Causal Distance-Metric-Based Assistance for Debugging After Compiler Fuzzing

Josie Holmes
School of Informatics, Computing & Cyber Systems
Northern Arizona University
Email: josie.holmes@nau.edu

Alex Groce
School of Informatics, Computing & Cyber Systems
Northern Arizona University
Email: agroce@gmail.com

Abstract—Measuring the distance between two program executions is a fundamental problem in dynamic analysis of software, and useful in many test generation and debugging algorithms. This paper proposes a metric for measuring distance between executions, and specializes it to an important application: determining similarity of failing test cases for the purpose of automated fault identification and localization in debugging based on automatically generated compiler tests. The metric is based on a causal concept of distance where executions are similar to the degree that changes in the program itself, introduced by mutation, cause similar changes in the correctness of the executions. Specifically, if two failing test cases (for the original compiler) become successful due to the same mutant, they are more likely to be due to the same fault. We evaluate our metric using more than 50 faults and 2,800 test cases for two widely-used real-world compilers, and demonstrate improvements over state-of-the-art methods for fault identification and localization.

I. INTRODUCTION

The proposed techniques in this paper are motivated in large part by progress in automated testing. Recent advances, some implemented in powerful tools [77], [68], [30], [32], [41], [42], have made it easier to find subtle faults in modern optimizing compilers. Unfortunately, the blessing of many failing test cases can become a curse for a developer. Simply discovering which test cases in a large set of failures represent distinct faults (or already known faults) is a very difficult problem [14], [19]. For simple crashes and assertion violations, bucketing failures is sometimes easy. When a failure is detected by differential testing [52], where two compilers or optimization levels disagree, the problem is so hard that it may turn potential users away from automated test generation [14]. Even once a fault is identified, fixing the problem can be very difficult. Due to complexity of intermediate representation, interaction of optimizations, subtle language semantics, and other issues, debugging compiler faults may often be harder than debugging faults in other kinds of software (this is probably not limited to compilers; debugging systems software in general is hard [53]). This paper proposes the use of mutants [3] to assist debugging based on large sets of automatically generated test cases. The underlying idea is that if two failures can be repaired in the same way, even if that repair only avoids the failure and does not truly fix it, they are likely due to the same fault. Repairs can also provide information for understanding and debugging discovered faults.

A. Fuzzer Taming and Debugging Assistance

Automated test generators tend to produce many failures, which correspond to far fewer faults. The distribution of faults in a set of failures tends to follow a strong power-law curve, where a few faults account for most failures, and many or even most faults have only 1 or 2 associated failures (even in a set of 1,000 or more test cases) [14], [28]. Looking at all the failures to find distinct faults is impractical. It is also impractical to apply the iterative approach: first, fix one test's fault; then re-run all tests, until all tests pass. There may be some (possibly known) faults that are very difficult to fix. As of 5/8/2017, clang had 107 unresolved, assigned bugs, some dating to 2008 [1]. Knowing if a randomly generated test case is an instance of a known failure can be difficult, and fixing long-standing problems is unlikely. Applying a novel fuzzing technique [77], [30], [41] may dump dozens of new faults on a project at once, and high priority faults may be hidden by stubborn low-priority faults.

Chen et al. defined the *fuzzer taming problem*: "Given a potentially large collection of test cases, each of which triggers a bug, rank them in such a way that test cases triggering distinct bugs are early in the list." [14]. Their proposed solution was to use the Furthest-Point-First [21] (FPF) algorithm. An FPF ranking of test cases requires a distance metric d on test cases, and ranks test cases so that dissimilar tests appear earlier. The hypothesis of Chen et al. was that dissimilar tests, by a well-chosen metric, will also fail due to different faults. FPF is a greedy algorithm that proceeds by repeatedly adding the item with the maximum minimum distance to all previously ranked items. Given an initial seed item r_0, a set S of items to rank, and a distance metric d, FPF computes r_i as $s \in S : \forall s' \in S : min_{j<i}(d(s, r_j)) \geq min_{j<i}(d(s', r_j))$.

Fuzzer taming results using FPF were promising, but limited. Rather than proposing a universal metric for use in fuzzer taming, Chen et al. offered many possible metrics (based on ideas in the literature, such as edit distances [44] or coverage spectra [67] methods), none of which performed notably well across all three of their benchmark data sets, and some of which were not even applicable to all data sets. For the two most difficult benchmarks, none of the metrics performed extremely well. FPF-based approaches remain in need of a universal but effective metric, particularly for the critical first

50 test cases examined [14].

One reason fuzzer taming is needed is that debugging compiler faults is, as noted, difficult. If developers were able to fix faults quickly once they were identified, it would not be as important to have very good fault identification. Many duplicate failures could be removed by fixing the underlying faults. Ideally, a technique able to provide effective fuzzer taming should be able to translate its underlying rationale into some form of automated debugging assistance. A variety of automated debugging methods [66], [25] have been based on distance, so there is reason to expect that a high-quality metric might also contribute to fault localization.

B. Comparison by Response to Mutation

The methods we propose are based on an idea with numerous possible applications: *if two test cases that fail can be made to succeed by the same small modification to the original program P, this provides evidence that they fail due to the same fault*. The idea is hardly controversial: most developers determine if they have fixed all faults in a set of failing test cases by re-running the tests after they fix at least one fault. As a method for fuzzer taming or debugging assistance, however, this is useless. Fortunately, developer-provided patches are not the only source of changes to a program. Mutation analysis [17], [8] applies large numbers of small syntactic changes to a program, usually to evaluate a test suite [3], [39]. Some recent work has also considered these changes as a source of fixes or localizations for program faults [64], [33], [54], [16]. Published empirical data [23], as well as our data, suggests that very few compiler faults are due to the kind of simple syntactic changes found in mutants. However, what should matter is that failures, which correspond to faults, can be made to succeed. When a test case t fails for program P but succeeds for mutant m of P we say that m *repairs* t (in practice, we will usually restrict this to unkilled mutants m). In most cases this does not mean that m is the inverse of a fault, but that m somehow avoids the consequence of a fault. It is not important that repairs actually fix anything, if all we seek is a way to decide whether failures are due to the same fault.

This idea is in fact a consequence of a more general (and difficult to investigate) hypothesis. What is interesting about a program execution? For many purposes, the only interesting properties of an execution are those that in some way relate to the correctness properties of the program, assuming we have a good set of correctness properties. These may include assertions, differential testing [52], [29], timing constraints, temporal logic formulas, or any other checkable properties. What, then, makes two executions similar? Our proposal is that *two executions of P are similar to the degree that changes to P produce the same changes in correctness of the executions.*

This leads to the general idea of comparing any two executions by their response to mutants, since a mutant can cause a passing or failing test to violate more/different properties. However, for practical purposes the most interesting implication is that if two failing test cases can be made to differ
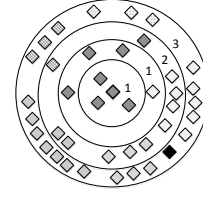

Fig. 1: Onion-ring shows all repairs for the center-most test case, and all other test cases repaired by same mutants. Rings represent repairing mutants, and diamonds test cases. Diamond pattern symbolizes fault. For innermost ring, all failures are due to the same fault. As rings expand, the set of faults grows (as does distance).

as to whether they fail by a mutant, this provides evidence that they fail due to different faults.

This not a claim that being repaired by the same mutant perfectly predicts fault equivalence. To understand why that seems highly unlikely, consider why a mutant of a compiler might cause a test case to stop failing. Many, likely most, compiler faults are due to incorrect optimizations. Turning an optimization off can cause a program to compile correctly. Optimizations are generally guarded by conditional switches — in some cases, so that the optimization can be turned off by a user, in other cases when a property of the code in question prevents the optimization. If two failures become successes due to turning off an optimization, it is presumably *some* level of evidence they are the same fault, compared to the baseline probability. Intuitively, the evidence provided seems much stronger than the fact that, e.g., the two test cases execute a shared line of code [67]. However, optimization guards range from guarding very specific fragments of code to guarding entire modules with many functions and related sub-optimizations. If a mutant prevents a very specific optimization (consisting of a few lines of code) from executing, and repairs two tests, that is powerful evidence they are related. If a mutant turns off a large number of optimizations, it is weak evidence. Must we then characterize mutant granularity? No.

Consider the structure of mutants repairing a failure due to some optimization. If many mutants repair this failure, by turning off the optimization or a whole family of optimizations, some will likely repair failures due to different faults. Repairs naturally produce an *onion-ring* structure (Figure 1), where a failure is repaired by (1) some mutants that are very specific to its fault (the inner rings) and repair few failures, as well as other mutants (2) that repair more and more faults, up to a hypothetical mutant (3) that turns off all optimization in the compiler (e.g., by effectively adding -O0), the outer ring of the onion. This does not introduce confusion, because the more repairs two tests have in common, the more likely they are to be due to the same fault, and the more they disagree on, the less likely they are to be due to the same fault. While especially intuitive as a consequence of disabling compiler optimizations, it seems likely this structure appears for repairs in many programs. Disabling an optimization is a code change that (usually) does not cause any test case that only concerns semantics, not speed, to fail, but can avoid some failures. For some oracles, many changes may fall into this classification: for example, if the only oracle is to detect crashes, a fault can

be avoided by aborting the program early. Turning off a data-structure invariant checker is another possible false repair that might cover many different faults. In all these cases, assuming more interesting and specific ways to repair faults exist, a distance metric can still work.

The idea of measuring the distance between failures (or other test cases) by their response to mutants is appealing. The most important aspect of most applications of distances between executions is some aspect of causality [45], [79]. Since at least the work of Hume [34], causality has been understood as answering some question of the form "what would have made a difference in this effect?" A program mutant is a very direct way to answer this question. The problem with determining causes of events in computer programs is that, as in most problems of causality, too many things can make a difference, most of which are not interesting. Program mutants are much more likely to be interesting causes for events in a program than changes to test cases, at least for fault identification and debugging purposes, because the practical use of knowing a cause is to change the cause and avoid or produce an effect. Debugging by changing test cases is not useful; debugging by altering the program is precisely how we seek to use the causes we discover. Using mutants as causes in order to measure similarity "inverts" the ideas of Lewis [45], [46], where similarity determines causality. One pleasant side-effect is that execution representation [66], [67] is not a difficulty for this kind of metric, since an execution is always represented as a set of correctness evaluations.

This paper investigates the utility of using distance metrics based on the causal approach suggested above. We show that such a metric can improve, in a statistically significant way, the ability to distinguish tests based on which faults they trigger. Furthermore, fault localization based on the same metrics can successfully pinpoint the location of some compiler faults sufficiently to be likely to substantially aid debugging, and, when applicable, performs very well for some faults studied in recent papers on mutant-based fault localization [33], [10].

The contributions of this paper include our ideas about how to measure distance between program executions, but more importantly some practical proposals for using such a metric in compiler fuzzer taming and fault localization. Our experimental results show that, for two realistic complex compilers, Mozilla's SpiderMonkey 1.6 JIT for JavaScript and GCC 4.3.0, using mutant response metrics improves FPF-based fuzzer taming over best previous results [14], in multiple ways. Our fault localization approach also compares favorably to state-of-the-art fault localization techniques [54], [33], [58], albeit with a more limited applicability (but also a lower cost).

II. A MUTANT-BASED DISTANCE METRIC

For every failing test case, its response to mutants defines a bit-vector, with length equal to the number of mutants that repaired any test case, where a 1 bit indicates that the mutant in question repairs this test case[1]. These cannot be effectively

[1]By *repairing* a failing test we here mean that a mutant causes the test to pass, *and* the mutant does not cause other, previously passing tests, to fail.

compared with Hamming distance. Consider t_1 and t_2, both repaired by 100 mutants. If these two test cases share 90 common repairing mutants, and disagree for 10, they have a Hamming distance of 20. This makes them "less similar" than two test cases, t'_1 and t'_2, where t'_1 is repaired by 5 mutants, t'_2 is repaired by 5 mutants, and *none of those mutants overlap*. A Jaccard distance, also used (as similarity) in some fault localization algorithms [11], is a better fit. For bitvectors u and v of length n:

$$d(u,v) = \begin{cases} \frac{\sum_{i=0}^{n-1} 1 \text{ if } u_i \neq v_i \text{ else } 0}{\sum_{i=0}^{n-1} 1 \text{ if } u_i \neq 0 \vee v_i \neq 0 \text{ else } 0} & \exists i : u_i \neq 0 \vee v_i \neq 0 \\ 0 & otherwise \end{cases}$$

This metric, the portion of mismatches over bits that are 1 in either vector, matches our intuitions about the above comparisons. It essentially formalizes the intuition of the onion-ring model, where more matching repairs indicates a much higher probability two failures are due to the same fault, even if not all repairs match.

Given such a metric, FPF [21] fuzzer taming [14] begins by ranking as first an arbitrary test case. FPF next ranks the test case, of all unranked tests, that has the largest minimum distance from all ranked test cases — the test case most dissimilar by *closest* already ranked test — until all tests are ranked. FPF is boundedly close to optimal under certain assumptions [21].

More complex metrics are possible, also combining information such as coverage and language features or test case tokens [14]. However, mixtures of metrics (combining different features) other than Levenshteins [44] over test case + output never performed well compared to more focused metrics in earlier work [14]. Therefore, in order to evaluate the contribution of our repair-based metric we compare it, alone, to the previous best metrics.

In Section VI we discuss uses of d requiring comparison of both failing and passing test cases: in this case, we would use three-valued vectors, with a bit for each *property*, where 1 indicates a failing property succeeds and -1 that a succeeding property fails. For such an application, we could remove the requirement that a repair not be a killed mutant, since the passing test vectors would encode that information

III. LOCALIZATION AND EXPLANATION

Automated fault localization [74], [38] provides users with information about the likely location of a fault in a program's source code. Parnin and Orso recently suggested that fault localization methods are not actually providing a great deal of assistance to real users in debugging [59]. Automated fault localization has not been widely adopted in industry.

Parnin and Orso note that most popular methods are statistical and tend to simply provide an ordered list of statements to examine. These statements are not guaranteed to be essentially related to the fault — with many techniques, the statements are, too often, code that executes as a consequence of the fault, code accidentally executed with the fault, and so forth. We note that most evaluations of fault localization have been

performed over programs where it is not clear that debugging is extremely hard, vs. the time spent applying automated localization and establishing effective (automated) testing to support automated localization. Compiler debugging, on the other hand, especially for subtle optimization-based problems that produce incorrect code (vs. simple crashes) is generally known to be difficult. Researchers reporting subtle compiler bugs often observe the time from reporting a fault to its correction, even once the fault is assigned, to be lengthy, and the resulting patches are sometimes thousands of lines [14]. Important compilers, whether JITs where security is paramount or C compilers used to compile core systems code, also tend to already be the subjects of extensive automated testing [68], [77].

Repairing mutants can be used for localization and explanation. While few will be equivalent to actual fixes, they will sometimes overlap the incorrect code, and will often be closely related to incorrect code, semantically. Since some tests are repaired by a large number of mutants, it is important to rank the mutants to produce a localization. Based on the onion-ring model proposed above, this is simple: each mutant is ranked as a localization/explanation based on the *most distant failure it also repairs*. If a given mutant only repairs failures that have similar repair vectors to the test case being localized, it will be highly ranked. If it repairs even one very dissimilar failure, then it will be considered a poor localization.

The **Repair** localization for a failing test case f ranks statements by the function r where:

$$r(m, f) = \begin{cases} \frac{1}{1 + max_{t \in F : m \in R(t)} d(t, f)} & m \in R(f) \\ 0 & m \notin R(f) \end{cases}$$

This ranks a mutant m (and associated statement) by the inverse of the distance from f to the most distant failing test also repaired by m, adding 1 so that if m only repairs failures at distance 0 r is well defined (and maximal). If m does not repair f, it is not ranked at all. One advantage of this approach over some statistical methods is that by ranking *mutants* the localization offers developers not simply a ranked statement but a kind of "explanation" [25] of the fault: if *this mutant* were applied, the fault would not exhibit. As Kochhar et al. show [40], a large majority (more than 85%) of developers considered the ability of a localization tool to provide a *rationale* or reason for a localization important. A mutation plus the tests that stop failing when it is applied is a practical, concrete explanation of a fault localization. Mutants that repair a failure provide insight into the nature of the fault, and, we hypothesize, more information the closer they are to the center of the onion-ring.

An advantage of this method over some statistical approaches to localization is that it is not potentially confused in settings with multiple faults. Each localization/explanation is based on a single failing test case, which in most cases fails due to only one fault. In fact, other faults (and their failures) theoretically *help* **Repair** rank the mutants.

In general, we expect **Repair** to be used in the context of compiler (or other complex system) fuzzing, where there are numerous failing tests, likely from multiple faults. In such cases, the f to be localized is presumably selected by FPF. However, in case our localization approach is used in a setting where single faults are expected, we propose an alternative method: let f be an arbitrary test with as few repairing mutants as possible, to allow the distance metric to focus attention on a few key mutants.

IV. EXPERIMENTAL RESULTS

Our primary experiments are based on subjects used in the only previous study of compiler fuzzer taming (from Chen et al.'s PLDI 2013 paper [14]), which we hereafter refer to as the FPF benchmark. Of the three data sets examined in that paper, this paper considers two: faults and tests cases for SpiderMonkey 1.6, Mozilla's JavaScript engine, with tests generated by jsfunfuzz [68], and *wrong code* faults and test cases for GCC 4.3.0, with tests generated by Csmith [77]. GCC 4.3.0 crash faults were essentially perfectly localized by previous approaches. In fact, on examination, only two of the 11 crash bugs are not distinguished by simply examining the crash message. Faults that produce a crash are also likely to be much easier to debug than semantic problems such as all of the GCC wrong code bugs and most of the JavaScript faults — e.g., following a dynamic slice might well suffice.

Our mutants were produced using the tool written by Jamie Andrews [3]. Andrews' tool applies only four operators: statement deletion, conditional negation, operator replacement, and constant replacement, chosen as a small set that still produces good results for C code. Experiments were performed on a MacBook Pro with 16GB RAM and dual-core 3.1GHz Intel Core i7; GCC executed on a VirtualBox-hosted Ubuntu 11.04. For GCC, some test cases from the FPF set no longer failed, presumably due to unknown differences in execution environment, OS, or memory layout. Discarding these reduced the set of test cases from 1,275 to 1,117 and the number of distinct faults to 27 rather than 35. For SpiderMonkey, all 1,749 test cases from the FPF benchmark failed in the new environment, representing 28 distinct faults.

These data sets, though similar in that both compilers are written in C, provide some interesting variance for testing our metrics. The oracle for SpiderMonkey executions is the set of checks built in to jsfunfuzz plus the requirement that the execution not crash. This is only a moderately strong oracle, and allows serious deviations from both the JavaScript language specification and normal SpiderMonkey behavior, while checking some complex details, such as eval round-trips. For GCC, the oracle is a differential check on a hash code: failure was defined as producing a executable with the -O3 flag that, when executed, produced a different checksum than code compiled by either of GCC 4.9.3 or clang 7.0.0, both using -O0. A repairing mutant must enable GCC 4.3.0 to actually compile code correctly, which is a very strict correctness property, making coincidental correctness [55] highly unlikely — only missed optimizations are allowed.
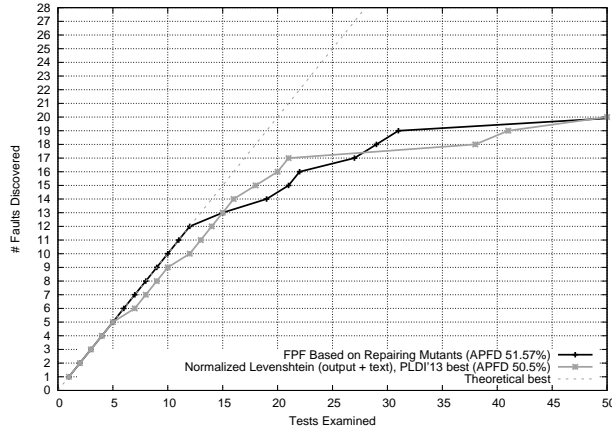
(a) Discovery curves for SpiderMonkey 1.6 faults
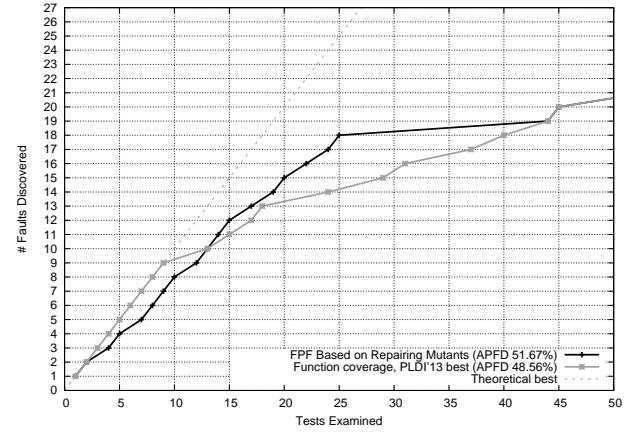
(b) Discovery curves for GCC 4.3.0 wrong-code faults

Fig. 2: Discovery curves compared to best curves from Chen et. al PLDI 2013 data (FPF benchmark)

We only show results for the first 50 tests in the ranking, and computed areas under curves for the same limit, since it seems very unlikely that a user will examine many more than 50 tests, especially given the decreasing slope of the discovery curves. In practice, after 50 tests, fixing a few faults and then re-running tests and FPF seems the most likely recourse, and we confirmed that after removing random subsets of faults, our metrics still outperform the previous best. We applied X-means [60] in an attempt to use clustering with our metrics, but, as with previous results [14] it did not compare well with FPF, and the runtime was much higher. We confirm the conclusion [14] that clustering is of limited value in fuzzer taming.

A. Mozilla SpiderMonkey 1.6 Results

There were 96,828 SpiderMonkey mutants, based on 69,634 lines of code in C and header files. Of these mutants, 12,666 were covered by some failing test case. Of these mutants, 10,525 survived a basic set of SpiderMonkey quick tests [26]. Of these mutants, 1,326 (12.6%) repaired at least one test case. Figure 2a shows the discovery curves for the mutant repair metric compared to the ideal discovery curve and the best curve from previous work using FPF, which used a normalized Levenshtein distance [44] over the failure output and test case text [14]. A discovery curve is a plot of the number of distinct faults that a user, examining the tests in the ranked order, would have seen after N tests (here, N goes up to 50).

The APFD (Average Percent Faults Detected) values in the graphs are based on the measure introduced by Rothermel et al. for evaluating test case prioritization methods [18]. APFD is a somewhat better summary of results than the raw curve areas used for evaluation in previous work [14]. APFD, as the name suggests, measures the percent of all faults discovered at the "average" point on the curve, by comparing the curve's area to an ideal curve, with interpolation. A simpler (but less informative) way to compare the curves is to note that the

mutation-based metric's curve is above the best previous curve at 60% of data points.

APFD results are useful summaries, but the curve itself is also worth examining, since (1) long sequences of tests with no new faults may discourage users more than an overall less effective but steadily climbing curve with few "plateaus" as we call these uninformative sequences of tests and (2) a good early curve is important to developers. The mutant repair curve climbs very rapidly in the early portion, with perfect discovery for the first 12 faults. The largest plateau is 5 tests without a new fault, ignoring the long period after the 31st test. In fact, a plateau at the end of the curve is not problematic. Assuming that few users will examine more than 50 test cases without fixing some faults, the user may give up after seeing 10 or more tests without a new fault based on the mutant repair curve, and in fact lose nothing by doing so. The best FPF benchmark curve, in contrast, has a plateau of size 17 after the 17th fault. If we assume a user stops examining tests after a size 10 or greater plateau, the user will only see 17 faults using the best benchmark metric, vs. 19 with our metric. Stopping at a plateau of size 6 produces the same results.

Over all mutants, and all pairs of test cases repaired by the same mutant (so the same pair may count many times, if many mutants repair both test cases), the probability of being due to the same fault was 42.77%, and the probability of being due to the same fault if two test cases disagreed on a repair (the mutant repaired one test case but not the other) was only 20.33%. The baseline rate for same-fault for test pairs was 33.64%. Just knowing that two test cases have *one* shared repairing mutant makes it 1.27x more likely that they are the same fault, and knowing they differ for one mutant makes it 0.6 times as likely they are due to the same fault. Matching non-repair, however, provides very weak evidence: only a 34.14% chance of matching fault, just over baseline.

An additional measure of effectiveness is to consider how effective a metric is in producing matched nearest neighbors. That is, how often is the nearest neighbor of a failure (that is not due to a singleton fault — a fault detected by only one test case) due to the same fault? For reduced [80], [14], [65] test cases, we assume that for a perfect metric, the nearest neighbor should almost always share the same fault, since there is little or no extraneous semantic content to each test beyond the cause of failure. For the SpiderMonkey failures, 96.3% of non-singleton failures matched their nearest neighbor(s). For mutants that repaired any failures, the mean and median number of test cases repaired were 120.6 and 4.0, respectively. Most mutants repaired a small number of tests, but a few mutants repaired a very large number of tests. A few mutants repaired *all* SpiderMonkey faults; obviously these mutants were not actual fault fixes, but effectively disabled the mechanism `jsfunfuzz` used to detect failure. The mean and median homogeneity for repairing mutants (% of failures repaired corresponding to the most common fault repaired) were 77.96% and 86.36%, respectively.

In order to check our results statistically, we sliced the FPF benchmark tests into 20 randomly selected equal-sized (as much as possible) distinct subsets. The repair metric had a mean APFD (89.23%) for these subsets that was 2.85% better than the mean APFD for the best benchmark metric (86.76%), over sets of ~ 10 faults. The result was statistically significant ($p < 0.05$ by Wilcoxon test). Median repair APFD was 90.05%, vs. 84.75% for the best benchmark metric.

1) Fault Localization: Table I shows a comparison of three fault localization methods for the SpiderMonkey (and GCC) faults. The first column is a fault ID. The next three columns show localization rankings. A dash for a column means that localization did not rank any faulty statements, or assigned all faulty statements suspiciousness of 0.0. The three rankings are: our **Repair** localization, the MUSE [54] localization, and the MUSEUM [33] localization. MUSEUM uses the same formula as MUSE, but works better for multiple faults because it uses only one failure. The MUSE/MUSEUM formulas normally make use of information from passing tests as well: when mutating a statement makes a passing test case fail, it makes the statement *less* suspicious, by a weighted amount. The weight assigned to information from passing tests in our setting would likely be low (due to the ratios of repairs to mutation kills). In a limited sense, information from passing tests is already incorporated in our results. Throwing out all mutants that are killed by any passing test as potential repairs/localizations ensures that the rankings of all statements that are in MUSE/MUSEUM rankings are correct, *relative to each other*. By definition, passing tests have no influence on the suspiciousness of these mutants. However, there may be other mutants that 1) repair a failure and 2) are killed by some test case: these could, if enough different ones repaired the same faulty statement, improve MUSE/MUSEUM results, though in practice this is extremely unlikely. We reject such mutants in part to keep costs low, and in part because we think that the causal information contained in such mutants,

that "fix" a failure but also break some passing test(s), is problematic. They seem likely to be less useful as explanations of the causes of a failure, since they do not impact the program semantics in a way that is only known to be beneficial, and could potentially even mislead a user if used as explanations.

Discussion with the MUSE/MUSEUM authors confirms that adding information for passing tests, while costly, would likely improve the MUSE and MUSEUM results, given the weakness of our oracles, particularly for SpiderMonkey, and should be considered essential for ideal application of their approach. Because the cost of recording the full mutant analysis matrix for all passing tests (rather than considering a mutant killed as soon as one test fails, as we do) is very high, and we would like to produce a comparison over a fixed computational cost, we give results for MUSE/MUSEUM over failing tests only, cautioning that we are not sure what the impact of this choice is on faults that were not localized. For similar reasons of keeping costs low, we also used the mutation operators of Andrews vs. the more extensive Proteum [50] operators used in MUSE/MUSEUM's evaluations. In Section IV-C we provide some comparisons with MUSEUM and MUSE fault localization in the context of a full mutation result matrix using their preferred set of operators.

Table I only includes 8 of the 28 SpiderMonkey faults. This is due to a problem with the data set: producing ground truth patches for a large application, in the sense of finding a minimal, clearly fault-fixing code change that can be back-ported to the original code is difficult [14]. While we believe the 28 faults identified in the FPF benchmark data are correct, it is very difficult to produce a valid patch of version 1.6 that captures the fix for most of these faults. In some cases the final commit that caused tests to stop failing appeared to only be the end of a complex series of changes that converged on a correct fix. In other cases, the code was modified so extensively before the fix that identifying "the incorrect part" of the original code seemed to owe more to guesswork than certainty. The evaluation of localization therefore only examines the 8 faults for which we could be reasonably certain that the patch to 1.6 was correct and characterized the fault in question accurately. In no case was the patch equivalent to one of the mutants (the smallest patch modified two statements).

No method performs extremely well — for half the faults, no methods produced what we consider a useful localization. Compiler faults are hard to debug, and any help is useful, but it seems unlikely developers will really benefit from a localization when it does not rank at least one faulty statement in the first 25-30 statements. By this measure, MUSE only provides a helpful localization once, and performs worst of the methods. **Repair** and MUSEUM both perform well, with **Repair** slightly better.

B. GCC 4.3.0 Wrong Code Results

There were 377,679 GCC mutants, based on 424,186 lines of code in C and header files. Of these mutants, 73,016 were covered by some failing test case. Of these mutants, 41,385 survived a GCC bootstrap build, compiled hello world, and
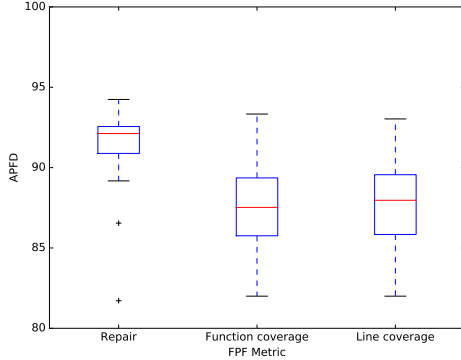
Fig. 3: APFD values for GCC suite slices

TABLE I: Fault localization for compiler faults

SpiderMonkey				GCC			
ID	Repair	MUSE	MUSEUM	ID	Repair	MUSE	MUSEUM
1	-	-	-	1	-	157	-
2	-	173	-	2	1	99	14
3	1	22	4	3	-	146	-
4	-	-	-	4	-	-	-
5	19	118	26	5	-	109	-
6	28	87	23	6	-	-	-
7	-	-	-	7	-	-	-
8	3	133	5	8	4	158	18
				9	-	-	-
				10	-	-	-
				11	10	167	6
				12	1	172	195
				13	-	-	-
				14	34	170	125
				15	5	159	31
				16	-	-	-

passed a GCC test suite. Of these mutants, 3,232 (7.8%) repaired at least one test case. Figure 2b shows discovery curves for the mutant repair metric vs. the best benchmark curve, Euclidean distance over function coverage vectors [14]. GCC's APFD improvement is larger than that for SpiderMonkey, but its early curve (first 15 tests) is worse compared to the benchmark curve, but still has a maximum gap of only 2 redundant tests vs. 4 for the benchmark. Over all, again the curve is better than the best benchmark curve at 60% of all points. Using the hypothetical model where a user stops examining tests after a plateau of size 10, the user of the mutant-based taming will see 18 distinct faults by examining 25 tests, and the user of the function coverage based taming will see 20 faults after examining 45 tests. If a user gives up after a plateau of size 5, our approach again lets a user examine 18 faults over 25 tests. Function coverage only yields 16 faults over 31 tests.

For GCC wrong code faults, the baseline chance that two test cases shared an underlying fault was 38.97%. Knowing that two test cases shared a repairing mutant raised the chance to 68.5%, and knowing they disagreed on a repair lowered it to only 19.01%. The respective increase and decrease in probability of matching fault compared to baseline was thus 1.75 times greater for matching repairs, and less than 0.5 the chance of being the same fault, given one mismatched repair. Knowing two test cases were both not fixed by a mutant again provided marginal evidence of sharing a fault: 39.2% chance of matching faults. For 99.3% (all but 8) of the 1,090 non-singleton fault failures in GCC wrong code, the closest test case(s) by the repair metric showed the same fault. The best previous reported FPF metric, function coverage vectors, had a matching rate of 92.2%. The mean and median numbers of repaired tests per mutant (for mutants that repaired any tests at all) were 18.4 and 3.0, respectively. A few mutants fixed a very large number of tests, up to 1,050. These all appear to be turning off all optimizations — essentially running gcc in -O0 mode. Most mutants fixed only a few tests, to a greater extent than was true with SpiderMonkey. Mean homogeneity (% of repaired tests corresponding to most common repaired fault) was 79.4%, median 100%.

Slicing the GCC tests into 20 random equal-sized test subsets and comparing with function and line coverage metrics (Figure 3) we find that differences in APFD values are statistically significant between our metric and both benchmark metrics, by Wilcoxon test, with $p < 0.0005$, and the mean APFD improvement — even for only 55 tests, exposing only 6-11 faults (mean of 7.75 faults) — is more than 3.8% better than either benchmark metric.

1) Fault Localization: Table I shows localization results for GCC 4.3.0. Again, only some faults were deemed to have strong enough ground truth patches for evaluation. MUSE performed poorly, with no useful (by the standard of having at least one faulty statement in the top 25) localizations. MUSEUM provided 3 useful localizations, but no very high quality localizations. Only **Repair** performed very well, with useful localizations for 5 faults, and the fault was in the top 5 statements for 4 of these.

C. Repair Localization for Non-Compiler Faults

The **Repair** localization was designed for use in compiler (or at least complex system software) fuzzing. It is not proposed (or at least, not here evaluated) as a general-purpose fault localization method. However, it is possible to compare **Repair** with other methods, using their full mutation and experimental set, to provide a more apples-to-apples comparison than the evaluation vs. MUSE and MUSEUM over compiler faults. We obtained the data from two evaluations of mutant-based fault localization methods: the set of faults used to evaluate MUSEUM [33], and a set of CoREBench [7] faults used in an evaluation of MUSE and Metallaxis-FL by Chekal et. al [10] (we were unable to obtain the original MUSE dataset, thus far). These data sets are not ideal for evaluating **Repair**: for all but 10 of the faults (all from the CoREBench set) there is only a single failing test case in the data. **Repair** for single failing tests is essentially MUSEUM without use of passing tests, except to reject any mutation that breaks a passing test. **Repair** is meant to operate in the case where a program either has numerous failing tests associated with open bugs (e.g., a typical production compiler or complex system software program) or where a program has just been fuzzed for the first time, so has numerous failures representing

different faults. The distance metric lets **Repair** make use of these other faults, by enabling it to focus on repairing mutants most relevant to the failing test.

1) MUSEUM Results: For the six faults used in the original MUSEUM paper (all with a single failing test), **Repair** gives the same (perfect) localization for three faults. One of the other three faults (Bug2) has no repairing mutants (that do not break some passing test) so **Repair** provides no localization information at all. For the remaining two faults, Bug5 and Bug6, **Repair** gives best rankings of 12 and 9, respectively, compared to 8 and 3 for MUSEUM.

2) CoREBench Results: For the full data set in the paper [10], **Repair** does not perform as well as MUSE and Metallaxis-FL, as expected. It is only able to rank a faulty line of code for 12 of 30 faults. However, for these faults, it performs very well indeed, perfectly localizing 7 of the 12 (vs. 3 total perfect localizations for Metallaxis-FL and 4 for MUSE). **Repair** is uniquely best for 5 of the 12, and is tied for best for another 6; for the remaining fault, it performs better than Metallaxis-FL but worse than MUSE. Table II shows full results. The fourth column shows, for those bugs where **Repair** did not rank any faulty statement, how many statements **Repair** proposed for a user to examine. Note that in most cases if **Repair** was not helpful it also provided little output to waste a user's time: it only once presented more than 3 statements to examine. In practice, we only suggest using **Repair** at all in cases where 1) there is at least one repairing mutant (not killed by any passing test) for a failing test and 2) there are multiple failing tests (whether from one or multiple faults). When the Stmts column shows a bold 0, it indicates there were no repairs (7 of the 30 faults). The 7 faults meeting both requirements are bolded. For 4 of these, **Repair** produces a perfect localization, and for the other three, it produces no more than 2 non-faulty statements to examine.

D. Discussion

In one sense, the discovery curve improvements here are practically very useful, but not extremely large in a relative sense. The SpiderMonkey curve has an APFD only slightly more than 2% better than the best result from previous FPF efforts. For GCC, APFD improves by 6.4%. However, the comparison is with the very *best* curve chosen after running more than 16 different metrics. In practice, users simply do not know ground truth to rank curves, thus Chen et al. [14] do not really give a practical approach to distance metric selection. The best methods for different subjects varied widely, even in such difficult-to-understand ways as less-fine-grained coverage providing better results in some cases, but worse in other cases. In practice, their work established that FPF could produce good curves, but gave very little useful guidance for choosing a metric for actual use of the technique, since trying multiple metrics is impractical: a user has to examine each curve. This paper presents a single curve using a universal metric, and achieves a 2-6% percentage point improvement over the best of more than 16 different metrics studied in the previous work; our improvements over any single "reasonable" method

TABLE II: Fault localization for CoREBench faults

Fault	#Failing Tests	Repair	Stmts	Metallaxis-FL	MUSE
Coreutils 1	2	1	-	1	1
Coreutils 2	3	-	**0**	86	186
Coreutils 3	1	25	-	47	27
Coreutils 4	1	-	**0**	28	431
Coreutils 5	5	-	2	4	7
Coreutils 6	1	-	1	5	206
Coreutils 7	1	1	-	5	9
Coreutils 8	1	-	1	22	160
Coreutils 9	5	-	1	10	157
Coreutils 11	1	-	1	2	1
Coreutils 12	2	-	**0**	6	9
Coreutils 13	1	-	**0**	905	833
Coreutils 14	1	14	-	19	11
Coreutils 15	2	1	-	1	6
Coreutils 16	1	-	**0**	569	447
Coreutils 17	3	-	2	37	195
Coreutils 18	1	-	3	27	3
Coreutils 19	1	1	-	3	1
Coreutils 20	7	1	-	12	3
Coreutils 21	2	-	**0**	20	191
Coreutils 22	2	1	-	6	2
Findutils 27	1	-	9	12	6
Findutils 32	1	-	1	73	70
Findutils 33	1	-	3	10	30
Findutils 35	1	1	-	1	1
Findutils 36	1	9	-	9	19
Findutils 37	1	11	-	22	34
Grep 46	1	-	2	7	10
Grep 47	1	4	-	4	4
Grep 48	1	-	**0**	26	497

applied to both problems would be considerably larger (for instance, using the most obvious basis, line coverage, we see more than 17% improvement). Moreover, the improvement is, for our subject programs, reliable: choosing random subsets of the full data set, the difference in mean APFD from best-previous method is around 3%, and statistically significant.

For fault localization and error explanation, the results show possible improvement on state-of-the-art mutation-based approaches. A practical impact of the results reported is that we suggest users of our techniques examine only the first few (at most 10, and we propose as few as 5) mutants and coverage-changing statements, and ignore localization if none of these results are helpful. This is analogous to our suggestion that a user abandon the FPF curve if 5-10 test cases in a row fail to reveal any new faults, as a heuristic. We suspect the information in the mutants/coverage changes is probably helpful in some cases where they do not localize a faulty line, but this is simply based on our highly incomplete understanding of the faults and patches in question, and not a solidly established claim. The expertise of compiler developers would be needed to confirm or reject this belief. Our suggestion that mutations themselves provide interesting error explanation is also applicable to MUSE and MUSEUM. There may be some potential for confusing explanations, however, if repairs include mutants that also cause some failures, as in the standard MUSE/MUSEUM approach.

For the compiler faults, if any repair for a failure modified a faulty statement, **Repair** always ranked it in the top 34 localizations, ranked it in the top 5 in 6 of 10 such cases, and 3 times ranked it 1st. MUSEUM, in contrast, ranked one such repair 195th, never ranked a fault in its top 3 statements,

and only ranked a fault in its top 5 statements twice. Of course, MUSEUM might be suffering from a lack of mutation operators, but in some cases this is unlikely due to the nature of the repairs. For the CoREBench faults, **Repair** produced more than twice as many perfect localizations as the other methods, though it also did not provide any ranking for a fault in a large number of cases.

There are two ways to consider the performance of **Repair**. First, it is using information that other techniques are not using. If each failure was only repaired by one mutant, MUSEUM and **Repair** would both rank that mutant maximally, and we speculate that it would usually be faulty code. But such cases are very rare in compilers: for SpiderMonkey the mean/median number of mutants repairing each failure were 90.8 and 78, respectively, and for GCC the mean and median were 54 and 43. **Repair** can distinguish repairing mutants in fine-grained ways that MUSE and MUSEUM cannot by relying on the onion-ring structure: when a repair also repairs failures that otherwise do not resemble the failure being localized (in terms of its repairing mutants), **Repair** assumes that repair is general to many different faults, and so ranks it lower than more "relevant" repairs.

The other way to think about **Repair** for the non-compiler faults especially, is as a very *cautious* localization. It only uses mutants that repair a failure, and, because it is clear that, in general, failing tests contain far more information about faults than passing tests, it only uses information from failures (other than using passing tests to prune mutants that kill some passing tests, again a principle of discarding potentially confusing information). This results in **Repair** either providing a "useful" localization or almost no localization information at all to mislead a user, for the non-compiler faults, which user studies and surveys suggest is the ideal behavior due to the cost of false positives in localization [59], [40]. This is basically a trade-off. MUSE, MUSEUM, and Metallaxis-FL provide localization for many more faults, in a more diverse range of settings (with single failing tests, no repairs, etc.), and using more diverse information — but, in cases where **Repair** provides a localization, it seems to often be higher quality due to its restriction to a high-signal source of information (mutants repairing failing tests, only). In settings, unlike compilers, where there are relatively few repairs, **Repair** also produces very little incorrect information, and so has a low cost to the user in terms of wasted attention. Note that our heuristic to stop reading after 5 wrong localizations is not even needed for all but one CoREBench fault — **Repair** suggests 1-3 statements at most.

E. Mutation Analysis Costs

The compiler experiments required a large computational budget. However, our approach is, by design, cheaper than MUSE or Metallaxis-FL, in that, for a program with up-to-date mutation testing results (just knowing which mutants are killed, not a full matrix), it only requires executing mutants over failing tests. Running each test case under each mutated version of the compiler is cheap: each execution requires

around 0.05 seconds for SpiderMonkey, and 0.12 seconds for GCC, on average; moreover, executions can be done in parallel, most failures do not cover most mutants, and vice versa, so the total number of repair checks needed is far less than the product of mutants and failures, and can be spread over many machines.

Mutation cost reduction techniques are also applicable. E.g., trivial compiler equivalence (TCE) [57] can reject equivalent and redundant mutants, removing almost 30% of mutants for benchmark subjects. Other techniques, such as using mutant schemas to avoid having to compile and store each mutant, are also applicable — almost all of the techniques characterized by Offutt and Untch [56] as *do faster* approaches apply. Whether sampling mutants [22] is applicable is less clear. Even if the FPF curve remains the same, a critical mutant for localization might be omitted. We performed some limited experiments, and found that for a few sample sizes ranging from 13% of mutants to 50% of mutants or using only some mutation operators (with the exception of operator-replacement-only), APFD was always better than benchmark set's best metric. However, the improvement in some cases was *much* smaller than for the full set, and varied considerably (in some cases it was up to 2% better than our full results). Fault localization results were much worse, however (with relative rankings preserved) for both our techniques and MUSE/MUSEUM.

F. Threats to Validity

The largest threat to validity is that this paper's conclusions rely on only two data sets for compilers, and a small number of non-compiler faults. The compiler ground truths are possibly imperfect, due to the complex change history, and it was not possible to produce good patches for all faults, limiting our analysis of compiler fault localization. The comparison with other fault localization methods is also limited: our compiler experiments limit the mutation budget in ways that may weaken MUSE and MUSEUM effectiveness, and the results using fault and mutant data sets from other studies provide few faults, no multi-fault problems, and very few multi-failure problems, limiting **Repair** effectiveness. It is also possible that faults in the framework used for experiments introduced errors into the data. In order to check for these problems, we will make the raw data for a re-analysis available on request.

V. RELATED WORK

The most closely related work is that of Chen et. al [14]. We use two of the same compiler data sets, and both apply the FPF [21] algorithm. The distance metrics used with FPF in the previous work, however, were *ad hoc*, chosen for each program by hand, produced poorer results for the critical portion of the curve for the two hard data sets, and were not extended to provide any fault localization. This paper uses the FPF approach in the context of a more causal comparison of program executions that applies in theory to any executions, not just failing test cases, and introduces a novel fault localization. Prior to the FPF-based work, Francis et al. [19], Podgurski et al. [62] and others [49], [48] used clustering

to attempt to solve similar problems in identifying distinct faults, on simpler human-generated data sets. Chen et. al [14] provide an in-depth comparison of clustering and ranking, and the advantages of ranking (as well as some possible limitations). Jones et al. discuss general issues in debugging multiple faults efficiently [36]. The work of Groce et al. [28] provides an alternative approach to fault identification, based on term rewriting to normalize failures. However, this approach is not currently applicable to compiler fuzzing, as it only support tests that are sequences of method calls.

Fault localization [74], [66], [47], [48], [15], [38], [37], [36], [25], [9], [49], [24], [69], [2], [76], [80], even specifically for compilers [73], is a long-standing field of research. Recently, fault localization researchers have used program mutants to improve statistical [38] fault localization techniques [54], [16], [20], [33], [58], [10]. The work of Hong et al. [33] on MUSEUM and Moon et al. [54] on MUSE provides a good summary of other work along these lines. Much fault localization work is largely (in many cases exclusively) based on single-fault scenarios; e.g., the MUSE [54] evaluation includes only 2 of 14 scenarios with multiple faults, and those scenarios include only a few faults. The statistical approaches may owe some of their superiority in experiments over other methods to the use of mostly single-fault evaluations [37]. By focusing on a single failing test case (for us, selected by fuzzer taming) as the locus of analysis, our work, like some early efforts [66], [15], [25] may be more suited for situations involving many faults.

Using program modifications or mutants broadly defined to repair programs has been a popular topic of recent work as well [43], [63], [72]. These *generate-and-validate* approaches to patching aim to fix programs by generating a large variety of potential fixes, then using test cases to prune out invalid fixes. Qi et al. [64] discuss some serious limitations of early work on this topic, but also demonstrate that effective patching is possible using a restricted search space that focuses on removing functionality.

The use of distance metrics in software engineering includes some of the localization and fault identification efforts discussed above (e.g., [66], [25], [9], [49]). Vangala et al. proposed using distance to cluster test cases to improve diversity and eliminate duplicates [71]. We speculate that using FPF with our causal metric over passing tests as a test prioritization (or selection) method might be a fruitful approach [78]. Adaptive random testing uses distances (usually between inputs) to choose tests [12], [13], [4], and Artzi et al. guided test generation for fault localization using path constraint metrics [5]. Sumner et al. evaluated approaches to generating execution peers for debugging and understanding [70] using tree edit distances (and evaluated other distances). Xin et al. [75] examined methods for indexing program executions, useful in all methods involving execution alignments. To our knowledge, most metrics used are essentially spectrum-based [67], [6] (using counts over structural entities), while our metric is essentially causal, based on mutants as counterfactual [45], [46] versions of a program.

VI. CONCLUSIONS AND FUTURE WORK

By using program changes (produced by mutants) as causes to determine distance, it is possible to improve on efforts to identify the set of faults in a large set of redundant compiler failures [14] and discover the source code locations of faults [54], [33]. For a set of more than 2,800 tests and more than 50 faults, over the Mozilla SpiderMonkey 1.6 JavaScript compiler and GCC 4.3.0, our methods improve fault identification ("fuzzer taming"), in a statistically significant way (with effect size of about 3% improvement), over the *best* (for each subject program) of over 16 metrics considered in previous work. Preliminary investigation also shows improvement (for a shared mutation testing budget) over previous mutant-based fault localization methods, for the same tests and a subset of 24 faults with known locations. Our **Repair** method, in particular, was the only localization approach to provide any perfect localizations of faults (it produced 3), and provided three times as many very-high-quality (ranking the fault in top 5 locations) localizations as the next best method [33] — 6 such localizations, compared to only 2. For GCC wrong-code faults, only our methods produced any very-high-quality fault localizations. When applied to non-compiler faults, the metric tended to either not produce a localization, produce a very small incorrect localization (1-3 statements), or produce a very high quality localization. Restricting to the cases where we consider **Repair** applicable (7 faults) **Repair** perfectly localized 4 of them, and produced a 1 or 2 statement incorrect localization for the others.

As future work, we plan to apply the mutation-based metric to other fuzzer taming and fault localization problems. The highly varying results for even the best fault localization methods in our experiments demonstrate that further advances are required before compiler debugging can consistently be made less onerous through automated assistance.

We also plan to explore other applications of mutant-based metrics. Many software engineering techniques rely on measuring distance between program executions [6], [66]. Not all of these concern only failing executions. For example, Zhang et al. [81] show that FPF based on a simple Hamming distance over branches covered can significantly improve the effectiveness of seeded symbolic execution [51], [61], [35]. Mutation response is likely too expensive to use for this purpose without modification, but sampling mutants to refine distance when coarser metrics plateau may be practical. Cost may be less of a concern when applying our metrics as a method for prioritizing or selecting regression tests, where the results can be repeatedly used in test suite execution, without recomputing distances based on every code change [78]. Our metrics may also be useful in determining execution peers [70], for example to help operators of spacecraft find similar behaviors to telemetry downlinks in testbed history [31], [27].

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