The Inversive Relationship Between Bugs and Patches: An Empirical Study

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***Abstract*—Software bugs[1] pose an ever-present concern for developers, and patching such bugs requires a considerable amount of costs through complex operations. In contrast, *introducing* bugs can be an effortless job, in that even a simple mutation can easily break the Program Under Test (PUT). Existing research has considered these two opposed activities largely separately, either trying to automatically generate realistic patches to help developers, or to find realistic bugs to simulate and prevent future defects. Despite the fundamental differences between them, however, we hypothesise that they do not syntactically differ from each other when considered simply as code changes. To examine this assumption systematically, we investigate the relationship between patches and buggy commits, both generated manually and automatically, using a clustering and pattern analysis. A large scale empirical evaluation reveals that up to 70% of patches and faults can be clustered together based on the similarity between their lexical patterns; further, 44% of the code changes can be abstracted into the identical change patterns. Moreover, we investigate whether code mutation tools can be used as Automated Program Repair (APR) tools, and APR tools as code mutation tools. In both cases, the inverted use of mutation and APR tools can perform surprisingly well, or even better, when compared to their original, intended uses. For example, 89% of patches found by SequenceR, a deep learning based APR tool, can also be found by its inversion, i.e., a model trained with faults and not patches. Similarly, real fault coupling study of mutants reveals that TBar, a template based APR tool, can generate 14% and 3% more fault couplings than traditional mutation tools, PIT and Major respectively, when used as a mutation tool. Our findings suggest that the valid scope of mining code changes for either mutation or APR can be wider than previously thought.**

***Index Terms*—Software bug, software patch**

I. Introduction

Software bugs are prevalent, long-lived, and expensive to fix, thus they have been a major concern in software maintenance [1], [2]. The high cost of bug fixes mainly stems from the need for the developers to deeply engage with the bugs by following such steps as 1) reading the bug report, 2) inspecting the possible buggy locations, and 3) making several attempts to write patches (which involve compiling and running tests). All of these steps require a deep understanding of both the bug and the Program Under Test (PUT). In addition, developers should be aware of the possibility that, despite fixing the initial buggy symptoms, they may have introduced a new bug with their patch [3], [4], further adding to the complexity of bug fixes.

On the contrary, we note that it is trivially easy to *introduce* bugs. Given access to the source code, bugs can be introduced effortlessly, mainly because there exist infinitely more incorrect programs than the ones that satisfy the given specification in the space of all programs. This fundamental difference between writing patches and introducing bugs also can be seen in two types of automated software testing and debugging techniques: Automated Program Repair (APR) [5], which automates the process of writing patches, and Mutation Testing [6], which automates the process of introducing bugs. To successfully repair a bug, the APR technique needs to overcome many different challenges: it first has to accurately localise the bug, then find a specific combination of existing ingredients to compose a patch, and finally apply and validate the patch by executing test cases. On the contrary, mutation testing is context insensitive and is allowed to make small syntactic changes to an arbitrary location in the source code.

However, if we look closely at how APR tools operate, they act like mutation tools. While trying to find a patch, it is natural for them to produce many *faults*, which are the by-product of the search algorithms adopted by APR techniques. On the relationship between mutation testing and APR, Weimer et al. observed that Generate & Validate (G&V) APR techniques form a dual of mutation testing [7]: APR aims to find mutants that pass the tests, while mutation testing aims to find mutants that fail the tests. One example that exploits this duality is PraPR [8], a recently proposed APR tool that directly augments the mutation operators of a Java mutation testing tool, PIT [9]. The evaluation of PraPR shows that the repair attempts using only PIT mutation operators perform surprisingly well: for Defects4J v1.2.0 subjects, it produced the plausible patches for 106 buggy versions, correct patches for 17 buggy versions out of 395 versions.

Given that APR and mutation testing tools have exactly opposite purposes, how can one be successfully used as another? We hypothesise that it is mainly because the patches and faults, when seen simply as changes made to the code, are not that different from each other. For example, consider a code change, from `a + b` to `a - b`. Although this is a

[1]In this paper, we use 'fault' and 'bug' interchangeably to refer to unwanted behaviour of a program during its execution.

```
@@ -64,9 +64,6 @@ protected
    DateTimeSerializerBase(Class<T> type,
      {
          ...
-         if (property == null) {
-             return this;
-         }
          JsonFormat.Value format = ...
          if (format == null) {
              return this;
```

Fig. 1: Fix commit of JacksonDatabind-102 in Defects4J

widely used arithmetic mutation operator, it can equally be a bug fixing patch. Similarly, even a code change that appears to be a common fix pattern, may eventually turn out to be a fault-inducing change, and vice versa. Figure 1 shows a such case where the developer-written patch is a deletion of a null checker. This might seem counter-intuitive as it is an inversion of a common fix pattern (i.e., adding a null checker) that becomes a fault-inducing pattern, but in this case, it turns out to be a correct patch.

Based on this observation, we design three empirical studies to investigate the relationship between the patches and faults from different angles. First, we compare their lexical and structural similarities by leveraging code change clustering and pattern inferring algorithms, then evaluate whether the patches and faults are grouped together. Second, we evaluate two mutation tools as APR tools and evaluate their effectiveness. Finally, turning the table, we directly convert APR tools into mutation tools and evaluate their ability to generate mutants that are coupled with real faults. The results of our empirical evaluation suggest that buggy and fixing code changes are in fact more similar to each other than we expect. The implication of this finding is that, when mining a particular type of code changes (i.e., either bug inducing commits, or bug fixing commits), we can consider wider range of code changes than we thought before. Further, it suggests a future direction of cross-purpose uses of both APR and mutation testing tools, or the development of a unified technique.

The main contributions of the paper are as follows:

- We empirically evaluate the similarities between patches and faults using 6k code changes in C projects and 7k code changes in JavaScript projects, mined from various open source repositories. The results suggest that patches and faults are in fact similar to each other, as they can be grouped together by the code change clustering and pattern inferring algorithms.
- We present an empirical evaluation of mutation tools as APR techniques. We employ mutation tools that are inversions of existing APR tools, and evaluate these variations by applying them to the buggy programs in Defects4J. The results shows that IBIR [10] (i.e., inverted TBar [11]) and inverted SequenceR [12] can still successfully generate 42 and 17 plausible patches, which are only eight and two fewer than their original forms.
- We also demonstrate that existing APR tools can be converted into mutation tools. A mutation coupling analysis using real faults in Defects4J shows that TBar, a template-based APR tool, can successfully generate 14% and 3% more fault couplings than widely studied mutation tools, PIT and Major, respectively.

The rest of the paper is organised as follows. Section II introduces a motivating example. Sections III, IV, and V provide a detailed experimental design and results of the three empirical studies respectively. Section VI discusses the threats to validity. Section VII presented related previous work and Section VIII concludes.

II. A Motivating Example

Figure 2 contains actual code changes from Defects4J. Let us begin with a simple question: are these patches, or bug inducing changes?

```
- if (dataset != null) {
+ if (dataset == null) {
```

(a) Fix change

```
- if (dataset == null) {
+ if (dataset != null) {
```

(b) Inverted fix change (i.e., bug inducing change)

```
1  public LegendItemCollection getLegendItems() {
2      ...
3      if (this.plot == null) {
4          return result;
5      }
6      int index = this.plot.getIndexOf(this);
7      CategoryDataset dataset =
   ↪   this.plot.getDataset(index);
8  -   if (dataset != null) {
9  +   if (dataset == null) {
10         return result;
11     }
12     int seriesCount = dataset.getRowCount();
13     ...
```

(c) Fix change with surrounding context

Fig. 2: A fix commit of jfreechart (Chart-1 in Defects4J)

Figure 2a contains an example fix change from jfreechart that modifies != to ==, while Figure 2b shows the inversion of Figure 2a. Without the captions, it is not easy to tell them apart. We hypothesise that a code change itself is context insensitive, which is why it is hard to distinguish a patch from a bug inducing change, and vice versa. The difference becomes clearer only when we consider the surrounding context of the change, as shown in Figure 2c. An earlier conditional statement `if (this.plot == null)` has a similar predicate and the same return statement; we also observe that the variable `dataset` is subsequently used in Line 13, suggesting that it is more natural to return when it is `null`. With the help from the context, we can guess that the change in Figure 2a is likely to be a fixing change, and not a bug inducing one. This example raises the questions of whether mining and interpreting the given code changes only as either fix or bug inducing is desirable, as well as how actually different they are

from each other. In the following three sections, we investigate and discuss those observations with three empirical studies, respectively.

```
@@
expression E0;
@@
- if (isupper((int )*E0))
+ if (isupper(*E0))
  {
-   *E0 = tolower((int )E0);
+   *E0 = tolower(*E0);
  }
```

(a) IfStatement/21/1/0

```
@@
expression E0;
@@
- if (isdigit((int )*E0))
+ if (isdigit(*E0))
  {
   ...
  }
```

(b) IfStatement/21/1/1

Fig. 3: Example clusters resulted from FlexiRepair

III. Similarity Study (RQ1)

RQ1. How similar are patches and faults to each other? We answer RQ1 using two approaches: clustering both patches and faults, and abstracting both as change patterns. With clustering, we investigate whether patches and faults can belong to the same cluster, whereas with pattern inferring, we investigate whether patches and faults can be abstracted into the same pattern. Answers to these questions would provide evidence of how much the patches and faults have similar structures and patterns. We perform the clustering analysis using FlexiRepair [13], and the pattern inferring analysis using SemSeed [14].

A. Cluster Analysis with FlexiRepair

FlexiRepair [13] is an extension and combination of FixMiner [15] and Spinfer [16]. Below, we will briefly introduce them with the procedure of how the clusters are formed.

1) FixMiner: To represent a code change, FixMiner [15] constructs a tree representation with three types of information: `Shape`, `Action`, and `Token`. Each of the information type corresponds to an abstraction level of the resulting representation. FixMiner starts the clustering at the highest abstraction level, which is `Shape`, and successively applies clustering to the results from the previous abstract level with `Action` and `Token`.[2] First, at abstraction level of `Shape`, FixMiner groups the code changes based on the type of the root node of AST and its depth (e.g., IfStatement/7), and identifies their clusters using algorithms of GumTree [17]. Next, the abstraction level of `Action` further divides the clusters generated by `Shape`, based on the change operations identified by GumTree. These clusters are labelled as node/depth/ShapeTreeClusterId (e.g., IfStatement/7/2).

[2]As FlexiRepair only considered `Shape` and `Action` for clustering, we exclude `Token` from the clustering process.

2) Spinfer: Spinfer [16] aims to infer semantic patches of Linux kernel by identifying the similar code fragments and control flows across code changes. Following FlexiRepair [13], we use Spinfer to finalise the clustering process on each cluster by FixMiner. As a result, the clusters generated by Spinfer have the lowest abstraction level and indexed as node/depth/ShapeTreeClusterId/ActionTreeClusterId (e.g., IfStatement/7/2/0).

The final output of Spinfer is the clusters of generic patches in the form of Coccinelle [18] transformation rules: Figure 3 shows two of generated clusters. They are assigned to the same cluster of IfStatement/21/1 at the `Action` abstraction level by FixMiner, but have been further divided into two smaller clusters, IFStatement/21/1/0 and IFStatement/21/1/1, by Spinfer because they have different structures of body of if statement.

TABLE I: C subject programs for cluster analysis using FlexiRepair

Repository	# Commits
libtiff	3,570
cmake	38,414
redis	8,770
gzip	604
libarchive	5,472
cairo	11,724
curl	26,967
tcl	17,145
nginx	6,858
apr	8,945
openssh-portable	11,004
gmp	16,782
lighttpd1.4	3,882
lighttpd2	1,551
git	46,715
MonetDBLite-C	48,461
freeradius-server	37,535
bind9	31,286
tmux	7,790
gstreamer	19,016

3) Dataset: We reuse the dataset presented by FlexiRepair: it contains mined commits in C projects from Github, Gitlab, and Savannah. However, it was not possible for us to process all repositories provided by the FlexiRepair dataset due to the limited computational resources. Instead, we randomly selected 20 repositories, as listed in Table I, and collect the code changes with the inverted changes, resulting in 720,000 changes. After applying the filtering rules of FlexiRepair to extract only fix commits, we have 3,000 fix changes that result in total 6,000 code changes considering inverted changes (i.e., bug inducing changes).

4) Evaluation: For the evaluation of FlexiRepair clusters, we use three levels of clusters from `Shape`, `Action`, and Spinfer, respectively. For the sake of simplicity, we hereafter denote the three levels as 'Level1', 'Level2', and 'Level3'. We report the number of clusters that include both the patches and faults for each level. Note that we cannot report other evaluation metrics such as cluster membership accuracy, as

there is no ground truth of correct cluster membership for code changes.

B. Pattern Analysis with SemSeed

SemSeed [14] presents an efficient algorithm for seeding realistic bugs by abstracting and matching bug inducing patterns. SemSeed selects an AST subtree of the changed line and extracts two token sequences (before and after the change) from the subtree. From them, the bug seeding patterns are inferred by abstracting all identifiers and literals into the placeholders, resulting in a pair of abstracted token sequences, $t = \langle t_x, t_y \rangle$. The tokens in t_x and t_y are either identifier or literals, or non-identifiers and non-literals.

As SemSeed only learns its patterns from faults, we modify it to consider both patches and faults, and see how many same $\langle t_x, t_y \rangle$ pairs are found in both of them. Let $t \in T_p$ be the pairs from the patches and $t \in T_f$ be the pairs from the faults. We report the number of pairs that exist in both T_p and T_f, which would be the code changes that represent both patches and faults. Based on the dataset presented by SemSeed that contains 3,600 inverted fix changes from 100 JavaScript projects in Github, we build our own that includes both fix and inverted changes, resulting in total 7,200 code changes.

TABLE II: Cluster analysis with FlexiRepair. x refers to the number of clusters having both patches and faults and y refers to the number of total clusters.

Cluster level	Level1 (node/depth)	Level2 (ShapeTreeClusterId)	Level3 (SpinferClusterId)
x / y	222 / 316 (70%)	569 / 1,800 (31%)	227 / 3,859 (7%)

```
@@
identifier I0;
expression E1;
@@
- char *I0 = rad_malloc(E1 + 1);
+ char *I0 = rad_malloc(E1);
```

(a) DeclStmt/10/0/0

```
@@
identifier I0;
expression E1;
@@
- const char *I0 = E1;
+ char *I0 = (char *)E1;
```

(b) DeclStmt/10/18/2

Fig. 4: Example Level1 cluster (DeclStmt/10)

C. Results of Cluster Analysis with FlexiRepair

Table II shows the number of clusters that have both patches and faults (x) and the total number of clusters (y). Among all clusters, 70% of Level1 clusters contain both patches and faults, followed by 31% of Level2 clusters, and 7% of the Level3 clusters: the trend confirms our expectation that the less we abstract the code changes, the more separated they would be. Figures 4 and 5 present a closer look at how clusters are formed at Level1 and Level2: Figure 4a and Figure 4b are in same cluster of DeclStmt/10, and Figure 5a and Figure 5b are in same cluster of ExprStmt/5/0. Both levels have placeholder identifiers or expressions that can be replaced, but Level2 clusters tend to show closer resemblance.

```
@@
identifier I1 = {getch,strequal};
expression list E2;
expression E0;
@@
- E0 = I1(E2);
+ E0 = (char )I1(E2);
```

(a) ExprStmt/5/0/1

```
@@
assignment operator A1;
expression E2, E0, E3;
@@
- E0 A1 strlen(*E2) + E3;
+ E0 A1 (int )strlen(*E2) + E3;
```

(b) ExprStmt/5/0/2

Fig. 5: Example Level2 cluster (ExprStmt/5/0)

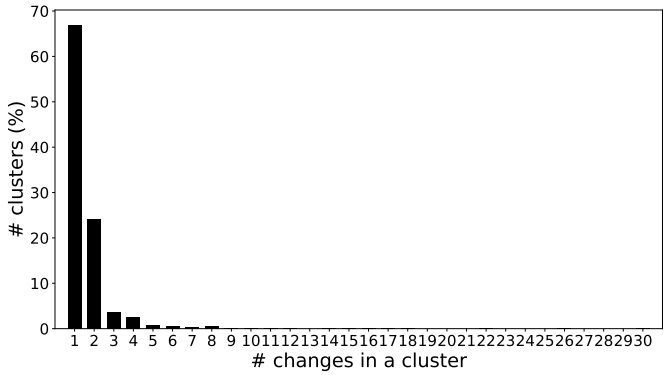


Fig. 6: The number of code changes for each Level3 cluster.

```
- reject(error);
+ reject(convertImapError(error));
```

(a) Fix commit of Adobe Brackets @a460c47

```
- callback(_err);
+ callback(mapError(_err));
```

(b) Inverted fix commit of nylas-mail @93942e7

Fig. 7: Two code changes having same SemSeed patterns

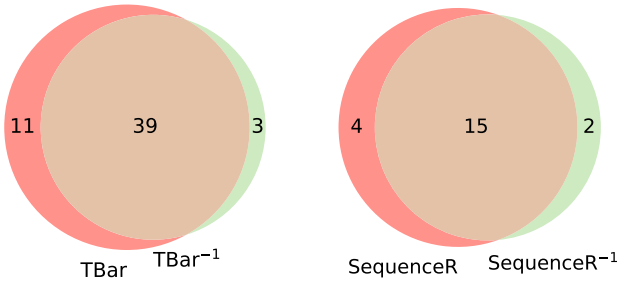
In addition, we examine how many code changes are in each Level3 cluster, since the overlaps of the patches and faults in Level3 clusters are only 227 out of 3,859 cases (7%). Figure 6 shows that 67% of Level3 clusters contain only one code change, failing to be grouped with any others due to the low abstraction level. Once we exclude such singletons, the proportion of the clusters that have both the patches and faults becomes 22%.

D. Results of Pattern Analysis with SemSeed

Next, we analyse the patterns of the patches and faults using SemSeed. We count the code changes whose patterns

(t) are found in both patches (T_p) and faults (T_f). Out of 3,951 code changes for each group, 1,752 code changes (44%) have patterns found in both T_p and T_f. For example, Figure 7 shows two code changes from different projects: Figure 7a is a fix change from Adobe Brackets which adds a method call `convertImapError` around `error`, and Figure 7b is a bug inducing change from nylas-mail which does similar modifications to Figure 7a. Both changes are abstracted into the pattern from $Idf_1(Idf_3)$; to $Idf_1(Idf_2(Idf_3))$; where Idf represents a placeholder identifier.

Answer to RQ1: Up to 70% of patches and faults can be clustered together by FlexiRepair; 44% of them can be abstracted into the same pattern by SemSeed. We conclude that patches and faults are not mutually exclusive and can be similar to each other.



(a) TBar and TBar^{-1} (b) SequenceR and SequenceR^{-1}

Fig. 8: The number of bugs for which mutation or APR can generate plausible patches.

IV. Mutation-to-APR Study (RQ2)

RQ2. Can mutation tools be APR tools? Given that the patches and faults have similar forms, we now move on to the cross-evaluation between them by investigating the patch effectiveness of the mutation. We treat the generated faults as patches and see whether they are actually plausible patches. If the patches and faults are fundamentally different and mutually exclusive, mutation tools will fail to generate any plausible patches.

Ideally, we would use existing mutation tools to perform this study. However, among the available and widely used Java mutation tools, PIT [9] has already been evaluated as an APR tool by PraPR [8], whereas Major [19] does not allow any modifications that are necessary for our experimentation as its source code is not available. As a result, we use IBIR [10], which mutates source code using inverted templates of TBar [11], and SequenceR^{-1}, which is originally the Neural Machine Translation (NMT) based APR tool, SequenceR [12], but trained with inverted dataset so that it 'translates' correct code to buggy code. Essentially, these two tools are direct inversions of APR tools, meaning that they are now designed to introduce bugs. Consequently, if these inversions can still patch bugs, it would show that our hypothesis about the similarities between patches and bugs is correct.

A. Modification Details

For this study, we introduce the following modifications to the original tools.

- *IBIR:* To use IBIR as an APR tool, we take the source code of TBar and replace the fix templates of TBar with the templates of IBIR. We switch templates instead of directly using IBIR, in order to take advantage of APR related support functionalities that already reside in TBar. We denote this modified tool TBar^{-1} to differentiate it from the original TBar as well as IBIR.
- *SequenceR:* SequenceR learns to translate a buggy code into a correct code using the fix changes. We follow the same training procedure, but we place the buggy code in the position of the correct code, and vice versa: this model will learn how to translate the correct code to the buggy code. We denote this model SequenceR^{-1}.

B. Evaluation Metrics and Configurations

We report and compare the number of bugs for which mutation and APR tools generate plausible patches. We use Defects4J v1.2.0, which includes four project and 231 bugs.[3] We use the old version of Defects4J, v1.2.0, as some of the studied tools are specifically designed to target Defects4J v1.2.0. Note that we compare SequenceR and SequenceR^{-1} using only 75 bugs, as the original tool is designed to target only one line patches.

C. Results

We run two mutation tools, TBar^{-1} and SequenceR^{-1}, and two corresponding APR tools, TBar and SequenceR, on the buggy programs in Defects4J. Figure 8 depicts two Venn diagrams that show the number of bugs for which each tool can generate plausible patches. TBar generates plausible patches for 50 bugs, which is eight more than those fixed by TBar^{-1}; SequenceR generates plausible patches for 19 bugs, which is two more than those fixed by SequenceR^{-1}. There are overlaps of 73% (39) between TBar and TBar^{-1}, and 71% (15) between SequenceR and SequenceR^{-1}, respectively. Although, APR tools are better than mutation tools at generating plausible patches in general, the results suggest that mutation tools can successfully perform as APR tools even though they learnt to operate in the opposite direction.

We also perform a qualitative analysis of the bugs that either APR or mutation tool exclusively fixes. Of 11 bugs fixed only by TBar, eight are fixed with the insertion operators, five of which are related to adding null pointer checkers. On the other hand, out of three bugs fixed only by TBar^{-1}, two are fixed with the deletions operators. We suspect that this is due to the difference in the number of operators in these tools: TBar includes 12 insertion operators but only two deletion operators, meaning that TBar^{-1} has 12 deletion operators and two insertion operators.

[3]See details at https://github.com/rjust/defects4j/tree/v1.2.0.

SequenceR exclusively fixes four bugs, three of which are fixed with the variable replacements. The two exclusive fixes by SequenceR^{-1} are also related to the variable replacement and changing the condition of a if statement. Since SequenceR is an NMT based tool, it is difficult to analyse and interpret its operations. Further analysis would require improved explainability of the underlying NMT models.

Answer to RQ2: Despite having learnt from faults, mutation tools can successfully find plausible patches for 42 and 17 buggy programs in Defects4J, compared to their counterpart APR tools that find plausible patches for 50 and 19 buggy programs.

V. APR-to-Mutation Study (RQ3)

RQ3. Can APR tools be mutation tools? We investigate whether the code changes produced by APR tools can be used as bug injections, despite the original intention of being patches. Conducting a real fault coupling study by following Just et al. [20], we evaluate the effectiveness of mutants produced by APR tools. As there are inherent differences between APR tools and mutation tools, we describe the challenges we faced, as well as how we dealt with them.

A. What kinds of APR tools can we modify?

Table III presents a list of Java APR tools we have considered. Our final selection criteria are as follows:

- *Availability*: we exclude the tools that are not publicly available or do not make their source code publicly available. Hercules [21], CapGen [22], and CURE [23] are excluded for this reason.
- *Executability*: We exclude ssFix [24] because it fails to connect to the private code search engine, and CoCoNut [25] because it has unresolved issues in preprocessing of training data as well as the model training.[4]
- *Failing tests*: We exclude the tools that require failing tests for patch generation, because we will apply them to correct programs for injecting faults. We exclude APR tools based on genetic programming, such as GenProg [26] and ARJA [27], because they use fitness functions that check whether tests that originally failed subsequently pass.

After filtering, we are left with four APR tools: SimFix [28], PraPR [8], TBar [11], and SequenceR [12]. These have all been recently published and open sourced; further, they do not require failing tests.

B. How to modify them?

Due to the differences in design goals between APR and mutation tools, we are forced to make a few modifications to the chosen APR tools:

[4]https://github.com/lin-tan/CoCoNut-Artifact/issues/2

TABLE III: Considered Java APR tools

Tool	Selected?	Public?	Working?	Failing tests
GenProg [26]	No	Yes	Yes	Yes
Angelix [29]	No	Yes	Yes	Yes
Nopol [30]	No	Yes	Yes	Yes
ssFix [24]	No	Yes	No	No
CapGen [22]	No	No	-	No
ARJA [27]	No	Yes	Yes	Yes
SketchFix [31]	No	Yes	Yes	Yes
SimFix [28]	Yes	Yes	Yes	No
Hercules [21]	No	No	-	No
PraPR [8]	Yes	Yes	Yes	No
TBar [11]	Yes	Yes	Yes	No
SequenceR [12]	Yes	Yes	Yes	No
CoCoNut [25]	No	Yes	No	No
CURE [23]	No	No	-	No

1) Where to fix (i.e., mutate): APR tools employ FL techniques to locate the buggy statements. In contrast, mutation tools usually have manual options for specifying the files to be mutated. Thus, we make APR tools to target the locations to be mutated by directly manipulating the FL results.

2) When to terminate: for mutation testing, we assume that PUT has no defects, i.e., it has a green test suite. However, APR tools assume that PUT has defects, so they terminate when the candidate patch passes all tests. As our mutation goal is to simulate all possible mutants, we modify the termination criterion of APR tools so that they generate all target mutants without considering test results.

3) Filtering some pre-defined patterns: TBar is based on the templates that have been collected from the fix patterns in the repositories, as well as patches generated by other APR tools. Among the collected patterns, there are some patterns that are highly likely fix-patterns, e.g., inserting a null pointer checker. Even if it is possible that the developers would insert a wrong or inappropriate null pointer checker, we exclude such patterns by default, as we posit that those cases are rare. The two APR tools, TBar and PraPR, are modified in this way. However, to evaluate the effect of this filtering strategy, we also include the versions without such filtering, which we denote TBar$_\alpha$ and PraPR$_\alpha$.

4) Sampling: APR tools are generally designed to focus their efforts into a single location that is believed to be the location of the fault (which is why their results are highly dependent on the FL results [32]). In contrast, mutation tools aim to evenly spread their efforts across the entire program, with ways to sample mutants to control their numbers. In this regard, we modify APR tools so that we randomly sample only five mutations per location.

C. Coupling Study

Coupling Effect Hypothesis (CEH) states that, if a test suite can detect and kill simple mutants, it will also be able to detect larger and more complex faults [33]. This is why mutation testing is supposed to work. As such, the effectiveness of a mutation testing tool can be precisely measured if we can measure the degree of coupling with real faults.

We follow the same procedure of the coupling study adopted by Just et al. [20]. The mutants are said to be *coupled* with real faults, if they are killed only by the test cases that reveal the real faults. Given that there are failing test cases, $ft_i \in FT$, let T_{pass} be a set of passing tests and T_{fail} be a set of tests that includes all tests in T_{pass} and a single failing test case, i.e., $T_{fail} = T_{pass} \cup \{ft_i\}$. We then compose the pairs of T_{pass} and T_{fail} denoted by $\langle T_{pass}, T_{fail} \rangle$ using the bugs and the corresponding failing test cases in Defects4J. For each pair, if there is at least one mutant that survives T_{pass} but is killed by T_{fail}, we mark the pair as *coupled*. Generating mutants using the modified APR tools, we compare the number of *coupled pairs* with the results from the two baseline mutation tools, PIT and Major.

Although we limit the mutant generation using random sampling at runtime, the number of generated mutants varies significantly between the studied tools. It would not be a fair comparison if there is a tool that generates many more mutants than others, as it will by definition have a higher chance of being coupled with real faults. Therefore, for any tool that generates more mutants than our reference mutation tool, Major, we further take a random sample out of those tools so that we consider the same number of mutants as Major. We repeat the experiment 30 times and report mean of the numbers. We will also report the unsampled total couplings, to see the impact of this additional sampling.

D. Configurations

We use the default set of mutation operators provided by Major in Defects4J, and the mutators in 'old defaults group' for PIT.[5] To compose $\langle T_{pass}, T_{fail} \rangle$ pairs from bug benchmark, we use Defects4J v1.2.0 and exclude some subjects that we fail to run all subject tools on, resulting in 493 pairs. We ignore any equivalent mutants, as we are only interested in coupled mutants, while equivalent mutants cannot be coupled by definition. For all APR tools, we follow the settings specified in their original study. To alleviate a huge cost of running tests against mutants, we generate mutants on the files that the target faults reside in.

TABLE IV: Results of a fixed-size fault coupling study

Tool	# Coupled Pairs	# Total Pairs	# Total Mutants
Major	300	493	31,877
PIT	249	493	29,855
TBar	316	493	31,679
TBar^{-1}	314	493	31,679
TBar$_{\alpha}$	306	493	31,731
SequenceR	90	493	29,291
SequenceR^{-1}	132	493	24,720
SimFix	99	493	29,411
PraPR	204	493	31,874
PraPR$_{\alpha}$	175	493	31,877

[5]https://pitest.org/quickstart/mutators/

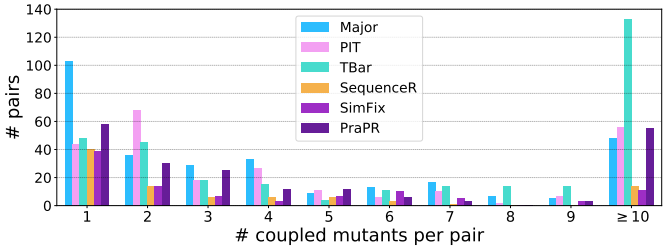


Fig. 9: A distribution of the number of coupled mutants for each coupled pair.

E. Results

We begin by presenting the results of the fixed-size fault coupling study, for which we sample the same number of mutants as Major from all studied mutation tools. Subsequently, we present the results of unrestricted coupling study, for which we use all mutants generated by each tool. Finally, we present a qualitative analysis of the new types of mutations introduced by APR tools.

1) Fixed-Size Coupling Study Results: Table IV presents the results of the fixed-size coupling study: it shows the total number of pairs, the number of coupled pairs, and the total number of mutants considered for each tool. TBar and its variants perform better than two traditional mutation tools by making up to 316 coupled pairs out of total 493 pairs (64%). In addition, we examine the number of coupled *mutants* for each coupled pair as shown in Figure 9. On the pairs that have more than ten coupled *mutants*, TBar makes 133 coupled paired whereas Major and PIT make 48 and 56 coupled pairs.[6] It is well known that the fix templates of TBar have been a powerful baseline for APR, and its effectiveness is shown to be valid for mutant generation.

APR tools other than TBar, on the other hand, are outperformed by the two mutation tools, Major and PIT. A closer inspection of the *mutants* generated by SequenceR and SimFix suggests that the degree of freedom these tools have sometimes results in mostly frivolous code changes. Mutations created by these tools include insertion of duplicate logical clauses (e.g., changing `if (a != 0)` to `if ((a != 0) && (a != 0))`) or insertion of pointless parentheses (e.g., changing `return m != null && m.containsKey(value)` to `return (m != null) && (m.containsKey(value))`). We hypothesise that these kinds of changes lack purpose, both as a patch and as a mutation: compared to the carefully curated change templates of TBar, the changes made by SequenceR are much more difficult to interpret as it depends on a sequence-to-sequence NMT model. Since the NMT model does not concern semantics of the produced token squences, some of the *translations* may lack purposes. Similarly, SimFix depends mostly on structural

[6]Note that the duplicated mutants [34], i.e., mutants that are semantically different from the original, but equivalent to each other, may inflate these numbers. However, since mutant equivalence is in general undecidable, we simply report the raw results. We expect TBar to be largely free from this effect, as each mutant it generates correspond to a unique template. We have excluded syntactic duplicates.

similarity to source the change to be applied as mutation. Without considering the surrounding semantic context, the changes are also likely to lack focus.

Interestingly, PraPR performs worse than PIT, producing 204 coupled pairs compared to 249 made by PIT, although it is built based on PIT by augmenting nine mutation operators of PIT with six additional operators. However, it is the six newly introduced operators that mainly contribute to the performance deterioration. These operators perform either addition of field and method guards, or addition of pre/post conditions, which we think are too specialised for the purpose of program repair to perform as generic fault injection.

Finally, we investigate whether filtering pre-defined patterns has any merits by comparing TBar with TBar$_\alpha$, and PraPR with PraPR$_\alpha$, respectively. Both comparisons show that the filtering helps generation of coupled mutants: TBar makes ten more coupled pairs, while PraPR makes 29 more.

TABLE V: Results of an unrestricted fault coupling study

Tool	# Coupled Pairs	# Total Pairs	# Total Mutants
Major	300	493	31,877
PIT	306	493	48,133
TBar	416	493	83,185
TBar^{-1}	413	493	82,943
TBar$_\alpha$	414	493	103,603
SequenceR	154	493	50,072
SequenceR^{-1}	164	493	30,773
SimFix	166	493	64,887
PraPR	326	493	186,640
PraPR$_\alpha$	328	493	242,258

2) Unrestricted Coupling Study Results: Table V shows the results of a coupling study using all generated mutants. TBar and its variants again outperform others by making up to 416 coupled pairs out of 493 total pairs (84%), showing that pre-defined fix patterns are capable of generating effective mutants. SequenceR and SimFix also perform better when we do not restrict the number of mutants: they generate 64 and 67 more coupled pairs compared to the fixed-size coupling study results. However, they are still significantly less effective than PIT and Major.

While unrestricted PraPR and PraPR$_\alpha$ outperform PIT and Major, this is mainly thanks to the huge number of mutants they generate: PraPR$_\alpha$ generates 200k mutants, compared to 40k generated by PIT. The operators newly introduced to PraPR are responsible for these extra 160k mutants, but they only contribute 20 additional coupled pairs.

In the unrestricted coupling study, TBar$_\alpha$ and PraPR$_\alpha$ subsume TBar and PraPR, respectively. Therefore, the effect of filtering some of the pre-defined patterns can be clearly observed by comparing them: both TBar$_\alpha$ and PraPR$_\alpha$ only make two more coupled pairs than TBar and PraPR, respectively, despite generating 1.2 times more mutants. Consequently, we conclude that the advantages of using all pre-defined patterns are negligible.

3) New and Stronger Mutation Operators: Just et al. [20] reported that 27% of real faults are not coupled to the

```
+ for (int i = 0; i < listSize; i++) {
+     if (!ShapeUtilities.equal ...
```

(a) Chart-6 (fix)

```
- ... Partial(iChronology, newTypes, newValues);
+ ... Partial(newTypes, newValues, iChronology);
```

(b) Time-4 (fix)

```
- ... convertLocalToUTC(localInstant, false);
+ ... convertLocalToUTC(localInstant, false, instant);
```

(c) Time-26 (fix)

```
- return allResultsMatch(n, MAY_BE_STRING_PREDICATE);
+ return anyResultsMatch(n, MAY_BE_STRING_PREDICATE);
```

(d) Closure-10 (fix)

```
+ removeDuplicateDeclarations(root);
...
- removeDuplicateDeclarations(root);
```

(e) Closure-102 (fix)

Fig. 10: The real faults in Defects4J that are exclusively coupled with the mutants of APR tools.

mutants generated by Major, and categorised the types of those uncoupled real faults. Based on it, we investigate the real faults to which PIT and Major cannot couple any mutants, but APR tools can, and report the operators used by the APR tools.

- *Statement or code deletion*: TBar can delete a single or multiple lines of code (see Figure 10a), whereas both PIT and Major lack the Statement Deletion (SDL) mutation operator to avoid compilation failures.
- *Argument swapping*: TBar and PraPR can generate mutants that swap the arguments to the method call (see Figure 10b), as they anticipate swapped arguments as a potential developer mistake.
- *Argument omission*: TBar, PraPR, and SequenceR can remove extra arguments from a method call (see Figure 10c), anticipating such omissions as a potential mistake.
- *Similar method called*: PraPR can change the method called to a similar method since PraPR has a method replacement operator that is not a part of PIT operators (see Figure 10d), in anticipation of developer mistakes.
- *Statement shifting*[7]: TBar can change the location of a statement to the other line thanks to its move statement operator (see Figure 10e).

Answer to RQ3: APR tools can successfully generate the mutants coupled with real faults, revealing new and stronger mutation operators.

VI. Threats to Validity

Threats to internal validity concern any factor that may influence the observed effects. To mitigate such threats, we limit the APR and mutation tools we study to those that

[7]This type was not originally listed by Just et al. [20].

are publicly available and widely studied. We also limit any modification we introduce to the minimum.

Threats to external validity concern the degree to which our results can be generalised. We tried to incorporate as many datasets and programming languages as possible, by studying C and JavaScript (RQ1) as well as Java (RQ2 and 3). While our idea is not inherently confined to a specific programming language, only further experimentations can generalise our results to new tools and languages. We adopt Defects4J as the standard benchmarks in both mutation testing and APR.

Threats to construct validity occur when the metrics we use fail to measure what we initially plan to observe. For RQ1, we simply count the number of clusters as there are no clear ground truths: we believe this is the simplest and the most direct measurement we can take. For RQ2, we only report the number of bugs for which subject tools can generate plausible patches. This is a simple count based metric that can be validated by test execution. For RQ3, we report the number of coupled pairs, following a widely accepted protocol.

VII. Related Work

Weimer et al. [7] explored the duality between APR and mutation testing. Specifically, they formalised and characterised Generate & Validate (G&V) program repair as a dual of mutation testing: the equivalent mutant problem is related to the redundant repairs, while the coupling effect is related to the hypothesis that simple operators can fix many complex faults. Both assume the competent programmer hypothesis, meaning that APR also assumes that a relatively simple patch can repair the bug. This is what PraPR explicitly exploits using the mutation operators implemented in PIT [8]. In this work, we further investigate the approach taken by PraPR using TBar^{-1} and SequenceR^{-1}. Moreover, our work is not confined to APR and mutation testing, but also considers the similarities between human written patches and faults (RQ1). While Weimer et al. proposed the duality as a theoretical framework, we present empirical evidence of the relationship.

Brown et al. [35] proposed a mutant mining technique that essentially inverts fix changes mined from open source repositories. In one of their experiments, they ask whether 'forward' and 'backward' patches are different. The forward patches refer to the original fix changes, whereas backward patches refer to their inversions (i.e., faults). Interestingly, their results showed an overlap of 1,710 mined operators, out of 13,929 operators mined from both directions. However, all operators were mined from a single project, Space [36], limiting the scope of generalisation. We have conducted a larger empirical evaluation with multiple programs and languages. Our results also suggest that mining mutation operators only from one direction may miss some relevant code changes.

While APR techniques can successfully patch many faults, it is known that they also produce many incorrect changes during the process [26]. This partly motivates our use of APR tools as a source of code mutation. Recently proposed NMT based APR techniques seek to avoid the generation of incorrect and wasted patches by incorporating the surrounding contexts better [23], [25]. The qualitative analysis of our results for RQ2 and 3 hints at the importance of contexts, calling for future work on ways to representing as well as comparing them.

VIII. Conclusion & Future Work

This paper aims to relate two seemingly opposite concepts in software testing, fixing (patch) and introducing bugs (fault). We highlight their syntactic similarities based on empirical evaluations. An analysis of 13k fix- and bug-inducing changes collected from open source repositories shows that, when abstracted and clustered together, it is difficult to distinguish patches from faults: up to 70% of the patches and faults are clustered together. Based on these results, we also show that mutation tools can be successfully used as APR tools, and vice versa. An evaluation using Defects4J bugs shows that mutation tools generate plausible patches for 42 and 17 bugs, only eight and two fewer than original APR tools, TBar and SequenceR, respectively. Finally, we also show that APR tools can successfully generate mutants that are coupled with real faults. Our findings suggest that the scope of code changes traditionally used to mine mutation operators, or to learn fix patterns and templates, may need to be widened to incorporate additional code changes that are relevant.

Future work could include a developer study where participants are asked to guess whether the given code change is a patch or a fault, to further understand the impact of context as well as a process of speculation. In addition, another interesting direction could be exploring the potential of using our findings to improve software testing tools. One example would be exploiting both directions of code changes to mine the real faults in the open source projects. Previous work has resorted to inverting the fix commits to generate the faults. This is due to the fact that identifying bug-inducing commits is challenging, as the developers may not be aware of introducing a bug and it is likely to contain the changes unrelated to the bug [37], [38]. On the other hand, identifying fix commits is relatively straightforward, as the developers intend to fix the bug with a commit message such as 'This handles the bug related to issue #10'. Our finding suggests that by reversing the code changes we can simply double the size of the existing dataset of real faults. A next step could be to conduct a study that utilises this larger dataset, such as comparing the performances of the learning-based static bug finders trained on the original dataset and the larger one, respectively.

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