Configuration-Aware Regression Testing: An Empirical Study of Sampling and Prioritization

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ABSTRACT

Configurable software lets users customize applications in many ways, and is becoming increasingly prevalent. Researchers have created techniques for testing configurable software, but to date, only a little research has addressed the problems of regression testing configurable systems as they evolve. Whereas problems such as selective retesting and test prioritization at the test case level have been extensively researched, these problems have rarely been considered at the configuration level. In this paper we address the problem of providing configuration-aware regression testing for evolving software systems. We use combinatorial interaction testing techniques to model and generate configuration samples for use in regression testing. We conduct an empirical study on a non-trivial evolving software system to measure the impact of configurations on testing effectiveness, and to compare the effectiveness of different configuration prioritization techniques on early fault detection during regression testing. Our results show that configurations can have a large impact on fault detection and that prioritization of configurations can be effective.

Categories and Subject Descriptors

D.2.5 [**Software Engineering**]: Testing and Debugging

General Terms

Experimentation

Keywords

Regression Testing, Configurable Software, Prioritization, Combinatorial Interaction Testing

1. INTRODUCTION

User configurable software — software that can be customized through a set of options by the user — is becoming increasingly prevalent. Often these options are read at program start-up or can be changed at run-time, meaning that configurations can be modified by users on-the-fly. A single user configurable software application can often be instantiated in an enormous number of ways. From a testing perspective, each configuration may appear largely similar, but the underlying execution of code for the same set of test cases may differ widely across configurations [10]. This increases the burden on software engineers, who must consider not just which inputs to utilize in testing, but also which configurations.

The impact of configurability can be particularly large in the context of regression testing, which is performed each time a system is modified and is often resource limited [2, 3, 19, 22]. To date, most regression testing research has treated software systems as if they possessed a single homogeneous configuration. A primary focus has been on techniques for reducing test suite size (regression test selection) (e.g., [6, 23, 26, 27]) or on ordering test cases (test case prioritization) (e.g., [13, 29, 32]). None of this research, however, has explicitly considered issues involving configurations.

We term the collection of all possible configurations of a software system the *configuration definition layer* (CDL) for that system. The CDL sits on top of the normal set of inputs to the system and therefore magnifies by a multiplicative factor the already large set of inputs needed for testing. Arguably, all possible settings of the CDL should be tested with each applicable input, but in practice this is infeasible. Alternatively, a sampling technique can be used to somehow "cover" the configurations in the CDL, and each input can be utilized on each of the selected configurations.

Recent work suggests that combinatorial interaction testing (CIT) may provide an effective way to sample configurations for testing [15, 18, 33]. Using CIT-based testing approaches, faults occurring under the sampled configurations can be revealed. Most of this prior work, however, has focused on the testing of single versions of software systems, rather than on the regression testing of consecutive versions as a system evolves.

In this paper we address this lack of focus. We quantify the impact of configurability on the effects of regression testing, compare the effectiveness of CIT sampling to that of random sampling in regression testing, and examine whether we can improve early fault detection during regression testing through configuration prioritization. To do this we study several versions of the open source text editor `vim`. Our results show that the CDL is important, and that the choice of configurations tested can impact the fault finding ability of regression test suites by as much as 70%. We also show that CIT test suites perform better than random ones based on the same configuration model, and that there is justification for configuration prioritization during testing.

2. BACKGROUND

We begin by providing background information on combinatorial interaction testing and test case prioritization.

2.1 Combinatorial Interaction Testing

CIT sampling models the inputs or configuration options (*factors*) for a software system and their associated *values* and combines these systematically so that all t-way ($t > 1$) combinations of inputs or options are tested together [7]. Here, t is called the strength of testing, and when t=2, we call this pair-wise testing.

CIT samples are defined by mathematical objects called covering arrays. A *covering array*, $CA(N; t, k, v)$, is an $N \times k$ array on v symbols with the property that every $N \times t$ sub-array contains all ordered subsets from v symbols of size t *at least* once [8]. Quite often in software testing the number of values for each factor is not the same. Therefore, we use the following expanded definition (often called a mixed level covering array) that uses a vector of vs for the factors.

A covering array, $CA(N; t, k, (v_1 v_2 ... v_k))$, is an $N \times k$ array on v symbols, where $v = \sum_{i=1}^{k} v_i$, where each column $i \quad (1 \leq i \leq k)$ contains only elements from a set S_i of size v_i and the rows of each $N \times t$ sub-array cover all t-tuples of values from the t columns at least once. We use a shorthand notation to describe these arrays with superscripts to indicate the number of factors with a particular number of values. For example, a pair-wise covering array with five factors, three of which are binary and two of which have four values, can be written as follows: $CA(N; 2, 3^2 2^4)$. (We remove the k since it is implicit). Covering arrays have been shown to be effective test suites [4, 7, 17, 33].

To model software using CIT sampling we first need to describe the relevant factors and their associated values. One way to do this is with the Test Specification Language (TSL) [24], a specification based method for defining the combinations of factors influencing program behavior that should be tested together. TSL partitions the system inputs into *parameters* and *environment factors* and within these into *categories*. For each category, a set of *choices* is defined based on equivalence classes.

Though TSL was created to define combinations of program parameters and environment factors, it is not limited to this. TSL can also be used to define factors for covering arrays from system configurations, each of which is considered a category. Each of the choices in the categories becomes a *value* in CIT terminology.

In TSL the large combinatorial space formed by considering all combinations of categories and choices is reduced through two approaches. The first approach sets specific choices as *single* or *error*, meaning that these are tested alone. The second approach adds properties to particular choices and defines *constraints* that relate other choices to these properties. These approaches can significantly reduce the final set of combinations associated with a TSL specification. In TSL all possible combinations are then generated given the set of specified constraints. CIT techniques do not directly use these methods, but rather reduce the combinatorial space by systematically testing only t-way combinations. It is possible, however, to combine these techniques by adding single test cases to the CIT test suite and to consider constraints if certain combinations are illegal [7, 9].

Partial TSL for `vim`

Properties

expandtab:	set et
	set noet
smarttab:	set sta
	set nosta
tabstop:	8
	1
textwidth:	0
	2
	78

Pair-wise CIT Sample

expandtab	smarttab	tabstop	textwidth
set et	set sta	8	78
set et	set nosta	1	78
set et	set nosta	1	2
set noet	set sta	1	0
set noet	set sta	8	2
set et	set nosta	8	0

Figure 1: A CIT Test Suite Defined using TSL

Figure 1 shows an example of a partial TSL definition (without any single or error notations, or constraints) for the text editor `vim`, and an applicable pair-wise test suite. The shaded boxes denote the 2-way combinations in which value "`set et`" has been chosen for factor `expandtab`.

2.2 Test Case Prioritization

Commonly, test case prioritization is used in regression testing, at the test suite level, with the goal of detecting faults as early as possible in the regression testing process, given a test suite inherited from previous versions of the system. There are many techniques (e.g., [14, 20, 28, 30]) for prioritizing test cases based on various forms of information such as code coverage or modification history.

Just as test cases can be prioritized, so can configurations, the motivation being to order the configurations in a manner that helps meet testing objectives (e.g., fault detection) earlier. To our knowledge this idea has not yet been systematically explored. Bryce and Colbourn [5] present an algorithm to prioritize CIT test suites, but theirs is a general algorithm that may or may not apply to configurable software. Moreover, their approach does not address a key aspect of prioritization, namely, how to weight the various elements that drive prioritization on real software systems.

In [25], we examined prioritization of CIT *test suites* and developed several ways to control the prioritization through weightings. We used methods that utilize code coverage data from prior releases, as well as one that is specification based. Further, we observed that Bryce and Colbourn's prioritization technique is a combined generation and prioritization technique, rather than pure prioritization, because it regenerates tests each time rather than simply reordering them. We modified the algorithm to perform pure prioritization and compared the two approaches. Our results showed that prioritized test suites detected faults earlier than unordered ones. We leverage this work in the rest of this paper, but now focus on configurations rather than test cases.

3. PRIORITIZING CONFIGURATIONS

Since our configuration model is based on combinations of configuration options, to prioritize configurations we need techniques that use data on the importance of interactions between options. In [25] we prioritized CIT test suites for regression testing; there, we did not consider the system configuration, but rather, ordered the test cases within a test suite. We modify that strategy to work on configurations.

We use Bryce and Colbourn's algorithm [5] to generate prioritized configurations. The CIT samples generated are a special kind of covering array called a *biased covering array*. The algorithm uses the *interaction benefit* or *importance* of individual factors and values to determine the final configuration order. The algorithm (see [5] for details) begins by computing a total interaction benefit for each factor. The factors are sorted in decreasing order of interaction benefit and then filled as follows. First, the individual interaction benefit for each of the factor's values is computed; this selects the value of the factor that has the greatest interaction benefit. After all factors have been fixed, a single configuration has been created, and the benefits for factors are recomputed and the process starts again. The algorithm is complete when all pairs have been covered.

As observed earlier, this type of prioritization is really a regeneration technique; it generates a new CIT sample for each new version. The implication is that a new set of configurations is tested each time we prioritize. In [25] we extended the concept of interaction benefit to apply to previously generated test suites as a basis of prioritization. We used the interaction benefit to simply *order* configurations from a given CIT sample; the same set of configurations is used each time. In this paper, we refer to the first technique as *regeneration* and the second as *pure prioritization*.

One requirement for using interaction benefit to drive regeneration or prioritization is to assign *weights of importance* to each factor and value in the CIT model. In [25] we used code coverage data from the previous version to weight the values of the model. Once we decided which test cases contributed most to code coverage or to finding faults we calculated the occurrence of the individual values in those test cases and gave them the highest weights. Additionally, we used a specification based approach and set weights based on the current version's specification. We take a similar approach at the configuration level.

In a configurable system our model is at a higher level of abstraction than the test suite/test case level, so we first describe some metrics that can help us assess how important an individual configuration is.

3.1 Metrics for Fault and Block Coverage

In [10] we developed a set of metrics for measuring the changes in a test suite's fault detection behavior across configurations. These metrics were derived from metrics developed by Elbaum et al. for code coverage [12]. We begin with a *block matrix* B and a *fault matrix* F. Let B be a $b \times t$ matrix, where b is the number of unique blocks in the program and t is the number of test cases in the test suite. If cell (i, j) in B contains a 1, this means that test case j executed the code in block i, otherwise the cell contains 0.

Let F be an $f \times t$ matrix, where f is the number of unique faults in the program and t is is the number of test cases in the test suite. If cell (i, j) in F contains a 1, this means that test case j detected fault i.

Figure 2 shows examples of a block matrix, B_1, and two fault matrices, F_1 and F_2. In this example, there are four faults, three blocks and five test cases.

We use these block and fault matrices to define and calculate two metrics: block coverage and fault detection. We include two additional related metrics explained here and used later in our experimentation, change across faults and change across tests.

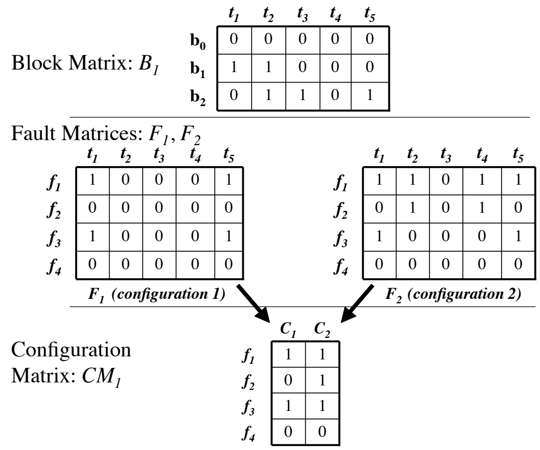


Figure 2: Block and Fault Matrices

Block Coverage (BC)

BC measures the percentage of blocks covered by a given test suite. In Figure 2, the BC for matrix B_1=66.7%.

Fault Detection (FD)

FD measures the percentage of faults found by a given test suite. Matrices F_1 and F_2 of Figure 2 have FD values of 50% and 75%, respectively.

Change Across Faults (CAF)

CAF compares a pair of fault matrices, F_1 and F_2, and measures the percentage of rows that differ between them. CAF measures the sensitivity of faults across configurations. A large CAF means that there is a large difference in the way in which individual faults are detected by test cases across configurations. If rows F_1i and F_2i have at least one cell (i, j) that differs, $diff\_Fcount_i = 1$. Then:

$$CAF = \frac{\sum_{i=1}^{f} diff\_Fcount_i}{f} \times 100$$

The CAF value for matrices F_1 and F_2 in Figure 2 is 50%. In our study, CAF is calculated for all $\binom{n}{2}$ pairs of configurations where n is the number of configurations.

Change Across Tests (CAT)

CAT compares two fault matrices, F_1 and F_2, and measures the percentage of columns that differ between them. CAT measures the sensitivity of test cases across configurations. A large CAT means that there is a large difference in the ability of individual test cases to detect particular faults across configurations. If columns F_1i and F_2i have at least one cell (j, i) that differs, $diff\_Tcount_i = 1$. Then:

$$CAT = \frac{\sum_{i=1}^{t} diff\_Tcount_i}{t} \times 100.$$

The CAT value for matrices F_1 and F_2 in Figure 2 is 40%. In our study we examine all $\binom{n}{2}$ pairs of configurations when using this metric.

3.2 Computing Weights of Importance

To prioritize or re-generate configurations, we first need to compute weights of importance for each value of the configuration model. We use a four step process (Figure 3):

Step 1. Given block and fault matrices as just described, we relate these metrics to our configuration specification model. Initially we have one block and one fault matrix for *each*

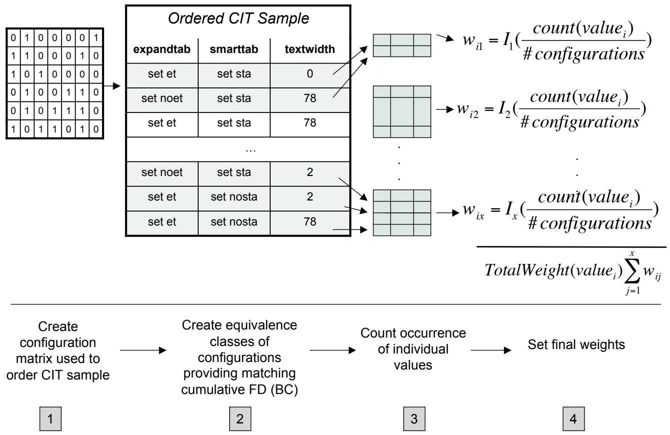


Figure 3: Setting the Weights for Interaction Benefit

configuration. We first aggregate these into *configuration matrices*. Figure 2 shows how we combine two fault matrices, F_1 and F_2, into a single configuration matrix CM_1 (the same can be done with block matrices). The rows of the matrix are still faults (or blocks), but the columns are now configurations. A "1" in a cell means that at least one test case for that configuration found the fault (or covered the block). We can determine an individual configuration's FD or BC by summing the columns of the configuration matrix associated with that configuration. In the example, Configuration 2 (C_2) has a higher FD than Configuration 1 (C_1).

Step 2. Given a configuration matrix, we use an iterative greedy approach to order our CIT sample in descending order. We order the configurations until we reach a high percentage of the cumulative FD or BC coverage for the full set of configurations (our threshold). This threshold is chosen empirically as the point at which the rate of contribution of new configurations is minimal. In our experiments the threshold was 100% for FD and 95% for BC.

To gather as much information as possible about the importance of individual values from the configuration model, we aggregate configurations into equivalence classes. For each selection of the next best configuration, we order our CIT sample in descending order of FD or BC. If there is more than one configuration that provides the best new cumulative FD or BC we select all of the equivalent configurations and put these into the same class. For example, if the first configuration has an FD value of 45%, we find all other configurations that also have this FD value. We then find the next best configuration. If this adds 5% more FD we find all other configurations with the same additional fault detection, and so on. We select configurations without replacement (each can belong to only a single class). At the end we have classes of configurations each associated with the same additional contribution to the overall FD or BC.

Step 3. Next, we calculate weights for each value. We sum the number of times a value occurs in each equivalence class and divide this by the number of configurations in that class. For example, if `set sta` occurs twice and we have four configurations in the class, this value has a weight of 0.5, and if `set noet` occurs twice this value has a weight of 1.0.

Step 4. In the final step, we first assign an importance, I, to each of the equivalence classes as a normalization. The importance is set to the cumulative FD or BC of that group divided by the total FD or BC (i.e. it is the percentage of FD or BC it provides in relation to the maximum for the CIT sample). Finally, the weight of each value is multiplied by its importance I in each of the classes, and the weights are summed to obtain the individual weight for that value.

The final weights for each value of the CIT model are then fed into the regeneration or prioritization algorithm, which computes the interaction benefit [5].

3.3 Specification Based Weightings

There may be instances where we do not have prior code coverage, but can prioritize based on the current system specification. To account for such instances, in [25] we used a specification based approach for prioritizing CIT test cases for regression testing. Since we define our configuration model using TSL we can use this for weighting as well. In TSL based prioritization we have the advantage of not needing a prior version; instead we rely on our existing version of the software to produce information to direct prioritization.

To set weights, for each category in the TSL specification we examine that category's possible choices. In the case of binary choices, where one choice turns a feature on and one turns a feature off, we set the ON option to a weight of 0.9 and the OFF option to 0.1. (Our intuition is that the ON option will cause more code to be executed.)

In cases where we have multiple choices for a category, we use a greater number of features or higher complexity of the choice as a proxy for higher code coverage. For instance, in the subject `vim` that we use in our studies described later, we have a category called **`laststatus`** which determines when the last window will contain a status line. There are three values: **`never`**, **`only when 2 windows`** and **`always`**. We assign the highest importance (0.5) to the third value, a medium importance to the second (0.3), and the smallest importance (0.1) to the first. The absolute values used in the prioritization algorithm are less important than the relative values, therefore we have chosen these to reflect a relative importance and have not fine tuned them to specific values. We realize that different heuristics and different absolute values may impact the quality of this method. We leave investigation of this for future work.

3.4 Pure Prioritization

This algorithm is the same as the approach used in [25], but the object of prioritization is the configuration rather than the test case. We iteratively select the configuration with the highest interaction benefit. The interaction benefit considers both the weights of values as well as which pairs from the CIT sample have been covered. We calculate the interaction benefit for each configuration at the start of each iteration, by summing the product of each pair of weights for the configuration (this differs from the approach used in [5] because their regeneration algorithm calculates benefit only for the factors and values). If a pair occurs in a configuration that has already been selected, its interaction benefit is set to zero. For instance, if a configuration has the values `set et`, `set sta`, 8 and 78 with associated weights, 0.2, 0.3, 0.1, 0.5, and the pair 8,78 has already occurred in a previously selected configuration, then the interaction benefit for this configuration is $0.06+0.02+0.10+0.03+0.15+0.00 = 0.36$. When more than one configuration provides the highest interaction benefit, we randomly select from among them. We continue this until all configurations have been selected.

version	# blocks	loc	# changed methods	faults hand+mut
v2	26,081	87,442	664	3+10
v3	30,217	105,225	830	3+10
v4	30,426	105,690	323	4+10
v5	30,553	106,391	317	4+10
v6	30,744	107,992	324	2+10
v7	30,764	105,944	311	3+10

Table 1: Object of Analysis

4. EMPIRICAL STUDIES

We have designed a set of empirical studies to investigate the impact that the CDL has on regression testing, to measure the applicability of using CIT as a sampling mechanism, and to assess the effectiveness of our prioritization approaches. Our studies address the following research questions:

RQ1: What is the effect of changing configurations on the outcome of regression testing across consecutive versions of a program?

RQ2: Is CIT an effective method for sampling configurations for testing?

RQ3: Can we improve fault detection effectiveness, when resources are constrained, through prioritization of configurations?

The rest of this section describes our objects of analysis, independent and dependent variables, methodology, and threats to validity. Subsequent sections present results.

4.1 Objects of Analysis

As an object of analysis we selected `vim`, an open source, multi-platform text editor extended from `vi` written in C [21]. The object was obtained from the Software Infrastructure Repository (SIR) [11] and is augmented with a test suite containing 975 test cases, organized into groups by functionality, and hand seeded faults (not seeded by the authors of this study). We selected this object because it is highly configurable, has a non-trivial code base and has both a default configuration and a test suite that was not developed for this study. This helps to reduce potential sources of bias.

We conduct our experiments on a set of consecutive versions of `vim`, including six releases of the software (version 2-7 from SIR). This corresponds to `vim` versions 5.3-5.8, developed over from 1998 to 2001.

Table 1 provides basic information on our object of analysis, including the number of basic blocks in system versions as calculated by `gcov`[16], the number of source lines of code calculated by `sloccount`[31], and the number of methods changed or added between versions. The number of faults in each version is also shown. Since relatively few faults (2-4) were seeded in any single version, we added 10 additional faults in each through the use of a mutation testing tool [1]. To avoid a potential source of bias, we generated all possible mutations for each version and then randomly selected (with replacement) 10 modules for mutation, in which at least one method had been changed between versions. (We focus only on changed methods to simulate regression faults.) Within each of these 10 modules a mutation from within the changed methods was randomly selected and added to our subject.

`Vim` has a user configuration file, `.vimrc`, that controls a set of between 146 and 187 user configurable options for the different versions of the software. We used the online documentation found at [21] along with the `-setall` option within the software to list and model the system's configuration space, using TSL to specify the configuration options. In each version there were some configuration options that either did not allow certain test cases in the original suite to run (for instance, an option that turns on interactive mode will not work because our test cases run in batch mode), or that we deemed to have little effect on the software under test, such as modifying a path location for a specific directory; we ignored these options. In total, we modeled 90 options in version 2, 93 options in versions 3, 4, 6, and 7, and 76 options in version 5. Note that in version 5, there was a significant decrease in the configurable option space which reduced the size of our model for this version.[1]

Next we used the TSL definition (following the process reported in [25]) to generate a CIT configuration sample for each version. Due to the size of the configuration space and time required for testing (7 hours per configuration) we used a strength 2 (or pair-wise) CIT sample. We used a $CA(60; 2, 2^{77}3^7 4^2 6^3 10^1)$ array for version 2, a $CA(60; 2, 2^{80}3^7 4^2 6^3 10^1)$ array for versions 3, 4, 6, and 7, and a $CA(60; 2, 2^{65} 3^6 4^1 6^3 10^1)$ array for version 5. Note that each sample created has 60 configurations. We used a simulated annealing algorithm [8] to generate the samples. The configurations were then mapped to the `.vimrc` file for manipulation during experimentation. In addition to the covering array samples we generated a comparison sample of the same size for each model consisting of 60 random configurations. The original `vim` test suite from SIR was designed to be run with a single default configuration. We tested this configuration as well as the baseline.

4.2 Independent Variables

Our independent variables for RQ1 and part of RQ2 are the individual configurations in our covering array samples or random samples generated without replacement from all possible configurations of the TSL model. We refer to the original unprioritized covering arrays as **ca** and refer to the sample of 60 random configurations as **rand**.

To answer part of RQ2, and for RQ3, we use the prioritization techniques described in Section 3 as well. We refer to these techniques in terms of both the weighting technique and whether or not the array was prioritized or regenerated: **pure-BC** and **regen-BC** for block coverage weighting, **pure-FD** and **regen-FD** for fault-detection weighting. The last two techniques, **pure-TSL** and **regen-TSL**, reflect the method that uses only the TSL to determine weightings for interaction benefit.

4.3 Dependent Variables

To address RQ1 and RQ2, we measure block coverage (**BC**), fault detection (**FD**), change across faults (**CAF**) and change across tests (**CAT**) as described in Section 3. For RQ2 we also compare the minimal number of configurations in an unordered sample needed to detect the same number of faults detected by all configurations in that sample (**NCF**).

[1]On further analysis, it appears that version 5 was set up by default to use a minimal subset of options, but that the other options are still available within the software. We chose to ignore this in order to maintain the integrity of our model developed through documentation, rather than by examination of source code.

Finally, to address RQ3, we examine the *Normalized Percentage of Faults Detected* (**NAPFD**) which was first described in [25]. In prior work [14, 28], a metric that has been commonly used for prioritization is the *Average Percentage of Faults Detected* or APFD. This metric measures the area under the curve when the percentage of faults found is plotted on the y-axis against the percentage of the test cases (in our case configurations) run on the x-axis. The difficulty with APFD, however, is that it assumes that the total number of configurations and the total number of faults detected in each sample is the same. This is often not the case when we regenerate CIT samples. Therefore NAPFD normalizes this metric to reflect the area under the curve when we have differing numbers of configurations or faults (see [25] for more details). The NAPFD formula is as follows:

$$NAPFD = p - \frac{CF_1 + CF_2 + ... + CF_m}{m \times n} + \frac{p}{2n}$$

In this formula, n represents the number of configurations we are able to run within budget, and m represents the total number of faults found by all configurations. Proportion p represents the number of faults detected by the set of configurations within our budget divided by the total number of faults detected in all of the configurations. CF_i stands for the index of the configuration (when testing in prioritization order) in which Fault i was found. If a fault i is never detected, $CF_i = 0$.

To illustrate, Figure 4 shows a fault matrix with five configurations and eight faults. Suppose that the ordering of the configurations is C_3, C_5, C_2, C_4, C_1. The first configuration (20% of the configurations) finds three faults. After the first three configurations (60% of the configurations) have been executed, five faults have been found (62.5%). If we are able to run the entire CIT sample, $m = 8$, $n = 5$, $p = 1$, $CF_1 = CF_3 = 4$, $CF_2 = CF_4 = CF_7 = 1$, $CF_5 = CF_6 = 2$ and $CF_8 = 5$, the NAPFD value is 0.6. But if we have a budget sufficient only to run 3 configurations, then n changes to 3 and CF_1, CF_3 and CF_8 change to 0. Now the NAPFD value is 0.44.

	C_1	C_2	C_3	C_4	C_5
F_1				x	
F_2		x	x		
F_3				x	
F_4			x		x
F_5					x
F_6					x
F_7	x		x		
F_8	x				

Figure 4: Configuration Order: C_3**,** C_5**,** C_2**,** C_4**,** C_1

4.4 Study Methodology

For our first set of experiments we run the entire test suite on each configuration without any faults, letting outputs serve as the oracle, and then we turn on each fault individually and measure fault detection. This alleviates the potential masking of one fault by another. For each model, we run each set of experiments under each configuration as is defined by the covering array or random array, as well as the default configuration (not created by us) from SIR. We collect block coverage on the fault free version using `gcov`.

For the prioritization experiments that use weights based on block coverage and fault detection ability, we use the prior version of the program to prioritize configurations for the current version, but for the TSL based weightings we use data on the current version as the source for prioritization information. When collecting data on the unordered CIT or random samples for NCF and for the unordered prioritization results, instead of using a single number we randomly select 50 orders and use the average of the results. This reduces sources of bias from the generation process.

4.5 Threats to Validity

Empirical studies are subject to threats to validity. We have attempted to reduce these through our experiment design, however, we outline the major threats here. With respect to external validity (or the difficulty of generalizing to other objects), we have examined only one software system, written in C, and results obtained with other systems may not match these. Similarly, we have utilized only one unconstrained TSL definition. The faults used in this study were both hand seeded and generated by mutation. We did not separate these out for our study because there were too few hand seeded faults to draw conclusions, but we note that the results shown follow those of the mutation faults alone which have been suggested by others as being sufficient to represent realistic faults [1].

With respect to internal validity (the possibility that factors other than variance in our independent variable is responsible for our results) our greatest concern is problems with our instrumentation, and thus we have manually cross-validated our analysis programs on small examples and manually validated random selections from the real results.

With respect to construct validity (the validity of measures), there may be other metrics that are more accurate in measuring the cost-benefits of regression testing techniques.

5. RESULTS

We now analyze the results of our study with respect to our three research questions.

5.1 RQ1: The Effect of Configurations on Regression Testing

To address RQ1 we examine the fault detection ability and code coverage of test suites under different configurations across the sequence of `vim` versions, by measuring the FD and BC data values for each configuration. Figures 5 and 6 provide box plots for FD and BC across all versions of `vim`. Table 2 shows the detailed FD data. In Figure 5 the dot on each box plot is the FD value for the default configuration provided from SIR. In most versions the default configuration has a level of performance well below that of the best configuration. For all versions, the FD values range from about 30% to 100%, representing a range of almost 10 faults, although the BC values are fairly constant. The lack of variance in BC may be due to the weakness of block coverage adequacy.

The CAF (Figure 7 and Table 3) and CAT (Figure 8 and Table 4) results are used to distinguish behaviors relative to individual faults and tests, focusing on a different granularity of our measures. The data shows a great deal of variation in CAF values. This means that some configurations match closely in fault detection effectiveness across versions while others vary greatly. The average CAF value for each ver-

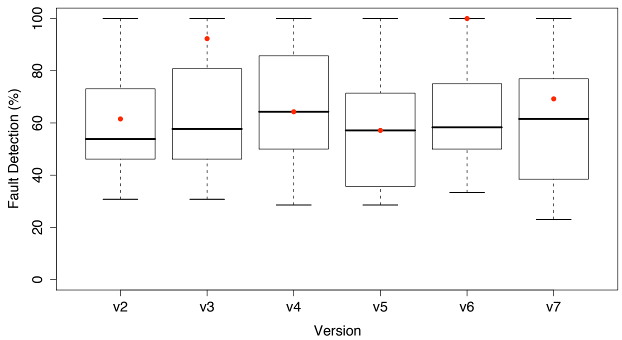


Figure 5: Fault Detection

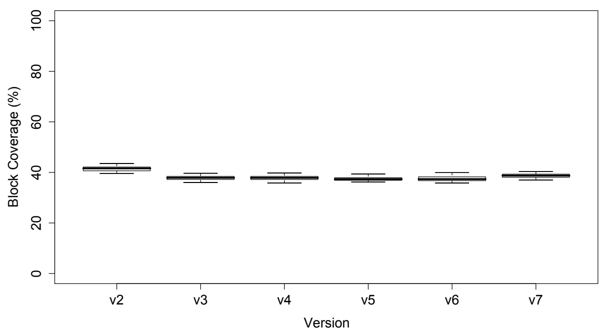


Figure 6: Block Coverage

	Min	Max	Mean	Median	StdDev
v2	**30.77**	**100.00**	**58.46**	**53.85**	**17.76**
v3	**30.77**	**100.00**	**61.15**	**57.69**	**21.22**
v4	**28.57**	**100.00**	**65.48**	**64.29**	**20.72**
v5	**28.57**	**100.00**	**58.81**	**57.14**	**21.70**
v6	**33.33**	**100.00**	**63.75**	**58.33**	**21.47**
v7	**23.08**	**100.00**	**60.00**	**61.54**	**21.84**

Table 2: Statistics for FD

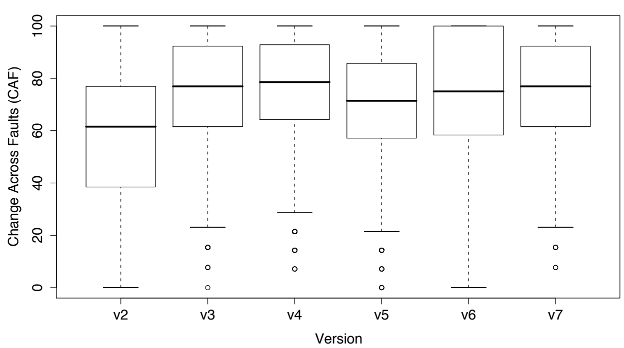


Figure 7: Coverage Across Faults

	Min	Max	Mean	Median	StdDev
v2	**0.00**	**100.00**	**59.55**	**61.54**	**24.23**
v3	**0.00**	**100.00**	**74.02**	**76.92**	**21.33**
v4	**7.14**	**100.00**	**75.23**	**78.57**	**22.29**
v5	**0.00**	**100.00**	**69.21**	**71.43**	**23.41**
v6	**0.00**	**100.00**	**73.96**	**75.00**	**24.06**
v7	**7.69**	**100.00**	**74.66**	**76.92**	**22.18**

Table 3: Statistics for CAF

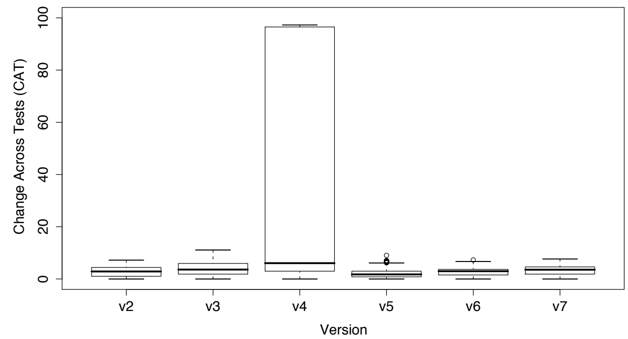


Figure 8: Coverage Across Tests

	Min	Max	Mean	Median	StdDev
v2	**0.00**	**7.18**	**2.79**	**2.87**	**1.82**
v3	**0.00**	**11.08**	**3.99**	**3.59**	**2.50**
v4	**0.00**	**97.33**	**45.41**	**6.05**	**46.41**
v5	**0.00**	**9.02**	**2.10**	**1.74**	**1.52**
v6	**0.00**	**7.28**	**2.66**	**2.97**	**1.55**
v7	**0.00**	**7.69**	**3.32**	**3.54**	**1.70**

Table 4: Statistics for CAT

sion is between 60% and 75%. The CAT values exhibit a much smaller difference, averaging only around 3% except on version 4 which exhibits a large fluctuation.

Our results bring us to the conclusion that the CDL plays an important role in fault detection on this system, and *which* configuration is tested greatly impacts the result of regression testing. There are large differences in fault detection at the test suite level, although there are only small differences in block coverage. We see the greatest fault detection variation at the test suite level of granularity.

5.2 RQ2: Effectiveness of CIT for Testing Configurations

To examine our second research question, we first compare fault detection abilities between two sets of configurations: our CIT sample and a randomly generated sample of the same size based on the same TSL model.

Both of these samples detect all seeded faults when all configurations are tested cumulatively. However, there are some differences in the distribution of fault detection effectiveness abilities for individual configurations. Figure 9 provides a box plot comparison of the two sets of configurations. We can see that the CIT sample seems to be distributed more towards the higher fault finding end.

We examined this further by finding the number of configurations needed to detect all faults within each sample if we were to just run each sample in an unprioritized order (NCF). The box plot in Figure 10 shows 50 randomly selected orders for each sample. We show this for all versions.

We also applied the Wilcoxon two sample test on each version, with an α level of 0.05, to examine whether there is a significant difference between these two groups (see Table 5). Our data shows a significant difference.

We can come to only a weak conclusion on this research question. Overall, CIT does appear to provide some benefits. The results of fault detection using CIT generated configurations show that in most cases, the median fault finding ability of the CIT sample is similar to or higher than that of the default SIR configuration with respect to the number of

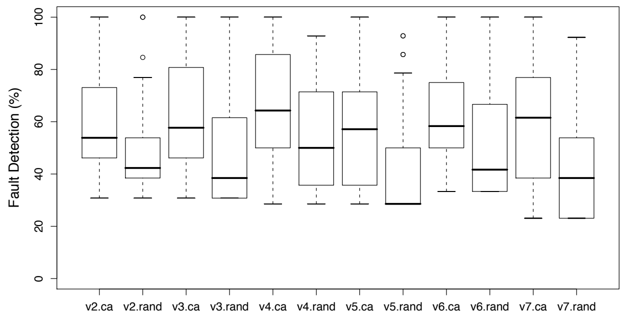


Figure 9: Fault Detection: CIT vs. Random

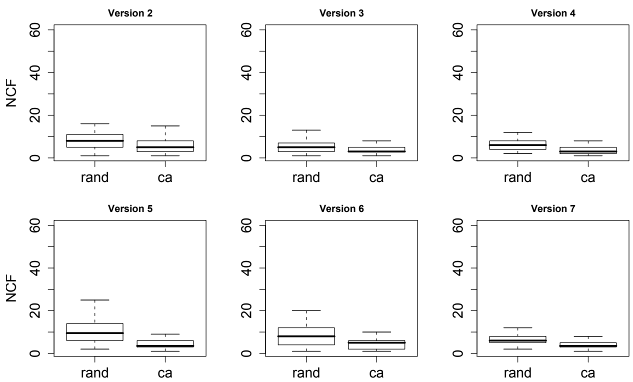


Figure 10: NCF: CIT vs. Random

Wilcoxon Two-Sample Test		
Statistic		**1843.5000**
Normal Approximation		
Z		**-4.7360**
One-Sided	**Pr < Z**	**< .0001**
Two-Sided	**Pr > |Z|**	**< .0001**
t Approximation		
One-Sided	**Pr < Z**	**< .0001**
Two-Sided	**Pr > |Z|**	**< .0001**

Table 5: Wilcoxon Test Results for v2

faults detected. Half of the CIT configurations detect more faults than the default SIR configuration. Though the cumulative fault detection effectiveness of the random sample is the same as that of the CIT sample, individually the CIT configurations appear to have slightly better fault detection capabilities.

5.3 RQ3: The Effectiveness of Prioritization

RQ3 examines whether prioritized CIT configurations yield faster fault detection than unprioritized ones.

We provide the NAPFD values for some of the prioritization techniques we considered in Table 6. (We omit the regenerated FD prioritization results since they closely follow those of the regenerated BC metric.) Recall that higher NAPFD value represent earlier detection of faults.

To simulate a resource-constrained testing environment in relation to this research question, we selected "budgets" of configurations in increments of five configurations for analysis. Each budget represents the maximum number of configurations that we are allowed to run. We calculated the NAPFD for each method at each budget level. For instance,

		pure			**regen**	
version	**unordered**	**FD**	**BC**	**TSL**	**BC**	**TSL**
	Budget: 5					
v3	**.76**	**.78**	**.78**	**.81**	**.90**	**.90**
v4	**.76**	**.90**	**.90**	**.90**	**.90**	**.90**
v5	**.74**	**.71**	**.69**	**.68**	**.90**	**.90**
v6	**.73**	**.90**	**.90**	**.90**	**.90**	**.90**
v7	**.76**	**.90**	**.73**	**.90**	**.90**	**.90**
	Budget: 10					
v3	**.88**	**.89**	**.89**	**.90**	**.95**	**.95**
v4	**.88**	**.95**	**.95**	**.95**	**.95**	**.95**
v5	**.86**	**.81**	**.79**	**.79**	**.95**	**.95**
v6	**.85**	**.95**	**.95**	**.95**	**.95**	**.95**
v7	**.88**	**.95**	**.87**	**.95**	**.95**	**.95**
	Budget: 15					
v3	**.92**	**.93**	**.93**	**.94**	**.97**	**.97**
v4	**.92**	**.97**	**.97**	**.97**	**.97**	**.97**
v5	**.91**	**.87**	**.86**	**.86**	**.97**	**.97**
v6	**.91**	**.97**	**.97**	**.97**	**97**	**.97**
v7	**92**	**97**	**.91**	**.97**	**.97**	**.97**
	Budget: 60					
v3	**.98**	**.98**	**.98**	**.99**	**.99**	**.99**
v4	**.98**	**.99**	**.99**	**.99**	**.99**	**.99**
v5	**.98**	**.97**	**.97**	**.97**	**.99**	**.99**
v6	**.98**	**.99**	**.99**	**.99**	**.99**	**.99**
v7	**.98**	**.99**	**.98**	**.99**	**.99**	**.99**

Table 6: NAPFD Values for Different Budgets

as the table shows, the NAPFD values for the unordered sample when we are allowed to run only five configurations ranged from .73 to .76, but the NAPFD values for regenerated CIT samples were higher at .90, indicating that prioritization was effective. After the 15th configuration (Budget 15), all faults are found in all versions, so NAPFD values stabilize and are very close to those measured for the full budget of 60; thus, we do not show the data for increments in between 15 and 60.

For all versions except version 5, the prioritized and regenerated configurations exhibited better NAPFD values than unordered CIT configurations, although the difference decreases as we run more samples in the configuration. We see very little difference between the different weighting schemes for pure prioritization. The regenerated samples, however, always seem to provide the highest NAPFD values.

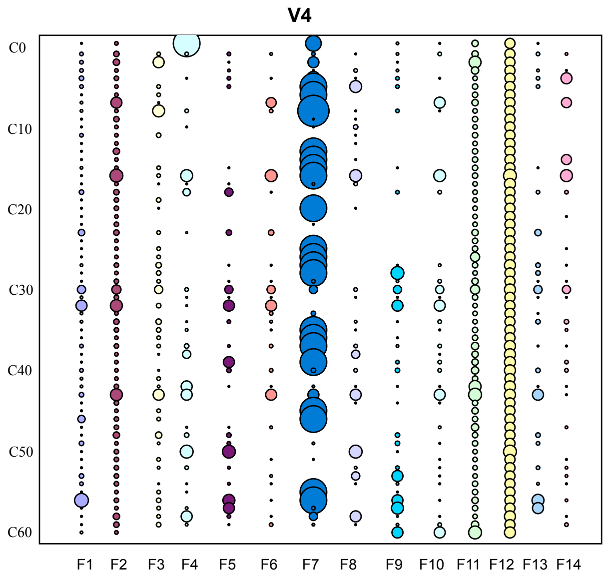
Our results suggest that both prioritized and regenerated configurations detect faults earlier than unordered configurations and that the regeneration techniques work better for early fault detection in application to our object of study. The choice of a specific prioritization technique seems less important than whether we choose pure prioritization or regeneration.

6. DISCUSSION AND FURTHER ANALYSIS

In this section, we provide additional discussion and analysis of the results just described. We also analyze some specific faults in detail to help further explain the results.

6.1 Configuration Dependent Faults

Inspecting our results and data further, it is apparent that some faults are found by every configuration while other faults are found by only some. To better understand this trend we plotted the faults for two versions of `vim` to show their distribution across configurations. Figure 11 shows the



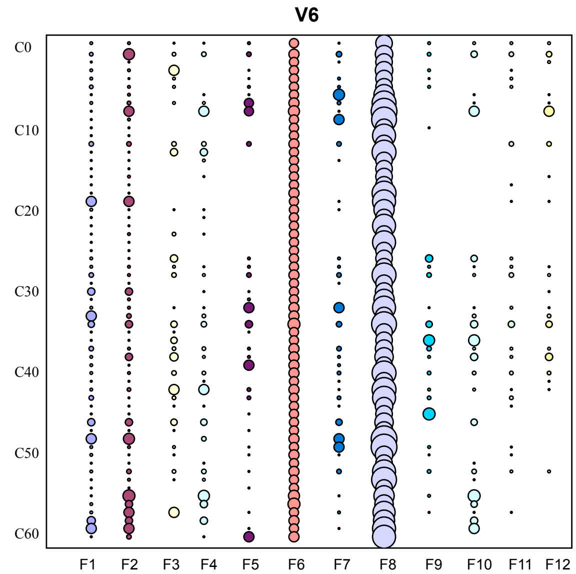


Figure 11: Fault Density

data for versions 4 and 6. On the `y-axis` we number the configurations and on the `x-axis` we list the individual faults. In version 4 the first four faults are hand seeded while the rest are mutants. In version 6 the first two faults are hand seeded. The size of the dots represents the number of test cases that detected a specific fault. The graphs show, for instance, that Faults 1, 2, 11 and 12 in version 4 are found during testing in all configurations. However, while Fault 12 seems very evenly handled across configurations, Fault 1 exhibits some variation. Other faults such as Fault 7 in version 4 are found with great frequency in some configurations but completely missed in others. We call the first type of fault *configuration-independent* and the second type *configuration-dependent*.

Note that there may be two definitions of configuration-independence, one at the test suite level and one at the test case level. It is possible for all configurations to find the same fault with at least one test case, but the actual test cases which find these may differ. This has implications for test case selection, since it implies that different test subsets may behave differently even when full test suites do not. All of the configuration-independent faults found in our subject are of the first type: they are configuration-independent at the level of test suites.

We next examine some specific faults to understand what makes them independent or dependent. Fault 11 in version 4, located in a routine named `window.c`, is a configuration independent fault. The faulty code incorrectly tests to see whether a buffer is NULL. The code is called unconditionally, and its execution status has no connection with any options that can be tuned by users. The test case that detects this fault under each configuration belongs to those that test program startup behavior, and thus it is run in each configuration.

Fault 7 in version 4 is configuration dependent; in fact, this fault plays a large role in the unusual fluctuation of the CAT metric for this version. This fault is located in a function named `my_sync()` in file `memfile.c`. This function is used to synchronize output of changed parts of the memory file to disk. Fault 7 is a particularly easy fault to find when its enclosing function is executed because it does not occur inside any conditional statements. Thus, we analyzed the functions that call `mf_sync()` and found that they are all called under certain conditions, controlled by the parameters `p_uc` or `b_may_swap` which relate back to our TSL model. The first parameter is associated with the number of synchronized characters, which is set by the configurable option `updatecount`, and the latter decides whether or not a swap file can be opened, which is turned on/off by another user configurable option called `swapfile`. In other words, if a swap file is not permitted by `set updatecount=0` and `set noswf`, then `mf_sync()` will never be executed and the fault will not be detected.

Now consider the alternative case where `updatecount`>`0`. The function `ml_open_file()`, which also calls `mf_sync()`, is called by another function, `set_bool_option()`. The `set_bool_option()` function is executed in any program run, because it is used to set Boolean options on startup. Hence, if we ever execute `set_bool_option()`, we will always execute `mf_sync()`, the faulty code, and detect the fault. Therefore, it is not surprising that a large proportion of test cases detect this fault when the required configurable options are set.

6.2 Analysis of Data Outliers

There are two places in our data that warrant further examination. First, in version 4 we see an unusual variation in the CAT metric. Second, we see an inverse in the results of NAPFD in version 5 for smaller budgets in the purely prioritized samples. We examine each of these in turn.

In version 4, there is a moderate range of CAF values but a very large range of CAT values. In fact, this metric varies from almost 100 down to 6 (see Table 4). We illustrate how this is possible. Suppose we have four test cases, three faults and two configurations as shown in the fault matrices in Table 7. In this case the value of CAF is 1/3 and the value of CAT is 1. The difference is caused by Fault 0. None of the test cases detect this fault under Configuration 1 but all four test cases detect it under Configuration 2. Hence this suggests that if there is a small range of CAF values but a

F/T	t_0	t_1	t_2	t_3	**F/T**	t_0	t_1	t_2	t_3
f_0	**0**	**0**	**0**	**0**	f_0	**1**	**1**	**1**	**1**
f_1	**1**	**0**	**0**	**0**	f_1	**1**	**0**	**0**	**0**
f_2	**1**	**1**	**0**	**0**	f_2	**1**	**1**	**0**	**0**
Fault Matrix for c1					Fault Matrix for c2				

Table 7: Fault Matrices for Configuration 1 and 2

large range of CAT values, there may exist faults that are triggered primarily by configuration options. This explains the phenomenon seen in version 4: Fault 7 is detected by more than 70% of the test cases under some configurations but never detected under others.

Finally, we examine the anomaly in prioritization for version 5. As can be seen in Table 6, this version has lower NAPFD values than the unordered samples; but we see this only for the purely prioritized sample, not for the regenerated one. Between version 4 and version 5 a large number of options were deleted from the model (this was based on documentation of the tool). Since we use version 4 to prioritize version 5 for the pure prioritization, a lot of information is lost. During the calculation of the interaction benefit we may have many options that do not contribute to the order of version 5. In the regenerated version we use the documentation from the current version, therefore, our prioritization seems to work as well as on the other versions.

6.3 Practical Significance

We believe that the results presented here provide some practical guidance for testers. In our study, we began with a test suite and a single configuration from SIR that correspond to an actual testing scenario set up for that system. By running the same set of test cases using additional configurations, we were able to uncover faults previously undetected. In practice, testers should consider configurations.

There is a trade-off, however, between the time needed to run additional configurations and the new faults that may be found by running them. The time required (over seven hours for a single configuration) to run all test cases of `vim` means that in practice, running configurations is an expensive proposition. We thus believe that prioritizing configurations will be most beneficial when resources are limited, such that testing effort may be foreshortened.

In our study we also found that only a relatively small percentage of configurations were needed to reach fault detection levels comparable to much larger percentages; therefore, other techniques for sampling configuration spaces may also work in practice. This implication, however, may be dependent on the object program considered, and the type of faults. In `vim`, for example, we observed the best prioritization results using regenerated CIT samples; in practice this means that we tested different configurations each time. Since our configurations did not require recompilation, but rather just a modification of a single file, we believe that this is the better approach to follow with this system. Systems that require extensive compilation or other mechanisms to change configurations, however, may find pure prioritization to be less expensive.

Finally, we did find that the CIT sample size is larger for regenerated CIT samples (65-95); when the full set of configurations must be run this may limit the regeneration approach.

7. RELATED WORK

There has been a large body of work on regression testing for both regression test selection (e.g., [6, 23, 26, 27]) and test case prioritization (e.g., [13, 29, 32]). Most of this work has focused on the test suite or test case as the object of selection or prioritization. Our work differs in that it focuses on prioritizing configurations.

Other work on CIT for testing configurable systems [15, 18, 33] examines the effectiveness of CIT to model configurations for testing, but it considers fault detection or localization only in single versions of a system, and does not try to quantify the impact on the CDL. In [10] we examine the impact of configurations on the CDL, but this is a preliminary study on a single subject for a single version of a web browser; it does not use prioritization or leverage CIT.

Recent work on prioritization of CIT test suites for evolving systems was reported in [25]. This is closely related to the current work, where we have used similar prioritization techniques. However, the object of prioritization in [25] is the test suite.

Finally, there has been some work on prioritization algorithms [5] for CIT. An algorithm from that work is used in our own work; however [5] does not present any empirical results of using the algorithm, nor does it provide methods for assigning weights to calculate interaction benefits.

8. CONCLUSIONS

In this paper we have presented the results of an empirical study to examine the effects of changing configurations on a user configurable system, `vim`, across multiple versions. We see compelling evidence that the CDL plays an important role in fault detection on this software subject: as many as 10 faults might be missed if the "wrong" configurations are omitted from testing. We see the greatest fault detection variation at the test case level, which may have implications for regression test selection. Not one of the faults in our study were detected by an equivalent subset of test cases across all configurations. We have also examined the effectiveness of CIT in sampling the configuration space. We conclude that it is effective, but the effects are not particularly strong. Finally, we have evaluated several techniques for CIT configuration prioritization. Our results suggest that both prioritized and regenerated configurations may detect faults earlier than unordered configurations and that the regeneration based techniques outperform the pure prioritization techniques.

In future work, we intend to examine additional configurable systems and apply these techniques. We are examining additional heuristics to be used for prioritization and examining the difference between random and CIT samples more thoroughly. We are also considering techniques for prioritizing higher strength covering arrays, and grouping homogeneous options for generating variable strength arrays. Finally, we are examining the differences between configuration dependent and independent faults.

Acknowledgments

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