**Responses to Referee: 1**

**Comment 1:**

One of my major concerns is the applicability of the proposed approach in practice, which is greatly hurt by the assumptions made in the paper, including all failures are deterministic and there are no inter-option constraints.  Needless to say, these assumptions generally do not hold true in practice. While the first assumption is mentioned in the paper (Assumption 1 on page 6), the second assumption is not mentioned. This is important because in the presence of inter-option constraints, which invalidate some option setting combinations, some of the propositions and proofs given in the paper seem to become invalid and the approach needs to go through a set of nontrivial modifications. These issues need to be discussed in the paper and at the very least, the authors should elaborate on how to relax these assumptions.

Response: It is really true as the reviewer suggested that we did not clarify the assumptions that may hurt the application of our approach. Hence, we explicitly clarify the assumptions. There are three assumptions that we propose, i.e., the result of test case is deterministic, failures can be distinguished and no inter-option constraints existed. Additionally, according to the comments, we add one more section (Section 7) to explain that the impacts to our approach, as well as the measures to relax them.

**Comment 2:**

I believe what is meant by Assumption 1 in the paper is that all failures are deterministic, rather than all test results are deterministic. If so, this assumption should be restated.

Response: Here what we mean by Assumption 1 is that the test results are deterministic. Actually, these two versions of deterministic testing are both existed in the studies of Combinatorial testing. For example, Zhang [1] and Ghandehari [2] are based on the assumption that test results are deterministic, while Yilmaz [3] and Fouché [4] referred to the deterministic failure. We believed they are all reasonable. In this paper, we use the first version of testing deterministic, as it is simpler to handle (Deterministic failures may introduce the problem of test case-aware covering array [5], which is beyond of this paper). Additionally, we believe that the non-deterministic of test results is caused by the non-deterministic failures, and hence we emphasize that in the Assumption 1 and later in section 7.

[1] Zhang, Zhiqiang, and Jian Zhang. "Characterizing failure-causing parameter interactions by adaptive testing." Proceedings of the 2011 International Symposium on Software Testing and Analysis. ACM, 2011.

[2] Ghandehari, Laleh Shikh Gholamhossein, et al. "Identifying failure-inducing combinations in a combinatorial test set." Software Testing, Verification and Validation (ICST), 2012 IEEE Fifth International Conference on. IEEE, 2012.

[3] Yilmaz, Cemal, Myra B. Cohen, and Adam Porter. "Covering arrays for efficient fault characterization in complex configuration spaces." Software Engineering, IEEE Transactions on 32.1 (2006): 20-34.

[4], Sandro, Myra B. Cohen, and Adam Porter. "Incremental covering array failure characterization in large configuration spaces." Proceedings of the eighteenth international symposium on Software testing and analysis. ACM, 2009.

[5] Yilmaz, Cemal. "Test case-aware combinatorial interaction testing." Software Engineering, IEEE Transactions on 39.5 (2013): 684-706.

**Comment 3:**

Furthermore, it is important to emphasize the fact that the proposed approach is concerned with option-related failures.  It would also be interesting to evaluate and/or discuss how the proposed approach could deal with non-option-related failures.

Response: Considering the Reviewer’s suggestion, we have emphasized that our approach can only handle the option-related failures (Section 3.1, Page 7). Additionally, we discussed the problems of non-option-related failure in Section 8.5.2, as well as some measures that can alleviate this problem.

**Comment 4:**

Another concern is that a number of definitions, propositions, and proofs are given in the paper, but the relationship between the theory and the proposed approach is not clear at all. How does the proposed approach benefit from these propositions? How does the proposed approach evolve from the given theory?

Response: We are very sorry for that we didn’t make it clear of relationships between the formal modeling and proposed approach. In fact, the definitions in Section 3.1 (Definition 3.1 to Definition 3.5) are given to formalize the subjects we discussed, that is, to formally define what is the MFS. Section 3.2 generally reveal the relationships between the test sets and schemas. These relationships, however, is the basic foundation of how to identify the MFS, and how masking effects will affect the MFS identification. To reach this target, Proposition 3.7 and 3.8 shows that any test sets has its corresponding minimal schemas. These two propositions show the theory to obtain the MFS, as MFS is exactly the minimal schemas of failing test sets. Proposition 3.10 and 3.11 show that for two test sets that has an inclusion relationship, then their minimal schemas also have some relationships. These two propositions give the foundation of the impacts of masking effects, as the masking effects will make us incorrectly judge the outcomes of test cases, which will result in that the failing test sets we thought are include or including those real failing test sets. Based on this, we can figure out the extent to which the MFS we obtained under the masking effects will be deviated from the real MFS. Other propositions in Section 3.2, i.e., Proposition 3.6, 3.9, are the auxiliary propositions to get those four propositions. Later in Section 4, we mainly formally define the masking effects, and show the impacts what masking effects will affect the MFS identification based on Proposition 3.10 and 3.11. One key observation from the impacts of masking effects is caused by the un-determination of the outcomes of some test cases which have triggered different failures other than the same failure under analysis. What’s was worse, according to the analysis in Section 4.3, this can result in incorrectly inferring results of some test cases. Hence, a natural idea is to reduce those test cases which trigger other failures as much as possible when we identify the MFS for some particular failure. In fact, as we do not need to execute all the test cases for MFS identification (See newly added section 3.3), hence, we can select some proper test cases that needs to identify the MFS. As a result, we should replace those test cases that are used to identify the MFS which trigger other failures with the test cases that either pass or trigger the same failure under analysis. This is the foundation of the approach we proposed in this paper.

In a short word, those propositions are proposed to show how MFS are identified, and how and why the MFS identification are affected by masking effects. The approach that is proposed to reduce the drawbacks that may introduce the masking effects, so we can obtain a higher-quality MFS. According to the reviewer’s comments, we have enhanced the description of the significance for these propositions in the last paragraph at Section 3.2. Additionally, we have added one more section, i.e., Section 3.3, to show the relationship between MFS identification and those propositions, and also prove the fact that we do not need to use all the text cases to identify the MFS. Additionally, we have shown that the test cases that trigger other test cases may significantly negatively affect the inferring result of FII approach (Section 4.3). Later on, we have enhanced the explanation why we should replace those test cases that trigger other test cases at the first paragraph at Section 5.

**Comment 5:**

A related concern is that it is not clear at all whether the proposed approach always guarantees to find the minimal failure inducing schema (MFS) as it is defined in Definition 3.5. Note that once a failure inducing schema is found, the number test cases required to prove that this schema is an MFS grows exponentially with the number of options in the schema. It is not clear whether the proposed approach actually tries all the subsets or not. A proof is needed here!

Response: Sorry about that we did not make it clear that our approach should need the exhaustive test cases. Hence, we have added one more section (Section 3.3) to formally show that, we do not need to execute all the test cases to identify the MFS. The proof is also given. What’s more, we show that we can further reduce the test cases with some assumptions (Safe value assumption in this paper). Similarly, the formal proof is given is Section 3.3.

**Comment 6:**

Masking affects are not only caused by failures. Unaccounted for control dependencies, for example, can also cause masking effects, e.g., the use of –help command line option may make the sut to display a help text and exit without exercising any of the remaining option setting combinations appearing in the underlying configuration.  Therefore, the authors can consider restating Definition 4.1. It is also important to mention that Definition 3.5 is not valid in the presence of masking effects when discussing this definition; not a couple of pages later.

Response: It’s true that masking effects can be triggered by other events, such as unaccounted for control dependencies. According to the reviewers’ comments, we have restated the definition of masking effects, making the cause of masking effects more generally such that it may not limited to the different failures. Additionally, we have re-emphasized that Definition 3.5 is not valid in the presence of masking effects in the first paragraph in Page 6.

**Comment 7:**

The proposed approach deserves a section of its own. As it is right now, the description of the approach is spread across a number of sections. For example, Section 5.2 titled “a case study using the replacement strategy” is the first section that attempts to describe how the approach works. However, as also indicated by its title, this section is supposed to introduce a feasibility study and the approach must have been introduced much more earlier. What is the input to the proposed approach? What is the exact algorithm implemented by the approach? When does the search for failure inducing combinations terminate?

Response: It’s a good advice that we should make the approach an independent section. Hence, after we describing the test case replacement strategy in Section 5, we added a new section, i.e., Section 6 to completely introduce our approach. In this section, we showed how our approach works, including the specific inputs and outputs, how to identify the MFS (with which algorithm), when to replace the test cases, and when to terminate our approach.

**Comment 8:**

The original FIC\_BS approach needs to be explained in the paper, which is not that big of a deal, so that readers are not forced to read other papers just to see what is going on.   For example, just by reading the paper it is not clear how the “fixed parts” are determined and after reading the FIC\_BS paper, one can only guess.

Response: According to this comment, we have explained how FIC\\_BS works in Section 3.3, as well as the exact algorithm which is listed in the Appendix A. We also explain what the “Fixed part” exactly mean in both these two parts (Section 3.3 and Appendix).

**Comment 9:**

The third and sixth paragraphs in Section 2 suggest that the proposed approach will use some sort of static analysis, which is not true. Therefore, these paragraphs should be rephrased to avoid misleading the reader.

Response: Sorry for the misleading to the fact that our approach may use some sort of static analysis, which our approach actually did not. Hence, we removed some confusing words, rephrased some sentences, and emphasize the conclusion to make it more clear.

**Comment 10:**   
  
To generate “desirable” configurations, a suspiciousness score is computed for every option and failure. What is the justification for using such a scoring scheme? Doesn’t it make more sense to compute the scores for combinations of option settings rather than for individual options?

Response: The idea to compute suspiciousness score for every option is first applied in Combinatorial testing in this study [1]. In fact, the suspiciousness score is based on the intuition that, if one option value appears more frequently in the test cases which trigger a particular failure, it is more likely to contribute to that failure. This idea is originally inspired by the general spectra-based fault location technique—Tarantula [2,3].

Besides, as the reviewer said, it is more reasonable to obtain the suspiciousness of schemas. In fact, our formula (EQ1), exactly gives the simplest way to compute the suspiciousness of a schema, i.e., Linear Averaging of the options in a schema. Although these exists more reasonable ways to define the suspiciousness of schema, e.g., considering the weight of different option value, however, we believe the linear averaging is the most common and straightforward way to compute the suspiciousness of the schema, and hence, our approach can work in more efficient way.

According to this comment, we have emphasized these two points in this paper (Section, page).

[1] Ghandehari, Laleh Shikh Gholamhossein, et al. "Identifying failure-inducing combinations in a combinatorial test set." Software Testing, Verification and Validation (ICST), 2012 IEEE Fifth International Conference on. IEEE, 2012.

[2] Jones, James A., Mary Jean Harrold, and John Stasko. "Visualization of test information to assist fault localization." Proceedings of the 24th international conference on Software engineering. ACM, 2002.

[3] Jones, James A., and Mary Jean Harrold. "Empirical evaluation of the tarantula automatic fault-localization technique." Proceedings of the 20th IEEE/ACM international Conference on Automated software engineering. ACM, 2005.

**Comment 11:**

It is quite confusing that the two strategies introduced by the paper, namely “regarded as one failure” and “distinguishing failures”, are explained using the OFOT approach, which is a different fault characterization approach, than the one used in the experiments, namely FIC\_BS. How was FIC\_BS augmented with these strategies in the experiments?  For example, with the “distinguishing failures” strategy what happens if a configuration reveals a different failure?

Response: Sorry for that we used OFOT approach to describe the impacts of masking effects on FII approaches, which is different than the approach, i.e., FIC\\_BS --- the FII approach we used in our approach in this paper. In fact, these two approach are very similar in theory, among which OFOT is simpler to explain. According to this comment, we have re-explained the masking effects impacts based on the FIC\_BS approach. What’s more, to be consistent in this paper, all the content that is related to specific FII approach in this paper are illustrated with the FIC\_BS approach, including the sections about explaining MFS identification, masking effects, and the approach proposed in this paper.

**Comment 12:**

In the first paragraph of Section 5.1, it is not clear what is meant by the following sentence: “The replacement must satisfy the condition that the newly generated ones will not negatively influence the original identifying process.”

Response: This sentence mainly means that the replacement operation should obey some rules, for example, the newly generated test case should keep the same fixed part, otherwise, the original FII approach may not work. As the next sentence (the first sentence of the second paragraph at this section) already explain this, so we have deleted this confusing sentence.

**The comments about the experiments.**

**Comment 13:**

a)      The authors need to further justify the use of synthesized subject applications and elaborate more on how these subjects were actually created.

Response: Considering this comment, and combining this comment of the third reviewer, we have removing all these synthesized subjects. Instead, we added wo more new real subjects in our experiment. We also enhanced the description of these subjects, including how to create them and how to run these experiments.

**Comment 14:**

b)      What was the MeetEndCriteria used in the experiments? Why? A sensitivity analysis for this parameter would greatly benefit the work.

Response: The MeetEndCriteria in the experiment is just the maximal number of trails (set to be 3 by default) to get a satisfied test case when running the replacement strategy. According to the comment, in Section 5, we added the sensitivity analysis to show why we choose this value, by considering the trade off the accurateness of the MFS identification and the cost we needed.

**Comment 15:**

c)      The configuration space models used in the experiments are relatively small. Since the authors have already used simulations, they could have also evaluated the proposed approach on larger configuration spaces.

Response: Yes, the configuration models used in the experiments are relatively small. This is mainly because we should perform the MFS identification for each failing test case. By doing this we can obtain a more general conclusion. If the we greatly increase the number of parameters in the testing model, the effort to conduct these experiments will increase exponentially. Also according to comment of the third reviewer, we remove all the synthetic subjects, so we do not build the subject with large parameters. As an alternative, to make the conclusion of the experiments more general, we created two more subjects with more bugs for each version, so that we can observe the impacts of masking effects under the condition of more failures.

**Comment 16:**

d)      The factors used for the real subject applications seem to be configuration options. Therefore, what were the actual test cases that were executed in the chosen configurations?

Response: In this paper, all the test cases are actual the test configurations. The actual test case for each experiment varies with subjects, for example, for the HSQLDB, our test case is one simple SQL command, while for JFlex, our test case is a text file. What’s more, for all the test configurations, the test case for one specific subject is the same. It is quite true that for one subject, with different configurations, we need to execute multiple different test cases to check the correctness of the software, and even some times, different configuration needs different test cases [1]. But here in this paper, we make the test cases simple, i.e., one for all the configurations. This is because we just want to check the configurations masking effects. This is common in other FII approaches studies [2] [3] [4]. The test case-aware combinatorial testing, however, is beyond the discuss of this paper.

[1] Yilmaz, Cemal. "Test case-aware combinatorial interaction testing." Software Engineering, IEEE Transactions on 39.5 (2013): 684-706.

[2] Nie, Changhai, and Hareton Leung. "The minimal failure-causing schema of combinatorial testing." ACM Transactions on Software Engineering and Methodology (TOSEM) 20.4 (2011): 15.

[3] Zhang, Zhiqiang, and Jian Zhang. "Characterizing failure-causing parameter interactions by adaptive testing." Proceedings of the 2011 International Symposium on Software Testing and Analysis. ACM, 2011.

[4] Ghandehari, Laleh Shikh Gholamhossein, et al. "Identifying failure-inducing combinations in a combinatorial test set." Software Testing, Verification and Validation (ICST), 2012 IEEE Fifth International Conference on. IEEE, 2012.

**Comment 17:**

e)      Please rephrase “to construct many real testing objects [for evaluations] is time-consuming”.

Response: Has been fixed.

**Comment 18:**

f)      What is the degree of an MFS?

Response: As MFS is a schema, so the degree of an MFS is just the degree of that schema (Definition 3.3 in Page 6), i.e., the number of option values in that MFS.

**Comment 19:**

g)      Why was the “ignored number” metric treated differently than the rest of the evaluation metrics?  What was the issue with none of the MFS being ignored (paragraph 3 in Section 6.2.2)?

Response: The issue that none of the MFS being ignored is because that, we merged all the MFS identified in all the failing test case. Hence, there's a strong possibility that we can determine all the MFS, for example, if one MFS cannot be determined in one test case, we may identify that MFS in another test case. Hence, we may not ignore any MFS. Other metrics has no such problem, so they are different. However, as the latter comment (**Comment 21**) said, the average performance metrics are more rational than the merged values, hence, we have revised values for all the metrics, such that only the average values are given.

**Comment 20:**

h)      In the Figures 3a-g, the points that belong to the ILP approach are almost impossible to distinguish from the rest.

Response: We have standardized the results for different approaches (ILP, random, regarded as one failure, and distinguishing failures), such that the comparison among them are more sensible.

**Comment 21:**

One major concern is that the experiments reported in Section 6.2 assume that for a given failure, all the test cases (configurations) revealing the failure are known a priori, which is not realistic at all. In the experiments, given a configuration space, the space is exhaustively tested, and then for each failure, every test case revealing the failure is fed to the proposed approach, and then the MFSs individually obtained for each test case are combined. Therefore, the results reported in this section are over-approximations. To be fair, the proposed approach should not work on any failures that it hasn’t discovered by itself, except for the input failure. For example, the average performance metrics obtained from failures could be reported.

Response: It’s really true that the evaluation for these approach was over-approximation. Instead merged all the identified MFS for one approach, it’s more rational to evaluate the approach against the MFS in the failing test case that it works on. Hence, we have separately evaluated the performance of these approaches in each failing test case, and only focusing on the MFS on that failing test case. At last, we will show the average performance among all the failing test cases.

**Comment 22:**

The discussion on page 24 states that ILP and “distinguishing failures” approaches showed the same or similar performance (i.e., similar exact number, sub number, super number, etc.), but ILP was more costly. I thought that ILP was one of the main contribution of this paper as “distinguishing failures” is a trivial extension for the existing FIC\_BS approach. The authors should elaborate more on this.

Response: The reason why the results of our approach does not stand out from those of strategy “distinguishing failures” is that, we only choose the subjects with a small amount of bugs, i.e., either two or three. This condition is favorable for the “distinguishing failures strategy”. For example, assume bug A masks bug B; then when we identify the MFS of bug A, the “distinguishing failures strategy” is the correct strategy, as if there is a test case trigger the bug B, then it must not trigger the bug A (otherwise, bug B will not be triggered). Hence, there is a probability that is up to 50% that “distinguishing failures strategy” makes the correct operation.

Considering this comment, i.e., to make the results of this two approach more recognizable, we added two more subjects with more bugs. Our new experiment results showed that in such conditions, our approach ILP gives an outstanding result when comparing with “distinguishing failures” strategy.

**Comment 23:**

In Section 6.3, it is stated that compared to the random test generation strategy, ILP reduced the number of test cases needed by 1 to 2, on average. What is the theoretical and practical significance of this reduction? Doesn’t it suggest that the masking effects used in the experiments were easy to avoid? The approach should be evaluated more rigorously.

Response: Sorry to not give the significance of this reduction. To revise this comment, we compute the statistical significance (t-test) of this reduction. The result shows that the reduction is significant.

Additionally, the sentence we said in the paragraph “Aggregative for the five metrics” in Section 8.2.2. (Originally Section 6.2.2 in Page 24) i.e., “the possibility of triggering a masking effect is relatively small”, may mislead readers to that “the masking effects used in the experiments were easy to avoid”. We are very sorry to not make it articulate. In fact, what we mean is that by using “distinguishing failures”, the masking effects can be easily avoid. The reason is the same as what we have said in the response in Comment 22. To revise it, we construct two new subjects which offer more bugs and more masking effects. As a result, these two strategies, i.e., “distinguishing failures” and “regarded as one” has a similar performance when handling masking effects, and also, our approach ILP performed better than these approaches significantly.

**Comment 24:**

Furthermore, I strongly believe that all the experiment sections should be rewritten, as it is quite difficult to follow what is going on and to reason about the results.

Response: According to the comments, we have changed almost all the contents in the experiment, which includes adding new software subjects, removing all the synthesized software, showing more about how to create these subjects, update the results of the experiments and the corresponding discussions.

**Comment 25:**

Section 6.4 compares the proposed approach with FDA-CIT. However, the comparisons don’t seem to be fair. First, the authors should clearly mention that FDA-CIT’s primary concern is to avoid masking effects and give every t-tuple a fair chance to be tested, not to perform fault characterization and that it can work with non-deterministic failures and in the presence of inter-option constraints. In particular, FDA-CIT uses fault characterization as a tool to reach its goals and it can work with other fault characterization approaches (as also noted by the authors).

Response: According to this comment, we have emphasized the following three parts in our paper (See the fourth paragraph of Section 8.4 for more detail), i.e., 1) the main concern of FDA-CIT is to avoid masking effects when generate a covering array. 2) it can handle more practical problems, such as non-deterministic failures and inter-option constraints. 3) work with other fault characterization approaches.

**Comment 26:**

Second, in the experiments, FDA-CIT is used with t-way covering arrays, where 2 <= t <= 4. When working with failures caused by the interactions of t options, the strength of the covering arrays to be used with FDA-CIT should be at least (t+1). When a t-way covering array is used, very few instances of the failure-inducing t-tuple would appear in the array (only one instance in the worst case).  When a lower-strength covering array is used, the failure inducing t-tuple may not even appear in the array, which prevents the approach to even reveal the failure. The comparisons are not fair because although there were 6-way failures (Table XIX), the highest strength used was 4. It should have been at least 7. This can clearly be seen in Figures 5a-c, where, as t increased, FDA-CIT’s overall performance increased. Considering that the proposed approach required significantly more number of test cases than FDA-CIT (e.g., 172 vs. 626), the extra test cases required for the higher strength covering arrays may not affect the cost relationship much.

Response: Yes, as the reviewer said, it’s not fair to just use t-way covering array for FDA-CIT to identify the MFS. Hence, for each experiment we took, we increased the strength by 1 for FDA-CIT, i.e., (t+1)-way covering array and updated all the results for FDA-CIT. Additionally, we have reduce some redundant test cases generated by our approach ILP (For those failing test cases that contained the existed test cases, we do not necessary conduct the MFS identification again). As a result, the difference of the amount of test cases between our approach and FDA-CIT is very small.

**Comment 27:**

The paper should rigorously address the cost benefit tradeoffs. Assume that we are concerned with highly configurable systems where each factor corresponds to a configuration option. Then, what the paper refers to as a test case will correspond to a configuration. In such systems, a configuration is tested by running a battery of test cases in the configuration. For example, if we have 1000 test cases, given a configuration, we may want to execute all of these test cases in the configuration and determine the failure-inducing option setting combinations on a per test case basis. In such cases, however, the proposed approach needs to be carried separately for every test case (and even for every failure of a test case). That is, the number of configurations required by the proposed approach grows linearly with the number of test cases.  However, this is not the case for FDA-CIT as test cases share configurations in FDA-CIT. Considering that the number of test cases required by the proposed approach is already significantly higher than FDA-CIT, this may cause scalability issues in real world scenarios.

Response: This is a good comment for considering the practical application. Yes, with no more adaptive control, the number of configurations needed by our approach will grows linearly with the number of test cases. However, in practice, there is no need to go on executing the test cases under the configuration which contain the identified MFS. This is because, according to the definition of MFS, each configuration containing MFS will fail. Hence, we just need to execute those test cases under other configurations which does not contain identified MFS. In this paper, we do not discuss this because, our paper does not focus on the test case-aware testing environment, and also, we can use some adaptive ways to reduce such redundancy, but it is far beyond the point of this paper.

**Comment 28:**

More details should be provided about how the FDA-CIT approach was implemented. For example, the first paragraph on page 29 mentions about an “over fitting” problem. Were the classification trees computed by using n-fold cross validation to avoid over fitting as suggested by FDA-CIT? What was n? The same paragraph also suggests that every path from the root to a leaf node is considered as MFS. However, FDA-CIT only selects those paths (those MFSs) whose accuracy is above a given cutoff value. What was the cutoff value used in the experiments? How was this cutoff value selected?

Response: Sorry to that we do not give enough details about the implementation of FDA-CIT. Yes, our implementation of FDA-CIT use n-fold cross validation. The n is set by default value (Weka). Additionally, the cut-off value is 1 in our experiment, that is, only the accuracy is 100 % will be set as MFS, others are omitted. In fact, for the cutoff value, we have tried other values, but their performances are not as good as the one which we set 1. According to the comment, these details are added to our paper. What’s more, we have posted the implementation of FDA-CIT in the website, including the details of the setting and process to run it.

**Comment 29:**

It is good that the authors published the subject applications and the configurations used in the experiments online. However, it is not quite clear how to reproduce the experiments, e.g., how to configure these subject applications and how to run the test cases. Furthermore, the synthesized subject applications and the respective test cases are missing.

Response: According to this comment, we have added more files to explain how to run the experiment and to re-produce the outcomes. Furthermore, according to the comment of the third reviewer, we have removed all the synthesized subjects.  
  
**Other Minor issues:**

**Comment 30:**  
  
+ Combinatorial interaction testing is often abbreviated as CIT, not as CT.

Response: Actually there two abbreviated versions are both existed. Specifically, CT is short for combinatorial testing [1][2], CIT is short for Combinatorial Interaction Testing [3][4]. In this paper, they are uniformly cited as Combinatorial testing (CT). According to this comment, we have emphasized these two versions in the footnote at Page 2.

[1] Nie, Changhai, and Hareton Leung. "A survey of combinatorial testing." ACM Computing Surveys (CSUR) 43.2 (2011): 11.

[2] Kuhn, D. Richard, Raghu N. Kacker, and Yu Lei. Introduction to combinatorial testing. CRC press, 2013.

[3] Yilmaz, Cemal. "Test case-aware combinatorial interaction testing." Software Engineering, IEEE Transactions on 39.5 (2013): 684-706.

[4] Cohen, Myra B., Matthew B. Dwyer, and Jiangfan Shi. "Constructing interaction test suites for highly-configurable systems in the presence of constraints: A greedy approach." Software Engineering, IEEE Transactions on 34.5 (2008): 633-650.

**Comment 31:**

+ Second paragraph on page 14, the second “the left part” should be “the right part”

Response: Has been fixed

**Comment 32:**

+ Fifth paragraph on page 14, “an scenario” -> “a scenario”

Response: Has been fixed

**Comment 33:**

+ Second paragraph in Section 6.1.2, the sentence that starts with “In fact,” is incomplete.  
Response: We have rephrased this sentence.

At last, special thanks to you for your good comments.

**Responses to Referee: 2**

**Comment 1:**

The major problem of the paper is the treatment of randomness in the empirical studies. Three of the four empirical studies involve some level of randomness (random replacement for studies 2 and 3, generation of 2-4 way coverage). The authors compute the average of 30 tries, but don't report on notions like confidence interval. Although I am not a statistician, I am convinced that there is not sufficient analysis to provide statistical evidence. Also, in study 4, you are comparing average values; so you should probably do ANOVA analysis or something similar.

Response: We absolutely agree with the reviewer. The lack of statistical evidence made our original results not convincible. Hence, for each of experiment which contains randomness, for example, the randomized generated covering arrays, we have conducted t-test of the data. Then, the statistical significance value is given to determine whether the results of each experiment are reliable or not.  
  
**Detailed remarks**

**Comment 2:**  
  
Page 2 "We can get five two-way suspicious interactions..." five should be six!  
Response: Has been fixed.

**Comment 3:**

Page 6 : definition 3.3, you should not use k in v\_k\_1 or v\_k\_t because k is intended to represent the number of parameters that influence the SUT.

Response: It’s a good comment and very sorry to make this mistake. We have replaced them with v\_b\_1 and v\_b\_t in this paper.

**Comment 4:**

Page 6: Definition 3.5  
The definition should be clarified. It states that "all test cases ... trigger ... failure F". This is not the case with the example of Table II where only 4 test cases trigger Ex1. In most cases, the failure is not triggered by ALL test cases. So you should make clear which is the set where all test cases trigger a failure.

Response: We agree with the reviewer’s comment. The original definition is confusing and not clear. We have rephrased the definition of failure-causing schema (Definition 3.5): “If for any test case, as long as it contains the schema c, it will trigger a particular failure F. We call c the failure-causing schema of Failure F”.

**Comment 5:**

page 9: Proposition 3.11 Could you give a proof of this proposition? Probably I don't understand the term "antithesis" correctly, but as far as I understand it, the antithesis is the exact opposite of the thesis. So if both thesis and antithesis are true, everything is true!

Response: Sorry for the misleading. Here we tried to describe the fact that the Proposition 3.11 can be Inferenced by Inversely based on Proposition 3.10. However, the misuse of word “antithesis” make the sentence confusing. As suggested of this comment, we gave a clear proof of this proposition.

**Comment 6:**

page 11: typo in section 4.3 "and the other test cases (t2,t3) failed" should be "(t2,t4)".

Response: Has been fixed

**Comment 7:**  
  
page 12, first line "t1 and t3 should..." should be "t1 and t2 should..."

Response: Has been fixed

**Comment 8:**  
  
page 13 section 5.1, 4th paragraph, 2nd line "as it may not always BE possible..."  
 Response: Has been fixed

**Comment 9:**

page 14 line 7 "the left part" should be "the right part"

Response: Has been fixed.

**Comment 10:**  
  
page 16 section 5.2 "fixed part needed to be testED in each iteration"

Response: Has been fixed

**Comment 11:**  
  
page 17, Figure 2, The suspiciousness matrix is related to e2 and e3 and not e1 and e2.

Response: Has been fixed

**Comment 12:**

page 17, Figure 2. I don't understand the choice of t4'. Why don't you choose "00001221", which has a better suspiciousness score?

Response: It’s a very good point. In fact, “00001221” obviously has a better suspiciousness score. But in our approach, the choosing of each test case under testing should obey some rule. Generally, some parts of the original failing test case should not be changed in the latter generated test case. These parts are called “fixed part” as mentioned in this paper. As for t4, (0, -, -, -, -, -, -, -) is the fixed part, hence, the t4 should keep this part, and the remaining part can be any values, as long as it is different from the original failing test case t0. According to this, all the test cases that replace t4, i.e., t4’ and t4’’, are all keep this fixed part. As a result, we choose (0 2 2 2 2 1 1 2) instead of (00001221), even though (00001221) has a better suspiciousness score.

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**Comment 13:**  
  
page 18, table XVI, what is the "bug pairs", how did you choose these pairs, why are they not the same in the versions of the same program? (were they fixed with the new version?).

Response: Yes, the bugs in the older version were fixed. Here, “bug pairs” are the IDs of two bugs in the same version of a software. As we have added newly subjects which contain more than two bugs, hence in the revised paper we have removed the notion of “bug pairs”. Instead, we directly use the notion “bugs”. These bugs are collected in the bug tracker of the corresponding software.

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**Comment 14:**

page 19, section 6.1.2 and table XVIII. How many tests of the "failures" count do contain the MFS?  
From your text, it seems that all failures did contain the MFS, and that none of the tests did contain the second fault alone. It also seems that there are only two faults in each version.

Response: It is really true that each version of the original software just has two faults. It is also true that all the failures contain the MFS. But there are some tests contain the second fault alone. This is because, if there is no test case that can detect the second fault alone, then we cannot know there is another fault, and consequently we cannot know there exists masking effects. To be more precise, we have re-write the original Table XVIII in page 18 to be the new Table , which is also attached as the following. In this table, we use the expression (#n) to represent the nth bug, hence we can easily find how many test cases are triggered with the particular failure. In the masked column, we use the expression (#n -> #m) to directly show how many test cases which should trigged failure (#m) but be masked with failure (#n).

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| Software | Version | All tests | Failure | | Masking | Total |
| HSQLDB | 2.0rc8 | 18432 | #1 () | #2 () | #1 -> #2（） |  |
|  | 2.2.5 | 6912 | #1 () | #2 () | #1 -> #2（） |  |
| 2.2.9 | 6912 | #1 () | #2 () | #1 -> #2（） |  |
| JFlex | 1.4.1 | 36864 | #1 () | #2 () | #1 -> #2（） |  |
| 1.4.2 | 73728 | #1 () | #2 () | #1 -> #2（） |  |

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**Comment 15:**  
  
page 20 section 6.2 "we observed that the number of parameters...". On which case studies did you make these observations? the ones of table XVIII? Probably not the case studies of table XVIII as the largest one has 15 parameters (and not 30). So you should make more precise from which observations you deduced the number of parameters.

Response: It’s a good comment and sorry to make such a mistake. We try to say that the number of parameters are ranged from (8 to 15), but we somehow write it to be (8 to 30). In fact, all the synthesized subjects we created has the number of parameters ranged from 8 to 15. This number is based on the observation of the original table XVII. Additionally, as suggested by the third reviewer, we have removed all these synthesized ones.

**Comment 16:**  
  
page 22: last two lines testing object 2 has no best strategy, testing object 3 has distinguishing failures as best strategy. So the numbers do not correspond between this paragraph and figure 3.

Response: Yes, we agree with the reviewer and sorry to not describe the results correct. We have revise this sentence as suggested.

At last, we are grateful for your good comments and your affirmation.  
  
  
**Responses to Referee: 3**

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**Comment 1:**

The improvement over simpler strategies is not substantial. For example, using the suspiciousness score only reduces 1-2 test cases on average compared to using a random selection approach. Given the overhead of computing and maintaining the suspiciousness scores, random selection may be a more cost-effective solution. The benefits of using mutation and ILP do not seem to go beyond the very simple distinguish-failure strategy much, which simply does not consider the failing cases that do not belong to the same fault. The two have almost the same performance for 4/5 metrics in Fig. 3. Even for the metric that the proposed method shows benefits, I wonder if one can easily improve the performance of the distinguish-failure strategy by having more test cases.

Response: It’s really true that in our original experiment, it does not show significant advantages of our approach over those simpler strategies. The reason is that the masking effects in our original subjects are very simple. In fact, almost all the subjects just have two bugs with one can mask another. This condition is favorable for the simple distinguish-failure strategy. For example, assume bug A masks bug B; then when we identify the MFS of bug A, the distinguishing-failures strategy is the correct strategy, as if there is a test case trigger the bug B, then it must not trigger the bug A (otherwise, bug B will not be triggered). Hence, there is a probability that is up to 50% that distinguishing-failure strategy makes the correct operation. To make the comparison fairer, we have added some new subjects with more bugs to make the masking effects more complex. As a result, our approach ILP perform obtain a relatively significantly better result than the distinguishing-failures strategy (See Table ).

On the other hand, as the newly added subjects contain more bugs, consequently, the masking effects is not that easy to avoid. Then our approach ILP reduces more than just 1 to 2 test cases than the random approach. What’s more, the statistical analysis value shows that improvement is not trivial (See Table ).

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**Comment 2:**

The evaluation can be improved. It is currently evaluated on only 2 programs with 5 versions, each version having two bugs. I wonder why not collect more bugs for each version and have more programs (and versions)? I would rather see the author’s trade the current space used in the over-detailed experiment set-up for more programs and bugs. The synthetic programs are not that useful.

Response: We absolutely agree with the reviewer. More real subjects with more bugs can make the evaluation of our approach more general. Hence, we collect more new subjects with more bugs. What’s more, we have improved the description of how to set-up these subjects, so that the re-produce of these experiments is easier. Additionally, we have removed some of the synthetic programs in the paper as suggested.

**Comment 3:**

It is unclear what is the termination condition of this failure inducing interaction identification process. The paper seems to indicate that it terminates when the MFS is computed. But from my point of view, the computation of MFS is determined by the test suite you have. This seems to make it a chicken-and-egg problem.

Response: We are sorry to not properly describe the end-condition of our approach. In fact, the failure-inducing interaction identification is the process to filter those interactions that are not failure-inducing. To filter those interactions, we need to find some passing test cases that contain them. Then ending condition, hence, is that no more interaction in the original failing test case can be filtered. Besides this, a formal description of this ending condition is to generate and execute test cases until Lemma 3.13 (See the newly added Section 3.3) is satisfied.

According to the comment, we have enhanced the description of this ending condition in the Section 6.1.

**Comment 4:**

Part of the technique hinges on properly classifying failures, which is a hard challenge in general. The authors should discuss how they achieve this.

Response: Yes, it’s true that our approach is based on the failures classification, and it is a very hard challenge. However, in this paper, our key point is to discuss the impacts of different failures on MFS identification. Hence, we do not focus on how to achieve this. In fact, in these experiments, we just simply treat these bugs with the same exception traces as the same failure, others as different. More complex techniques to handle this problem, such use the clustering techniques to classify the failures according to available information [1][2][3], are far beyond of this paper and not be discussed

[1] Zheng, Alice X., et al. "Statistical debugging: simultaneous identification of multiple bugs." Proceedings of the 23rd international conference on Machine learning. ACM, 2006.

[2] Podgurski, Andy, et al. "Automated support for classifying software failure reports." Software Engineering, 2003. Proceedings. 25th International Conference on. IEEE, 2003.

[3] Jones, James A., James F. Bowring, and Mary Jean Harrold. "Debugging in parallel." Proceedings of the 2007 international symposium on Software testing and analysis. ACM, 2007.

**Comment 5:**

I don't understand why ILP is needed. A simple linear search algorithm shall do the work. Please explain.

Response: It’s really true that a simpler linear search algorithm can also be applied in our approach to find a proper test case. However, as this problem to search a proper test case is related to Integer (a test case consists of discrete parameters with discrete values), we believe using Integer Linear Programming (ILP) technique is more appropriate. As suggested, we have emphasized this point in Section 5.1 at Page 18 .

**Comment 6:**

The paper still contains a lot of grammatical problems. It has to go through very rigorous proof-reading.

Response: We agree with this comment. We have re-vised all these grammatical problems and repeatedly checked the paper.

**Specifics**

**Comment 7:**  
  
Abstract: "theory lack"=> "theory lacks"

Response: Has been fixed.

**Comment 8:**

page 3: "newly regenerated" => "newly generated"

Response: Has been fixed.

**Comment 9:**

page 3: what is a factor?

Response: A factor is a parameter value in a test case (Or a schema).

**Comment 10:**

page 3: "suffered multiple failures" => "encountered multiple failures"

Response: Has been fixed.

**Comment 11:**

page 3: "import masking effects" => "induce masking effects"  
Response: Has been fixed.

**Comment 12:**

Table II, there seems to be a soundness issue here. For ex2, it is possible that the programs just fails with  
<7,2,4,5> and <11,2,4,5> but not <5,2,4,5>. But know the technique seems to think that <-,2,4,5> would fail  
the program. How can your technique address this in general?

Response: In this motivation example, as the first parameter va just has two possible values, i.e., 7 or 11. Hence if this program fails with <7,2,4,5> and <11,2,4,5> for ex2, then <-, 2, 4, 5> must be a failure-inducing schema for ex2. On the other hand, if va has the third value 5, and <5, 2, 4, 5> does not fail, then we can just determine <7,2,4,5> and <11,2,4,5> are the failure-inducing schemas for ex2, not the schema <-, 2, 4, 5>. The determination of a schema to be a failure-inducing schema is just to find that if any test case, as long as contain this schema, will fail with one failure.

**Comment 13:**

page 7: "L: The number of failures ...". You should distinguish faults and failures (through out the paper).  
They have separate meaning in software engineering but the paper simply uses failures in all cases.

Response: We have revised them throughout the paper. Additionally, we emphasize the relationship between the notion failure and fault in the first paragraph at Page 6 and the last paragraph (the sentence under "L: The number of faults ...".) of Section 3.1 at Page 7, so that these two words will not confuse readers.

**Comment 14:**

page 7: "T(c)<=T\_Fm", should it not be "T(c) >=T\_Fm"?

Response: No, as T\_Fm are all the test cases that fail with Fm, and c is just one MFS of Fm, hence T(c) <= Fm. This is because there may be multiple MFS for Fm. For the following example (2^3), T\_F1 = (0, 0, 0), (0, 0, 1), (0, 1, 0). Two MFS are (0, 0, -) and (0, -, 0). Then we can find that T\_F1 > T ( (0, 0, -) ), which are (0, 0, 0), (0, 0, 1). According to this comment, we have emphasized this point.

|  |  |
| --- | --- |
| Test case | Outcome |
| (0, 0, 0) | F1 |
| (0, 0, 1) | F1 |
| (0, 1, 0) | F1 |
| (0, 1, 1) | Pass |
| (1, 0, 0) | Pass |
| (1, 0, 1) | Pass |
| (1, 1, 0) | Pass |
| (1, 1, 1) | Pass |
| MFS for F1 | |
| (0, 0, -) | (0, -, 0) |

**Comment 15:**

pages 8-9: The proofs are not that useful. They are quite obvious.

Response: We have removed some proof that is obvious.

**Comment 16:**

page 9: "impacts of masking..." => "impact of masking", the same problem occurs a few times.

Response: Has been fixed.

**Comment 17:**

page 10: "one failure-fail"=> "one-fault failure"

Response: Has been fixed.

**Comment 18:**

page 11: "significantly impact on" => "has significant impact on", the same occurs a few times.

Response: Has been fixed.

**Comment 19:**

page 11: "We offer..." => "Consider"

Response: Has been fixed.

**Comment 20:**

page 11: "The pass of..." => "The passings"

Response: Has been fixed.

**Comment 21:**

page 12: "In other word",=> "In other words", the same happens a few times

Response: Has been fixed, all the "In other word" are replaced with "In other words".

**Comment 22:**

page 14: "between test case" => "between test cases"

Response: Has been fixed.

**Comment 23:**

page 14: "triggers other failure"=> "triggers other failures"

Response: Has been fixed.

**Comment 24:**

page 14: what do you mean by "the maximal possible failure"?

Response: According to this comment, we recognize the term “maximal possible failure” is confusing, hence, we changed it to be “the most likely fault”. For example, in table XIII in page 17 (the new version paper we submit) for test case t3, F2 is such a failure, as the suspiciousness between t3 and F2 is higher than F3.

**Comment 25:**

page 14: "the corresponding failure", do you mean "all other failures" here?

Response: No. Here we mean the “most likely fault it can trigger” as discussed in the comment 24. We have rephrased this in the paper.

**Comment 26:**

page 16: "number of attempts is"

Response: We have rephrased it to be “the number of attempts to find a proper test case is” .

**Comment 27:**

page 19: "SUT have" => "SUTs have"

Response: Has been fixed.

**Comment 28:**

The observations in 6.1 are not new. So you are not gaining much out of this experiment

Response: Yes, as the reviewer said, the observation in Section 6.1 (Now Section 8.1) is similar to the existing study [1]. However, there is an essential difference. As we stated in the fourth paragraph of Section 8.1.2. The main difference between that work and ours is the way that the masking effects is quantified. In that work, the masking effect is the number of τ-degree schemas that only appear in the test cases that triggered other failures, where, the τ-degree schemas can be either MFS or not. Our work, however, quantifies the masking effects as the number of test cases that are masked by different failures. These test cases should contain some MFS, i.e., they should have triggered the expected failure if they did not trigger any other failure. As a result, the observation that “masking effects exist widely” does not has the same meaning of the two studies. Hence, we believe that the observation in Section 6.1 (Now Section 8.1) is new to some extent.

[1] Yilmaz, Cemal, et al. "Reducing Masking Effects in Combinatorial Interaction Testing: A Feedback Driven Adaptive Approach." Software Engineering, IEEE Transactions on 40.1 (2014): 43-66.

**Comment 29:**

page 20: "can happened"

Has been fixed to be “can be”.

**Comment 30:**

page 24: what do you mean by "One issue is the redundancy...", please rephrase.

Response: We have rephrased that sentence to be “We believe this is an improvement, because too many sub or super schemas in fact point to the same actual MFS, which results in duplication and makes it hard to identify the actual MFS.”.

**Comment 31:**

page 26: "produce the generated..."

Response: Has been rephrased to be “guarantee that the generated test cases should cover all the…”.

**Comment 32:**

page 32: "of constrains"

Response: Has been fixed, and changed to be “constraints”.

**Comment 33:**

page 32: "covering array with considering" => "covering array while considering"

Response: Has been fixed.

**Comment 34:**

page 32: "this constraint" => "these constraints"

Response: Has been fixed.

**Comment 35:**

page 32: "First, the work that aims..", not a sentence.

Response: We have rephrased it to be “First, we discuss the works that aim to identifying the MFS in the SUT”.

At last, we are appreciated to your comments, which are very useful to improve the quality of this paper.