Statistical Errors in Software Engineering Experiments: A Preliminary Literature Review

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ABSTRACT

Background: Statistical concepts and techniques are often applied incorrectly, even in mature disciplines such as medicine or psychology. Surprisingly, there are very few works that study statistical problems in software engineering (SE). ***Aim:*** Assess the existence of statistical errors in SE experiments. ***Method:*** Compile the most common statistical errors in experimental disciplines. Survey experiments published in ICSE to assess whether errors occur in high quality SE publications. ***Results:*** The same errors as identified in others disciplines were found in ICSE experiments, where 30% of the reviewed papers included several error types such as: a) missing statistical hypotheses, b) missing sample size calculation, c) failure to assess statistical test assumptions, and d) uncorrected multiple testing. This rather large error rate is greater for research papers where experiments are confined to the validation section. The origin of the errors can be traced back to: a) researchers not having sufficient statistical training, and, b) a profusion of exploratory research. ***Conclusions:*** This paper provides preliminary evidence that SE research suffers from the same statistical problems as other experimental disciplines. However, the SE community appears to be unaware of any shortcomings in its experiments, whereas other disciplines work hard to avoid these threats. Further research is necessary to find the underlying causes and set up corrective measures, but there are some potentially effective actions and are a priori easy to implement: a) improve the statistical training of SE researchers, and b) enforce quality assessment and reporting guidelines in SE publications.

CCS CONCEPTS

• **General and reference → Surveys and overviews**;

KEYWORDS

Literature review, Survey, Prevalence, Statistical errors

ICSE '18, May 27-June 3, 2018, Gothenburg, Sweden

ACM ISBN 978-1-4503-5638-1/18/05…$15.00
https://doi.org/10.1145/3180155.3180161

1 INTRODUCTION

Experimentation makes extensive use of statistics. Several studies warn about the existence of scientific articles using inappropriate statistical procedures [5, 32, 62]. This happens even in mature disciplines, such as the health sciences [6].

In turn, there are very few papers studying statistical errors in software engineering (SE) articles. There are papers discussing statistical power [23], heterogeneity in meta-analysis [52], and the relative strengths and weaknesses of cross-over designs [41, 81] in SE. This stands in contrast to other disciplines where papers warning about problems in simple statistical concepts such as hypothesis statement [16, 58], interpretation of p-values [62], sample size calculation [2, 25], significance levels [58], etc., are quite common.

We aim to assess the prevalence of these problems in the SE literature. We have compiled the most common statistical errors in experimental disciplines and surveyed empirical papers published in ICSE between 2006 and 2015, to check whether or not these papers are subject to the compiled errors. Our results suggest that SE experiments have the same weaknesses as in other sciences. SE researchers do not use relatively simple concepts like hypothesis statement, sample size estimation, inference, and post-hoc testing correctly. These problems seem to be related to poor statistical training, and the use of exploratory research.

Our contributions confirm the shortcomings in experimental SE research and identify their origin. In our opinion, the SE community should improve researchers' statistical training and, more importantly, establish mechanisms (e.g., quality assessment tools, reporting guidelines) to identify and correct statistical problems in SE experiments before they are published.

The structure of this paper is as follows. Section 2 provides background on the topic of statistical errors in science and SE. Section 3 presents a short literature review identifying several statistical errors. We screen articles reporting experiments for a subset of the above errors in Section 4. The origin of the identified

errors is evaluated in Section 5. Section 6 offers a critical appraisal of this. Finally, the conclusions are reported in Section 7.

2 BACKGROUND

2.1 Statistical Errors in Experimental Disciplines

Scientific and engineering researchers apply statistical techniques to analyze and interpret many of their research results. Hence, statistical techniques have experienced an increase in use, particularly in medicine [2, 63, 83], psychology [5], education [21], and social science [25, 58].

There is a relatively large collection of publications that provide information about the existence of statistical problems in virtually all disciplines. Not all publications are recent; they have been available since the widespread adoption of experimental research in their respective areas. The reported problems have a broad scope [51], including: the definition of statistical hypotheses [16, 58], interpretation of p-values [62], sample size calculation [2, 25], significance levels [58], and confidence intervals [16], and so on.

Papers about statistical shortcomings in other disciplines have derived their results from some type of literature review of primary studies from one or more specialized journals. Their conclusions are surprising and worrying since they report high error rates:

- Welch [83] studied 145 articles from one of the most renowned medical journals, the *American Journal of Obstetrics and Gynecology*, and found that 52.6% of the articles contained inadequate or incomplete statistical descriptions.
- Bakker [5] evaluated 218 articles from high and low impact psychology journals. The author reported that low impact journals exhibit statistical inconsistencies more frequently than high impact journals. Bakker determined that about 15% of all the papers from both high and low impact journals have at least one incorrect statistical conclusion.
- Ercan et al. [25] evaluated 164 and 145 articules in psychiatry and obstetrics, respectively. Of the psychiatric and obstetrics publications, 40% and 19%, respectively, contained mistakes regarding: sampling, sample size calculation, and contradictory interpretations of inferential tests.
- Kilkenny et al. [39] assessed the experimental design of 271 papers published in *Medline* and *EMBASE* from 2003 to 2005. More than 60% of the papers are subject to biases during the assembly of the study cohort, weak statistical analysis, missing information, etc.

The origin of the statistical errors can be traced back to several causes:

- According to Castro et al. [75], the analysis and interpretation of empirical results in any scientific discipline depend primarily on how well researchers understand inferential statistics. The authors suggested that researchers in the education community, especially PhD students, are prone to misconceptions, particularly when they are using abstract statistical concepts, such as confidence intervals, sampling distributions with small numbers, sampling variability, different types of distributions, and hypothesis tests.
- Cohen et al. [19] conducted an empirical study with degree students. They found that students lack statistical knowledge, which leads to the misinterpretation of statistical concepts and biased judgements.
- Brewer [12] evaluated 18 statistical handbooks by renowned publishers, e.g., *Academic Press, Addison-Wesley, McGraw-Hill, Prentice-Hall, John Wiley*, etc. These books contained inaccurate statements in topics such as sampling distributions, hypothesis testing, and confidence levels.

2.2 Statistical Errors in SE

The SE community apparently has limited awareness of the existence and impact of statistical shortcomings in its publications. When we searched for SE papers related to statistical problems, the only results were: Dybå et al.'s paper regarding statistical power [23], Miller's paper on meta-analysis [52], and two papers by Kitchenham [41] and Vegas et al. [81] that focused on within-subject designs.

Several other papers discuss specific statistical issues. For instance, Kitchenham introduced robust statistical methods [42], while Arcuri and Briand discussed statistical tests for the assessment of randomized algorithms [4]. These papers do not assess the weaknesses in current research. They suggest opportunities for improvement in the toolset that SE researchers currently use.

There is a manifest difference between SE and other experimental disciplines regarding statistical errors. In medicine and other sciences, statistical problems are routinely identified in publications; this issue is almost completely overlooked in SE.

Other disciplines have not addressed statistical defects and methodological problems until relatively late on. For instance, while the first formal randomized clinical trial in medicine was conducted in the 1940s [8], the first publication about statistical defects in this field that we are aware of was published in the 1970s [30]. Given that SE is only just adopting experimental methods and the associated statistical techniques, its failure to pay attention to the assessment of statistical issues should come as no surprise.

This paper reports an exploratory study aiming to answer the following research questions:

RQ1: What are the most common problems associated with the use of experimental procedures in experimental disciplines?

RQ2: What is the rate of statistical errors in SE research?

3 STATISTICAL ERRORS IN EXPERIMENTAL DISCIPLINES

3.1 Review Strategy

To answer RQ1, we reviewed several specialized books published on the topic, such as Good et al. [29], Vickers [82], and Huck [34]. These books provide a good starting point for our exploratory study because they are not related to any specific discipline (although there is some bias toward the health sciences) and they focus on serious errors often inspired by real research.

3.2 Collected Data

Two researchers (O. Dieste and R. P. Reyes) reviewed the above three books. They found 93 text sections clearly pointing to some type of error that can be frequently found in the literature. Discrepancies were solved by consensus. The complete listing of paragraphs is available at https://goo.gl/8zb9LU, including links to the reference books and related literature.

3.3 Analysis Method

We applied thematic synthesis to classify the statistical errors. We applied the guidelines by Creswell [21] and Cruzes et al. [22] to avoid bias and achieve methodological rigor in the synthesis and interpretation of results [11]. The analysis consisted of two stages: coding and theme definition. It was conducted by the same two researchers than collected the data.

During the coding stage, both researchers independently assigned low-level codes to each text section, which were later reviewed and harmonized. We created 93 different codes. During the theme definition stage, codes were grouped together by means of higher-level codes. This procedure was aligned with our purposes since the high-level themes represent error-prone areas. Both researchers worked collaboratively. They organized the low-level concepts into high-level themes according to a directed graph, shown in Fig. 1. Themes and connections between themes and concepts are available at https://goo.gl/8zb9LU.

Nodes represent categories of statistical errors. Categories become progressively more abstract as the tree is traversed from right to left. For instance, the node *study design* (Fig. 1, bottom left) is connected to the nodes *assignment* and *sampling*. This means that the high-level category *study design* contains two types of errors: *assignment* and *sampling* errors. Likewise, *assignment* splits into further lower-level error types, such as *matching*, *randomization*, etc. Notice that Fig. 1 shows only one subset of the error types that we have identified to keep the graph within page limits. The full graph is available at https://goo.gl/qovXQw.

There may be multiple connections between codes and high-level themes and between one high-level theme and another because they are mentioned in several books, or more than once in the same book in different contexts. For instance, *randomization* is discussed twice in terms of the representativeness of the random samples:

> (item #40) *Misconception: If a truly random process is used to select a sample from a population, the resulting sample will turn out to be just like the population, but smaller.* [34, pp. 123]

> (item #41) *Misconception: A sample of individuals drawn from a larger, finite group of people deserves to be called a random sample so long as (1) everyone in the larger group has an equal chance of receiving an invitation to participate in the study and (2) random replacements are found for any of the initial invitees who decline to be involved.* [34, pp. 127]

and once again with regard to the equivalence of experimental groups formed by random assignment:

> (item #90) *The idea behind randomization is to make the groups as similar as possible [...]. Baseline differences at the beginning of the trial, such as in age o gender, are due to chance. [...] giving a p-value for baseline difference between groups created by randomization is testing a null hypothesis that we know to be true.* [82, pp. 100]

These repeated associations are an indication of relevance, and thus the arcs connecting the corresponding nodes have been made proportionally wider. The number next to the arc indicates the number of times that the connection appears in the raw data. Dotted lines represent connections that appear just once.

3.4 Review Results

Statistical errors can be classified in to three groups: a) experimentation, b) meta-analysis, and c) prediction. Most errors are related to experimentation. Nevertheless, it is noticeable that meta-analysis appears three times in connection with subgroup analysis and the combination of studies with different designs. Prediction appears just once, with respect to with linear modeling. In what follows, we focus on problems associated exclusively with experiments.

Analysis is the experimentation facet most often mentioned in connection with statistical errors. In the three reviewed books, analysis errors appear 63 times. There are two main sources of problems with analysis: the application of inferential techniques and the interpretation of results:

- The inferential techniques most often used during experimental data analysis are classical tests, such as t-tests, and their related concepts, such as p-values and tails. Researchers often make wrong assumptions about the tests (e.g., robustness of t-test), and they select tests in circumstances in which they cannot be applied (e.g., ordered alternative hypotheses) or are sub-optimal (e.g., low power tests). All common tests, including t-tests, correlations, and ANOVA, are mentioned in this context.
- Another frequently mentioned inferential technique is linear modeling; multiple linear regression is the best known example of linear modeling. The most frequently mentioned problem is the rationale behind the definition of the linear model. Other issues, such as the violation of assumptions and usage beyond limits (e.g., outside the linear phase), are also reported.
- Many supposedly basic concepts, such as confidence intervals, statistical significance, or p-values are frequently misinterpreted.

Study design is second to analysis. This concept includes methodological issues connected to the management of experimental units, such as sampling and assignment. In both cases, the sources of the problems are inappropriate or missing randomization and sample size calculation.

Reporting is another troublesome issue, which is mentioned the same number of times (ten) as study design. There are many sources of reporting defects (e.g., overlooking experimental incidents or multiple testing), although the absence of descriptive statistics (e.g., means) is emphasized (tree times) in the reviewed books.

The last prominent issue is goal definition. Researchers often do not state statistical hypotheses. Failure to explicitly define *null* hypotheses appears three different times in Fig. 1.

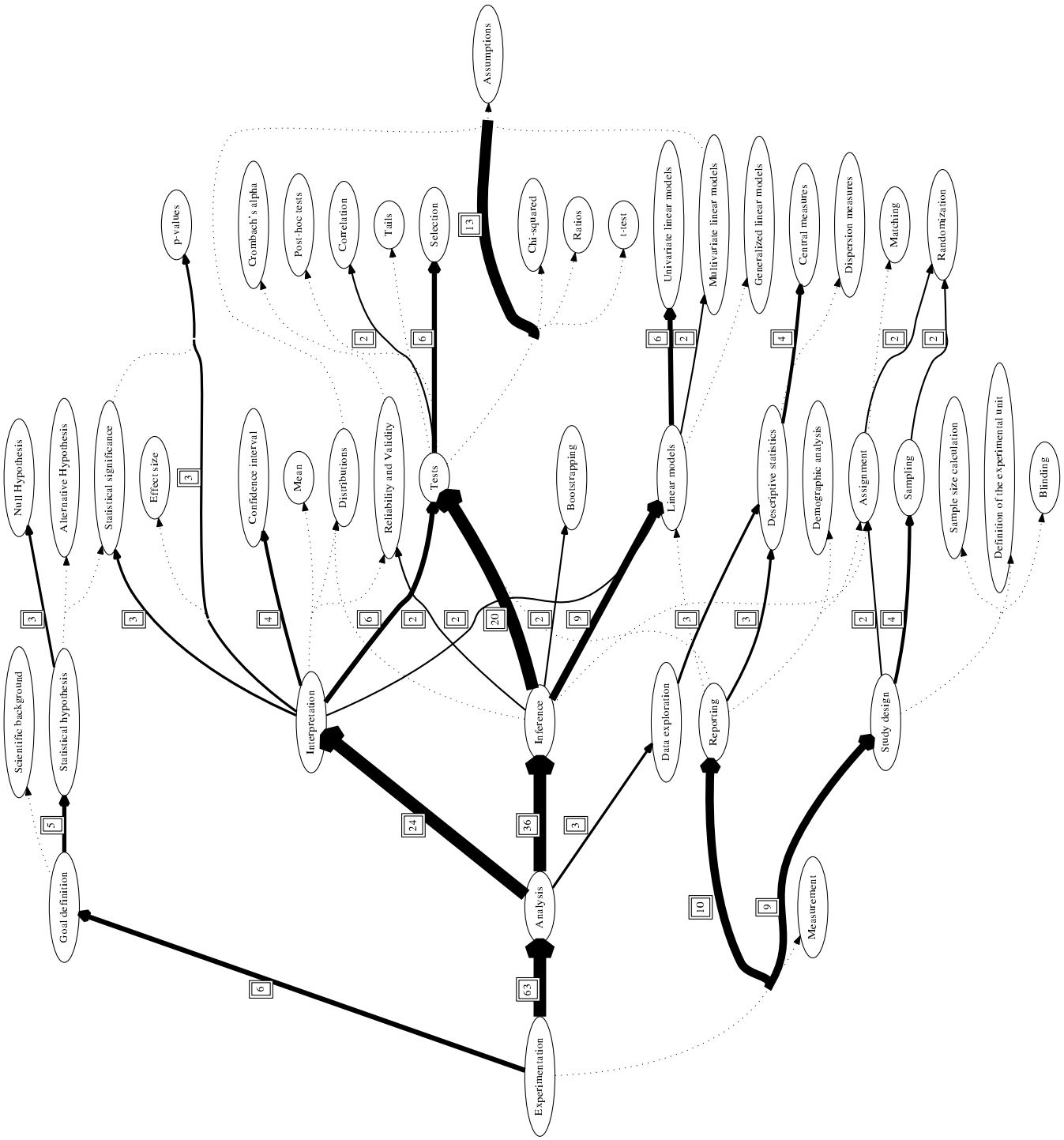


Figure 1: Classification of statistical errors in experimental research papers

4 STATISTICAL ERRORS IN ICSE EXPERIMENTS

The aim of RQ2 is to find out the rate of statistical errors in SE research. To answer RQ2, we evaluated the experiments published in ICSE over 10 years (2006-2015). ICSE is SE's flagship conference. Our evaluation should identify the rate of common statistical errors in the best SE research; the situation of lower quality SE research is likely to be worse.

4.1 Evaluation Instrument

The complete list of statistical errors that we have compiled contains almost 100 items. Since statistical errors are ubiquitous in the general research literature, it is highly likely that several of the ~ 100 problem types would appear in virtually any SE paper as well. Therefore, an exhaustive review of SE experiments would draw a too pessimistic picture of our field.

We have focused on recurrent types of errors (denoted by wide arrows in Fig. 1. For instance, null hypothesis -related problems are referenced multiple times in Fig. 1, as well as test assumptions or central measures. We have selected the most error-prone statistical concepts, developed appropriate questions, and created the 10-question checklist shown in Table 1. All these questions can be easily traced back to Fig. 1 (or the online version at https://goo.gl/qovXQw). Further clarification is required at this point:

- Q1.1 and Q1.2 may look outdated due to the increasing criticisms of the null-hypothesis and significance testing (NHST), and the recommendations to adopt other statistical approaches such as confidence intervals and effect size indices [10, 71, 77]. However, SE research has not yet taken up these recommendations. For instance, only four out of 21 experiments published in ICSE from 2006 to 2015 report any measure of effect size, and two out of 21 refer to confidence intervals. Nowadays, NHST is still the main statistical approach used in SE.
- Q4 (*Have subjects been randomly assigned to treatments?*) may not be applicable to some types of experiments, e.g., when two defect prediction algorithms are applied to the same code, that is, matched pairs or similar designs. In such cases, the question is answered as N/A. A similar strategy is applied for any question that does not make sense for a given experiment, e.g., Q5 (*Have the test assumptions (i.e., normality and heteroskedasticity) been checked or, at least, discussed?*) when an experiment does not use statistical tests.
- Test assumptions vary from test to test. In many cases, reference books state incomplete or even questionable assumptions. Thus, in Q5 (*Have the test assumptions (i.e., normality and heteroskedasticity) been checked or, at least, discussed?*), we will pay attention only to the most usual conditions (normality and heteroskedasticity) that have to be examined before applying virtually any parametric test.
- Q7 (*Have the analysis results been interpreted with reference to applicable statistical concepts, such as p-values, confidence intervals, and power?*) would appear to be a rather crucial question. Fig. 1 shows that the node *interpretation* is connected by wide arcs with nodes representing relatively simple statistical concepts, such as power, confidence interval, p-value, and so on. However, we doubt that we can answer this question objectively. While authors typically discuss their results at length, they may simplify or omit some statistical issues required to clearly transmit their message to readers. Thus, we face the risk of making mistakes, e.g., evaluating Q7 negatively due to incomplete reporting. We decided to skip this question (and there is a line through it in Table 1).
- Multiple testing does not appear to be a key issue in Fig. 1. However, it was cited three times as a source of problems during both analysis and reporting; note that there are three incoming arcs for this node in Fig. 1. This justifies Q9 (*Is multiple testing, e.g. Bonferroni correction, reported and accounted for?*).
- Q10 (*Are descriptive statistics, such as means and counts, reported?*) is important for both analysis and reporting. We consider this issue in the context of reporting only, so as not to inflate the number of defects found.

Table 1: Evaluation checklist

#	Question
Q1.1	Are null hypotheses explicitly defined?
Q1.2	Are alternative hypotheses explicitly defined?
Q2	Has the required sample size been calculated?
Q3	Have subjects been randomly selected?
Q4	Have subjects been randomly assigned to treatments?
Q5	Have the test assumptions (i.e., normality and heteroskedasticity) been checked or, at least, discussed?
Q6	Has the definition of linear models been discussed?
~~Q7~~	~~Have the analysis results been interpreted by making reference to relevant statistical concepts, such as p-values, confidence intervals, and power?~~
Q8	Do researchers avoid calculating and discussing post hoc power ?
Q9	Is multiple testing, e.g. Bonferroni correction, reported and accounted for, ?
Q10	Are descriptive statistics, such as means and counts, reported?

4.2 Target studies

Our original aim was to survey only ICSE experimental papers from 2006 to 2015. However, the decision soon proved to be questionable. We conducted a pilot study on ICSE 2012 edition to check the feasibility of our study. We immediately realized that the number of fully-fledged experiments was quite low: we found only four experiments. In turn, we found many small-scale experiments aimed at evaluating the properties of new techniques or methods, typically reported in research paper Evaluation Sections. To be precise, we identified 16 experiments as evaluations (18.4% of the total number of papers in ICSE 2012).

The question was whether the survey should be extended to experiments as evaluations, or restricted to standalone experiments. Experiments as evaluations often apply an experimental methodology, but are typically only one to three pages long. The compressed reporting format may lead to writing practices that may be misconceived as statistical errors by reviewers. On the other hand, experiments as evaluations account for a large share of empirical research, and the results of this survey would be incomplete if they were overlooked.

We decided to evaluate both types of studies separately. In a first stage, we searched for all standalone experiments published in ICSE from 2006 to 2015. We found 21 a total of papers. We then collected a similar number[1] of experiments as evaluations to avoid over-representation.

4.3 Study selection

Two researchers (O. Dieste, E. R. Fonseca, and R. P. Reyes) worked separately to screen the tables of contents of the ICSE 2006-2015 Technical Tracks. They reviewed the title and abstract for any indication that the paper reported an experiment. If in doubt, they examined the full text in search of further evidence of at least two treatments being compared, that is, the minimum condition to be met by any experiment.

The total number of papers and the papers pre-selected after screening are shown in Table 2. The pre-selection agreement was calculated using Fleiss' κ, as recommended by K. L. Gwet [31, pp. 52]. $\kappa = 0.45$, typically considered as *moderate* [27]. This implies that we may have failed to identify some experiments. Note that it is not straightforward to identify experiments using metadata, such as titles and abstracts, due to missing methodological descriptors.

Three researchers (O. Dieste, E. R. Fonseca, and R. P. Reyes) individually reviewed the pre-selected papers and classified them into the *experiment* and *non-experiment* categories. Disagreement was resolved by consensus. A total of 21 papers were classified as standalone experiments. This represents 2.7% of the total number of papers published in ICSE. Previous research has already pointed out the low number of controlled experiments published in ICSE [91]. The agreement level for this step of the selection process was Fleiss' $\kappa = 0.52$, typically considered as *moderate* [27, 31]. As reported below, this low agreement is due to the existence of missing information (e.g., hypotheses or randomization procedures) in the manuscripts. Further details are available at https://goo.gl/jHWpq3.

Table 2: Summary of the selection process. Experiments as evaluations between parentheses

Year	Total papers (TP)	After screening	Selected	%
2006	72	8	2 (3)	2.8% (4.1%)
2007	64	7	2 (3)	3.1% (4.7%)
2008	85	8	1 (3)	1.2% (3.5%)
2009	70	7	0 (3)	0.0% (4.3%)
2010	62	5	1 (3)	1.6% (4.9%)
2011	62	5	1 (3)	1.6% (4.9%)
2012	87	31	4 (3)	4.6% (3.5%)
2013	85	8	1 (3)	1.2% (3.5%)
2014	99	11	5 (3)	5.0% (3.1%)
2015	84	11	4 (3)	4.8% (3.5%)
Total	770	101	21 (30)	2.7% (3.9%)

Finally, three ICSE experiments as evaluations per year were selected at random from the tables of contents of the ICSE Technical Track. The three researchers independently reviewed these papers, and discrepancies were resolved by consensus. The process was repeated until three experiments as evaluations had been identified for each ICSE conference from 2006 to 2015.

[1]We rounded up from 21 to 30, i.e., three papers per edition × 10 ICSE years = 30 papers.

4.4 Execution

The three researchers individually evaluated all papers and gave a *yes/no/not applicable* answer to each checklist question (see Table 7). The level of agreement was *substantial* to *almost perfect* in many cases, which increases the reliability of our results. Details of the evaluation are available at https://goo.gl/3iy9eL (standalone experiments) and https://goo.gl/qCboSX (experiments as evaluations).

Table 3: Agreement levels per question

		Standalone Exp.		Exp. as Eval. Sec.	
Stage		κ	**Agree**	κ	**Agree**
Goal definition	Q1.1	0.839	Almost perfect	0.643	Substantial
	Q1.2	0.746	Substantial	0.788	Substantial
Study design	Q2	1,000	Perfect	1.000	Perfect
	Q3	0.092	Slight	0.389	Fair
	Q4	0.541	Moderate	0.585	Moderate
Analysis	Q5	0.752	Substantial	0.662	Substantial
	Q6	1.000	Perfect	0.558	Moderate
	Q8	0.894	Almost perfect	0.803	Almost perfect
Reporting	Q9	0.592	Moderate	0.659	Substantial
	Q10	1.000	Perfect	0.480	Moderate

4.5 Survey Results

Table 4 summarizes the survey results. Percentages are calculated as $\frac{\{Yes|No|N/A\}}{9}$. The No column represents the percentage of papers in the sample that are affected by the error indicated by the corresponding question, i.e., the prevalence of the statistical error rate. Q1 was split into two parts to differentiate the problems related to the null (Q1.1) and the alternative (Q1.2) hypotheses.

Table 4: Defect rates

		Standalone Experiments			Experiments as *Evaluation Sections*		
Stage		Yes	No	N/A	Yes	No	N/A
Goal definition	Q1.1	66.7%	33.3%	0.0%	13.3%	83.3%	3.3%
	Q1.2	57.1%	42.9%	0.0%	6.7%	90.0%	3.3%
Study design	Q2	0.0%	100.0%	0.0%	3.3%	96.7%	0.0%
	Q3	28.6%	71.4%	0.0%	13.3%	86.7%	0.0%
	Q4	66.7%	28.6%	4.76%	20.0%	0.0%	80.0%
Analysis	Q5	61.9%	33.3%	4.76%	13.3%	20.0%	66.7%
	Q6	4.8%	0.0%	95.24%	3.3%	0.0%	96.7%
	Q8	85.7%	9.5%	4.76%	36.7%	0.0%	63.3%
Reporting	Q9	9.5%	71.4%	19.07%	3.3%	26.7%	70.0%
	Q10	95.2%	0.0%	4.76%	76.7%	13.3%	10.0%

We found clear evidence of the existence of statistical errors in ICSE papers. The rate of the different errors varies, but it is rather large in many cases, e.g., Q1, Q2, Q3 and Q5 (hypothesis definition, sample size calculation, random selection and assumption checking, respectively). Although the current situation is rather serious, it has improved as compared to previous reports [91]. The results are

somewhat different for standalone experiments and experiments as evaluations. For experiments as evaluations, the number of N/A responses is much higher. There appear to be reasons for this:

(1) Most of the experiments as evaluations apply a matched pairs design. Random assignment (Q4) is typically not applicable in this case, e.g., two different bug prediction algorithms are applied to the same code [78, 85].
(2) A large number of studies, e.g., [46, 90], conduct the analysis using descriptive statistics only. Descriptive statistics do not have assumptions to check (Q5). When inferential statistics are not used, Q6-9 (linear modelling, power, and post-hoc testing) are not applicable either.

We ran a CHAID tree[2] classification to confirm the above observations. A N/A value in Q4 generates a subset containing 80% of all the experiments as evaluations studies ($\chi^2 = 29.7, df = 2, p - value < 0.001$). The classification tree confirms that non-random assignment due to matching is a differential characteristic of the experiments as evaluations.

Focusing on the error rate, we found that both standalone experiments and experiments as evaluations yield similar values[3] when examined using *Question* × *Study Type* contingency tables, with the exception of Q1.1 ($\chi^2 = 15.4, df = 1, p - value < 0.001$) and Q1.2 ($\chi^2 = 20.7, df = 1, p - value < 0.001$). In both cases, standalone experiments define null hypotheses (Q1.1) often and alternative hypotheses (Q1.2) five (66.7% vs. 13.3%) and eight times (57.1% vs. 6.7%), respectively, more often than experiments as evaluations.

Both types of studies do not show statistically significant differences for the remaining questions, although some may be false negatives. There are a large number of N/As for several questions, which reduces the amount of usable data, thus lowering the power of the tests. However, the low p-values for both the χ^2 and the Fisher's exact test suggest that Q3, Q4, Q5, Q10 could achieve statistical significance with larger samples. In all cases, standalone experiments perform random selection (Q3[4], random assignment (Q4), assumption checking (Q5) and reporting of descriptive statistics (Q10) more frequently than experiments as evaluations. Albeit not as large as in the case of Q1.1 and Q1.2, differences are still substantial, e.g., 61.9% vs. 13.3% for Q5.

We also find from Table 4 that:

- The required sample size (Q2) has been calculated in just one study. The definition of the linear model (Q6) has been considered in just two cases.
- Multiple testing (Q9) is a pervasive problem in SE research. Most studies fail to report or correct for multiple testing using adequate, e.g., Bonferroni or False Discovery Rate, methods.
- There is a high rate of random selection (Q3). Nevertheless, this problem is not easy to solve in human experiments because it is troublesome to assemble cohorts. In turn, random selection could be effectively applied in non-human research, e.g., when data is extracted from code repositories.

This survey shows that common statistical errors that occur in other sciences happen in SE as well. We have been able to survey a very limited number of experimental papers in one SE conference. However, both the type and number of problems found suggest that SE is facing the same challenges as in other sciences.

5 DISCUSSION

The most likely explanation for the occurrence of the statistical errors associated with Q1-10 is the recent adoption of experimental methods in SE. Many researchers have not taken formal courses on experimental methodology and inferential statistics as part of their postgraduate training. Self-education tends to lead to major differences among individuals. If these assumptions were true, two scenarios would be logical consequences:

(1) The studies conducted by skilled researchers would be of higher quality (where quality means error freeness, e.g., $\frac{\text{Yes answers}}{\text{All answers}}$) than experiments conducted by less skilled researchers. We could thus expect the quality values spread to range 0% to 100%. As errors are independent, quality follows a normal distribution[5].
(2) Any statistical concepts closely related to practice, e.g., random assignment (Q4), assumption checking (Q5), and reporting (Q10), would have a lower error probability than theoretical notions, e.g., hypotheses definition (Q1), sample size calculation (Q2), random selection (Q3), linear modeling (Q6), post-hoc power calculation (Q8), and post-hoc testing (Q9).

In order to check Scenario 1 above, Fig. 2 shows the histograms for both types of studies. In the case of standalone experiments (Fig. 2a), the histogram matches the assumption: the quality scores range across the 0% to 100% interval, and the distribution is normal ($Shapiro - Wilk = .947, df = 21, p - value = .300$). Experiments as evaluations (Fig. 2b) paint a rather different picture. The distribution is skewed to the left ($skewness = 1.02$), indicating that paper quality is concentrated around the low scores. This is a clearly non-normal distribution ($Shapiro - Wilk = .863, df = 30, p - value = .001$).

The above analysis suggests that the causes behind the statistical errors differ depending on the study type. In the case of standalone experiments, poor statistical training may explain the observed errors.

In the case of experiments as evaluations, training alone cannot explain the data. In our opinion, the low scores point to the secondary role of statistics and experimental methodology in these papers. Not only do experiments as evaluations take up a relatively small space of papers (which provides an excuse for summarizing "unnecessary stuff"), but statistical rigor also probably takes second

[2]The response variable was the study type (standalone experiments and experiments as evaluations) and the predictors were the questions Q1-10. We used the SPSS default CHAID parameters, with the exception of the parent and child nodes, which were set to 10 and 5 cases, respectively, due to the small number of cases.

[3]Notice that N/A values may suggest misleading relations. For instance, Q9 yields Yes/No values of 9.5% and 71.4% for standalone experiments, and of 3.3% and 26.7% for experiments as evaluations. Values differ greatly, but the odds $\frac{71.4}{9.5} = 7.5 \sim \frac{26.7}{3.3} = 8.1$ are rather similar.

[4]Notice that Q3 yields $\kappa = 0.09$ and $\kappa = 0.39$ for standalone experiments and experiments as evaluations, respectively. Random sampling is a controversial issue in SE. Results for Q3 should be viewed with caution.

[5]Statistical errors are probably dependent. When a researcher learns a statistical topic, e.g., sample size calculation, this knowledge is likely to lead to the avoidance of other errors, e.g., post-hoc power calculation. However, the errors underlying Q1-10 are too wide-ranging to appear strongly clustered in papers.

place in the authors' objectives (they are probably more interested in providing a convincing case for their proposals).

To check the Scenario 2 above, Table 5 contains the odds of making an error. The odds are the same concept as introduced in footnote 3; they represent the probability of an event occurring (providing a negative response to the Q_i question, i.e., the paper includes a statistical error) rather than another (providing a positive response to Q_i, i.e., there is no such error).

Table 5: Odds of making the error indicated in Q1-10

		Odds ($No \div Yes$)	
Concept type	Q	Standalone experiments	Experiments as evaluations
Practical concepts	Q4	0.4	0.0
	Q5	0.5	1.4
	Q10	0.0	0.2
Theoretical concepts	Q1.1	0.5	5.0
	Q1.2	0.8	10.0
	Q2	+Inf	+Inf
	Q3	2.5	5.0
	~~Q6~~	~~0.0~~	~~0.0~~
	~~Q8~~	~~0.1~~	~~0.0~~
	Q9	10.0	10.0

In the case of experiments as evaluations, the data *exactly* matches our assumption[6]. All theoretical concepts have large odds (≥ 5.0), as opposed to practical notions whose odds are small (≤ 1.4). For standalone experiments, the situation is *more or less* the same. For the theoretical concepts, odds are smaller than for experiments as evaluations, with the only exception of Q9. This is consistent with the higher error rate of the experiments as evaluations studies. However, Q1.1 and Q1.2 odds are much smaller (0.5 and 0.8, respectively) and comparable to the odds that appear in the group of practical concepts.

The above analysis confirms that poor training is the most likely explanation for the presence of statistical errors in experiments. In the case of experiments as evaluations studies, a more casual usage of statistics increases the error rate, but the final outcome is the same.

One anomaly in Table 5 is the large odd that Q9 exhibits for standalone experiments. It has the same value as in experiments as evaluations. This value is even less plausible given the small odds for Q1.1 and Q1.2: any researcher with a good knowledge of statistical hypotheses should be aware of the impact of multiple testing on α levels. The most likely reason is that, in addition to testing the statistical hypotheses, standalone experiments also perform exploratory research (which shows up as a large number of uncorrected post-hoc tests). Exploratory research is a common feature of many SE experiments, e.g., [7, 86].

Post-hoc testing is associated to *p-hacking*, that is, the acceptance of outcomes that fit expectations [55]. p-hacking leads to publication bias. Jørgensen et al. [38] evaluated the existence of publication bias in SE publications following Ioannidis' critical perspective for medicine [35]. Both papers came to a similar conclusion: the likelihood of publication bias is rather high. More importantly for our purposes, both papers report that the underlying reasons for publication bias are statistical, for example, multiple inference tests and a preference for statistically significance testing. Our data supports Jørgensen et al.'s observations: post-hoc testing increases the number of tests, and the failure to use correction methods for multiple testing probably inflates the number of false positives, thus leading to publication bias.

6 THREATS TO VALIDITY

This study applied two research protocols: a literature review and a paper survey. The two protocols are very similar. They have to meet a number of criteria concerning the relevance of the primary studies with respect to the research questions and the consistency across studies. Table 6 shows an assessment according to the appraisal criteria suggested by Thompson et al. [80][7]. On the whole, the results of the evaluation were positive. We can be relatively confident that the literature review and the survey results are trustworthy. However, they are incomplete due to the limited number of the primary sources used; three well-known books about statistical errors and experimental papers from one SE conference were used in the study. The external validity of this research is thus limited. Additionally, the literature review followed a simplified, but well-defined protocol. We took note of the page numbers of the books from which we extracted information about statistical errors. We disclosed the entire thematic analysis, including codes and high-level themes. All decisions were made by at least two researchers. These precautions increased the validity of the literature review.

With regard to the paper survey, we have taken reasonable precautions to avoid biases. Three researchers participated in the paper selection and evaluation. All decisions were recorded and made public for review. Agreement levels (using Fleiss' κ) were calculated and disclosed.

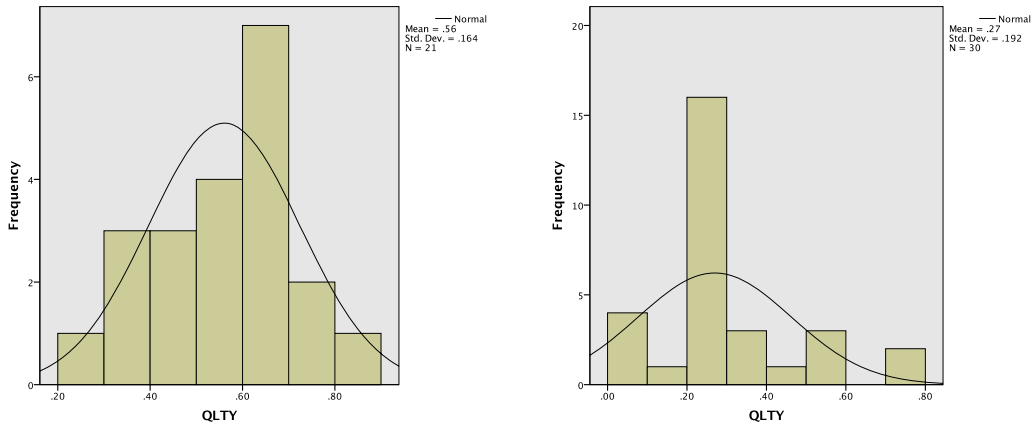
However, the precautions taken did not mean that we performed a correct assessment in all cases. The selection process yielded a low Fleiss κ value, which suggests that we may have skipped some experiments and, thus, potentially biased the results. Even so, there is no question about there being statistical errors in SE, although their occurrence rates or percentages may vary.

We do not claim that the reported rates are representative of all types of SE research. Actually, the rates reported in this paper probably represent the best practice in SE research, with the possible exceptions of the ESEM and EASE conferences, and maybe some journals, like Empirical Software Engineering. As we move away from outlets of repute, the number and severity of statistical errors is likely to increase.

Finally, we should point out that the results addressed in the Discussion Section are somewhat speculative. We cannot rule out alternative explanations for the distribution of quality scores. As usual, further research will be required to confirm our deductions.

[6] We are crossing out Q6 and Q8 because: a) Q6 was applicable only in two out of 51 studies, and b) post-hoc power analysis (Q8) is a commission, not omission, error; authors may perform correctly simply by not conducting a power analysis. Their inclusion would not have challenged our conclusions.

[7] There are many appraisal procedures; we have chosen [80] because it is quite simple and domain-independent.



(a) Standalone experiments **(b) "Experiments as evaluations"**

Figure 2: Histograms

Table 6: Appraisal criteria used for review

Appraisal criteria	RQ1 assessment	RQ2 assessment
Was the literature search comprehensive?	No	No
Were appropriate criteria used to select articles for inclusion?	Partially: The selected books were appropriate, but were not specialized. The books may have omitted the discussion of specific statistical errors that probably appear in other sources, such as research papers.	Partially: ICSE is the flagship conference in SE. Other conferences may publish lower quality experiments.
Were studies that were sufficiently valid for the type of question asked included?	Yes: The three books specifically addressed the topic of statistical errors.	Yes: Experiments published in ICSE represent the best practice in ESE.
Were the results similar from study to study?	Yes: They were very consistent. Several errors were identified by two or three books simultaneously. The same high-level themes were synthetized from the different books.	Yes: Statistical problems repeated across experiments.

7 CONCLUSIONS

The results of this preliminary review suggest that SE is subject to the same type of statistical errors as are found in other scientific disciplines. These problems are not complicated or sophisticated. They are surprisingly simple and include undefined hypotheses, missing sample size calculations, randomization, and multiple testing, among others. It is rather surprising that there is no information about the existence of such problems in SE. The SE methodological literature has not widely addressed this topic; only some papers [23, 41, 52, 81] have scratched the surface. Researchers may not be aware of the existence of statistical errors, and much less so of their prevalence and potential impact.

There are two reasons that appear to explain the presence of statistical errors in SE research: a) the recent widespread adoption of experimentation in SE, and b) the frequent use of exploratory research. In our opinion, the rapid adoption of experimental methods in SE research has forced researchers into statistical self-education. Additionally, it is rather unlikely that SE research teams include or have access to statistical consultants. As a result, errors tend slip into designs and ultimately published papers. This situation matches other sciences that have a long experimental tradition, such as medicine and ecology, which have only recently paid attention to statistical errors.

As empirical research in SE approaches a mature stage, there will be a greater awareness about statistical errors and the need to avoid them. However, it would be unwise for the SE community sit back and wait for the day to come. Besides setting up formal training courses at universities and professional societies (which is now afoot), the SE community shall enforce good practices, such as reporting guidelines and quality standards, that have proved to be useful in other sciences, e.g., medicine [72], psychology [74] and education [28]. Furthermore, these good practices can be easily enforced by journal editors and conference PC chairs at relatively little cost and effort.

Exploratory research is another source of problems. From the viewpoint of this research, exploratory research takes the form of missing statistical hypotheses and the execution of multiple uncorrected tests. However, these errors lead to publication bias, as already detected in SE [38]. Experiment pre-registration is probably the best way to fight against publication bias [15], but it is not easy to set up and enforce. To the best of our knowledge, pre-registration has not been discussed so far in SE. Further research is needed to find out effective ways to combat publication bias in SE. In the meantime, the establishment of reporting guidelines and quality standards may improve the situation.

8 ACKNOWLEDGMENTS

This work was partially supported by the Spanish Ministry of Economy and Competitiveness research grant TIN2014-60490-P, Empirical Software Engineering Research Group (GrISE), Laboratorio Industrial en Ingeniería del Software Empírica (LI2SE) and SENESCYT.

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Table 7: Problems found in standaline experiments and "experiments as evaluations" published in ICSE between 2006-2015

Empirical Studies	Code	Goal definition	Study design			Analysis			Reporting	
		(Q1) *Null hipothesis*	(Q2) *Sample size calculation*	(Q3) *Random sampling*	(Q4) *Random assignment*	(Q5) *Assumptions*	(Q6) *Model definition*	(Q8) *Post-hoc power*	(Q9) *Multiple testing*	(Q10) *Means*
Standalone Experiments	2006-EX01 [56]	No/Yes	No	No	Yes	No	N/A	No	No	Yes
	2006-EX02 [44]	Yes/Yes	No	Yes	No	Yes	N/A	Yes	No	Yes
	2007-EX03 [57]	Yes/No	No	No	No	No	N/A	Yes	No	Yes
	2007-EX04 [68]	Yes/Yes	No	No	Yes	No	N/A	Yes	No	Yes
	2008-EX05 [90]	No/No	No	Yes	Yes	N/A	N/A	N/A	N/A	Yes
	2010-EX06 [70]	Yes/Yes	No	No	Yes	No	N/A	Yes	No	Yes
	2011-EX07 [84]	Yes/Yes	No	No	Yes	Yes	N/A	Yes	No	Yes
	2012-EX08 [14]	Yes/Yes	No	No	Yes	Yes	N/A	Yes	No	Yes
	2012-EX09 [88]	Yes/Yes	No	No	Yes	Yes	N/A	Yes	No	Yes
	2012-EX10 [61]	Yes/Yes	No	No	No	Yes	N/A	Yes	N/A	Yes
	2012-EX11 [45]	Yes/Yes	No	No	Yes	Yes	N/A	Yes	No	Yes
	2013-EX12 [7]	No/No	No	No	No	Yes	N/A	Yes	Yes	Yes
	2014-EX13 [93]	Yes/Yes	No	No	Yes	Yes	N/A	Yes	No	Yes
	2014-EX14 [48]	Yes/Yes	No	Yes	Yes	Yes	N/A	Yes	No	Yes
	2014-EX15 [73]	No/No	No	Yes	No	No	N/A	Yes	N/A	N/A
	2014-EX16 [24]	No/No	No	No	Yes	Yes	N/A	Yes	No	Yes
	2014-EX17 [53]	Yes/Yes	No	Yes	Yes	Yes	N/A	Yes	N/A	Yes
	2015-EX18 [86]	Yes/Yes	No	Yes	N/A	No	N/A	Yes	Yes	Yes
	2015-EX19 [89]	Yes/No	No	No	Yes	Yes	N/A	Yes	No	Yes
	2015-EX20 [54]	No/No	No	No	Yes	Yes	Yes	No	No	Yes
	2015-EX21 [3]	No/Yes	No	No	No	No	N/A	Yes	No	Yes
"Experiments as Evaluations"	2006-CM01 [78]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	N/A
	2006-CM02 [87]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	No
	2006-CM03 [47]	No/No	No	No	Yes	N/A	N/A	N/A	N/A	No
	2007-CM04 [92]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2007-CM05 [37]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	N/A
	2007-CM06 [46]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2008-CM07 [50]	No/No	No	Yes	N/A	N/A	N/A	N/A	N/A	Yes
	2008-CM08 [67]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2008-CM09 [17]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2009-CM10 [40]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2009-CM11 [33]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2009-CM12 [9]	Yes/No	No	No	N/A	Yes	Yes	No	N/A	Yes
	2010-CM13 [60]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2010-CM14 [18]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2010-CM15 [64]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2011-CM16 [43]	N/A/N/A	No	No	N/A	N/A	N/A	N/A	N/A	N/A
	2011-CM17 [49]	Yes/Yes	No	No	Yes	No	N/A	Yes	No	Yes
	2011-CM18 [20]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2012-CM19 [13]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2012-CM20 [59]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2012-CM21 [76]	Yes/Yes	No	Yes	N/A	N/A	N/A	Yes	No	Yes
	2013-CM22 [26]	No/No	No	No	N/A	Yes	N/A	Yes	Yes	Yes
	2013-CM23 [36]	No/No	No	No	Yes	No	N/A	Yes	No	Yes
	2013-CM24 [94]	No/No	No	Yes	N/A	No	N/A	Yes	No	No
	2014-CM25 [66]	No/No	No	No	N/A	No	N/A	Yes	No	Yes
	2014-CM26 [1]	No/No	No	No	Yes	Yes	N/A	Yes	No	No
	2014-CM27 [69]	Yes/No	Yes	Yes	Yes	Yes	N/A	Yes	No	Yes
	2015-CM28 [79]	No/No	No	No	N/A	N/A	N/A	N/A	N/A	Yes
	2015-CM29 [85]	No/No	No	No	Yes	No	N/A	Yes	N/A	Yes
	2015-CM30 [65]	No/No	No	No	N/A	No	N/A	Yes	N/A	Yes