Tables XV and XVII). Faulty code with fewer EFG predecessors—fewer degrees of freedom—was consistently more likely to be covered and detected for both applications. For CrosswordSage, F.MaxSucc was the metric chosen to represent the degrees-of-freedom characteristic in the multivariate models of Det and of Det given Cov (Tables XV and XVII). In contrast to the results for F.MinPred, faulty code with more EFG successors—more degrees of freedom—was consistently more likely to be detected.

6.1.5. Size of event handlers. The size of event handlers executing a faulty line could either be measured by the minimum or the maximum number of lines executed in the same event (F.MinWith or F.MaxWith). For CrosswordSage, F.MinWith was the better predictor of fault detection, with faults in larger event handlers being harder to detect. For FreeMind, the results are inconsistent. In the univariate models of the SUITECOVFAULTS data set, for example, event handlers with a larger F.MinWith are associated with greater fault detection, but event handlers with a larger F.MaxWith are associated with less fault detection.

6.2. Test-suite characteristics

6.2.1. Proportion of coverage. The code-coverage metrics—T.Class, T.Meth, T.Block, and T.Line—were, of course, associated with the probability of covering a given faulty line. Interestingly, greater coverage did not necessarily increase the likelihood of detecting a given fault—at least for the range of coverage levels considered in this experiment (see tables in Section 4). For FreeMind, in fact, code coverage seemed to add no statistically significant value to testing. Had a broader range of code coverage levels been studied, however, code coverage would almost certainly have been shown to increase the likelihood of fault detection significantly.

The event-coverage metrics—T.Pairs and T.Triples—behaved in nearly the opposite fashion. While they did not increase the likelihood of covering a given faulty line, they did (for CrosswordSage and sometimes for FreeMind) significantly increase the likelihood of detecting a given fault.

6.2.2. Size. As expected, test suites with a greater size (T.Events) were more likely to detect a given fault. (They were not any more likely to cover a given faulty line.) An important question in studies of test-suite characteristics is whether code coverage still affects fault detection when test-suite size is controlled for. In this study, the only case where test-suite size influenced fault detection more than coverage (i.e., when T.Events

was chosen for a multivariate model) was in the model for FreeMind in Table XV, in which no coverage metrics were significant at all.

6.2.3. Granularity. Contrary to previous experiments [Rothermel et al. 2004; Xie and Memon 2006], granularity (T.Len) had no effect whatsoever on coverage or detection. In this experiment, the minimum test-case length was equal to the depth of the EFG. In the previous experiment on GUI testing [Xie and Memon 2006], shorter test cases were allowed. Thus, test-case length appears to only affect fault detection to the extent that higher-granularity test suites reach deeper into the EFG; once the granularity exceeds the EFG depth, no further benefit is gained.

6.3. Interactions between test-suite and fault characteristics

Interaction effects between test-suite and fault characteristics indicate cases where certain kinds of test suites are better at detecting certain kinds of faults. In practice, this information may be used to design test suites that target kinds of faults that tend to be more prevalent or more severe. Interaction effects can also influence the results of evaluations of testing techniques because, if certain techniques are better at detecting certain kinds of faults, the sample of faults may be biased for or against a technique.

Only for CrosswordSage did interactions between test-suite and fault characteristics significantly affect fault detection. For the POOLCOVFAULTS data set (Table XV), method-level mutation faults (F.MutType $= 1$), faults lying farther from the initial state (higher F.CovBef), and faults lying in larger event handlers (higher F.MinWith) were better targeted by test suites with greater line coverage (T.Line). Both for that data set and for the SUITECOVFAULTS data set (Table XVII), faults lying farther from the initial state (higher F.CovBef) were also better targeted by test suites with greater event-pair coverage (T.Pairs).

6.4. Model fit

The multivariate models of Cov for the POOLCOVFAULTS data set (Table XI) fit the data well, with sensitivity and specificity between 81% and 99%. For CrosswordSage, the fit was nearly perfect. This suggests that the set of metrics studied is nearly sufficient to predict whether a test suite will cover a given line—a prerequisite to predicting whether the suite will detect a fault in the line. But, at least for applications like FreeMind, some influential test-suite or fault metrics remain to be identified, as part of future work.

The multivariate models of Det for the POOLCOVFAULTS data set (Table XV) and of Det given Cov for the SUITECOVFAULTS data set (Table XVII) did not fit as well, with sensitivity and specificity between 60% and 82%. This shows that predicting whether a test suite will detect a fault is harder than predicting whether the test suite covers the faulty code; more factors are at work.

Finally, the multivariate models of Cov and Det for the POOLCOVFAULTS data set, which includes all fault characteristics, fit much better than those for the ALLFAULTS data set, which includes just one fault characteristic (F.MutType). This provides additional evidence that at least some of the fault characteristics studied besides F.MutType are important predictors of fault coverage and detection.

Naturally, the fit of the multivariate models differed for different data sets. When more fault metrics were available as independent variables—in the POOLCOVFAULTS and SUITECOVFAULTS data sets, as opposed to the ALLFAULTS data set—the models, not surprisingly, fit better. The models also fit better when the dependent variable was Cov than when the dependent variable was Det (a variable influenced by Cov), and better when the dependent variable was Det than when it was Det given Cov (a variable not influenced by Cov). One reason is that some of the characteristics studied—e.g.,

fault's distance from initial state and test suite's code coverage—are inherently more related to fault coverage than to fault detection. (Empirically, this is shown by the larger coefficient magnitudes in Table IX than in Table XIII for distance from initial state (both applications) and code coverage (CrosswordSage only).) Another reason may be that fault detection is inherently harder to predict than fault coverage, being a product of more variables.

The fit of the multivariate models differed also between the two applications. The models of Cov fit the data for CrosswordSage somewhat better, while the models of Det and of Det given Cov fit the data for FreeMind somewhat better (based on the percentage of data points correctly classified). The largest difference, and the only case where both sensitivity and specificity are greater for one application than for another, is the models of Cov for the POOLCOVFAULTS data set (Table XI). The model for CrosswordSage may fit markedly better in this case because of some additional, unknown test-suite or fault characteristics affecting coverage for FreeMind, which is the larger and more complex of the two applications.

Even though the set of fault and test-suite characteristics studied did not result in perfectly-fitting models, the experiment results are still of value. While the characteristics studied do not comprise a complete list of factors that influence fault detection in software testing, they are certainly *some* of the factors that empirical studies of testing should account for.

6.5. Differences between applications

There were numerous minor differences between the results for CrosswordSage and FreeMind—cases where the significance, magnitude, or even the direction of a metric's influence on coverage or fault detection differed for the two applications. But there was only one metric, mutation type (F.MutType), that was influential enough to be included in the multivariate models yet influenced fault detection in opposite ways for the two applications. For CrosswordSage, method-level mutation faults were consistently *more* likely to be detected (even though they were less likely to be covered). For FreeMind, method-level mutation faults were *less* likely to be detected. The proportion of faults that were method-level mutations also differed widely between the applications: 47% for CrosswordSage and 70% for FreeMind.

These differences seem to result from the structure of the applications and the nature of the test oracle, with FreeMind having more opportunities to seed method-level mutation faults and with those opportunities lying in code that is less likely to affect the GUI state (as checked by GUITAR). For example, FreeMind contains a method called `ccw` that helps calculate the coordinates of an object in the GUI. Since the test oracle ignored the coordinates of GUI objects, faults in `ccw` were unlikely to be detected if they only changed the calculated coordinate values. And since `ccw` mainly operates on primitive types, nearly all of the mutation opportunities were for method-level faults. Thus, in the experiment, all 21 of the faults sampled from `ccw` were method-level mutations, and only 2 were detected by their corresponding test suite.

Another notable difference between the applications was in the total percentage of faults covered and detected in each data set (Table VII). Even though the percentage of all faults covered was nearly the same (49% for CrosswordSage, 48% for FreeMind), the percentage of faults detected was lower for FreeMind in every data set. This suggests at least one of the following explanations:

(1) the sample of faults selected for FreeMind, out of the population of mutation faults for that application, happened to be especially hard to detect;
(2) the structure of FreeMind makes it harder to detect randomly seeded faults; or

(3) event-coverage-adequate GUI test suites are less effective for FreeMind than for CrosswordSage.

The experiment offers some evidence of the second explanation (e.g., the mean value of F.CovBef is greater for FreeMind), but the other explanations cannot be ruled out. Further investigation is a subject for future work.

6.6. Threats to validity

Four kinds of threats to validity are possible for this experiment.

Threats to internal validity are possible alternative causes for experiment results. Since one aim of the experiment was to identify characteristics of faults that make them easier or harder to detect, threats to internal validity here would cause certain kinds of faults to seem easier or harder to detect when in fact they are not. For example, it could be that faults seeded nearer to the initial state of the program happened to be easier for reasons independent of their location in the program. But, because faults were seeded objectively and randomly, there is no reason to suspect that this is the case. The faults considered in this work are those that can be mapped to "pieces of code." It should be noted that faults are often more complex and involve not just single (presumably contiguous) "pieces" of code but also interactions between pieces of code [Frankl et al. 1998]. Furthermore, studying Cov separately from Det (Section 5.1) helped to nullify one potential threat to validity: that class-level mutation faults appeared to be significantly easier to cover, even though this is not an inherent property of such faults. Because only two applications were studied, and each had different results, some of those inconsistent results may have arisen by chance or through quirks of one or the other of the applications.

Threats to construct validity are discrepancies between the concepts intended to be measured and the actual measures used. The experiment studied metrics of faults and test suites that were intended to measure some more abstract characteristics (Table I). These characteristics could have been measured in other ways. Indeed, if static analysis had been feasible for the applications studied, then it would have been a better way to measure certain fault metrics than using estimates based on the test pool. However, because care was taken to define the metrics such that they would not depend too much on the test pool (e.g., by using minima and maxima rather than averages), and because the test pool was much larger than any test suite, the authors contend that the threats to validity posed by these estimates are not severe.

Threats to conclusion validity are problems with the way statistics are used. Because the experiment was designed to meet the requirements of logistic regression, it does not violate any of those requirements. The only known threat to conclusion validity is the sample size. While it is not recommended that the sample size or power be calculated retrospectively from the experiment data [Lenth 2000], the required theoretical sample size would probably be very large because so many independent variables were used. (Sample sizes calculated from the experiment data for use in for future experiments are presented by Strecker [Strecker 2009].) The sample size for this experiment was chosen not to achieve some desired level of power but to generate as many data points as possible in a reasonable amount of time and then perform an exploratory analysis, focusing on the fault characteristics. Since the fault characteristics frequently showed up as statistically significant, the sample size apparently served its purpose.

Threats to external validity limit the generalizability of experiment results. Any empirical study must suffer from these threats to some degree because only a limited sample of all possible objects of study (here, software, faults, and test suites) can be considered. In this experiment, only two GUI-intensive applications, mutation faults,

and event-coverage-adequate GUI test suites (with line and block coverage around 50%) containing test cases of equal length were studied; different results might be obtained for different objects of study. Besides the limitations of the objects of study, the experiment is also unrealistic in that some of the faults are trivial to detect (they cause a major failure for every test case) and in real life would likely be eliminated before the system-testing phase. However, it is crucial to note that this experiment is more generalizable in one respect than most other studies of software testing. This work characterized the faults used in the experiment, and it presented the results in terms of those characteristics, to make clear what impact the particular sample of faults had on the results. The same was done for test suites and their characteristics. This work can serve as a model to help other researchers improve the external validity of their experiments.

7. CONCLUSIONS

The experiment in this work tested hypotheses about faults in GUI testing. It showed that, in the context studied, certain kinds of faults were consistently harder to detect:

— faults in code that lies further from the initial program state ("deep faults"),
— faults in code that is *not* always executed more than once by an event handler, and
— faults in event handlers with more predecessors (in-edges) in the event-flow graph.

These results have implications for stake-holders in GUI testing. First, the results provide a picture of the faults most likely to go undetected. For users of GUI testing, this gives valuable information about reliability. (For example, deep faults are more likely to survive testing, but they may also be less likely to affect users of the software; therefore, additional effort to target them may not be warranted.) For researchers, understanding the faults often missed by GUI testing can guide the development of new testing techniques. The second implication is that the results suggest ways to target harder-to-detect faults. They show that these faults are more likely to reside in certain parts of the code (e.g., event handlers with more predecessors), so testers can focus on these parts. They also show that increasing the line coverage or event-pair coverage of GUI test suites may boost detection of deep faults.

The experiment also tested hypotheses about GUI test suites, with several surprising results. Test suites with modestly greater code coverage (line, block, method, or class) did *not* necessarily detect more faults. Test suites with greater event-pair and event-triple coverage were *not* more likely to execute faulty code, yet they sometimes *were* more likely to detect faults; evidently, these suites executed more different paths within the covered portion of the code. Test-case granularity did *not* affect fault detection.

While results like these may interest the GUI-testing community, the experiment has broader implications. It shows that fault characteristics *can* be accounted for when evaluating testing techniques—using the experiment architecture presented here—and they *should* be accounted for because they can impact the results. It is imperative that evaluations of testing techniques be as accurate and effective as possible. Effective evaluations can convince practitioners that a new technique is worth adopting, or prevent them from wasting resources if it is not. Effective evaluations can illuminate the strengths and weaknesses of different, possibly complementary, techniques. They can even direct the invention of new techniques. Only if evaluations of testing techniques account for fault characteristics can they satisfactorily explain and predict the performance of testing techniques.

To account for fault characteristics in this experiment, a new fault characterization was proposed. Unlike previous attempts to characterize faults, this work used characteristics that are objective, practical to measure, and—as the experiment shows—can

significantly affect faults' susceptibility to detection. While the characterization here is not intended to be complete or definitive, it is an important first step from which further progress can be made.

8. FUTURE WORK

The experiment, the experiment architecture, and the fault characterization can all be improved upon in future work. **The experiment** should be replicated in different contexts, and replications should study different test-suite and fault metrics. To lend external validity to the experiment results on GUI testing, the experiment should be replicated with different GUI-based applications. One weakness of this experiment was that it explored only a narrow range of test-suite coverage criteria; there are many other code coverage measures (e.g. decision, condition, def-use) that might be better predictors than block and line coverage; future work will need to incorporate these criteria into the study. Moreover, a different strategy for GUI-test-suite generation, such as augmenting automatically-generated test suites with manually-generated test cases, could be used. In terms of seeded faults, we have performed coarse-grained analysis by examining only class and method-level mutants; to provide finer-grained analysis, in future work, we intend to model mutation operators as characteristics of faults.

To lend even broader external validity to the experiment, it should be replicated in domains other than GUI testing and with faults other than one-line mutations. Especially interesting would be natural faults made by programmers. To make this possible, analogues of the GUI-testing-specific metrics used in this experiment will need to be found for other testing domains. One metric, test-case length, already has an analogue: test granularity. For other metrics based on GUI events and the event-flow graph, function calls or "test grains" [Rothermel et al. 2004] might be substituted for events. A different graph representation, such as the control-flow graph, might be substituted for the event-flow graph.

The assumption that faults affect just one line of code will also need to be relaxed. This should be straightforward. For example, "coverage of the line containing a fault" could be generalized to "coverage of all the lines altered by a fault".

The experiment architecture should be enhanced and validated. It should be extended to account for program characteristics as well as test-suite and fault characteristics. One solution *seems* to be to look at ⟨*test suite, fault, program*⟩ triples rather than ⟨*test suite, fault*⟩ pairs. But the solution is not so straightforward: the fundamental rule of logistic regression—independent data points—would dictate that each ⟨*test suite, fault, program*⟩ tuple must refer to a different program. Clearly, a better way to account for program characteristics is needed.

Instantiations or variations of the experiment architecture could be developed to perform experiments more efficiently, requiring fewer test cases or fewer faults. For example, a factorial design may be more appropriate in some situations than this experiment's method of randomly generating data points. For efficiency, goodness of fit, and ease of interpreting results, alternatives to logistic-regression analysis, such as decision trees or Bayesian networks, may be tried.

In some cases, it may be more interesting to study test cases than test suites. A variation of the experiment architecture could be developed that replaces test suites with test cases. Since a single test case would not usually be expected to exercise all of the software under test, restrictions might be placed on the faults that may be paired with a test case. Last but not least, the experiment architecture should be validated by using it in evaluations of testing techniques conducted by people other than the authors.

The fault characterization should be expanded and refined. Additional relevant characteristics should be identified to more fully explain faults' susceptibility to detec-

tion. Even better, an underlying theory of fault types in software testing—rather than a somewhat ad hoc list of characteristics—should be developed.

The characterization could be made even more applicable to practice by incorporating fault severity. Although severity is highly subjective, perhaps one or more fault characteristics (such as the distance of faulty code from the initial program state) could approximate it. Characteristics of the failures caused by a fault should be identified and studied. These may help explain faults' susceptibility to detection in ways that static fault characteristics cannot, and they may also establish a connection to fault severity.

Eventually, the fault characterization, as well as test-suite and software characterizations, should mature to the point where these characteristics can accurately predict whether a given kind of fault in an application will be detected by a given test suite. The more accurate those predictions are, the more effective evaluations of testing techniques can be.

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