Attention: Not Just Another Dataset for Patch-Correctness Checking

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ABSTRACT
Automated Program Repair (APR) techniques have drawn wide attention from both academia and industry. Meanwhile, one main limitation with the current state-of-the-art APR tools is that patches passing all the original tests are not necessarily the correct ones wanted by developers, i.e., the plausible patch problem. To date, various Patch-Correctness Checking (PCC) techniques have been proposed to address this important issue. However, they are only evaluated on very limited datasets as the APR tools used for generating such patches can only explore a small subset of the search space of possible patches, posing serious threats to external validity to existing PCC studies. In this paper, we construct an extensive PCC dataset (the largest manually labeled PCC dataset to our knowledge) to revisit all state-of-the-art PCC techniques. More specifically, our PCC dataset includes 1,988 patches generated from the recent PraPR APR tool, which leverages highly-optimized bytecode-level patch executions and can exhaustively explore all possible plausible patches within its large predefined search space (including well-known fixing patterns from various prior APR tools). Our extensive study of representative PCC techniques on the new dataset has revealed various surprising findings, including: 1) the assumption made by existing static PCC techniques that correct patches are more similar to buggy code than incorrect plausible patches no longer holds, 2) state-of-the-art learning-based techniques tend to suffer from the dataset overfitting problem, 3) while dynamic techniques overall retain their effectiveness on our new dataset, their performance drops substantially on patches with more complicated changes and 4) the very recent naturalness-based techniques can substantially outperform traditional static techniques and could be a promising direction for PCC. Based on our findings, we also provide various guidelines/suggestions for advancing PCC in the near future.

1 INTRODUCTION
Automated Program Repair (APR) [17, 22, 44, 78] aims to directly fix software bugs with minimal human intervention and thus can substantially speed up software development. Recently various APR techniques have been proposed and extensively studied in academia [20, 26, 35, 37, 57, 76]; furthermore, APR techniques have also drawn wide attention from the industry (e.g., Facebook [42], Alibaba [7, 39], and Fujitsu [53, 54]). A typical APR technique first generates various candidate patches based on different strategies, such as template-based [26, 34, 35], heuristic-based [22, 78], constraint-based [17, 44, 71] and learning-based [10, 32, 40] ones; then, it validates all the candidate patches via software testing [20, 22, 40], static analysis [60], or even formal verification [9]. To date, most APR systems leverage software testing for patch validation due to the popularity and effectiveness of testing in practice.

Although test-based patch validation has been shown to be practical for real-world systems, it suffers from the test overfitting issue – patches passing all the tests (i.e., plausible patches) may not always be semantically equivalent to the corresponding developer patches [51]. The reason is that software tests can hardly cover all possible program behaviors for real-world systems. Therefore, it is usually recommended that the developers should manually inspect the plausible patches to find the final correct ones. However, such manual inspection can be extremely challenging and time consuming given the large number of potentially plausible patches and code complexity for real-world systems [20]. To relieve the burden on developers, various techniques have been proposed to automate Patch-Correctness Checking (PCC) – while static techniques aim to infer patch correctness based on analyzing the patched code snippets [27, 64, 68], dynamic techniques rely on runtime information to determine patch correctness [70, 73]. In recent years, researchers have also leveraged the recent advances in deep code embedding for PCC [59].

While existing PCC techniques have shown promising results, they are mostly studied on limited PCC datasets since the APR tools used for generating patches for such datasets can only explore a very small portion of the possible patch search space due to the following reasons. First, the leveraged APR tools have been shown to be overfitting to the studied subjects, e.g., Defects4J V1.2 [16, 20]. Such APR tools usually apply very aggressive patch pruning strategies to fix more bugs on the studied subjects, but may fail to fix any bugs on other subjects, e.g., the recent SimFix/CapGen tools [22, 64] widely adopted in existing PCC datasets can only fix 0/2 more bugs on a newer version of Defects4J with ~200 more bugs [20]. In this way, the possible plausible patches that should be generated are...
also missed by such APR tools, and thus absent in the resulting PCC dataset. Second, due to the efficiency issue, most studied APR tools (e.g., SimFix) terminate patch exploration as soon as they find the first plausible patch. In this way, a large number of possible plausible patches can also be missed by such techniques. For example, even including 21 APR tools, the current largest manually labeled PCC dataset [61] only has 902 patches for Defects4J V1.2 in total. Third, most of the PCC datasets are built on the popular Defects4J V1.2, making it unclear whether the findings can generalize to other subjects. As a result, it is totally unclear whether the promising experimental results of existing PCC techniques can generalize to more realistic datasets and real-world software development.

In this paper, we aim to revisit existing state-of-the-art PCC techniques with a more extensive and real-world dataset to more faithfully evaluate their effectiveness in practice. To this end, we first construct an extensive dataset for PCC based on the recent PraPR repair system [20]. We choose PraPR given the following reasons: (1) its predefined patch search space is large since it applies popular well-known fixing templates from various prior APR work [3, 49, 56], (2) it is the only available APR tool (to our knowledge) that can exhaustively explore the entire predefined patch search space since it generates and validates patches based on its highly optimized on-the-fly bytecode manipulation. In this way, PraPR can generate all plausible patches within its clearly defined patch search space, which can largely avoid the dataset overfitting issue [20] and can work as an ideal candidate for constructing PCC datasets. We apply PraPR to both the widely studied Defects4J V1.2 dataset [23] with 395 bugs and the latest Defects4J V2.0 dataset with 11 additional subject systems and 401 additional bugs. In total, our dataset contains 1,988 plausible patches, including 83 correct patches and 1,905 overfitting patches after manual labeling. To our knowledge, this is the largest manually labeled dataset for PCC. Then, based on our new datasets, we perform an extensive study of prior PCC techniques, including state-of-the-art static [27, 58, 64, 65, 68], dynamic [70, 73], and learning-based [59, 74] PCC techniques. Our empirical study reveals various surprising findings that the community should be aware of, and based on the findings, we provide various guidelines/suggestions for further advancing PCC techniques in the near future. Overall, this paper makes the following contributions:

- **Real-world Dataset.** We create a large-scale realistic dataset for PCC. To our knowledge, this is the first dataset based on exhaustive patch search space exploration, as well as the largest manually labeled patch dataset for PCC to date.
- **Extensive Study.** We perform an extensive study of state-of-the-art PCC techniques, including static, dynamic, and learning-based ones, on our newly constructed dataset. In total, our experiments cost 580 CPU days, including static analysis, code embedding, test generation, and test execution for all patches within our dataset.
- **Practical Impacts.** Our empirical study reveals various surprising results the community should be aware of, as well as providing various practical guidelines for future PCC work, e.g., the dynamic PCC techniques can no longer work for patches on more complex and real-world bugs and should be largely improved.

## 2 BACKGROUND AND RELATED WORK

In this section, we first introduce the background on automated program repair and patch correctness checking, and then motivate our study by systematically revisiting datasets used in the literature.

### Automated Program Repair

Automated Program Repair (APR) [17, 22, 26, 35, 44, 57, 76, 78] aims at fixing bugs with minimal human intervention. Due to its promising future, researchers have proposed various APR techniques, which can be categorized according to how patches are generated: (1) **Heuristic-based APR** [31, 50, 77] leverages heuristics to explore the search space of patches; (2) **Template-based APR** [26, 34, 35] incorporates patterns summarized from historical developer patches to guide patch generation; (3) **Constraint-based APR** [17, 44, 71] leverages constraint solving to directly synthesize patches; (4) **Learning-based APR** [10, 32, 40] utilizes learning techniques (e.g., neural machine translation [6] to generate patches. APR tools then leverage software testing [20, 22, 40], static analysis [60], or even formal verification [9] to validate the patches.

### Patch Correctness Checking

Most APR techniques use software tests for patch validation, regarding the patches that can pass all tests as potentially correct. However, such an assumption can be problematic, since tests often fail to detect all possible bugs. Therefore, a plausible patch may be overfitting to the existing tests and yet be incorrect. To address such test-overfitting issue, various Patch Correctness Checking (PCC) [29] techniques have been proposed to differentiate overfitting patches from correct patches.

There are two application scenarios for PCC techniques, with oracle patch information or without. In the oracle-based scenario, the plausible patches that exhibit differently from the oracle patch are deemed as overfitting. Note that oracle-based PCC is usually used for APR experimentation, and cannot be applied to real-world bug fixing where the oracle patch information is unavailable. In contrast, oracle-absent PCC can identify potential overfitting patches without oracle, thus can be applied in real-world bug fixing. Therefore, we only focus on the oracle-absent techniques.

According to whether dynamic patch execution information is required, traditional PCC techniques can be categorized into static and dynamic techniques. In particular, static techniques leverage static code features [27, 64, 68], pre-defined anti-patterns [58] or even code naturalness [65] to predict patch correctness. Dynamic techniques leverage run-time information, such as crash or memory-safety issues used in Opad [73] and test execution traces used in Patch-Sim [70]. Recently, researchers have also started exploring advanced machine learning and deep learning techniques for PCC. Seminal research [74] characterizes patch code with pre-defined features, while more recent work [11, 59] directly encodes patch code via embedding models (e.g., BERT [15]) and learns to predict the probability of each patches being overfitting.

### Existing PCC Datasets

We revisit the existing peer-referred literatures on automated program repair and PCC based on the APR review [46] before 2022, and summarize the datasets used in their evaluation. Table 1 presents the characteristics of datasets used in existing work. Note that for the patches generated by APR tools, we check whether the early stop mechanism is enabled during APR procedure as shown in Column "Early stop". From the table, we can find that although involving various APR tools, subjects, and
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| Work            | Studied technique | Scale | Ratio | Subject source | Early stop |
|-----------------|-------------------|-------|-------|----------------|------------|
| Tan et al. [58] | Anti-patterns     | 269   | 30/299| CoREBench [8], GenProg [13] | 4/4 |
| Xin et al. [61] | DfTGen            | 89    | 10/79 | All from APR tools | 4/4 |
| Le et al. [25]  | SS                | 982   | 25/40 | All from APR tools | 4/4 |
| Yang et al. [27] | Opsi              | 449   | 22/427| All from APR tools | 4/4 |
| Xin et al. [61] | sFix              | 122   | 3/3 | All from APR tools | 4/5 |
| Xiong et al. [20] | Patch-Sim        | 119   | 29/14 | All from APR tools | 5/5 |
| Ye et al. [70]  | RGT               | 618   | 207/281| All from APR tools | 10/14 |
| Wen et al. [61] | CapGen            | 302   | 28/174| All from APR tools | 0/1 |
| Xin et al. [61] | sharpFix          | 82    | 56/28 | All from APR tools | 2/2 |
| Tan et al. [20] | Embodiment-based  | 1,400 | 48/552| Defects4J V1.2 | 12/17 |
| Wang et al. [11] | PCC techniques   | 1,060 | 249/354| Defects4J V1.2 | 16/21 |
| Lin et al. [15] | Cache             | 902   | 25,899/24,105| Defects4J V1.2 | 11/11 |
| Ye et al. [70]  | ODS               | 10,382| 2,081/2,299| Defects4J V2.0 | 11/11 |

Table 1: Datasets used in existing PCC work

patches, existing PCC datasets can still be insufficient for evaluating PCC, since they can only explore a very small portion of the possible patch search space due to the following reasons.

First, existing datasets mainly include the plausible patches generated by overfitting APR tools. As shown in recent work [16, 20], most existing APR tools are overfitting to the Defects4J V1.2 [23] benchmark. Such APR tools often leverage aggressive and specific patch pruning strategies so as to fix more bugs, but may suffer from the dataset-overfitting issue [37]. For example, the recent SimFix/CapGen tools [22, 64] can only fix 0/2 more bugs on a tool than its “real-world” APR tool [20]; also, 11 APR tools have been shown to perform substantially worse on other benchmark tools than the widely-used Defects4J V1.2 [16]. In this way, the possible plausible patches that should be generated are missed, and thus absent in the resulting PCC dataset.

Second, existing datasets only include the first one/few plausible patches generated by APR tools due to efficiency issue. APR tools (e.g., SimFix [22]) enable early stop mechanism, terminating patch execution as soon as they find the first plausible patch for the sake of efficiency. As shown in the table, most APR tools in existing PCC datasets enable early stop. In this way, existing PCC dataset may miss a large number of possible plausible patches that can be potentially generated by these APR tools. According to Noller et al.’s study [47], developers expect APR quickly (30-60 min) generate 5-10 patches each bug. But among the 16 APR tools investigated by Liu et al. [37], the most effective one, SimFix, set a timeout of 300 min for each bug but only generated 68 patches on Defects4J V1.2. Therefore, APR tools for previous datasets are not practical nor efficient. In fact, even including 21 APR tools, the existing largest manually labeled PCC dataset [61] has only 902 plausible patches for Defects4J V1.2; meanwhile a single APR (i.e., SequenceR [10]) can only generate at most 73 patches.

Third, most PCC datasets are built on the popular Defects4J V1.2 (old version), making it unclear whether the findings can generalize to other subjects. As shown in the table, the majority of existing PCC datasets are generated from the subjects in the most widely used benchmark Defects4J V1.2. Therefore, it remains unknown how existing PCC techniques perform on patches from other subjects.

To address the limitations above, in this work, we construct a more extensive and real-world dataset so as to revisit all representative PCC techniques. In particular, our dataset is built on the plausible patches generated by the recent byte-code level APR tool PraPR [20]. We choose PraPR since 1) its predefined patch search space is large since it applies popular well-known fixing templates from various prior APR work [3, 49, 56], and 2) it is the baseline APR tool that can exhaustively explore the predefined patch search space due to its highly optimized on-the-fly bytecode manipulation. In this way, PraPR can generate all plausible patches within the clearly defined patch search space, which other APR tools fail to reach. Furthermore, in addition to the most widely used Defects4J V1.2 dataset [23], we further apply PraPR to the most recent version of Defects4J dataset, i.e., Defects4J V2.0 with 11 additional subject systems and 401 additional bugs.

3 DATASET CONSTRUCTION

3.1 Subjects

Our datasets are constructed with patches generated for the subjects in Defects4J [23] due to the following reasons: 1) Defects4J contains hundreds of real bugs for real-world projects and has become the most widely studied APR benchmark in the literature, 2) most prior PCC studies were performed on Defects4J V1.2 [23, 59, 61, 67, 70, 75], thus enabling a more direct/fair comparison with prior work. We include all subjects from Defects4J V1.2 (with 395 bugs from 6 projects) since it is the most widely used Defects4J version in prior APR and PCC work [23, 59, 61, 64, 67, 68, 70, 75]. Furthermore, we also study the latest Defects4J V2.0, which includes 11 additional projects with 401 additional bugs, to study the dataset overfitting issue of existing PCC work, i.e., whether the prior PCC experimental results can generalize to the newer Defects4J version. Please refer to our website [4] for detailed statistics of Defects4J versions.
3.2 Patch Collection

Plausible Patch Collection. For each studied buggy version from Defects4J, we first run PraPR on it to generate all possible plausible patches (i.e., the patches passing all the tests). Since PraPR is based on bytecode manipulation, the resulting plausible patches are in the bytecode level. We further decompile these bytecode-level plausible patches into source-code-level patches since existing PCC techniques work at the source-code level. We make the following efforts to ensure the decompilation process as precise as possible. First, we configure PraPR to include all the required debugging information in the resulting bytecode-level patches. Second, we leverage the state-of-the-art decompiler JD-Core [2] to decompile patched bytecode files. Third, we only locate the patched line in the decompiled file (note that PraPR only changes one line on bytecode level patches), and patch this line into the original buggy source file with the help of debugging information to obtain a potential patch at the source-code level. Finally, we perform the sanity check to ensure the decompiled source-code-level patches indeed pass all the tests. Finally, we obtain 1,988 plausible patches.

Patch Correctness Identification. For each plausible patch, we then manually determine its correctness by comparing it to the developer patch. In particular, we follow the labeling procedure in previous work [37] and the patches that satisfy the following criteria are labeled as correct: (1) the patches that are syntactically identical to the developer patches, or (2) the patches that are semantically equivalent to the developer patches according to the rules summarized from existing work [37]. Otherwise, the patches are labeled as overfitting. We involve multiple participants with 3+ years Java development experience in the manual labelling procedure: two participants first label each plausible patch individually, and a third participant would be introduced to resolve the conflicts by discussing based on the corresponding bug report or issue link. In total, among all the 1,988 new plausible patches, 83 are correct and 1,905 are overfitting ones.

3.3 New Patch Datasets

The plausible patches above constitute our main dataset PraPRV2.0, with 1,988 plausible patches generated by PraPR on Defects4J V2.0. Based on the main dataset, we further construct three sub-datasets for more thorough study of PCC work. Table 2 presents the details.

PraPRV1.2. This dataset includes all 1,311 plausible patches generated by PraPR on Defects4J V1.2, separated from the main dataset to compare with prior PCC studies on the same Defects4J version.

MergeV2.0. Though PraPR contains overlapping patch fixing patterns with many other APR tools, it is still important to consider patches from other APR tools for more comprehensive PCC evaluation. Hence, we further combine our PraPRV2.0 with the existing largest labeled dataset derived from APR tools, i.e., WangV1.2 [61] that includes 902 plausible patches generated by 21 other APR tools. After carefully removing the duplicates between WangV1.2 and our dataset, we finally obtain 2,763 plausible patches in this merged dataset.

BalanceV2.0. Table 2 shows that the MergeV2.0 dataset is largely imbalanced with mostly overfitting patches. While PCC studies frequently leverage imbalanced datasets, we further construct a balanced dataset based on MergeV2.0 for more thorough evaluations. Specifically, we keep all correct patches from MergeV2.0 and randomly sample the same number of overfitting patches. To mitigate the bias in randomness, we repeat the random sampling process for ten times and present the average results on BalanceV2.0.

To our knowledge, the datasets newly constructed in this work are the largest manually labeled datasets for evaluating PCC. For example, PraPR alone generates 1,988 plausible patches while the existing largest PCC dataset has only 902 plausible patches (Lin et al. [33] and Ye et al. [74] have datasets with ~50,000 and ~10,000 patches but they are not manually labeled). In addition, merging the patches generated by PraPR with the existing datasets could further form a larger labeled dataset (i.e., MergeV2.0). The large scale of our new datasets is credited to the high efficiency and exhaustive search strategies of PraPR, which help collect many plausible patches that are missed by the other APR tools due to their efficiency issues or their early termination. For example, PraPR alone generates 39 unique plausible patches for the Defects4J bug Math-28, while the other existing 21 APR tools generate 7 plausible patches in total (5 of them are unique). The main reason could be that other tools early stop after finding the first plausible patch while PraPR exhaustively explores the whole search space. Therefore, at least 34 out of the 39 plausible patches generated by PraPR are not visited by the existing 21 APR tools.

4 STUDY DESIGN

4.1 Research Questions

Based on our newly constructed datasets, we revisit all state-of-the-art PCC techniques via the following research questions:

- RQ1: How does static PCC perform on our datasets?
- RQ2: How does learning-based PCC perform on our datasets?
- RQ3: How does dynamic PCC perform on our datasets?

4.2 Studied Techniques

Our study selects existing state-of-the-art techniques that are designed for or can be adapted to the PCC task. Specifically, the selected techniques can be broadly categorized into three categories, including static, dynamic, and learning-based techniques. We only include those techniques that do not require the oracle information (i.e., the developer patches) since the practical usefulness of those techniques requiring the oracle information is compromised [61].

4.2.1 Static Techniques. Wang et al. have empirically investigated the effectiveness of static features extracted from three tools [61], namely ssFix [68], S3 [27] and CapGen [64], and then utilized such features to check patch correctness. We use identical experiment settings and methodology to assess patch correctness in new dataset.

S3: S3 proposed six features to measure the syntactic and semantic distance between patched code and the original buggy code [30]. Among them, AST differencing and cosine similarity are utilized to prioritize and identify correct patches. Specifically, the sum of
these features is computed as the suspiciousness score. Patches with higher scores are more likely to be overfitting according to the previous study [5] which claims correct patches are more similar to the original buggy code, and thus possess fewer modifications. In addition, we exclude the other three features in this study following prior work [61]. Specifically, model counting and output coverage are excluded since they cannot be generalized to all the generated patches [61]. Besides, Anti-patterns utilized in S3 is excluded here since we utilize it as a stand-alone tool, following the previous study [61].

ssFix: ssFix focused on the token-based syntax representation of code to identify syntax-related code fragments to generate correct patches. Specifically, structural token similarity and conceptual token similarity are calculated to measure the similarity between the buggy code and patched code. Similar to S3, the sum of these features is used to rank patches. Patches with higher scores are ranked higher.

CapGen: CapGen designed three context-aware features to prioritize correct patches over overfitting ones, namely the genealogy similarity, variable similarity, and dependency similarity respectively. Although such features are not initially proposed for PCC, they can still be adapted to assess patch correctness from the view of static features. Specifically, following the existing study [61], the product of these similarity scores is used to rank and prioritize patches, and patches with higher scores are considered more likely to be correct.

For the above three static techniques, we apply the Top-N strategy to identify correct patches following prior work [61]. Specifically, the Top-N prioritized patches are labeled as correct while others as overfitting, where N is the number of correct patches in our dataset following prior studies [61]. To mitigate the effect of N, we also show the average ranking of correct patches per bug (i.e., denoted as AVR) to evaluate the ability of prioritizing correct patches (detailed in Section 4.3).

Anti-patterns: Anti-patterns provides a set of generic forbidden transformations, which can be applied on search-based repair tools [58]. It has been shown that Anti-patterns could help obtain program patches with higher qualities with minimal effort. To apply these Anti-patterns on our own dataset, we first map the Anti-patterns to PraPR’s mutation rules [20] through manual inspection. If the rules fall into a specific pattern, we deem those patches generated by the mutation rule as overfitting. Such a strategy is the same as that adopted by the existing study [61]. In this way, we observe that patches generated by PraPR mainly fall into four of the seven anti-patterns, including A1: Anti-delete CFG Exit Node, A2: Anti-delete Control Statement, A5: Anti-delete Loop-Counter and A6: Anti-append Early Exit. Therefore, we classify the patches that fall into these rules as overfitting and the others as correct.

Code naturalness: Cross entropy [14] for code measures the naturalness of code against the code language model [21]. Very recently, researchers have shown that code naturalness in terms of entropy values computed by LLMs can help rank patches for faster program repair [25, 66]. Furthermore, Xia et al. [65] demonstrated for the first time that it is possible to use such entropy values to perform patch correctness checking, i.e., correct patches can be more natural than overfitting patches. However, there is no comprehensive study on how such entropy-based techniques perform in PCC datasets to our knowledge. For a list of tokens in the corpus of LLM, the mean entropy can be calculated as the negative log probability of each token as mean_entropy = −∑_{i=1}^{n} log(p_{ti})/n, where p_{ti} refers to the ith token of the sequence and p_{ti} refers to the model probability of token t_{i}. Similarly, the sum entropy can be computed as sum_entropy = −∑_{i=1}^{n} log(p_{ti}). Patches with lower sum or mean entropies are considered more natural and will be ranked higher. In our evaluation, we use the model CodeT5-large [62], and follow the same experimental setting with prior work [63]. For entropy-based techniques, we only use AVR (as described in section 4.2.1) to evaluate the effectiveness of patch ranking since the entropy is dependent on the context of bugs and improper for comparing patches across different bugs.

4.2.2 Dynamic Techniques. Dynamic techniques are designed to capture testing behavior and result for patch assessment. Specifically, we focus on state-of-the-art dynamic techniques widely studied in the literature [61, 70, 73]:

Opad: Opad is designed based on the hypothesis that patches should not introduce new crash or memory-safety problems [73]. Therefore, any patch violating those rules is regarded as overfitting. Note that Opad is originally designed for C programs utilizing fuzzing techniques to generate new test cases. To adapt it on Java, we leverage two state-of-the-art test generation tools, i.e., Evo-suite [19] and Randoop [48], to generate test cases based on the buggy version. Opad based on EvoSuite and Randoop are denoted as E-Opad and R-Opad respectively, following the previous study [61]. Specifically, for EvoSuite and Randoop, we generate 30 test suites for each buggy program with a time budget of 600 seconds for each test suite utilizing existing test generation module in Defects4J framework. We finally successfully generate 44,937 test suites on 796 buggy programs in total. After test generation, we first run the generated tests (for five times) on the original buggy programs to observe that, we run the remaining ones on the patched programs. If any crash occurs or any exception is thrown, Opad will identify the patch as overfitting, otherwise it will identify the patch as correct.

Patch-Sim: Patch-Sim is a similarity-based PCC tool, which utilizes the tests generated by automated test generation tools (Randoop in the original study [70] and both Randoop and EvoSuite in Wang et al.’s study [61]) as the test inputs. It assumes that tests with similar executions are likely to have similar results. Therefore, given that a correct patch may behave similarly on the passing tests and differently on the failing ones compared with the buggy program, Patch-Sim could assess patch correctness. Note that in the original study, Xiong et al. used Randoop to generate test suites [70]. Besides, according to Wang et al.’s study [61], the tests generated by Randoop perform better than those by EvoSuite. We, thus, choose Randoop as the test generation tool in this study. To apply PatchSim on PraPR_{1.2}, we generate 6,570 test cases for related projects in total by ourselves. Combining with the test suites generated by the existing study [61], we obtain 10,404 test cases for Patch-Sim.
al. [74] which aims to learn patch correctness via feature engineering. Lin et al. [33] proposed Cache with similar ideas but their tools are currently not executable due to broken scripts and incomplete artifacts.

**Embedding:** Code Embedding is the technique that can transfer source code into distributed representations as fixed-length vectors. Consequently, various software engineering tasks can take the advantages of such distributed representations by utilizing supervised machine learning algorithms. Tian et al. initially proposed utilizing code embedding techniques to identify correct patches among plausible ones [59]. There are also other techniques utilizing embedding to classify patches [11, 12], but they mostly follow similar ideas and cannot outperform Tian et al. ’s work [59]. Therefore, we utilize the best-performing model in Tian et al. ’s study, denoted as BERT-LR (i.e., the pre-trained models of BERT [15] with logistic regression), to investigate the performance of embedding-based technique. We follow the same setting as adopted in the existing study by utilizing the dataset used in [59] for training and our collected benchmark for testing. Note that we filter the overlapped patches for the training process.

**ODS:** ODS (Overfitting Detection System) extracts a large number of static code features and leverages ensemble learning based on decision trees to predict patch correctness [74]. For a given patch, ODS extracts 202 code features, including 150 code description features representing the characteristics of the patch’s ingredients and its context at different granularity, 26 repair pattern features encoded with human knowledge on repair strategies and 26 contextual syntactic features that encode the scope and similarity information in the source code. To evaluate ODS in our dataset, we follow the experimental setup in the original paper [74]. We first use the code analyzer provided by ODS to extract features of patches in our dataset and then reuse the model trained by the authors for prediction.

### 4.3 Metrics

Following the recent works on patch correctness checking [59, 61, 70, 73], we evaluate PCC techniques against following metrics:

- **TP:** # of truly overfitting patches identified as overfitting.
- **TN:** # of truly correct patches identified as correct.
- **FP:** # of truly correct patches identified as overfitting.
- **FN:** # of truly overfitting patches identified as correct.
- **Precision:** \( = \frac{TP}{TP + FP} \). Denoted as Precision.
- **Recall:** \( = \frac{TP}{TP + FN} \). Denoted as Recall.
- **Recall of Correct:** \( = \frac{TN}{TN + FP} \). Denoted as Recall_c.
- **PR-AUC:** Area Under Precision-Recall Curve [13].
- **AVR:** the Average Ranking of correct patches for bugs which have both overfitting and correct patches.
- **Correct patch ratio:** \( = \frac{TN}{TN + FN} \). Denoted as CPR.
- **Accuracy:** \( = \frac{(TP + TN)}{(TP + FP + TN + FN)} \). Denoted as ACC.

Studies suggest that PCC techniques should not mistakenly exclude correct patches [61, 70, 74], i.e., the FP should be as low as possible. However, when the dataset is imbalanced (i.e., mostly overfitting patches), Precision can be biased and overrated since FP is often much smaller than TP, thus posing significant effects to the final results. Therefore, following the previous work on imbalanced datasets [70], we further include the metric Recall_c (the ratio of correctly identified correct patches) for complementary fair comparisons. In fact, Recall_c is crucial since rejecting a correct patch may cause a bug to be unfixed. Besides, We also utilize the PR-AUC [13] metric to eliminate the effects caused by imbalanced datasets [55].

Note that PR-AUC is not applicable for rule-based techniques including Opad, Patch-Sim, Anti-patterns and ODS since the results are discrete (either 0 or 1). For the static techniques (S3, CapGen, ssFix and code naturalness) based on feature score ranking, we leverage AVR to show their average ranking of correct patches, i.e., the smaller ranking is better. For example, given AVR \( a(b) \), \( a \) is the average ranking of the first correct patch in each bug while \( b \) is the average number of patches in each bug. In summary, an effective PCC technique should achieve high Precision, Recall, Recall_c, and PR-AUC, while small AVR. For ODS evaluation we reuse their metrics with two additional ones CPR and ACC.

### 4.4 Threats to Validity

The threats to internal validity mainly lie in the implementation of studied PCC techniques. To reduce such threats, we reuse existing implementations whenever possible, and have carefully reviewed all our code and script. In addition, to mitigate the bias in the patch correctness labeling, we involve multiple participants, follow widely-used criteria summarized from existing work [37], and have released all our patches for public review. To mitigate the potential bias of randomness in sampling, we repeat the sampling process for ten times and use the average results. The threats to external validity are mainly concerned with the generalizability of our findings. We mitigate the threats from single APR tool by further merging our datasets with previous datasets. The threats to construct validity mainly lie in the metrics used in our study. To mitigate the threats, we have included all popular metrics for PCC, including Precision, Recall, Recall_c, AVR, PR-AUC, CPR and ACC.

### 5 RESULT ANALYSIS

#### 5.1 RQ1: Static Techniques

##### 5.1.1 Static Code features.

For studying all the static code features, we follow the same experimental setting as the recent study [61]. The detailed experimental results for the three studied techniques are shown in Table 3. From the table, we can observe that compared with original scores on Wang et al.1,2, the Recall scores all increase while the Recall_c scores all substantially decrease on the PraPR_d and PraPR_d datasets. One direct reason is that the two new datasets are extremely imbalanced (with mostly overfitting patches). Meanwhile, the extremely low Recall_c scores (i.e., only 5%-20% of correct patches are identified as correct) still demonstrate that such techniques can hardly be useful in practice. The PR-AUC scores of all features for all datasets are below or around 50%. Taking Balance_d_d dataset for example, two of the features get a PR-AUC score slightly over 50% (the proportion of positive sample and the expected PR-AUC for random classification model) and one of them gets less than 50%, indicating that these features are hardly useful to identify overfitting patches. The experimental results on the more balanced Merge_d and Balance_d datasets
further confirm this observation. For example, on the balanced dataset Balance220, the Recall remains similar to the prior study on Wang212 [61] (i.e., ~45%), while the Recall substantially drops over 30 percentage points for all three static techniques (e.g., 78.7-46.5% for SSFix). AVR is a better metric for simulating actual efforts of selecting one correct patch from plausible patches. From the tables, we can observe that for Wang212 dataset, developers would examine 2.1–2.8 patches on average (depending on the chosen technique) until the first correct patch is found. However, when PraPR dataset is considered or merged with Wang212, the AVRs rise to 5.5–8.2. In other words, when larger patch space is included, effectiveness of all similarity-based static tools significantly degrades and the cost to identify correct patch for each bug significantly increases.

Table 3: Performance of static features on different datasets

| Dataset | TP | FP | TN | FN | Precision | Recall | PR-AUC | AVR |
|---------|----|----|----|----|-----------|--------|--------|-----|
| S3      |     |    |    |    |           |        |        |     |
| Wang212 | 512 | 137| 111| 137| 79.1%     | 79.1%  | 44.8%  | 45.3%|
| PraPR20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| PraPR20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| Merge20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| Balance20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| SSFix   |     |    |    |    |           |        |        |     |
| Wang212 | 515 | 139| 109| 139| 78.7%     | 78.7%  | 44.0%  | 43.8%|
| PraPR20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| PraPR20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| Merge20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| Balance20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| CapGen  |     |    |    |    |           |        |        |     |
| Wang212 | 510 | 144| 144| 144| 78.8%     | 78.8%  | 41.2%  | 41.9%|
| PraPR20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| PraPR20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| Merge20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |
| Balance20 | 1222| 42 | 72 | 66 | 96.7%     | 96.7%  | 10.5%  | 4.1% |

Table 4: Average scores based on static features

| Patches          | SSfix | S3 | CapGen |
|------------------|-------|----|--------|
| PraPR20_correct  | 1.52  | 10.60 | 0.37 |
| PraPR20_overfitting | 1.56 | 15.73 | 0.41 |
| Wang212_correct  | 1.49  | 11.71 | 0.44 |
| Wang212_overfitting | 1.35 | 26.30 | 0.25 |
| Developer        | 1.16  | 40.64 | 0.26 |

Listing 1: A PraPR patch replacing condition with false

Finding 1: The widely-accepted assumption made by existing similarity-based static techniques that correct patches in general share higher similarities with the buggy code is no longer valid on our new dataset with exhaustive patch generation.

Through further investigation, we observe that one major issue of similarity-based static techniques is that they can produce diverse similarity scores for semantic-equivalent patches. For instance, Listing 1 shows a simple example patch. This patch simply replaces the conditional expression with false, which is semantically equivalent to removing the whole if block (including its large body). The scores generated by the three static techniques are totally different, as shown in Table 5. The left column displays the scores of the original patch, i.e., replace the conditional expression with false, while the right column denotes the scores of the simplified patch, i.e., simply removing the whole if block. Among the eight static features, the similarity scores of SSFix features of original patch are significantly higher compared with the simplified patch (i.e., the patch with the whole if block removed) 140.99% and 84.49% respectively, while the edit distance scores for S3 of the original patch have much lower scores than the simplified patch. Therefore, the widely-accepted assumption can be assumed to be a promising result.
For CapGen features, the scores are also completely different from each other. These cases are frequently observed in our dataset, i.e., a patch can be simplified to its equivalent patch. There are 54 mutating condition to false patches among the 1.311 PraPR\textsubscript{A2} patches, in addition to other patches with similar patterns. Such results provide stronger evidence that merely considering the syntactic similarity is inadequate for PCC while more advanced semantics-based techniques should be proposed.

**Finding 2:** All three studied static techniques can compute totally different scores for semantically equivalent patches, indicating that future static PCC techniques should incorporate more advanced semantics analysis.

### Table 6: Performance of Anti-patterns

| Datasets | TP | FP | FN | PN | Precision | Recall | Recallc |
|----------|----|----|----|----|-----------|--------|---------|
| Wang\textsubscript{A1.2} | 219 | 37 | 211 | 435 | 85.55% | 31.49% | 85.68% |
| PraPR\textsubscript{A1.2} | 174 | 10 | 37 | 1,090 | 94.57% | 13.77% | 78.72% |
| PraPR\textsubscript{A2.0} | 363 | 28 | 55 | 1,544 | 92.80% | 18.95% | 66.27% |
| Merge\textsubscript{A2.0} | 339 | 56 | 215 | 1,930 | 90.89% | 22.46% | 79.34% |
| Balance\textsubscript{A2.0} | 41 | 56 | 215 | 230 | 42.27% | 15.13% | 79.34% |

### 5.1.2 Anti-patterns

Table 6 presents the results of Anti-patterns [58] on our four new datasets, along with its original results in previous work [61] (i.e., Wang\textsubscript{A1.2}). From the table, we can observe that Anti-patterns performs significantly worse on PraPR patches by misidentifying a larger proportion of correct patches as overfitting and omitting more overfitting patches. Compared to the previous dataset, Anti-patterns exhibits both lower Recallc and Recall on PraPR\textsubscript{A1.2}. Although Precision has a suspected improvement, the actual cause is the heavily imbalanced datasets, i.e., the overwhelming number of overfitting patches over the correct ones. Actually, the results on the balanced dataset Balance\textsubscript{A2.0} further confirm the worse performance.

We further looked into the root cause of Anti-patterns’ poor performance on our datasets with two misclassified examples of Anti-patterns. Note that these two patches are generated exclusively by PraPR and have not been included in any previous dataset. Listing 2 is an FP example where Anti-patterns mistakenly identifies a correct patch generated by PraPR as overfitting. In particular, the correct patch shown in Listing 2 falls in the anti-pattern A2 which disallows any removal of control statements such as if-statement. In fact, in order to enable general and efficient searching attempts in the patch space, fixing operations leveraged in existing APR (especially the basic fixing operations in PraPR) are often much less sophisticated than manual fixes, which may even appear “simple” and “crude” sometimes. Therefore, Anti-patterns, which distinguishes overfitting and correct patches by the pattern of manual patches, inherently becomes less effective in identifying the developer-unlike but correct patches (which are largely missed by prior APR tools but captured by PraPR).

### Finding 2: All three studied static techniques can compute totally different scores for semantically equivalent patches, indicating that future static PCC techniques should incorporate more advanced semantics analysis.

### Listing 2: A correct patch misclassified by Anti-patterns

```c
if (childType.className()) {...
    } else {
    ...
    } else if (...)
```

### Finding 3: Anti-patterns performs much worse on our datasets due to its limited capability of identifying (i) overfitting patches with subtle modifications and (ii) correct patches with developer-unlike modifications. Such problem is largely amplified on our datasets due to PraPR’s exhaustive patch generation.

### Table 7: Performance of code naturalness

| AVR | sP | p | cogen | Sum Entropy | Mean Entropy |
|-----|----|---|-------|-------------|-------------|
| Wang\textsubscript{A1.2} | 2.72(9.1) | 2.89(10.5) | 2.14(9.1) | 1.99(9.1) | 1.99(9.1) |
| PraPR\textsubscript{A1.2} | 6.77(10.69) | 8.15(10.69) | 7.38(10.69) | 4.73(10.69) | 4.65(10.69) |
| PraPR\textsubscript{A2.0} | 8.26(9.64) | 7.15(9.64) | 6.62(9.64) | 4.69(9.64) | 4.67(9.64) |
| Merge\textsubscript{A2.0} | 6.12(12.87) | 7.01(12.87) | 5.49(12.87) | 3.60(12.87) | 3.47(12.87) |

### 5.1.3 Naturalness-based techniques

Table 7 shows that in all datasets, both Sum Entropy and Mean Entropy can outperform all three static techniques. Specifically, in terms of AVRs, the sum/mean entropy outperforms the best-performed static technique by 11.21%/11.21% (on Wang\textsubscript{A1.2}), 30.13%/31.31% (on PraPR\textsubscript{A1.2}), 34.82%/25.40% (on PraPR\textsubscript{A2.0}), and 44.08%/36.79% (on Merge\textsubscript{A2.0}). Interestingly, our results also confirm prior work [65] that Sum Entropy performs slightly better than Mean Entropy for PCC. The reason is that Sum Entropy calculates the entire sequence entropy for code naturalness computation, and considers both code naturalness and length information. To conclude, naturalness-based techniques show greater potential in PCC and this is the first, to our best knowledge, evaluation of naturalness-based techniques on PCC datasets. We appeal to the community that more attention should be drawn into this direction.

**Finding 4:** naturalness-based techniques can substantially outperform all static code features in patch ranking, and Sum Entropy performs slightly better than Mean Entropy.

### 5.2 RQ2: Learning-based Techniques

#### 5.2.1 Embedding technique

Table 8 presents the results of the embedding-based technique [59] on our datasets, i.e., PraPR\textsubscript{A1.2} and PraPR\textsubscript{A2.0}. Since there is a large overlap between the training set of the embedding technique and Merge\textsubscript{A2.0}/Balance\textsubscript{A2.0}, i.e., patches collected by Liu et al. [37], we do not consider Merge\textsubscript{A2.0}
or Balance2.0 in this RQ. For comparison, we also present the results of the embedding-based technique in its original paper (i.e., Tian01.2).

To evaluate the performance more precisely, we pay more attention to PR-AUC since the applicable datasets are heavily imbalanced. Obviously, the embedding-based technique tends to perform badly on PraPR patches with PR-AUC to be less than 20% on all our new datasets. Furthermore, with the datasets to be gradually more balanced through PraPR01.2, PraPR2.0 and PraPR2.0-ε1.2 (i.e., exclusive bugs in PraPR2.0), the performance of Precision, Recall and Recallc keeps dropping while the PR-AUC increases, which indicates the worse performance is more credible. The results further confirms that the embedding-based technique suffers from the dataset overfitting issue: it performs worse on the patches of subjects that are different from training set. Thus, we encourage future learning-based PCC work to consider across-dataset evaluation.

We then look into the potential reasons for such a decrement. The intuition of embedding-based PCC techniques is that the cosine similarity between the embedding vectors of correct patches and buggy code should be larger than the cosine similarity between the embedding vectors of overfitting patches and buggy code. Tian et al. [59] show that correct/overfitting patches in their dataset perfectly follow such an assumption. However, this assumption no longer holds on the additional patches generated by PraPR. Figure 1 presents the distribution of the cosine similarity between the embedding vectors of correct/overfitting patches and buggy code on the original dataset in the embedding work [59] (i.e., Tian01.2) and our datasets built on Defects4J V1.2 and additional bugs in Defects4J V2.0 (i.e., PraPR01.2 and PraPR2.0-ε1.2). From the figure, we observe that different from the prior work [59], correct patches and overfitting patches share very close distributions of similarity scores on our datasets, which explains why the embedding-based technique exhibits worse performance on PraPR patches. Furthermore, correct patches even tend to have lower median similarity than overfitting patches on PraPR2.0-ε1.2, demonstrating that the assumption made by the embedding-based work no longer holds on our new dataset.

### Finding 5: The assumption that the cosine similarity between the embeddings of correct patches and buggy code should be larger than that between the embeddings of overfitting patches and buggy code no longer holds. Also, the embedding-based technique tends to suffer from the dataset overfitting issue.

| Dataset       | TP  | FP  | TN  | FN  | Precision | Recall | Recallc | PR-AUC |
|---------------|-----|-----|-----|-----|-----------|--------|---------|--------|
| Tian01.2      | 85  | 14  | 16  | 24  | 85.86%    | 77.98% | 53.33%  | N/A    |
| PraPR01.2     | 822 | 15  | 27  | 227 | 98.21%    | 78.36% | 64.29%  | 11.86% |
| PraPR2.0      | 1,220 | 23 | 51  | 443 | 97.52%    | 72.81% | 59.21%  | 15.51% |
| PraPR2.0-ε1.2 | 298 | 8   | 26  | 206 | 96.14%    | 65.89% | 52.94%  | 19.69% |

### Table 8: Performance of the embedding-based technique

Finding 5: The assumption that the cosine similarity between the embeddings of correct patches and buggy code should be larger than that between the embeddings of overfitting patches and buggy code no longer holds. Also, the embedding-based technique tends to suffer from the dataset overfitting issue.

### 5.2.2 ODS. Table 9 shows the result of ODS in our dataset. For Wang01.2, we directly reuse the results from [74]. It turns out that the performance on PraPR01.2 and PraPR2.0 significantly drop for Recallc. CPR and ACC, e.g., CPR drops from ~84% to ~6%, which means a lot of overfitting patches ‘escape’ ODS and get misidentified as correct. Similarly, the Recallc drops because more correct patches are classified as overfitting. More interestingly, we evaluate ODS on developer patches and found that the FP rate (ratio of developer patches identified as overfitting) is 43.52% and 41.56% on developer patches v1.2 and v2.0. To figure out the potential reason that the performance drops, we carefully check the feature definitions in ODS and understand why PraPR patches can escape ODS. ODS defines 202 features in total (in three groups). According to the feature analysis of ODS [74], the order of importance of three groups of features is: Contextual Syntactic Features (150) > Code Description Features (26) > Repair Pattern Features (26). Note that ODS features are specific characteristics of programs, and therefore, only patches with the same variable methods. Thus, we can observe that developer patches are generally more complex than those in the training set. On the other hand, the features in ODS are specific to the training set and might not be effective for new datasets.

| Feature Group | No. of Features | CPR |
|---------------|----------------|-----|
| Contextual Syntactic | 150 | 88.91 |
| Code Description | 26 | 80.06 |
| Repair Pattern | 26 | 79.19 |

### Table 9: Performance of ODS

Finding 6: Features of ODS are too weak to identify overfitting patches and miss a lot of overfitting patches in our dataset. Plus, the high FP rate on developer patches is unbearable for PCC.

### 5.3 RQ3: Dynamic Techniques

#### 5.3.1 Opad. Table 10 and Table 11 respectively show the results for E-Opad (Opad with EvoSuite) and R-Opad (Opad with Randoop). We can observe that except for R-Opad on Balance2.0, Opad achieves...
a precision over 95% on all the datasets, while the achieved recall ranges from 11.71% to 18.83%. The high precision with low recall

Table 10: Performance of E-Opad on different datasets

| E-Opad | TP | FP | TN | FN | Precision | Recall |
|--------|----|----|----|----|-----------|--------|
| WarPR1.2 | 92 | 9 | 248 | 52 | 100.00% | 14.07% |
| PrPR1.2 | 148 | 47 | 1,116 | 100.00% | 11.71% | 100.00% |
| PrPR2.0 | 207 | 2 | 1,638 | 99.26% | 14.02% | 97.59% |
| MergePR2.0 | 344 | 269 | 2,145 | 99.62% | 13.82% | 99.26% |
| Balance2.0 | 45 | 269 | 223 | 96.00% | 17.71% | 99.26% |

Table 11: Performance of R-Opad on different datasets

| R-Opad | TP | FP | TN | FN | Precision | Recall |
|--------|----|----|----|----|-----------|--------|
| R-Opad1.2 | 67 | 0 | 248 | 547 | 100.00% | 10.24% |
| R-Opad2.0 | 12 | 12 | 35 | 1,026 | 95.20% | 18.83% |
| R-Opad2.0 | 146 | 58 | 1,559 | 95.84% | 18.16% |
| MergePR2.0 | 408 | 256 | 2,081 | 96.45% | 16.39% |
| Balance2.0 | 44 | 256 | 227 | 74.58% | 16.24% |

The performance of Opad overall tends to remain similar on our new datasets, demonstrating the robustness of such dynamic techniques. Meanwhile, consistent with the findings in prior work [61], Opad achieves a rather low recall on our datasets, indicating that a substantial ratio of overfitting patches are not detected. This is still a critical issue in PCC since manually filtering out overfitting patches can be extremely costly for developers [70].

Finding 7: The performance of Opad overall tends to remain similar on our new datasets, demonstrating the robustness of such dynamic techniques. Meanwhile, consistent with the findings in prior work, it achieves a rather low recall for incorrect patches, which compromises its practical usefulness.

Though the precision achieved by Opad is consistent with previous studies [61, 73]. Besides, the Recall also significantly outperforms other techniques with respect to precision. Such results indicate that Opad can identify most of the correct patches and rarely classifies correct patches as overfitting ones. In other words, the performance does not degrade much in our new datasets, and Opad is more stable than static techniques. This falls into our intuition since such dynamic tools based on test generation concern more towards code semantics instead of syntactic elements. Meanwhile, similar to the findings from prior work [61], PrPR achieves a rather low recall on our datasets, demonstrating the robustness of such techniques.

Listing 5: A correct patch misclassified by Opad

```java
public static int findFormat(Object serializers, Property handledType());
```

Finding 8: The effectiveness of Opad might decay and it might mistakenly identify correct patches as incorrect when the semantic changes of the patches break certain conditions.

Since developer patches might even incur larger semantic changes, we are curious to see whether such similar cases could also happen on developer patches. Unfortunately, there are no existing studies investigating Opad on developer patches. Fortunately, this is an important study since APR techniques can potentially fix more and more bugs and produce more and more developer patches in the near future. As shown in Table 12, E-Opad and R-Opad achieve Recall of 86.06% and 79.02%, respectively, which is significantly lower than that on the other datasets except for PrPR1.2. Specifically, Opad produces more false-positives and achieves lower Recall compared with previous study [61, 73].

Finding 9: Opad performs worse on developer patches and should be applied with caution on (future) automated APR tools that can generate more complicated patches.

Listing 6: A developer patch misclassified by Opad

```
if (property == null) {
    return this;
}
```

Finding 8: The effectiveness of Opad might decay and it might mistakenly identify correct patches as incorrect when the semantic changes of the patches break certain conditions.

Since developer patches might even incur larger semantic changes, we are curious to see whether such similar cases could also happen on developer patches. Unfortunately, there are no existing studies investigating Opad on developer patches. Fortunately, this is an important study since APR techniques can potentially fix more and more bugs and produce more and more developer patches in the near future. As shown in Table 12, E-Opad and R-Opad achieve Recall of 86.06% and 79.02%, respectively, which is significantly lower than that on the other datasets except for PrPR1.2. Specifically, Opad produces more false-positives and achieves lower Recall compared with previous study [61, 73].

Finding 9: Opad performs worse on developer patches and should be applied with caution on (future) automated APR tools that can generate more complicated patches.

Table 12: Opad/Patch-Sim on developer patches

| E-Opad | TP | FP | TN | FN | Recall |
|--------|----|----|----|----|--------|
| WarPR1.2 | 0 | 111 | 685 | 0 | 86.06% |
| PrPR1.2 | 0 | 167 | 629 | 0 | 79.02% |

Further manually checked some false positive cases in developer patches, and Listing 6 shows an example. Specifically, the developer patch moved a statement into an else block with a condition length >= 9. The failing test expects an IllegalArgumentException triggered by invalid parameters of formatBigIntegerBinary. When stepping into the method in Listing 6, the original statement throwing that exception was not executed in the patched program. Therefore, the program stepped over that statement and when executing the next statement (line 7), the parameter offset is -1, triggering an ArrayIndexOutOfBoundsException. Similar to the previous example, Opad is not aware of such semantic changes and misclassified that patch as overfitting. Actually, Opad could produce more false-positives when the patches get more complicated, i.e., different code structure or logic. This can be an important finding for the community since eventually APR techniques will be conquering more and more complicated bugs, with more and more complicated patches.

Finding 9: Opad performs worse on developer patches, and should be applied with caution on (future) automated APR tools that can generate more complicated patches.
5.3.2 Patch-Sim. Table 13 presents the performance of Patch-Sim on previous datasets (i.e., Xionga1.2 from its original paper [70] and Wannga1.2 from the previous study [61]) and our dataset.

Table 13: Performance of Patch-Sim on different datasets

| Datasets     | TP  | FP  | TN  | FN  | Precision | Recall | Recallc |
|--------------|-----|-----|-----|-----|-----------|--------|---------|
| Xionga1.2    | 62  | 0   | 29  | 48  | 100.00%   | 56.36% | 100.00% |
| Wannga1.2    | 249 | 51  | 186 | 392 | 62.85%    | 38.85% | 78.48%  |
| PrAPR1.2     | 210 | 3   | 7   | 283 | 95.39%    | 94.66% | 70.66%  |

Listing 7: A correct patch classified by Patch-Sim

Note that due to an implementation issue [70], Patch-Sim does not support Defects4j V2.0 and subjects Closure and Mockito from Defects4j V1.2. Hence, following prior work [70], we only consider the patches supported by Patch-Sim from Defects4j V1.2. Overall, 493 overfitting and 10 correct patches in PrAPR1.2 are used. As shown in the table, Patch-Sim performs much worse on our dataset compared with initial Xionga1.2 results. For example, Patch-Sim achieves 100% Precision and Recallc on its original dataset, whereas on PrAPR patches, both metrics are degraded. Such a finding is similar to results in previous work [61]. Listing 7 presents a sample FP that Patch-Sim mis-identifies the correct patch as overfitting. Patch-Sim is designed based on the mild consequence assumption that a passing test should behave similarly on the correct patch and the buggy code. Therefore, the patch substantially changing the behavior of passing tests would be regarded as overfitting by Patch-Sim. In this example, the correct patch modifies the condition, leading to significant control-flow changes: on the buggy code, some passing tests can enter the if block and then throw the exception, whereas on the patch code these tests skip the if block and exception statement. Therefore, Patch-Sim considers the behaviors of these passing tests changed substantially, and further regards the patch as overfitting. However, correct patches are also likely to introduce large behavioral changes whereas overfitting patches can also induce tiny impact on passing tests.

Finding 10: The assumption that passing tests should behave similarly on correct patch code and buggy code can be easily broken when a larger patch space is considered, especially for the correct patches with non-trivial modifications on control flow.

To further investigate the performance of Patch-Sim, Table 12 presents the performance of Patch-Sim on 175 developer patches from Defects4j V1.2 where Patch-Sim can be successfully applied. Interestingly, Patch-Sim performs even worse on developer patches with a Recallc of only 62.86%, i.e., mis-classifying 37.14% correct patches as incorrect. After manual inspection, we find that developer patches often involve sophisticated modifications and thus passing tests behave very differently from buggy code. For example, Listing 8 presents a developer patch which mis-identified as overfitting by Patch-Sim. The patch modifies multiple lines, and several if-else statements. Thus, it significantly affects the paths of some passing tests. In fact, it is prevalent that developer patches contain such sophisticated changes, and thus the assumption made by Patch-Sim can be easily violated on developer patches. Such results show that future PCC work should exercise complex patches since advanced APR tools can generate more complex patches in the near future.

Listing 8: A developer patch misclassified by Patch-Sim

Finding 11: Patch-Sim misclassifies 30% correct patches as incorrect on PrAPR1.2 and tends perform even worse with 36.14% misclassification rate on complicated developer patches. Our results also suggest that future dynamic PCC techniques should consider more complex patches for evaluation.

6 CONCLUSION

We have constructed a comprehensive PCC dataset for revisiting state-of-the-art PCC techniques. Our study of PCC techniques on the new dataset has revealed various surprising findings and practical guidelines for future PCC work, e.g., various assumptions from prior studies [5, 27, 61, 64, 68] no longer hold in our new dataset and semantics analysis should be included to determine patch correctness.

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