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Improving the Utility of Non-Significant Results for Educational Research

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Abstract

Non-significant results have the potential to further our understanding of what does not work in education, and why. We make three contributions to harness this potential and to improve the usage and interpretation of non-significant results. To evaluate current practices, we conduct a review of misinterpretations of non-significant *p*-values in recent educational research. The review indicates that over 90% of non-significant results are erroneously interpreted as indicating the absence of an effect, or a difference compared to a significant effect. Researchers sometimes link these misinterpretations with potentially erroneous conclusions for educational theory, practice, or policy. To improve the status quo and make non-significant results more informative, we provide a detailed framework based on which researchers can design, conduct, and analyze studies that yield reliable evidence regarding the actual absence of an effect. In addition, we provide a competence model that researchers can use to guide their own research and teaching.

Keywords: Non-significant results; misinterpretations; equivalence testing; framework; competence model

Improving the Utility of Non-Significant Results for Educational Research

Statistical hypothesis testing by use of *p*-values is omnipresent in educational research, and researchers rely upon its results for informing evidence-based decisions regarding educational theory, practice, and policy (Clearinghouse, 2019; Keselman et al., 1998; Kirk, 1996; Kraft, 2019; Olsen, Unlu, Price, Jaciw, & Bachman, 2011). Consequently, it is of major relevance to ensure that the information gained from *p*-values is valid and reliable, and used as a fertile tool for advancing educational insights and their application.

In the present research, we focus on the case of non-significant results, as indicated by non-significant *p*-values. A non-significant *p*-value is often seen to inevitably leave inconclusive evidence regarding the existence of an effect, or, in the worst case, it might be misinterpreted as indicating evidence for the actual absence of an effect (Mehler, Edelsbrunner, & Matic, 2019). However, non-significant results do not inevitably leave inconclusive evidence. They do actually have great potential for furthering our understanding of what works in education, and of what does not work, and why (Biesta, 2007; Lakens, 2017; Lakens, McLatchie, Isager, Scheel, & Dienes, 2020; Mehler et al., 2019). If well-designed studies probing an educational intervention repeatedly yield non-significant results, this might indicate its relative ineffectiveness. In addition, more principled information can be gained by testing statistically whether a non-significant result indicates the actual absence of an effect. This can be directly evaluated with modern statistical methods (Lakens, 2017; Lakens et al., 2020). As we will show, finding out reliably what a non-significant result really indicates requires the implementation of three measures in the conduct of educational research: A potentially non-significant result has to be considered appropriately already during study design, the result itself has to be interpreted correctly, and its actual implications have to be statistically scrutinized.

The current article provides three contributions to optimizing the utility of non-significant results for education. First, it portrays a critical aspect of the current practices in handling non-significant results in education. Specifically, a review is undertaken in order to determine the frequency of two consequential misinterpretations of non-significant *p*-values in recent educational research. The review shows that in the majority of recent articles, non-significant *p*-values are frequently misinterpreted as indicating the absence of an effect, or a difference compared to a significant effect. These misinterpretations are in turn commonly linked with potentially erroneous conclusions for educational theory, practice, and policy. In order to ameliorate this situation, we present a framework for the design and implementation of studies that have the potential to yield reliable information about what a non-significant result actually indicates. Based on this framework, researchers can ensure that they will very likely be able to successfully determine, based on appropriate statistical tests, whether a non-significant result really indicates the absence of an effect, or whether it indicates that the evidence is yet inconclusive. As a final contribution, we try to support researchers in implementing these practices by providing a competence model. The model outlines central aspects of reliable hypothesis testing based on *p*-values and alternative statistical approaches, focusing on the handling of non-significant results. With the competence model, we aim at supporting researchers in yielding informative non-significant results in their own studies, and in optimizing related aspects of their statistics education for tomorrow's educational researchers. With these three contributions - the review, framework, and competence model - we aim to support researchers in handling non-significant results in a way such that they provide informative insights for education. Following, we first introduce the two misinterpretations of non-significant results based on a fictitious example, before we present our three contributions.

Two Common Misinterpretations of Non-Significant Statistical Results

Imagine a researcher conducting a study on the effectiveness of an educational intervention, as depicted in Figure 1. The researcher assigns learners randomly to two different intervention conditions, employing Teaching method A and Teaching method B, respectively, hypothesizing that Teaching method B is more effective, because Teaching method A lacks a critical learning element. Both groups receive an assessment of the learning outcome as pretest before the intervention, and again as posttest after the intervention.

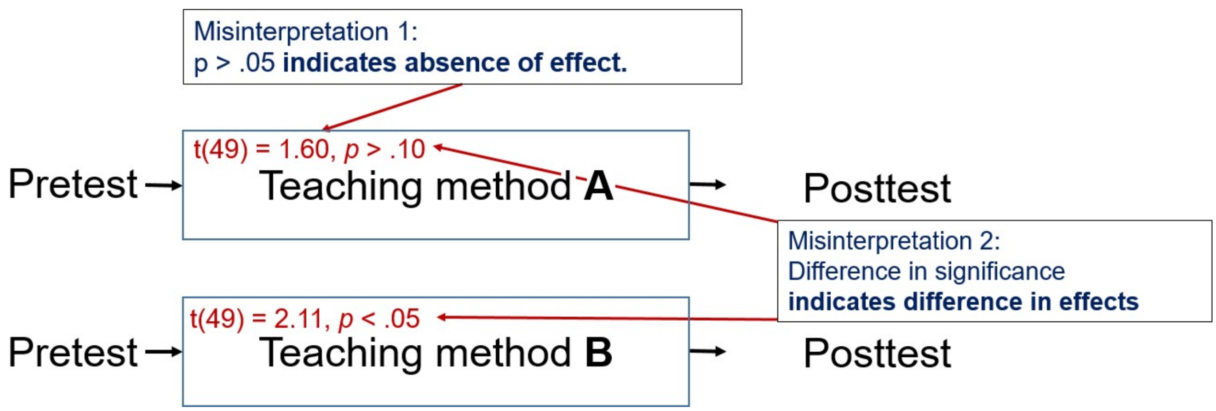

Figure 1. Depiction of the two misinterpretations of non-significant p-values in an educational context.

Based on a sample of 100 learners, 50 in each group, the researcher conducts two repeated measures *t*-tests, and, employing the common significance criterion $p < .05$, finds a significant difference between pretest and posttest for Teaching method B ($t(49) = 2.11, p < .05$), but not for Teaching method A ($t(49) = 1.60, p > .10$). The researcher interprets the two *p*-values as follows: “My hypothesis is correct; Teaching method B works better and caused significant learning gains, whereas no learning took place under Teaching method A!”. Putting aside that *p*-

values do not directly inform about the adequacy of hypotheses (Greenland et al., 2016; Wagenmakers, 2007), two common misinterpretations are apparent from this interpretation. Figure 1 indicates the two misinterpretations.

The first misinterpretation is that the non-significant *p*-value indicates evidence for the absence of a learning effect under Teaching method A (Alderson, 2004; Mehler et al., 2019). Observing a non-significant *p*-value by itself does not provide support for accepting the null hypothesis of no effect (Greenland et al., 2016). While a non-significant *p*-value *might* indicate evidence for the absence of an effect, it might as well represent a Type II error (also called beta-error; overlooking/missing an effect that really exists). Which of these two is the case cannot be determined based on a non-significant *p*-value. Even with a relatively large sample, an effect might simply have been overlooked, particularly if the population effect exists but is smaller than expected (Mehler et al., 2019; Richard D Morey & Lakens, 2016). This inconclusiveness holds even if the obtained *p*-value seems really large (e.g., $p = .999$); if no effect exists, $p < .001$ is as likely as $p = .999$ (Mehler et al., 2019). For these reasons, a non-significant p-value really leaves inconclusive evidence (Altman & Bland, 1995; Greenland et al., 2016).

The second misinterpretation is that the non-significant effect under Teaching method A differs from the significant one under Teaching method B (Gelman & Stern, 2006; Nieuwenhuis, Forstmann, & Wagenmakers, 2011). The misinterpretation is that if one effect is significant ($p < .05$) and another one is not significant ($p > .05$), this supports the assumption that the two effects differ from each other. This misinterpretation is linked with the first one; researchers who assume that a non-significant p-value indicates the absence of an effect might also assume that it differs from a significant one (which presumably "exists"). A difference in significances might however be caused by differences in the sample alone that do not indicate systematic population differences. Even if two *p*-values differ rather strongly from each other, this is not yet a reliable indicator that the underlying population effects are significantly different from each other. The appropriate way to test for the difference between two effects is an interaction test, which would test for example for an interaction between time-point and teaching method. Researchers do often not conduct such an interaction test (Gelman & Stern, 2006; Nieuwenhuis et al., 2011).

These and related misinterpretations of p-values are common across fields such as psychology, neuroscience, biology, eating disorders research, and medicine (Chmura Kraemer, 2017; Fidler, Burgman, Cumming, Buttrose, & Thomason, 2006; Hoekstra, Finch, Kiers, & Johnson, 2006; Hoekstra, Monden, Ravenzwaaij, & Wagenmakers, 2018; Hoekstra et al., 2018; Kline, 2004; Nieuwenhuis et al., 2011). These misinterpretations and further issues around *p*-values have led to decades of criticism and calls for alternative statistical approaches to hypothesis testing (McShane, Gal, Gelman, Robert, & Tackett, 2019; Nuzzo, 2014; Thompson, 1993; Wasserstein, Schirm, & Lazar, 2019). Still, despite frequent and ongoing criticisms, *p*-values will likely remain the prevalent statistical means for hypothesis testing in the foreseeable future. Therefore, the question arises whether and to which extent misinterpretations of non-significant results are prevalent in educational research. In addition, if these misinterpretations turn out to be rather highly prevalent, it remains open how to ameliorate the situation. Consequently, the two objectives of this research are to evaluate the prevalence and potential consequences of these misinterpretations in current educational research, and to provide measures in order to improve the utility of non-significant results.

A question remains before we get to the specific contributions of the present study: What is the correct interpretation of a non-significant *p*-value? For the non-significant *p*-value under Teaching method B, the correct interpretation and handling would be that it leaves inconclusive evidence. This result does neither support the decision to reject the hypothesis of no effect, nor does it support accepting it. This inconclusive situation is obviously discomforting. There are however statistical means for improving this situation, by testing whether non-significant *p*-values really indicate evidence for the absence of an effect, or for its negligible size (e.g., an effect which is so small that is has negligible educational consequences; Greenland et al., 2016; Dienes, 2014; Lakens, 2017). These statistical means will be in focus of the later part of this article, in which we describe a framework and competence model for the reliable implementation of these approaches. Regarding the second misinterpretation, as discussed before, an interaction test would have to be implemented.

The *p*-value resulting from the interaction test (which might again be non-significant and therefore leave inconclusive evidence) offers the basis for a statistical inference about this difference. Without an interaction test, a difference may appear to be present based on descriptive statistics, but a direct hypothesis test of the difference is required for principled statistical inference (Morey, Rouder, Verhagen, & Wagenmakers, 2014).

Following, we first present the review examining the prevalence and potential consequences of these misinterpretations in current educational research. In the next part, based on the results of the review, we will outline different opportunities for the handling of non-significant results, and provide the framework for maximizing their information value. Finally, to further support researchers in the implementation of appropriate strategies, we will provide the competence model to guide their own research and teaching.

A Review of Misinterpretations in Recent Educational Research

The first objective of the present study was to conduct a review in order to shed light on the prevalence of the two described misinterpretations about *p*-values in educational research. We examine the prevalence of these misinterpretations but we also believe that more persuasive evidence beyond their mere occurrence is needed. Therefore, in addition, we try to vet the extent to which these misinterpretations might actually impact educational research and its implications for education. The review was conducted on a sample of journal articles from 2016- and 2017-volumes of overall four different educational journals. The research questions resulting from the two objectives of the review were:

What is the prevalence of two misinterpretations of non-significant results ($p > .05$ implies evidence for absence of effect, $p < .05$ vs. $p > .05$ implies significant difference in effects) in recent issues of low-, medium-, and high-impact journals in the field of education?

In how many of the articles are these misinterpretations linked with potentially erroneous conclusions regarding educational theory, practice, and policy, and what are typical implications that researchers derive based on these misinterpretations?

Method

To answer these research questions, we reviewed a sample of 60 articles from four different journals. We are not aware of any prior research that has examined the prevalence and consequences of misinterpretations of non-significant results in educational research. We decided to stick to a limited number of articles and journals to conduct detailed in-depth reviews of these articles. This in-depth work allowed developing and refining our coding approach. From each of the journal volumes, 10 empirical articles were randomly drawn. The resulting sample of 60 journals articles stemmed from the following journals:

- German Journal of Educational Psychology (Zeitschrift für Pädagogische Psychologie): This journal was chosen as a rather low-impact journal (five-year impact factor in 2017 according to Thomson Reuters Journal Citation Reports: 1.01). This journal has some significance for a rather broad readership in central Europe, including stakeholders in academia and policy. The 2016 journal volume encompassed 21 empirical articles, the 2017 journal volume 12 empirical articles and one empirical short contribution.

- Instructional Science: This journal was chosen as a medium-impact journal (5-year IF: 2.62) because it publishes a broad range of educational studies. The 2016 journal volume encompassed 29 empirical articles.
- For the 2017-volumes, the journal Instructional Science was replaced by the British Journal of Educational Psychology (5-year IF: 3.32) in order to vary one of the journals and check whether the results depend on the choice of journal. This journal volume contained 41 empirical articles in 2017.
- Journal of Educational Psychology: This journal was chosen as a high-impact journal (5-year IF: 6.2) because it is among the most prestigious journals in education and might therefore have high impact on education and its research. The 2016 journal volume encompassed 76 empirical articles, the 2017 journal volume 73 empirical articles.

Both authors of this study identified focal hypotheses in all of the 60 articles. We read all of the articles in full. In the extraction of hypotheses, we experienced the issue that we were not able to develop a definition of *hypothesis* that would fit all the expressions we found in the reviewed articles. Thus, we decided not to define a priori what we mean by a hypothesis. Instead, we read the articles in detail and ensured a high validity of our codings of hypotheses the following way: Besides reading the articles and scanning for hypotheses subjectively based on our own expertise in educational research, we conducted key string-searches for the expressions “predict*”, “hypothes*”, and “expect*”. For all results from these searches, we checked whether we had already identified the according hypotheses. In almost all cases, results from these searches were perfectly coherent with the hypotheses that we had already identified and coded.

In a next step, statistical tests concerning the focal hypotheses which yielded *p*-values $> .05$ were identified. The alpha error-level (at which a result is interpreted as significant) was clearly at .05 in all reviewed articles. Although this was seldom explicitly mentioned, it became clear when authors started interpreting results with $p < .05$ as significant and results with $p > .05$ as non-significant. The non-significant *p*-values that were linked with the identified hypotheses represented the basis for potential misinterpretations. We then noted the statistical inferences and theoretical, practical, and policy implications linked to these non-significant *p*-values from the results- and discussion-sections of the articles.

As a measure of interrater-reliability, based on the share of the 2017-articles, after reading only the introduction of each article, we noted down all hypotheses that we could find, supported by a key string-search for “predict*”, “hypothes*”, or “expect*”. We noted all hypotheses that we had identified independently of each other in each of the articles, and resolved unmatched hypotheses. In the 2017 volumes, the number of hypotheses identified by rater 1 was 218, whereas rater 2 identified 216 hypotheses. The number of hypotheses identified correlated with $r = 0.60$ across the thirty articles, with an average of 0.07 difference in the number of hypotheses. While for most articles, almost all hypotheses identified were the same between the two raters, there were discrepancies of 10 hypotheses, and 15 hypotheses, respectively, for two of the articles. We discussed inconsistencies and could resolve all of these after discussion. We noted two reasons for inconsistencies that will be described in the discussion.

Results

The results across all reviewed articles are depicted in Figure 2. In total, in the 60 journal articles 501 times *p*-values were used to test the predictions of hypotheses. Of these, 198 (40%)

yielded $p > .05$. Thus, this number was the potential basis for finding instances of the two misinterpretations. Regarding the first misinterpretation, of the 198 non-significant p-values 181 (91%) were statistically misinterpreted as implying evidence for the absence of an effect. Regarding the second misinterpretation, 84 times researchers interpreted a difference between an effect with $p > .05$ and another one with $p < .05$ without conducting an appropriate test.

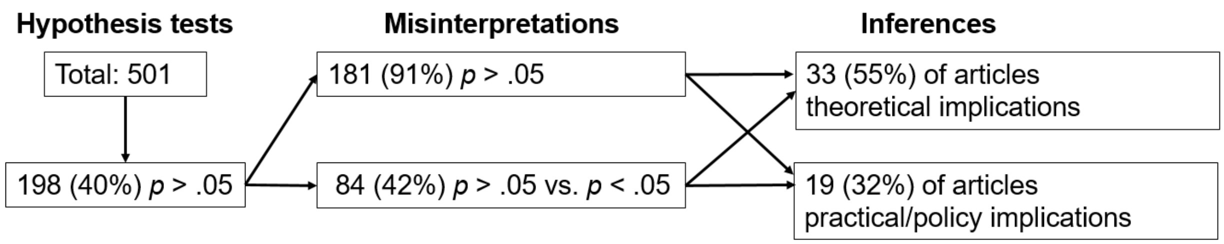

Figure 2. Results based on hypotheses tests from all of the 60 reviewed journal articles. Numbers for "Hypothesis tests" and "Misinterpretations" indicate absolute numbers, whereas those for "Inferences" indicate numbers of articles with at least one occurrence of respective inference.

Figure 3 also depicts results individually for the four journals. It appears that in the German Journal of Educational Psychology, less hypothesis tests were reported, which might be explained by the relatively short manuscript length in this journal (max. 70000 characters for empirical contributions). Overall, in each of the journals about one third to half of the p-values were non-significant, and there were no noticeable big differences in numbers of misinterpretations and related conclusions.

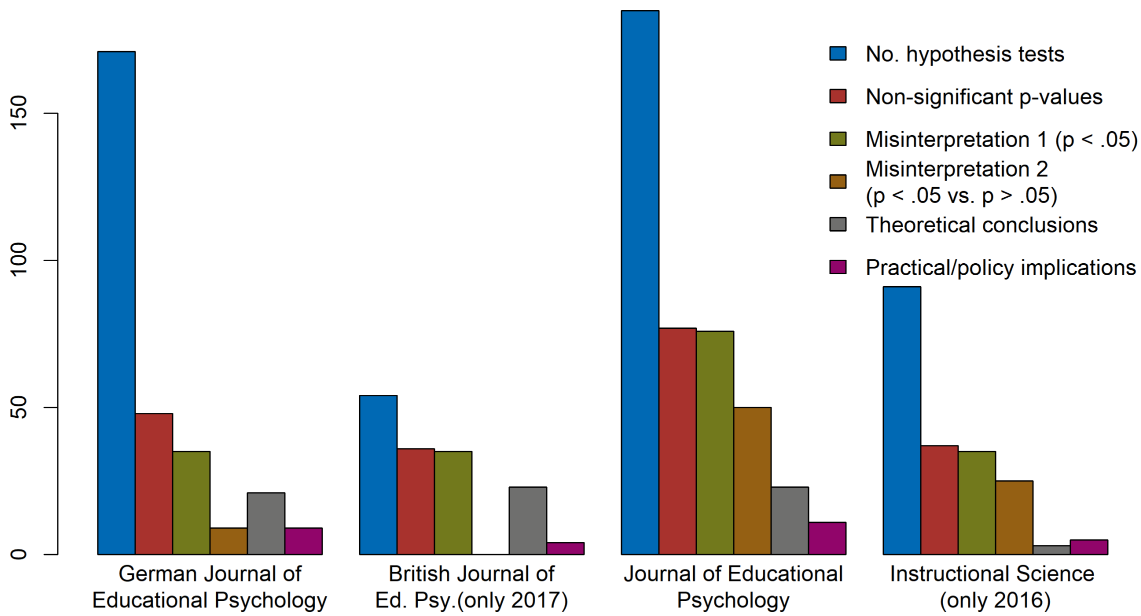

Figure 3. Results of codings of the 60 randomly drawn articles by journal. Instructional Science and German Journal of Educational Psychology volumes from 2016 and 2017, Journal of Educational Psychology volume from 2016, British Journal of Educational Psychology volume from 2017.

Regarding the second research question, there was a great variety of theoretical, practical, and policy implications that researchers inferred based on these misinterpretations.

As indicated in Figure 2 and Figure 3, whereas the overall number of potentially misled inferences based on misinterpretations was rather low, there were at least one such theoretical inference in more than half of the articles, and at least one such inference with practical or policy implications in about a third of the articles. A common practice was that in the case of $p > .05$, researchers discussed theoretical reasons for the absence of an effect. As potential explanation, researchers would most commonly point out differences in the operationalization of variables between their own study and others'. Of the 60 articles, 33 articles contained at least one theoretical conclusion based on the investigated misinterpretations. Furthermore, regarding both practical and policy implications, the second misinterpretation seemed to bear more weight than the first one: When finding $p > .05$ for the effect of a Teaching method B, however $p < .05$ for another Teaching method A, in various cases researchers pointed out the practical significance of teachers undergoing training in method B because of its apparent great importance, a suggestion with potential impact on classroom practices and teacher education policies. Such practical implications drawn from misinterpretations appeared in 19 of the 60 articles.

In some articles, we directly observed a misleading formulation of research hypotheses that emphasizes the nature of the misinterpretations. Specifically, in eleven cases hypotheses were formulated to predict no difference (e.g., “we expected learning transfer to be similar in both conditions”), yet without using the appropriate tests for such hypotheses of equivalence. In all cases, results from these hypotheses were linked to the misinterpretation that a non-significant *p*-value supported the respective hypothesis.

In some articles, authors indeed raised the assumption that $p > .05$ might stem from too small sample sizes. While sample size is implicitly linked to the frequency of a type II/beta-error, this possibility was never explicitly discussed. In very few articles a power analysis was conducted to decide on sample size. Furthermore, in some case, non-significant *p*-values were fully neglected and the related results not discussed at all.

These results from the review indicate that misinterpretations of non-significant *p*-values have a high prevalence in recent educational research. The core of the misinterpretations is that *p*-values in their typical application cannot support the decision to accept the null hypothesis; in other words, they cannot provide evidence for the absence of an effect. Recently, an approach has been introduced into psychology and related fields that actually allows for making such inferences based on *p*-values in combination with confidence intervals. This approach is called equivalence testing (Da Silva, Logan, & Klein, 2009). In order to enable educational researchers to make non-significant *p*-values more informative, guidance is required in how to apply this approach to educational research.

A Framework for Obtaining Informative Non-Significant Results

We provide a framework to support researchers in the process of designing informative studies to test whether non-significant *p*-values indicate the absence of a (meaningful) effect. The simple idea behind the underlying statistical approach, called equivalence testing, is that if a confidence interval lies fully the smallest effect size deemed consequential for a certain research question, this indicates significant evidence for the absence, respectively negligible size, of the effect in question (the approach is called slightly differently for one-sided and two-sided tests and different questions, see Wellek, 2010). Following, we will describe why implementing such an approach requires its consideration already during study design, and then provide the framework for its informative implementation.

The most common application of *p*-values is the long-term control of alpha- (type I) and beta- (type II) errors (Daniel Lakens, 2019). As with all regular hypothesis tests within this approach, hypothesis tests have to be considered during study design. Only in this case, hypothesis tests can assure proper error control, in contrast to exploratory post-hoc tests (Rubin, 2017). In addition, control of type II-errors requires the implementation of a power analysis in order to adapt the sample size, which should be optimally done before statistical analyses are conducted (Perugini, Gallucci, & Costantini, 2018). For these reasons, also the testing of non-significant *p*-values for evidence for the absence of an effect has to be considered already during study design. The framework for how to achieve this is depicted in Figure 4. Encompassing three major steps, it is based on two different concepts: The smallest effect size of interest, and the relevant frames of reference.

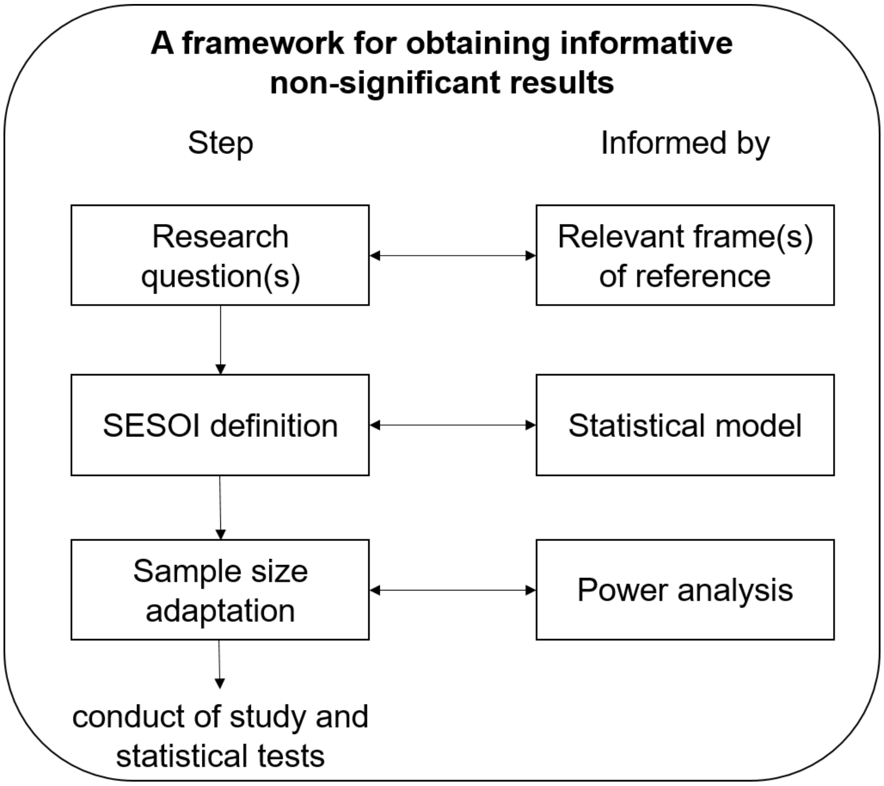

Figure 4. A framework for obtaining informative non-significant results in educational research. SESOI stands for smallest effect size of interest.

The first concept that the framework is focused around is figuring out the *smallest effect size of interest* (SESOI). A smallest effect size of interest is the minimal effect at which researchers would answer their research question with a clear "yes", or, in other words, at which they would care about the effect (Lakens, 2017; Lakens et al., 2020). From the complementary perspective, effects below this cut-off would be negligible. In educational settings, this concept can be applied for example to intervention studies: "What size would the intervention effect have to exceed in order to be regarded as meaningful, either for educational theory, practice, or policy?". An equivalence test, then, is a statistical significance test which indicates whether the obtained effect size lies below this cut-off. Its principle is really simple: If the confidence interval around an effect estimate lies below the smallest effect size of interest, one would conclude statistical equivalence; the absence of a meaningful effect (Lakens, 2017).

Determining the smallest effect size of interest is challenging. The advantage of this challenge is that it encourages researchers to reflect upon their precise research question, because a detailed research question provides hints towards the smallest effect size of interest. This is the first step in the framework: Determining a contextualized, precise research question as a prerequisite for determining a smallest effect size of interest. Frequently, despite ongoing criticisms against this practice, effect sizes are labelled as "small" for $d = 0.2$, "medium" $d = 0.5$, and "large" $d = 0.8$, following Cohen (1988). As Cohen (1988) himself noted extensively, it is generally not useful to base interpretations of effect sizes on rules-of-thumb that lack adjustments to the specific context. However, how is it possible to decide without relating to established guidelines on which effects would be of interest to find, and which not? In order to specify a sufficiently precise research question to decide on this question, we suggest the concept of *relevant frames of reference*.

A relevant frame of reference is any factor that contributes to determining the research question with sufficient precision such that it points towards a study's smallest effect size of interest. Relevant frames of reference are any factors that educational researchers would consider in deciding on the importance of an intervention effect. There is vast literature hinting towards factors that might be considered for judging effect sizes in education (e.g., Bakker et al., 2019; Schäfer and Schwarz, 2019; Fritz, Morris, and Richler, 2012; Hattie, 2010; Kraft, 2019). Based on this literature, we identified the key characteristics presented in Table 1 of an intervention, its context, and the employed research methods that researchers can consider as relevant frames of reference. The study characteristics are presented grouped into four categories: Relative comparisons to prior research or student development, impact on learners, balance of intervention effect with required resources, and theoretical relevance of the research question.

There are prior guidelines available for judging the effect sizes of educational interventions (Bakker et al., 2019; Clearinghouse, 2019; Kraft, 2019; Lipsey et al., 2012). However, these guidelines focus on the perspective of large-scale implementations of educational interventions. That is, these guides focus on educational policy, presuming that the aim of a specific type of intervention will be its later implementation in a schooling system.

For the present objectives, we assume that for individual research studies, research aims can widely differ. Researchers might conduct studies to tackle a purely theoretical question, they might aim at replicating or extending prior research, or they might want to try out a new educational intervention without yet considering its later potential for large-scale implementation. For this reason, for the purpose of judging the effect sizes of interest for individual studies, we suggest researchers to take into account the research aim and context at hand. The seventeen suggested factors on the provided list and additional ones can be considered one by one in order to help specifying a detailed, contextualized research question. Following, we provide an example for the determination of a sufficiently specific research question, and how this translates into a smallest effect size of interest.

Table 1

Overview of Some Central Relevant Frames of Reference for Carving out a Contextualized Research Question and Smallest Effect Size of Interest

Relative comparisons
Typical effect sizes of educational interventions (Fritz et al., 2012; Hattie, 2010; Schäfer & Schwarz, 2019)
Comparison to effects from similar prior interventions (Lipsey et al., 2012)
Comparison to regular growth during an academic period (Lipsey et al., 2012; note that interpreting effect sizes from this "years-of-learning perspective" has been criticized, see Baird & Pane, 2019)
Impact
Developmental impact (effects on outcome variable of interest in the long run; Dumas and McNeish, 2017; Funder and Ozer, 2019)
Comparison to policy-relevant achievement gaps between groups (e.g., impact of intervention on gender differences or decrease between at-risk students and not-at-risk students, absolute decrease in number of at-risk students; Lipsey et al., 2012; Yeager et al., 2019)
Percentile rank changes on a standardized test instrument (Clearinghouse, 2019)
Changes in probability to score above a certain relevant proficiency-threshold (Baird & Pane, 2019)
Resources
Duration (Cortina & Landis, 2011; Kraft, 2019; Lipsey et al., 2012; Watson, 1999; Yeager et al., 2019)
Implementation effort (Cortina & Landis, 2011; Kraft, 2019; Lipsey et al., 2012; Watson, 1999; Yeager et al., 2019)
Costs (Cortina & Landis, 2011; Kraft, 2019; Lipsey et al., 2012; Watson, 1999; Yeager et al., 2019)
Theoretical relevance
Cognitive processes involved (e.g., far transfer across contexts; Barnett & Ceci, 2002; Schalk, Edelsbrunner, Deiglmayr, Schumacher, & Stern, 2019)
Tests of the generalizability and robustness of effects (Prentice & Miller, 1992)
The stability of effects/transfer over time (Bailey, Duncan, Odgers, & Yu, 2017; Funder & Ozer, 2019)
Study design
Nature of the control group (e.g., a waiting list-condition or an active control group differing only in a key element; Cheung and Slavin, 2016; Schalk et al., 2019)
Specifics of the assessment instruments (e.g., non-standardized or standardized instrument; Cheung and Slavin, 2016)
Sample size: Small-scale experiment or large-scale implementation (Bakker et al., 2019)
The predicted scalability of an intervention to larger populations (Kraft, 2019)

A contextualized example for the application of the framework is depicted in Figure 5. In the example, an educational researcher aims at testing the effectiveness of a training of the control-of-variables strategy, a well-researched scientific inquiry strategy that is based on the principle to vary only one thing at a time in the investigation of causal effects (Schwichow et al., 2016).

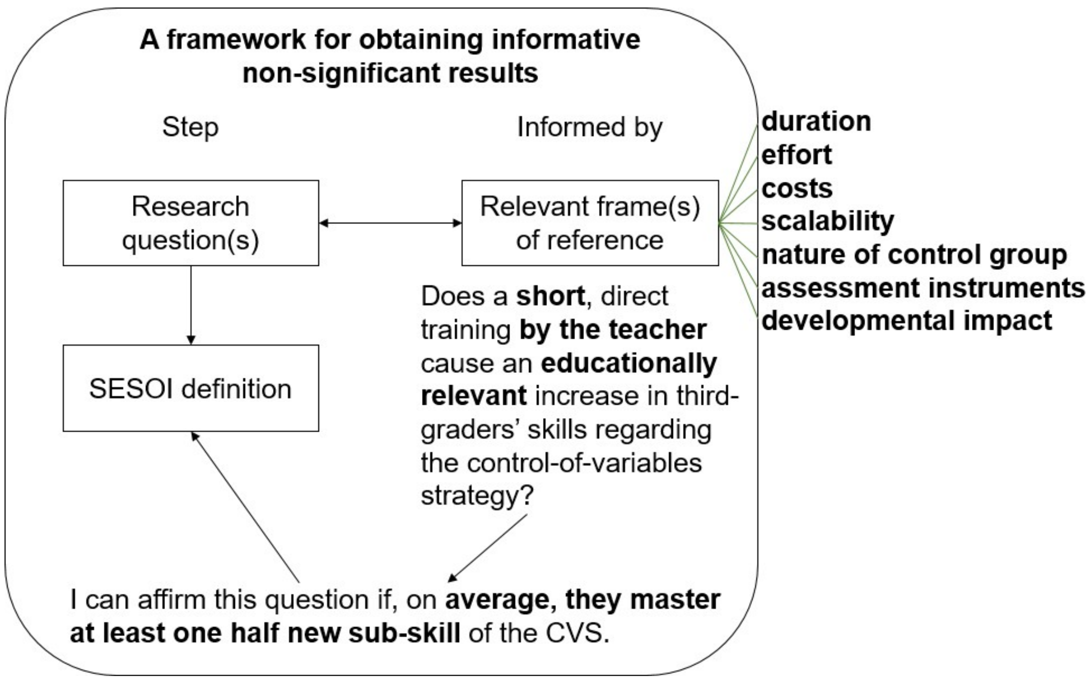

Figure 5. Example for implementation of the framework in a training study on the control-of-variables strategy.

The aim of the hypothetical study is to lead to a conclusion about the effectiveness of the training, or a lack thereof. A lack of effectiveness would be indicated by evidence for the absence of a meaningful training effect. Now, in order to be able to test for such evidence statistically, the researcher first has to specifiy a precise, contextualized research question. In the present case, the researcher builds the training study on a lot of prior existing literature. Dozens of training studies have been developed to increase students' understanding of the control-of-variables strategy (Schwichow et al., 2016). Therefore, the researcher specifies in more detail that the aim of the training is that it should be relatively easy to be implemented by teachers on their own, it should have rather low costs, and within the duration of one school lesson it should have visible impact on students' skills. The impact of the intervention in the experimental group would be compared to a control group which does not receive a training but receives the same material to engage in self-guided inquiry. As visible in Figure 5, these aspects go into specifying the research question in so much detail that the actual aim of the study becomes much more clearly apparent.

The next step is to determine the smallest effect size of interest. For assessing students' skills regarding the control-of-variables strategy, the researcher plans to employ an established measure of four sub-skills of this strategy, each of which is covered by six items (see Schwichow et al., 2016) for the description of such a measure). Based on own expert knowledge and relevant literature, the researcher decides that the smallest effect size of interest would be an increase of half a sub-skill on this measure. This translates into an advantage of three items on pretest-posttest gains on the measure of the experimental group in comparison to the control group, which does not receive any training and instead engages in self-guided inquiry during the same time. We believe that thinking in raw and concrete measures such as "three items difference" instead of standardized effect sizes is the preferred way to engage in study planning in most contexts in educational research. In the present case, according to prior findings with the same measure, a difference of three items would correspond to a half of a standard deviation, which also appears like a reasonable expectation to the researcher. If no prior information on the typical standard deviation on a measure is available, we suggest researchers to estimate it on the already assessed study sample or the first part thereof (and conduct the final hypothesis tests on the remaining part of participants). In such cases, when the smallest effect size of interest is determined based on the sample at hand, we suggest researchers to preregister this procedure (Mertens & Krypotos, 2019). A preregistration ensures to peer researchers that this procedure has been planned beforehand and therefore allows for confirmatory hypothesis tests (Rubin, 2017). So, the fully contextualized research question and the smallest effect size of interest have been specified.

In the third step, the researcher can now determine the appropriate statistical model to test the statistical effect of the training. The researcher decides on a multilevel regression model. Based on a power analysis of the exact model, the researcher finds that if the effect of the training is really zero, in order to determine reliably (with a probability/statistical power of $>$ .90; another cut-off that should be reflected upon in detail; see Bacchetti, 2019) that a confidence interval lies below the smallest effect size of interest of three items difference/a Cohen's *d* of 0.5, a sample of 18 school classes with overall 260 students should be assessed. Finally, the researcher conducts the study and estimates the confidence interval around the regression parameter indicating the difference in gains in solved items between the intervention group and the control group. If the researchers finds a non-significant group difference and the 90% confidence interval is fully below a group difference of 3 items, this supports the hypothesis that there are negligible differences in the two groups' gains at an alpha-error level of 5% (see Lakens et al., 2020 on notes about one-sided and two-sided equivalence testing). So, using a confidence interval of 90% for an equivalence test does something similar to a *p*-value with a cut-off for significance of .05 (Lakens et al., 2020).

Thus, the researcher has managed to make more of the non-significant result by finding that it does indeed support the assumption of no meaningful group difference. If the 90% confidence interval had included the three items difference in solution rates across groups, the equivalence test would have left inconclusive evidence. However, if the a priori power analysis has been conducted properly and yielded an informative estimate of study power, this will rarely happen.

With this example, it becomes clear that the framework we provide here increases the information value and therefore the utility of non-significant results. We suggest readers who are interested in applying this or similar approaches to consult Mehler et al. (2019), Lakens (2017), and Lakens et al. (2020) for further information and tutorials on equivalence testing.

With this framework involving three steps that build on each other, we hope to support researchers in managing to improve the information value of non-significant *p*-values. In addition, we believe that this framework actuates a broad array of important practices in the planning of informative quantitative studies, such as ensuring that research design and methods map onto theoretical and eventually related statistical questions, and conducting an informative power analysis in order to ensure the reliability of hypothesis tests. According to the main aim of the present research, more importantly we believe that it helps researchers to prevent misinterpretations and distinguish whether non-significant *p*-values obtained in their focal hypothesis tests indicate the actual absence of a meaningful effect, or that results are yet inconclusive.

A Competence Model for Research and Teaching

A last contribution to improving the utility of non-significant results in education remains: Ensuring that future researchers profit from the present insights and suggestions. To this end, we developed a competence model that provides educational researchers with an overview of important aspects of hypothesis testing based on *p*-values and alternative approaches. This competence model might serve as an informative guide for original research, and in statistical courses for the teaching of hypothesis testing with an emphasis on the correct application and interpretation of *p*-values.

In the competence model and the teaching of hypothesis testing, also alternative approaches to *p*-values should play their due role. While the typical application of *p*-values (the Pearson-Neyman approach, mixed with the Fisherian approach; see Gigerenzer, 2004) is part of the most classical approach to statistics, the Bayesian approach and the likelihood approach represent alternative statistical approaches that can be of help with the present issues (Dienes, 2008; Glover & Dixon, 2004). The three approaches in general serve to answer slightly different questions (e.g., *p*-values are mostly considered to optimize error-control in statistical decision making, whereas the Bayesian approach operates under the conceptualization of estimating uncertainty; Dienes, 2008), and it might be the case that what researchers are interested in is better served by other approaches than *p*-values. For example, Bayesian approaches can inform us about the confidence in null- and alternative hypotheses by means of Bayes factors (Wagenmakers, Morey, & Lee, 2016). We recommend researchers interested in these alternative approaches to p-values to consult Dienes (2008) for an excellent conceptual overview of all three approaches, and Wagenmakers et al. (2016), Wagenmakers et al. (2018), Dienes (2014), and Etz, Gronau, Dablander, Edelsbrunner, and Baribault (2018) for an extended overview of Bayesian approaches.

The competence model is presented in Figure 6. We based its conceptualization on multiple sources. First, the model is based on our own expertise regarding the correct application and interpretation of *p*-values, and multiple years of related teaching. The competence model is based on the question *what should researchers know or be able to do in order to be seen as competent users of p-values?* Based on this question, we took the following additional steps in developing the model. We conducted literature searches in the Google Scholar, PsycInfo and

ERIC databases with the keywords “p-value” AND “interpretation”, collecting information related to the present goal from the obtained literature (e.g., Greenland, 2017; Gagnier & Morgenstern, 2017, 2017; Greenland et al., 2016; Kühberger, Fritz, Lermer, & Scherndl, 2015; Lyu, Peng, & Hu, 2018; Wasserstein & Lazar, 2016). Furthermore, we gathered expert opinions from researchers on the social media platforms Twitter and Facebook, posing the question “*What should a researcher know, or be able to do, in order to be seen as a competent user of p-values?*”, providing them with more context about our competence model upon request. We also posed the same question on 14 graduate- and postgraduate-researchers in our local area who were part of an interest group for applied statistics. Finally, we introduced the present research at educational and methodological conferences, asking for further input from the expert audiences.

Competences	Level 1 Appropriate Application	Level 2 Elaborate Justification	Level 3 Informed Choice
Application	• Specify H_0 and H_1 • Determine significance level • Conduct simple power analysis and adapt sample size1 • Compare *p*-value with significance level • Decide: Reject or maintain H_0 • Decide: Accept or not accept H_1 • Apply corrections for multiple testing	• Specify and test non-Nilhypothesis as H_0 • Specify equivalence bounds and conduct equivalence test • Estimate statistical power for advanced statistical models and equivalence testing	• Select among *p*-values, effect sizes, confidence intervals, and alternative hypothesis testing approaches (e.g., Bayesian credible interval, Bayes factor, model information criteria, likelihood statistics)
Interpretation	• Interpret a calculated *p*-value • Describe rejection, maintenance, or acceptance of H_0 and H_1 with regard to context • Decline that non-significant *p*-value indicates evidence against H_1 • Decline that non-significant *p*-value indicates significant difference from significant *p*-value	• Interpret equivalence test • Decline that rejection of a H_0 can be interpreted as evidence against H_0 • Decline that maintenance of a H_0 can be interpreted as evidence in favor of H_0 • Decline that rejection of a H_0 can be interpreted as evidence in favor of H_1 • Decline that acceptance of the H_1 can be interpreted as evidence for H_1 • Decline that a *p*-value indicates the strength of effect • Provide correct interpretation of confidence interval • Decline that confidence interval indicates precision or likelihood of population effect	• Interpret and compare interpretations of *p*-values, confidence intervals, effect sizes, and alternative hypothesis testing approaches
Argumentation	• Provide definition of *p*-value • Provide frequentist definition of probability • Provide rationales for determined levels of significance and power • Explain that effect size estimate/*df* results in test statistic • Explain link between estimated distribution of test statistic and significance • Justify acceptance/maintenance of H_0 and H_1 based on the definition of a *p*-value and on the frequentist definition of probability • Argue that difference between effects has to be directly tested	• Justify H_0 with respect to content • Justify desired power with respect to content • Acceptance/maintenance of the H_0 and H_1 are elucidated based on distribution of *p*-values under H_0 and H_1 • Provide definition of confidence interval • Justify procedure and interpretation of equivalence tests • The importance of replications for NHST is elucidated with respect to the definition and distribution of *p*-values and frequentist probability • Elaborate with respect to frequentist probability, why confidence interval does not provide inform about probability of population effect. • Elaborate how effect size and sample size relate to *p*-values • Elucidate why multiple testing leads to alpha-error-inflation • Justify correction for multiple testing • Elaborate comparison of meaning of *p*-values between confirmatory and exploratory testing with and without a priori power analysis	• Justify choice among *p*-values, effect sizes, confidence intervals, and alternative hypothesis testing approaches in light of research question and study design • Explain statistical relations between *p*-values, effect sizes, confidence intervals, and alternative hypothesis testing approaches

Figure 6. Competence model for teaching of *p*-values and alternative approaches to hypothesis testing. Rows indicate different sub-competencies, columns different competence levels.

In the resulting model (Figure 6), we conceptualized the adequate usage of *p*-value as consisting of three sub-competencies: Being able to correctly apply hypothesis tests, being able to interpret them correctly, and being able to provide arguments for the correct application and interpretation, and against incorrect applications and interpretations. In addition to these three dimensions of competence, we conceptualized three levels of competence for each of these. The levels can be used to guide the teaching according to the level of competence that teachers deem appropriate to aim at with their students. If learners reach the first level, it means that they can validly apply, interpret, and argue about *p*-values in their most frequent ways of application. They can, for example, set up null- and alternative hypotheses, consider these and pre-determined alpha-error-levels and study-power-levels in a basic power analysis (i.e., a power analysis based on a common statistical model such as a *t*-test, linear regression, or ANOVA), interpret conducted hypothesis tests appropriately (which includes refutation of the two misinterpretations), and provide basic arguments for these procedures based on definitions of *p*-values and frequentist probability.

On the second competence level, learners are not just able to engage in the practices demanded at the first level; in addition, they are now able to provide detailed justifications for these procedures. They can refer for example to conceptual explanations of distributions of *p*-values under different hypotheses and deduce from these the rationales for the interpretation and usage of *p*-values. They also refute various additional misinterpretations of *p*-values and can provide conceptual explanations for their falsehood. On this level, another competence is mastered that goes beyond the most frequent applications of *p*-values: Learners are able to set up, interpret, and justify equivalence tests in order to make the most of non-significant *p*-values.

Finally, on the third competence level, researchers or learners are able to make an informed decision about whether they prefer using *p*-values, effect sizes, confidence intervals, equivalence tests, or other means of hypothesis testing (e.g., Bayesian credible intervals, Bayes factors, likelihood statistics, model information criteria, model fit criteria).

In order to support teachers in getting into the different aspects of the competence levels and their teaching, we provide descriptions of all levels of the sub-competencies in the Appendix and a list of reliable sources of background information and teaching materials for each of these levels. These include for example well-written and easily accessible explanations of misinterpretations and the correct understanding of *p*-values, free online statistical simulation tools that illustrate the rationales behind the different competencies, and links to free online lectures and courses.

Discussion

In this article, we provided three contributions for surveying and improving the utility of non-significant results for educational research. A literature review showed that in published educational research, non-significant results are frequently misinterpreted as indicating the absence of an effect, or a difference to a significant effect. Consequently, we provided a framework for the implementation of studies that can test whether a non-significant result really indicates the absence of an effect via equivalence testing, and a competence model for the correct usage of *p*-values, equivalence testing and related statistical methods. Our aim was to help researchers in implementing studies that can yield informative non-significant results, and in teaching the appropriate methods. Following, we first discuss the findings from the review, then the limitations of our contributions, and how these might be further developed and refined in future research.

Discussion of review results

The results from the reviews of both samples, from 2016- and 2017-volumes of less and more prestiguous peer-reviewed journals in educational research, suggested that researchers frequently misinterpret non-significant *p*-values as indicating evidence for the absence of an effect, or that the respective effect differs from another significant effect. Importantly, the review showed that these misinterpretations potentially impact the research process, and also educational practice and policy. We found that in more than half of the reviewed articles (overall: 52% - 31 out of 60), either of these misinterpretations was linked with conclusions for educational theory or research method, and in about a third of the reviewed articles (overall: 32% - 19 out of 60) with suggestions for educational practice or policy.

These results, first, indicate that misinterpretations of *p*-values do exist in educational science, with a rather high frequency in the reviewed journals. In addition, misinterpretations do rather frequently lead to unfounded theoretical conclusions and in various cases to practical educational suggestions and policy implications. The conclusion therefore is that misinterpretations in the reviewed journals, and likely in the broader field, are common, and that they have significant potential to hinder theoretical progress, inspire changes in classroom practices that might be empirically unfounded, and to also inspire potentially ineffective policy measures. Since previous reviews investigated other fields than education, we decided to conduct a rather in-depth-review of recent publications in educational science from sources with varying impact factor. Thus, the aim of this study was not to conduct a survey of the whole field. However, a careful selection of publication sources allowed a study of exemplary character that offers relevant and useful information for the field as a whole. We believe that these results show that complaints by methodologists pointing out these misinterpretations are not just statistical nitpicking, but they actually point towards an issue that demands attention because it has rather tangible consequences for researchers' conclusions.

From reviewing the articles, potential reasons became apparent *why* researchers tend to make the reviewed misinterpretations quite frequently, and even link them to different kinds of inferences: One possibility is that researchers apply and misinterpret non-significant *p*-values because they just want a tool for *knowing what is going on*. That is, they might want to use the *p*-value as an indicator of whether an effect was reliably present, or not. As our discussion of the meaning of *p*-values has shown, this is not what the *p*-value is equipped for. Its information value is highly limited and it can be reliably used only under highly controlled circumstances. The *p*-value only works properly for the control of alpha- and beta-errors if it is based on a proper power analysis with accordingly adjusted sample size, if the assumptions of the statistical model are met, and corrections for multiple testing are applied (Daniel Lakens, 2019). These prerequisites are for example not given when *p*-values are applied to statistical tests that have not been planned a priori in an exploratory manner (Chmura Kraemer, 2017; Rubin, 2017).

Another reason why researchers might engage in these misinterpretations is because they follow peer researchers' practices in published research. The causes for such processes are system-immanent: Authors and editors learn from each other and use articles as guidance that get a lot of attention and have high impact, and thus seem to work. One example where this is also frequently done is the comparison of experimentally assigned groups on background characteristics or on pretest measures (i.e., baseline comparisons). Such tests that yield non-significant results are commonly misinterpreted as evidence for the groups being equal. These unfounded randomization checks might work as a deceptive example, fostering the misconception of non-significant *p*-values implying the absence of effects (Gruijters, 2016; Schulz, Altman, & Moher, 2010). The same applies to a broader range of tests for which a non-significant result is often used as a hint for the null-hypothesis being true, such as the Levene test for homogeneity of variances, and the Shapiro-Wilk and Kolmogornov-Smirnov tests for deviations from normality (Gastwirth, Gel, Miao, & others, 2009). In order to prevent misinterpretations in future research, we recommend reviewers and editors to not ask for these practices and to make authors aware of these issues during peer review (Chmura Kraemer, 2017; Gruijters, 2016). For authors, we propose to think about other appropriate methods and to not apply commonly used methods just because others have applied them.

Another case which we would like to point out is that students are often discouraged when they find non-significant results in their thesis research. We would like to point out that a non-significant result is not a “bad” one, and refer students and their supervisors to Mehler et al. (2019) and D Lakens (2019) as informative resources regarding the handling of such situations in student research and further contexts with limited resources.

During reviewing the articles, we noted two reasons for inconsistencies in the codings of hypotheses and misinterpretations. We discuss these here because they might inform other researchers interested in coding hypotheses and related statistical tests and interpretations from published articles. The main reason for inconsistencies in the coding of hypotheses was that researchers tended to express hypotheses on different levels of granularity. Researchers employ varying levels of detail when describing their hypotheses, leaving room for interpretation of what accounts as a single hypothesis. An example would be a researcher expressing “Hypothesis 1: We expected that independently of gender, students would achieve learning gains on all three measures of achievement”. At first sight, this might seem like a single hypothesis. However, in all of the cases where researchers stated a hypothesis in this or a similar manner in the reviewed articles, they would then apply multiple statistical tests; six in this case: One for each of the three achievement measures, followed by an interaction test with gender on each of the three measures. After finding out that this was the main reason leading to differences in the number of counted hypotheses, we decided to code such hypotheses on the smallest level of granularity. That is, we would count such an expression as six different hypotheses. While this leads to the maximum number of hypotheses that one might count, coding in this way also ensures that all hypotheses from all sampled articles were coded on the same level of granularity.

The second main reason for inconsistencies were difficulties in deciding whether researchers actively misinterpreted non-significant *p*-values as indicating evidence for the absence of an effect. In many cases, it was not clear whether researchers in their interpretations aimed at expressing that a result was not significant or an effect was not reliably found, or whether they aimed at expressing that the result actually indicates evidence for the absence of the effect. We coded interpretations only as misinterpretations if, at some point, it became clear that researchers aimed at expressing the latter. An example where this is not clear would be a researcher finding a non-significant result and providing the interpretation "we could not find an effect of x". We would not count this as a misinterpretation. However, sometimes at a later point, usually in the discussion, researchers would put forward more detailed discussions of such a finding, such as "it is not clear why we could not find an effect of x. In contrast to earlier studies, we kept the study time shorter, which might have limited students' potential to thoroughly work on the demanding learning materials". In such a case, it becomes rather clear that researchers misinterpreted the absence of significant evidence as evidence for the absence of the effect, and then started discussing why the effect was not there. In general, we were rather conservative in our codings and we did not code interpretations as misinterpretations if their meaning was not clear.

Limitations and Future Research

In order to ensure interrater-reliability, we identified and coded hypotheses in the reviewed papers independently and blinded from each other. This allowed examining the degree to which focused review work might be biased by subjective codings. This goes beyond prior studies in which the identified hypotheses were not compared between multiple raters. We also compared codings of the further steps, however in an informal manner by comparing tabular sheets in which we had noted down the hypotheses and related tests and interpretations. We suggest that in similar future reviews, further measures of interrater-reliability should be taken, for example for the further steps in coding such as assigning statistical tests to hypotheses and coding whether a misinterpretation was present.

A further limitation is the limited number of reviewed articles from a selective sample of educational research journals. We decided to remain with a rather small sample but to read and discuss each article in detail, which was demanding regarding the time and expertise required for properly coding the different features. Future research might broaden the selection of journals to a random sample and take a random sample of articles from all of these. Random sampling, particularly with a sufficiently high number of reviewed articles, might enable conducting generalizable, targeted statistical analyses on the prevalence of misinterpretations and how they vary across journals. However, random sampling comes with other difficulties, such as defining the population of articles from a field that fall into the category of interest. Another option would be to investigate the development of misinterpretations across time, potentially in interaction with the respective journal.

Another limitation is that we only gauged the potential impact of misinterpretations within the respective articles. It could be argued that interpretations found in published articles are not directly linked to educational theory, practice, and policy. We do believe, however, that researchers reading each others' work are influenced by misinterpreted findings and will therefore likely develop research projects which are themselves based on the shaky foundation of misinterpreted results. In addition, interpretations of individual studies are commonly portrayed in media reports in which researchers' interpretations are taken at face value. Policy makers themselves might not always rely on media reports but rather on other sources such as meta-analyses conducted on the research on specific educational topics. It could be argued that the misinterpretations we identified will not go into meta-analyses, because usually there only statistical estimates are used. Meta-analyses, however, suffer from the same misinterpretations of p-values as regular reports of individual research studies.

Therefore, we would like to point out that even meta-analyses are not less susceptible to the present misinterpretations. What we additionally commonly observe is that in meta-analyses, more weight is put on meta-analytic effect sizes rather than statistical significance; here, we would like to point out that using the common rules of thumb on classifying effect sizes as *small*, *medium* or *large* (such as those attributed to Cohen (1988)) in educational meta-analyses is not necessarily more informative than *p*-values, be they misinterpreted or correctly used.

The frequency of non-significant *p*-values, which made up for 40% of all reviewed hypotheses, was much higher than the number of negative results (i.e., outcomes not in line with predictions) reported elsewhere. For example, Cristea and Ioannidis (2018) and Fanelli (2012) reported much lower numbers of just 6–14% of negative results in published literature. From one further study where this was not in the focus of analyses, a number of almost 30% of non-significant results among *p*-values independently of whether these were related to explicit hypotheses or not is apparent (Aczel, Palfi, & Szaszi, 2017). The influential study by Fanelli (2012) used a different method for counting negative results: Abstracts and/or full texts were screened until the first hypothesis appeared, and then it was coded whether for that hypothesis the authors concluded to have found a partially or fully positive result (which were both counted as positive results), or a negative result. Since the study by Fanelli (2012) has had profound impact on the methodological and meta-science communities, we wanted to investigate potential reasons for the deviation of the findings of Fanelli (2012) from ours. In order to determine whether our results were inconsistent with those by Fanelli (2012) due to the methodological differences, we re-coded the 60 articles in the same manner as described there. The result from this re-coding was that according to Fanelli's criteria, only 7 articles (12%) presented negative results, yielding a percentage that lies well within the reported results (6–14%) from Fanelli (2012). Yet, this is in stark contrast to our codings. We would like to point out that the coding based on Fanelli's method appeared very demanding to us, and we could not imagine using this approach to code articles from fields we are not experts in.

In future research, the competence model we developed could be used to inform educational interventions to foster students' or researchers' understanding of *p*-values and alternative approaches. For example, depending on the specific contents of an educational sequence, it might then be assessed whether learners do actually develop through the levels of the competence model in a step-wise manner. We believe that this highly depends on the educational approach and goal chosen: At universities where quantiative education is not strongly in focus, probably there is not sufficient time to delve into the specifics of frequentist statistical theory, and instructors might focus on reaching level 1 at the end of their courses. Reaching level 1 might be sufficient in order to become a critical consumer of published research articles, and a practically skilled user of *p*-values. At more research-intensive universities, however, particularly for graduate and post-graduate education, instructors might want to aim directly at supporting students' competencies at level 2 and level 3.

Education aimed at these higher goals will probably use different educational means (e.g., using statistical simulations), which will trigger different developmental trajectories in learners. We hope that researchers and instructors will use the sources provided in the appendix as a useful assistance for their teaching endeavors on the different levels.

We would like to clarify that we do not want to accuse specific researchers of having misconceptions of *p*-values. All of us have misconceptions, regarding various topics in statistics and beyond, and engaging in educational research requires life-long learning. Both of us have put many years into developing our understanding of *p*-values and alternative statistical methods. By exploring and exemplifying misinterpretations, we aim at shedding light on common formulations that might entrench misconceptions, and critically discuss the content and implications of works that include forms of misconceptions, as criticism is the bedrock of scientific method (Bishop, 2018).

In order to set a more positive mood and future outlook, we end with interesting facts about *p*-values that are related to the topics that we have treated in this paper. We think this is the right way to end this article because it started with a rather daunting perspective; showing up that so many educational researchers misinterpret *p*-values, and this actually has adverse consequences. We would like to set the focus on treating *p*-values as an interesting but limited tool among many others that we can use to try to advance education and its research.

Fun-Facts You Always Wanted to Know (?) About *p*-Values

- The *p*-value has a *p* because it stands for a certain *p*robability, but in frequentist statistics, *probability* means a frequency of non-observed events (Wagenmakers, 2007); so, the *p*-value really means something like a hypothetical *rareness*.
- There is a full Wikipedia article devoted to misinterpretations of *p*-values (Misuse of p-values,

2019).

- Under the null-hypothesis, *p*-values are uniformly distributed. In other words, if there really is no effect, it is equally likely to obtain any specific *p*-value. For many researchers, this is counter-intuitive, because they assume that if there is no effect, high *p*-values occur much less frequently than lower ones. This is also the reason why people tend to say that when the null-hypothesis is true, you will find 1 in 20 effects being significant (if α equals 5%). For a simple illustrative simulation of this fact, set the real effect to 0 and look at the distribution of resulting *p*-values under http://bit.ly/simplepvaluesim.
- There are tools that check for inconsistencies in reported *p*-values and their respective test statistics and degrees of freedom (e.g. statcheck; Epskamp and Nuijten, 2018). Running such a tool on the 60 reviewed articles detected 229 *p*-values and their respective statistics, and it indicated inconsistencies or reporting errors in 45 (19.6%) of these cases (for a report see https://osf.io/da7x3/).
- With enough statistical information provided, it is possible to compute via the statistical tool of *Bayes factors* the extent to which the null hypothesis is more likely than the alternative hypothesis, for basic statistical tests in published articles (Faulkenberry, 2019); unfortunately, this is not possible for most of the reviewed articles, because they report too few details or too complex statistical models.
- We like the *p*-value because it is so difficult to use and interpret that we could write this whole, hopefully useful, article about it.
- We used the term "*p*-value" 100 times in this article.

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Appendix

Sources and References for the Competence Model

A descriptive version of the different levels of the sub-competencies of the competence model for hypothesis testing in educational research with a focus on non-significant results is provided in Figure A1.

Competence Level I: Correct Implementation and Interpretation

The sources for the first level of the competence model describe misinterpretations of *p*-values and confidence intervals and suggested solutions, as well as central issues around effect sizes and power analysis.

1. **Cassidy, Dimova, Giguere, Spence, & Stanley (2019)**: *Failing grade: 89% of introduction-to-psychology textbooks that define or explain statistical significance do so incorrectly*. A review of the definition of "significance" in 30 textbooks used in the US and Canada, finding that undergraduate students are often confronted with incorrect explanations of significance.

2. **Alderson (2004)**: *Absence of evidence is not evidence of absence. We need to report uncertain results and do it clearly*. A concise editorial discussing an example where non-significant effects with rather big confidence intervals are interpreted to indicate that a treatment does not reduce HIV-1 incidence. Discusses how the specification of equivalence bounds might help in such cases, and how non-significant results might be interpreted in light of estimated confidence intervals around effect sizes.

3. **Gelman & Stern (2006)**: *The difference between "significant" and not significant" is not itself statistically significant*. Addresses the misinterpretation that one significant and another non-

significant effect differ significantly from each other. Shows interesting examples of this fallacy on the birth order effect on sexuality as well as on effects of low-frequency electromagnetic fields.

4. **Spence & Stanley (2018)**: *Concise, simple, and not wrong: In search of a short-hand interpretation of statistical significance*. Looking for a concise and valid interpretation of statistical significance, the authors come up with the solution of saying “the true effect may not be zero” or “there is reason to doubt that the true effect is zero”. Includes discussions of many misinterpretations and linguistic nuances that might help statistics teachers improve their language to prevent the development of misconceptions in learners.

5. **Krueger & Heck (2017)**: *The heuristic value of p in inductive statistical inference*. Discusses how *p*-values perform from the heuristic perspective of whether they indicate to researchers whether the alternative hypothesis they seek to test is true, as well as the probability of reproducing a significant result, depending on various factors such as sample size. Also relates the heuristic value of *p*-values to alternative statistics and discusses how these might complement each other from an applied research perspective.

6. **Lakens (2013)**: *Calculating and reporting effect sizes to facilitate cumulative science: A practical primer for t-tests and ANOVAs*. Describes how to calculate effect sizes for difference hypotheses in various contexts and designs. Explains basic constructs as power, the beta-error as well as the use and interpretation of effect sizes to inform about the magnitude of an effect in meta-analyses or for future research.

7. **Fern & Monroe (1996)**: *Effect-size estimates: Issues and problems in interpretation*. Explains why effect sizes should be interpreted within the context of the research question and study field. Discusses factors that affect the judgment of the size of an effect.

8. **Greenland et al. (2016)**: *Statistical tests, p values, confidence intervals, and power: A*

guide to misinterpretations. Easy-to-read theoretical guide. Provides an overview of 25 misinterpretations of inferential statistics and some critical discussion around these.

9. **Castro Sotos, Vanhoof, Van den Noortgate, & Onghena (2007)**: *Students' misconceptions of statistical inference: A review of the empirical evidence from research on statistics education*. An excellent review and discussion of empirical research on university students' misconceptions about sampling distributions, hypotheses tests, and confidence intervals, including papers from 1990 to 2006.

10. **Perugini, Gallucci, & Costantini (2018)**: *A practical primer to power analysis for simple experimental designs*. Explains the rationale of power analysis and its application on a whole spectrum of designs with detailed explanations in G*Power.

Competence Level II: Equivalence Testing, Advanced Power Analysis, and Conceptual Foundations

These sources discuss equivalence testing, power analyses of advanced yet common types of statistical models, and they dive deeper into the conceptual foundations underlying p-values, effect sizes, and confidence intervals.

11. **Lakens, McLatchie, Isager, Scheel, & Dienes (2020)**: *Improving inferences about null effects with Bayes factors and equivalence tests*. Presents and describes the SESOI approach and as well as the use of Bayes factors to evaluate whether an effect is absent or not. The authors apply both approaches on 4 examples, where descriptive analysis allowed a re-analysis.

12. **Ly et al. (2018)**: *Bayesian reanalyses from summary statistics: A guide for academic consumers*. Describes tools for Bayesian reanalysis of frequentist results, in order to judge their robustness. Based on famous examples from social psychology, the reanalysis shows how frequentist results can be interpreted differently with Bayesian methods. The paper also explains

how to interpret Bayes Factors and plots for robustness checks with the free JASP software package.

13. **Aczel et al. (2018)**: *Quantifying support for the null hypothesis in psychology: An empirical investigation*. Surveys the prevalence of the misinterpretation that $p > .05$ indicates absence of evidence in prestigious psychology-journals, with the result that 72% of non-significant results are misinterpreted. Also finds by means of Bayes factors that only in about 5% of cases, the reviewed non-significant results provide substantial evidence in favor of the null hypothesis.

14. **Wetzels et al. (2011)**: *Statistical evidence in experimental psychology: An empirical comparison using 855 t-tests*. The authors compare *p*-values, effect sizes and Bayes factors calculated from 855 *t*-tests and conclude that in most cases these estimates agree but differ in the magnitude of their support. The three tools are also explained and compared in detail.

15. **Arend & Schaefer (2019)**: *Statistical power in two-level models: A tutorial based on Monte Carlo simulation*. Describes in a tutorial how to calculate power in common two-level hierarchical models and shows via simulation studies, which sample sizes are required to reliably estimate effects at each level as well as cross-level effects.

16. **Schoemann, Boulton, & Short (2017)**: *Determining power and sample size for simple and complex mediation models*. Conducting power analysis in mediation models is complex. This article discusses the concept of power and how power is usually calculated for mediation models. The authors present a Shiny app to conduct power analysis on mediation designs.

17. **Bolger, Stadler, & Laurenceau (2012)**: *Power analysis for intensive longitudinal studies*. A gentle tutorial on how to conduct power analyses for autoregressive models in the SAS, SPSS, and Mplus software packages.

18. **Pfannkuch, Wild, & Parsonage (2012)**: *A conceptual pathway to confidence intervals*. Theoretical paper. Proposes a sequence of technology-enhanced practice tasks aimed at developing conceptual understanding of confidence intervals in students from age 14 to first-year university students. Provides a good overview of literature on the teaching of confidence intervals, but also some debatable conceptual depictions that should be read with care (e.g., equating level of confidence with probability, which might lead to the mixing up of Bayesian and frequentist ideas, see Morey et al., 2016).

19. **Morey et al. (2016)**: *The fallacy of placing confidence in confidence intervals*. A critical and excellent discussion of interpretations of confidence intervals as indexing the precision of estimates, of plausible or reasonable parameters values, and of their width (e.g., 95%) as indexing the plausibility that the true parameter value is included. In our perspective a central reference for boosting one's understanding of how the frequentist conceptualization of probability critically influences and limits interpretations of inferential statistics.

20. **Rubin (2017)**: *Do p values lose their meaning in exploratory analyses? It depends how you define the familywise error rate.* Discusses the applicability of *p*-values in exploratory analyses, which has been criticized as uninformative due to inflated alpha-errors. The author argues that the alpha-level only needs to be adjusted on the level of the same null hypotheses and not for a whole study.

Competence Level III: Comparison and Integration of Multiple Approaches

The following sources focus on the combined discussion of multiple statistical approaches to hypothesis testing and effect size estimation, their advantages and disadvantages, and statistical and conceptual relations. On this level, free online-resources for exploring and teaching related concepts come into play that can support researchers and learners in developing deeper conceptual understanding of multiple approaches to statistical inference. The online-sources provide free online-tools such as Shiny-apps that provide researchers with written discussions and informative visualizations of different statistical approaches to hypothesis testing and effect size indicators. We have used all sources for informing ourselves and for teaching, and we find them excellent.

21. **Fidler & Cumming (2005)**: *Teaching confidence intervals: Problems and potential solutions*. A discussion on the advantages of switching the focus of teaching from *p*-values to effect sizes with confidence intervals, based on empirical literature and extensive teaching experience of two experienced researchers.

22. **Morey, Rouder, Verhagen, & Wagenmakers (2014)**: *Why hypothesis tests are essential for psychological science: A comment on Cumming*. A concise, critical discussion of the replacement of *p*-values by effect sizes and confidence intervals, encompassing important issues on the roles of parameter estimation and hypothesis testing.

23. **Wagenmakers (2007)**: *A practical solution to the pervasive problems of p-values*. Discusses frequent misinterpretations of *p*-values and suggests applied researchers to apply instead an approximation of the Bayes factor, based on the BIC, a common model information criterion.

24. **Etz, Gronau, Dablander, Edelsbrunner, & Baribault (2018)**: *How to become a Bayesian in eight easy steps: An annotated reading list*: Provides an overview of literature that introduces central concepts of Bayesian statistics. Some of the discussed sources provide excellent discussions of how Bayesian statistical tools compare to frequentist and other tools, statistically and conceptually.

25. **McElreath (2018)**: *Statistical rethinking: A Bayesian course with examples in R and Stan*. A great statistics textboook on an intermediate level that manages to provide an insightful overview

of different approaches to statistical inference from a Bayesian perspective. Perhaps one of few books that manage to avoid many misconceptions that can often be found in introductory statistics textbooks (Cassidy et al., 2019).

26. **Kruschke (2011)**. *Bayesian assessment of null values via parameter estimation and model comparison*. An excellent and brief comparison of Bayesian estimation, Bayesian model comparison, and frequentist hypothesis testing as three approaches to the evaluation of null results. Be aware that this article is written by a strong proponent of the Bayesian estimation-approach!

27. **rpsychologist.com**: *RPsychologist*. A blog by Kristoffer Magnusson in which he provides free and excellent visualization tools that can be used for the teaching and furthering one's own understanding of equivalence testing, Cohen's *d*, and further statistical indices and tests.

28. **shinyapps.org**: *ShinyApps: Experience Statistics*. A compendium of various useful and free online-apps for computing and visualizing statistical indices and data distributions.

29. **coursera.org/learn/improving-statistical-questions:** *Improving your statistical questions*. A free online course by Daniel Lakens covering, amongst many other topics included in the competence model, how to design studies that are informative even if the results lead to non-significant effects, how to define the SESOI and how to justify the sample size based on that.

30. **Dienes (2008)**: *Understanding psychology as a science: An introduction to scientific and statistical inference*. A book, yet a rather concise one, embedding the frequentist, Bayesian, and likelihood statistical approaches in the general endeavor of creating scientific knowledge from the perspective of psychological science.

Competences	Level 1 (sources 1 - 10)	Level 2 (sources 11 - 20)	Level 3 (sources 21 - 30)
Application	On Level 1, concering the Application of *p*-values, they are used to test a specified Null-hypothesis based on a simple power analysis and adaptation of sample site and based on the *p*-value this H_0 is rejected and the H_1 is accepted or the Null-hypothesis is maintained and the Alternative hypothesis is not accepted.	On Level 2, Application of p-values is extended to non-nil hypotheses, equivalence tests are correctly set up and conducted, and power analyses are implemented for advanced statistical models.	On Level 3, individuals make an informed choice between Application of p-values and alternative hypothesis testing strategies and apply these correctly.
Interpretation	Interpretation: Calculated p-values can be interpreted with respect to content (and context), rejection of the H_0 and acception of the H_1 can be elucidated with respect to content, common misinterpretations are avoided.	Interpretation: Interpretations of *p*-value as evidence for or against hypotheses or as strength of evidence are rejected. Equivalence tests are appropriately interpreted.	Interpretation: Alternatives to p-values can be interpreted and the interpretations obtained from the different approaches are compared with regard to research context and research question.
Argumentation	Argumentation: Definition of a *p*-value and the frequentist definition of probability can be expressed.	Argumentation: It is elucidated why *p*-values cannot be interpreted as evidence for or against the H_0 and H_1 and why a *p*-value does not indicate the strength of an effect, based on the frequentist definition of probability and the distribution of *p*-values under the H_0 and $H_{1.}$ Implementation and interpretation of equivalence tests are elucidated conceptually. Correct interpretation of a confidence interval is justified conceptually, considering the frequent conception of probability. Correction for multiple testing is elucidated conceptually.	Argumentation: Selection among p-values and alternatives is substantiated and justified in light of research question and study design, as well as based on explanations of commonalities and differences between the different approaches.

Figure A1. Descriptions of the three levels of sub-competencies of the competence model for hypothesis testing.