How (not) to Correct a “Mistake”: A True Tale from the Journal of Applied Psychology

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Abstract

With this commentary, we pursue the following four goals: (1) Publicly acknowledging that we—of course—care about the correctness and integrity of the scientific record and therefore also support the correction of clear mistakes in our own manuscripts. We further also acknowledge the important function that anonymous readers play in this context who identify possible mistakes in the scientific record. (2) Providing transparency concerning the correction of our article (Breuer et al., 2016) that was issued by the Journal of Applied Psychology (JAP) on the 6th of April 2026. To do so, we provide a timeline with all details that are necessary to comprehend the whole process preceding the issuing of the correction, starting from an anonymous reader’s message to the then Editor-in-Chief of JAP in late 2024 until the publication of the correction in April 2026. (3) Pointing out the problems, which occurred during the exchange with the then Editor-in-Chief of JAP and which still remain. (4) Raising questions and providing suggestions on (i) how the process of correcting mistakes should unfold, (ii) how corrections should be formulated, and (iii) where possible limits of corrections of published scientific papers are. We thereby want to contribute to the needed transparency in the case of the correction of our article, but also to the question of how the process and results of corrections should—and perhaps also should not—be designed.

How (not) to Correct a “Mistake”: A True Tale from the Journal of Applied Psychology

Preamble

Before delving into the details of how the correction of our manuscript (Breuer et al., 2016) came about, we want to clarify our position with regard to three aspects. First, we (of course) do care about the correctness and integrity of the scientific record, and therefore strive for correctness in our own work. Thus, we are ready to admit and correct own mistakes. Second, we believe that readers can play an important role in this context even after an article was accepted for publication after a thorough peer review process. They often detect mistakes and point them out to journal editors who can then check these mistakes and, if they were correctly identified and deemed sufficiently consequential, address them for instance by issuing corrections. Third, we do believe that science is best served if the involved parties focus on the subject matter, act respectfully and avoid “ad hominem attacks”, and follow the scientific principles of transparent and referenced argumentation. This is what we want to do in this commentary. We therefore try to report the events leading up to the correction of our manuscript as matter-of-factly as possible by focusing on what was written and argued, while focusing as little as possible on the tone that was used in the exchange or on the identity or behavior of the involved persons. Furthermore, to avoid offending people involved in the process, we only cite from the continued email exchange we had with the then Editor-in-Chief of the Journal of Applied Psychology (JAP) if it is truly necessary to understand important details of the process.

The Methods Used in Breuer et al. (2016)

In 2016, we published our meta-analysis in the JAP (Breuer et al., 2016). The meta-analysis investigates the relation of trust (predictor) with different indicators of team effectiveness (criterion) while also testing moderators of this relation such as team virtuality and documentation of team processes. Overall, we conducted eight categorical moderator

analyses (see Table 3 in Breuer et al.), and all eight moderator analyses turned out to be statistically significant.

In the course of the review process of our meta-analysis (for details, see Table 1), we had been asked to use the Schmidt and Hunter approach to meta-analyses (Hunter & Schmidt, 2004) instead of the multi-level meta-analysis which we had used in our first submission to accommodate the nestedness of the data (i.e., effect sizes within studies and studies within manuscripts). We followed this request, and carefully implemented the steps described by Hunter and Schmidt (2004) in their chapter “Meta-Analysis of Individually Corrected Correlations” (starting on p. 120). Specifically, we took the following three steps to arrive at the results from our categorical moderator analyses (as reported in Table 3 in Breuer et al., 2016):

First, we individually corrected the predictor–criterion relationship from each study for unreliability. To do so, the compound attenuation factor A (see equation 3.28 in Hunter & Schmidt, 2004) was used to determine the corrected study correlation r_c (see equation 3.29 in Hunter & Schmidt, 2004). Also following Hunter and Schmidt (2004), we computed the sampling error variance in the uncorrected correlation, Var*(eo)* (see equation 3.30 in Hunter & Schmidt, 2004), which we then also corrected for measurement error in predictor and criterion (resulting in Var*[ec]*). In order to aggregate effects across primary studies, we determined a weight for each primary study, again following Hunter and Schmidt (2004, see pp. 123-124). Second, to obtain the best estimate of the population correlation ρ, we used the SPSS macro by meta-analysis expert David B. Wilson (see https://mason.gmu.edu/~dwilsonb/MetaAnal.html). We made sure that we did what the macro requires (“the macro assumes that you have already applied any needed transformations to analyze the effect size. For example, you have converted […] correlations (r) to Fisher’s z_r […]”; see the website). Third, we used the output obtained from the SPSS macro by Wilson as inputs for a *t*-test formula that is provided in Aguinis et al. (2008) specifically for categorical

moderator analyses. It should be noted that we submitted our meta-analysis originally as a full paper, but it had been accepted as research note requiring considerable cuts also in the description of the methods used.

The Process Leading up to the Publication of the Correction of Breuer et al. (2016)

In late 2024 (nearly nine years after the publication of Breuer et al., 2016), we were contacted by the then Editor-in-Chief of the JAP, who informed us that a concern was brought to her attention (an anonymous reader as it later turned out). In our first reaction to her initial email, we could successfully address this concern, which was not part of our conversation again. In addition, the then Editor-in-Chief raised two other concerns from her own reading of our published meta-analysis. One of them was also successfully addressed in our first email response and was not part of our conversation again. Only the second concern of the then Editor-in-Chief remained. Unfortunately, the then Editor-in-Chief did not provide us with any references or literature on why she believed that aspects of our meta-analyses could be problematic. This feature remained unchanged for a long time in our exchange, which overall lasted about twelve months.

The remaining concern focuses on our use of the *t*-test formula provided by Aguinis et al. (2008) for categorical moderator analyses. In their manuscript published in *Organizational Research Methods*, Aguinis et al. (pp. 16-17) introduced their formula as follows:

> “Specifically, we used the following equation (Neter, Wasserman, & Whitmore, 1988, p. 402):
>
> $$t = \frac{|\bar{r}_1 - \bar{r}_2|}{\sqrt{\frac{Var(r_1)}{k} + \frac{Var(r_2)}{k}}}$$
>
> where $\bar{r}_1$ and $\bar{r}_2$ are the average corrected correlations for each of the moderator-based subgroups and Var(r_1) and Var(r_2) are the variance in these correlations. These values are all calculated using the same method as the overall estimates (from step 6 above),

> except that there is a separate estimate for each of the groups under examination in the study. Note that either $\bar{r}$ −based or ρ−based notations can be used because $\bar{r}$ is the best estimate of the population correlation ρ.”

For our commentary, the last sentence of this quotation is particularly relevant, because Aguinis et al. (2008) clearly state that the formula can be used for two types of data: First, it can be used for data that are corrected for measurement error ($\bar{r}$ in the Aguinis et al. formula and in our manuscript or rc in Hunter & Schmidt, 2004) and second, it can be used for data corrected for both, measurement error and sampling error (ρ). Please note at this point that the formula along with these explanations has been published in the probably most prominent methods journal in the field (*Organizational Research Methods*) and there has been no criticism, addition, modification, or correction of the formula in the literature that we could find.

In accordance with Aguinis et al. (2008), we applied the formula in the following way—that is, we used data that was corrected for measurement error and sampling error (ρ) in the numerator and denominator:

Equation 1:
$$t = \frac{|\rho_1 - \rho_2|}{\sqrt{\frac{Var_{\rho 1}}{k1} + \frac{Var_{\rho 2}}{k2}}}$$

The then Editor-in-Chief of JAP and her associate editor and meta-analysis specialist, whom she added to the process early in the ongoing conversation (see Table 1, for details), however, claimed that this formula can only be correctly used as follows:

Equation 2:
$$t = \frac{|\rho_1 - \rho_2|}{\sqrt{\frac{Var_{rc1}}{k1} + \frac{Var_{rc2}}{k2}}}$$

Thus, interestingly, they claim that using a “mismatch” of estimates is the right way to use this formula, i.e., estimates in the numerator corrected for measurement and sampling

error (ρ), and variances in the denominator only corrected for measurement error (Var_{rc}). Indeed, this version of the formula can be delineated when considering that independent sample *t*-tests use the standard error as the denominator. A formula for the standard error of $\bar{\rho}$ was introduced by Hunter and Schmidt (2004, p. 206):

Equation 3: $$SE\bar{\rho} = SD_{rc} / \sqrt{k}$$

“where “$SE\bar{\rho}$ is the *SE* of $\bar{\rho}$, SD_{rc} is the *SD* of the correlations after each has been individually corrected for measurement error and other artifacts, and *k* is the number of studies“ (p. 206). Hence, the SE for the mean difference of ρs would indeed result in

Equation 4: $$SE = \sqrt{\frac{Var_{rc1}}{k1} + \frac{Var_{rc2}}{k2}}.$$

The right part of the equation could then be implemented in the denominator of the Aguinis et al. (2008) formula.

Using either the formula presented as equation 1 (as done by Breuer et al., 2016) versus the formula presented as equation 2 (as done by the then Editor-in-Chief of JAP and her associate editor) produces differing results. This is due to the variance estimates in the two versions of the formula (Equation 1 and 2) being vastly different. Hunter and Schmidt (2004, pp. 206-207) accordingly write that “It is important to note that SD(rc) ≠ SD(ρ). SD(rc) is much larger than SD(ρ), because SD(rc) has not been corrected for sampling error—and rc values typically have much sampling error“. Thus, if the denominator increases (because of the use of rc) while the numerator remains the same (when using ρ), the resulting *t*-values become smaller and the chance of observing significant results from the *t*-tests shrinks accordingly. Consequently, five of the previously eight statistically significant moderators in our meta-analysis (see Table 3 in Breuer et al., 2016) were not statistically significant when using the formula as interpreted by the then Editor-in-Chief of JAP. Please note that this is the central point of the correction published on April 6th 2026.

The central question here is, which version of the formula is more appropriate or whether it is possible at all to say that the use of one formula is more appropriate than the other. After a debate of about a year, we believe that—based on the current scientific state-of-the-art—there are two possible answers to the question. Our perspective is based on three elements: (i) the published formula by Aguinis et al. (2008) that was not corrected to this day—Aguinis et al. clearly state that it is possible to use their formula for “either $\bar{r}$ −based or ρ−based notations” (p. 17). Importantly, they do not mention any necessary adaptations or corrections to their formula when using ρ−based notations. (ii) Various meta-analyses using “our” approach (for details, see below), and on the expertise of various colleagues that were so kind to consult us in the process (for details, see below).

The perspective of the then Editor-in-Chief of JAP and her associate editor is different. Their argument rests on the formula for $SE\bar{\rho}$ as introduced by Hunter and Schmidt (2004, see equation 3 above).

From a neutral perspective, one may thus argue that, since the introduction of the formula by Aguinis et al. (2008), the scientific community was left with two alternative approaches on how the formula can be used. In our view, both approaches may be considered legitimate given the current state-of-the-art and the information available from the relevant literature. However, with the current correction of our meta-analysis, the then Editor-in-Chief of JAP did not acknowledge this state-of-the-art and the various meta-analyses that took “our” approach. Instead, they decided that our approach is incorrect, thereby taking a strong position in a matter that—at least in our opinion and in the opinion of those that have used “our” approach before—should be up for debate. Obviously, this decision raises various questions and problems.

Questions about and Problems with the Process and the Correction of Breuer et al. (2016)

In the following, we will list two questions and four problems concerning the way the correction of Breuer et al. (2016) came about (see Table 2, for an overview). We will list these questions and problems in the order, in which they emerged in the process leading up to the correction. Especially our (preliminary) answers to the questions point at possible improvements of the process and decision whether and how to issue a correction of a mistake in the scientific record.

First question: **Should possible methodological mistakes in manuscripts be corrected if the methods were transparently reported, peer-reviewed, and accepted by a journal? And if so, for which time frames?** In the case of Breuer et al. (2016), we were transparent during the review process in our response letters and in the accepted manuscript about the methods we used and on how we analyzed our data. In particular, the categorical moderator analyses in question were explicitly addressed already during the review process back in 2015. Admittedly, our descriptions in the published meta-analysis are somewhat less extensive than in the response letters because we had to change the format of our manuscript in the review process from a feature article (i.e., a full-length research article) to a research report (< 19 pages). We are not sure whether issuing corrections for articles is the best choice if these articles' methods and results had been fully peer-reviewed and accepted, but we agree that this can be justified. However, if the answer to the first question is "yes", the next question is trickier: For which time frames should corrections be issued? It is well-known that the scientific record contains quite a few mistakes concerning methods and results and that knowledge about adequate methods is constantly rising. Our article was corrected after about ten years—what would be a reasonable time frame for issuing corrections? We do not have an answer to this question, but there are obviously clear limits to this approach.

First problem: **Our efforts to provide information and arguments for our use of the Aguinis et al. formular were not sufficiently heard and integrated in the process leading to the correction.** In our extensive exchange with the then Editor-in-Chief of JAP, we took many measures to provide information and arguments for why our version (Equation 1, see above) of the Aguinis et al. (2008) formula was justified. Among other things, we (i) consulted with various experts on whether our use of the formula was correct or not, (ii) we re-calculated the results of another meta-analysis published in JAP (Rockstuhl et al., 2012)[1] with our version of the Aguinis et al. (2008) formula (Equation 1), leading to the same results as in the published paper (Rockstuhl et al., 2012), and (iii) we searched for other meta-analyses that in all likelihood had used the formula in the same way as we did. And while all these efforts suggested that the question of “right” and “wrong” is not easy or even clear to answer in our case, these efforts did not seem to play a role in the decision-making process of the then Editor-in-Chief of JAP.

Concerning (i): In our consultations with several experts, it quickly became obvious that there are different perspectives on the question of whether our use of the Aguinis et al. (2008) formula was correct or not. For instance, a renowned professor for psychological methods confirmed that our reading of the introduction of the formula by Aguinis et al. (2008; see the quotation above) is correct, and that our use of the formula accords with this introduction. Moreover, another professor for statistics who also does research on methods of meta-analyses provided mathematical arguments why our use of the formula was correct. Another expert, a professor for Industrial and Organizational Psychology, who is a renowned meta-analysis expert who also teaches meta-analysis as a research method, noted that the

[1] We appreciate the important work by Rockstuhl et al. (2012) and it was helpful for us when we conducted our own research. When we mention their meta-analysis in different places of our commentary, these various mentions are due to the fact that we were recommended the meta-analysis in the original review process and that a substantial part of our exchange with the then Editor-in-Chief of JAP was about it. Importantly, however, we do not mean to single it out and we do not suggest that it needs a correction. Their approach to categorical moderator analyses is widespread in the literature and we believe that correcting single meta-analyses in such a situation (as in our case) is no reasonable approach to a widespread research practice.

mismatch of using ρs in the numerator and rcs in the denominator within the same formula—as applied by the then Editor-of-chief of JAP—appears dubious to him. Last but not least, we reached out to Prof. Aguinis as the first author of the published formula in question, and he wrote that he agreed with our interpretation of their article. Of course, we forwarded all this information to the JAP (including the mathematical arguments provided by the professor for statistics). The perspectives of these experts were hardly referred to and appeared to have been quickly discounted or even completely dismissed, although these perspectives clearly question whether we in fact made a mistake. Thus, there seems to be much more unclarity regarding a possible mistake of ours than the JAP communicates. Moreover, none of these issues and unclarities are mentioned in the correction to our meta-analysis issued in April 2026.

Concerning (ii): In the first round of our review process in 2014, the responsible Associate Editor of JAP had recommended the meta-analysis by Rockstuhl et al. (2012, JAP) as an example for how we could, in addition to the corrected effect sizes, also report uncorrected effect sizes (see Table 1, for details). We liked how Rockstuhl et al. dealt with this methodological detail and also checked their general methodological approach. Their analyses further paralleled in other respects what we were told to do in the review process, so we decided to model our approach after theirs in other aspects as well. For instance, Rockstuhl et al. had also used the formula by Aguinis et al. (2008) and we decided to also follow their example in this respect and to use this formula for our categorical moderator analyses.

After informing the then Editor-in-Chief of JAP about this aspect, she wrote that she and her colleague from the editorial board had “largely reproduced” the results reported in Rockstuhl et al. (2012) when using their version of the Aguinis et al. (2008) formula (see Equation 2 above). We also tried to reproduce the findings reported by Rockstuhl et al. and it turned out that we could fully reproduce their results by using our version of the formula (see Equation 1 above). Thus, Rockstuhl et al. had in all likelihood used exactly the same version

of the formula that we used (i.e., Equation 1), and very likely *not* Equation 2. After informing the then Editor-in-Chief of JAP about our results, she admitted that we were right in our demonstration of Rockstuhl et al.'s likely use of Equation 1, however, this insight had no noticeable consequence for the whole process leading to the correction. This is remarkable because if we had used a wrong version of the formula by Aguinis et al. (2008) in our meta-analysis, the Rockstuhl et al. (2012) meta-analysis would also have needed a correction.[2]

Concerning (iii): Although there is already precedent for our use of the Aguinis et al. (2008) formula given that the meta-analysis by Rockstuhl et al. (2012) probably used it identically, we searched the literature for further examples of meta-analyses using the same approach. Although we did not conduct a fully exhaustive literature search, we quickly found several examples of meta-analyses that probably used exactly the same approach, including a meta-analysis that was published in JAP much more recently than ours.[3] We, of course, forwarded this information to the JAP. However, we received the reply that the whole clarification process was exclusively about our meta-analysis, but not about others. This is a questionable answer because—if several meta-analyses used the same approach—this could be considered as a conventional approach in our field, if not a standard.

Second problem: **Correcting a single study using a certain methodological approach versus critiquing the general use of this approach in the field.** The then Editor-in-chief of JAP decided to issue a correction only of Breuer et al. (2016) even though there was evidence that more meta-analyses (including meta-analyses published in JAP and also meta-analyses more recently published in JAP, see above) used the same approach. Thus, it is hard to understand why our meta-analysis was in focus. Indeed, we believe that if a statistical

[2] Once again, we do not suggest that the meta-analysis of Rockstuhl et al. (2012) needs a correction for using a widespread methodological approach to categorical moderator analysis. Mentioning this meta-analysis only serves the purpose that correcting our meta-analysis was not a consequent choice.

[3] We sent three related manuscripts to the then Editor-in-Chief to show that this is a widespread approach to categorical moderator analyses in meta-analyses. We decided, however, not to cite theses manuscripts here as there are likely many more studies that follow this approach and it is not our intention to single out specific studies.

formula is not correct, it would be appropriate to issue corrections for all meta-analyses published in the same outlet (JAP), and write a commentary critiquing the Aguinis et al. (2008) formula or the, from their perspective, wrong use of this formula.

Third problem: **Lacking references to the literature describing why our approach may have been wrong.** The exchange between us as the authors of Breuer et al. (2016) and the JAP was characterized by our efforts to not only provide arguments, expert opinions, and precedent information that supported our approach, but also to refer to the scientific sources where these pieces of information could be found. Unfortunately, the communication by the JAP did hardly provide any references to scientific works, which made it hard to understand the claims brought forward and also to judge whether they are in fact correct. As a result (and, admittedly, also due to the opinions of the consulted experts and other meta-analyses using the same approach), we are still not sure whether we in fact made a mistake or not by using the Aguinis et al. (2008) formula in the way we did, which is also the reason why we as authors do not agree with the published correction.

The approach of not providing scientific sources, which would support the claims made, is also reflected in the published correction. It states the following without providing any justification for the claim that we made a mistake:

> “In the article, “Does trust matter more in virtual teams? A meta-analysis of trust and team effectiveness considering virtuality and documentation as moderators,” by Christina Breuer, Joachim Hüffmeier, and Guido Hertel (*Journal of Applied Psychology,* 2016, Vol. 101, No. 8, pp. 1151-1177, http://dx.doi.org/10.1037/apl0000113), the formula used to calculate t-values to examine categorical moderators for the association between team trust and task performance is incorrect.“

Perhaps the APA requirements for corrections do not allow citing any references (we do not know this). We believe, however, this is not good scientific practice, regardless of

whether the underlying reason is a decision of the editor or APA requirements for corrections. By simply making such a claim without providing scientific references, it is difficult for others to (i) understand the claim and (ii) to examine whether the claim is correct. As corrections are also scientific records, they should of course be transparent and it should be possible to scientifically scrutinize them.

Fourth problem: **Lacking author involvement in the formulation of the correction.** The correction was formulated by the then Editor-in-Chief of the JAP and the involved Associate Editor and in a second step approved by the APA. We as authors then received this correction via email and only could choose whether we wanted to be mentioned as agreeing or as not agreeing with the correction. We were neither allowed to participate in the formulation of the correction nor were we allowed to publish a commentary associated with the correction that provides information from our perspective. Given the questions and problems raised in this section, it was obvious that we could not agree with the correction.

Second question: **What would be a good procedure to decide whether to issue a correction to a possible mistake in the scientific record?** While we do not have a full answer to this complex question, our true tale from the experiences we made in this process result in initial suggestions. (i) Claims by a journal editorship that a mistake was made should be backed up by scientific sources that allow for a clear assessment whether a mistake did in fact occur or not (if such sources are available). If it is not possible to clearly establish whether a mistake was made, no correction should be issued. In our case, the arguments that we brought forward tended to be discounted or even dismissed without considering their factual weight and we felt that it was not possible to openly discuss and commonly determine whether a mistake had occurred or not.

(ii) When making decisions about possible corrections, editors should factor in whether the mistake was part of the prior peer-review process and whether the manuscript was

accepted with the possible mistake being clearly visible for the responsible handling editor and the reviewers (and, thus, subject to peer-review).

(iii) A discussion in the scientific community regarding the maximum time frames for corrections is needed (i.e., how old should a manuscript be to still warrant a correction?). Such a discussion would hopefully result in standards, which would also improve related processes. Please note that this discussion not only relates to the validity of our knowledge base, but also to the accessibility and reproducibility of older research findings.

(iv) If a potential mistake or a problematic methodological approach is not only part of a specific manuscript, but of many manuscripts, the initial course of action should be changed. Even if the process started with a specific manuscript, the other manuscripts can no longer be ignored. Instead of singling out the first manuscript, all concerned manuscripts should be corrected and a commentary should be published in which the widespread mistake or problematic methodological choice is transparently discussed. (vi) If a correction is issued, it should contain not only the claim that a mistake was made, but also a clear explanation (including scientific sources) why the incident in question is a mistake. (vii) If it cannot be established with certainty that a mistake has occurred and a correction is nevertheless issued, authors should be given the opportunity to weigh in and articulate their perspective. While their response to the correction should of course be peer-reviewed for correctness, providing the authors with this opportunity satisfies their procedural fairness concerns.

In closing, we like to repeat that this commentary is intended to initiate a constructive discussion of corrections and development of sustainable standards in this process. Obviously, we are not very happy with the correction process in which we were involved in the past months. Nevertheless, by making the process and the related general questions transparent, we hope that our research community can reflect on and optimize the described processes with the goals to maintain the highest possible reliability of our research base, but also fairness in the interaction between editors and authors in our field.

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Table 1

Chronology of the Events Preceding the Correction of Breuer et al. (2016)

Date	Event
29th of September 2014	First submission of Breuer et al. (2016) to the JAP (submitted as part of C. Breuer's doctoral dissertation, supervised by G. Hertel [1st supervisor] and J. Hüffmeier [2nd supervisor])
From the 30th of September 2014 to the 11th of March 2016	Several rounds of revision where we, among other things, (i) changed our approach from a multi-level meta-analytical approach in the original submission to the Schmidt and Hunter approach as requested by the review team and (ii) were recommended by the Associate Editor a meta-analysis by Rockstuhl et al. (2012) as a model for how to report corrected and uncorrected effect sizes as part of our meta-analytical results.
11th of March 2016	Last revision of Breuer et al. (2016) sent to JAP. We were highly transparent in our response letter and the manuscript that we used a *t*-test formula that was provided by Aguinis et al. (2008; *Organizational Research Methods*) for categorical moderator tests in meta-analyses. We were also very transparent that we modelled our approach after Rockstuhl et al. (2012). We specifically wrote (citation from our response letter): "In fact, when we re-analyzed our data based on the Hunter & Schmidt approach in our last revision (following the suggestion of Reviewer 2), we followed your recommendation to consult the JAP paper of Rockstuhl et al. (2012) who also interpreted significance tests even though CIs overlapped."
14th of March 2016	Breuer et al. (2016) accepted for publication by the JAP – that is, not only our meta-analysis was accepted, but also the approach we chose methodologically.
1st of December 2024	First email from the then Editor-in-Chief of JAP, informing us that a concern was brought to her attention with respect to Breuer et al. (2016), and that when following up on this concern, in her own reading of the article two other concerns appeared. However, no scientific reasons or sources were provided that would confirm why our approach should be wrong.
27th of February 2025	Our reply, where we could clarify the first concern (from an anonymous reader) and the first of two concerns from the then Editor-in-Chief of JAP after successful reproduction of our results – these clarified concerns were never mentioned again in the exchange.
28th of February 2025	Second email from the then Editor-in-Chief of JAP, asking us how we calculated the *t*-tests for our categorical moderator analysis (the second concern from the then Editor-in-Chief).
7th of March 2025	Our reply, where we described that we used a formula provided by Aguinis et al. (2008) for exactly that purpose.
21st of March 2025	Third email from the then Editor-in-Chief of JAP together with an Associate Editor of JAP who is a meta-analysis expert (like

	all her following emails in this process), claiming (i) that our use of the Aguinis et al. (2008) *t*-test formula was wrong and that the formula would have needed adaptation without providing any explanation or references for why it should be wrong and (ii) that the prior meta-analysis by Rockstuhl et al. (2012) did use the formula differently and correctly.
26th of May 2025	Our reply, where we described that (i) we contacted three experts (a statistician, a psychological methods expert, and Prof. Aguinis, as the first author suggesting the formula in question). All three experts confirmed that we could use the formula for our purposes. (ii) We further described that many other meta-analyses used the formula in the same way as we did (including a more recently published meta-analysis in JAP). We attached three of these meta-analyses to our email.
28th of May 2025	Fourth email from the then Editor-in-Chief of JAP and her associate editor, claiming that the Aguinis et al. (2008) formula is accurate but needs to be adapted if we want to use it for the purposes of our meta-analyses in the way we did. They again did not provide any references supporting this claim or explaining how this adjustment should be made. They further mentioned that they were able “to approximate Rockstuhl et al.’s t-values” using their own version of the Aguinis et al. formula (see Equation 2 above). Finally, they denied the relevance of other meta-analyses citing Aguinis et al. (2008) while also using ρ within their moderator in view of the concern with Breuer et al. (2016).
24th of June 2025	Our reply, where we provided mathematical arguments that our application of the Aguinis et al. (2008) formula was correct and justified. These mathematical arguments were kindly provided by an established professor for statistics. We further explained that we successfully reproduced the results reported in Breuer et al. (2016).
27th of June 2025	Fifth email from the then Editor-in-Chief of JAP in which she, for the first time, provided a concrete reference for their claim that our application of the Aguinis et al. (2008) formula is incorrect (the 2015 version of the Schmidt & Hunter book on meta-analyses).
27th of July 2025	Our reply, where we pointed out that we could fully reproduce the results by Rockstuhl et al. (2012) using our version of the Aguinis et al. formula (see Equation 1 above). We also pointed out that we found it problematic that the perspectives of the experts that we consulted were hardly considered in the exchange so far. We further noted that we found it not justified to dismiss other meta-analyses that quite likely used the same approach as we did (the above Equation 1). Finally, we suggested an addendum containing an extended Table 3, now also including confidence intervals based on corrected correlations that consider measurement errors in addition to sampling errors to prevent further confusion among readers. We added a draft of such a table to our email.

29th of July 2025	Sixth email from the then Editor-in-Chief of JAP informing us that the first concern came from a scholar, the name was kept anonymous.
26th of August 2025	Our reply, where we offered the following solution to the concern: “To avoid potential confusion in the future, we would be happy to provide an extended Table 3 as addendum to our meta-analysis, explaining again the approach of our moderator analyses and also including confidence intervals based on corrected correlations that consider measurement errors in addition to sampling errors (see Rockstuhl et al., 2012, for a very similar solution).” We again added the draft of such a table to our email.
3th of September 2025	Seventh email by the then Editor-in-Chief of JAP, admitting that we were right that Rockstuhl et al. (2012) used the same approach to conduct their categorical moderator analyses (see Equation 1 above). However, she further noted that “[…] the fact that other published work (at least Rockstuhl et al., but perhaps others) used the Aguinis et al. formula without adaptation does not provide statistical evidence that this is the correct approach. As noted above, the accuracy of these other papers is beyond the current issue.“ She further argued that we cannot know whether other papers used (or did not use) adaptations of the Aguinis et al. formula.
13th of October 2025	Our reply, where we pointed out (among other things) that we do not agree with the then Editor-in-Chief’s argument that we cannot know whether other authors (e.g., Rockstuhl et al., 2012) did or did not use adaptations of the Aguinis et al. (2008) formula. Given that Aguinis et al. (2008) did not mention any required adaptations of the formula, it is reasonable to assume that authors such as Rockstuhl et al. (2012) would have mentioned any adaptations if they made any to the formula.
1st of December 2025	Seventh email by the then Editor-in-Chief of JAP informing us about her decision to issue a correction of Breuer et al. (2016).
2nd of December 2025	Eighth email by the then Editor-in-Chief of JAP, asking us whether we agree with the concrete correction she issued. She informed us that this is a step that the APA requires when issuing a correction.
7th and 8th of December 2025	Independent replies from all three authors, where we stated that we do not agree with the correction.
6th of April 2026	Online publication of a correction of Breuer et al. (2016). A publication presenting our perspective alongside the correction was denied due to the journal’s policy.

Table 2

Questions and Problems that Emerged in the Process Leading to the Correction of Breuer et al. (2016)

Question or problem	Content
First question	Should possible methodological mistakes in manuscripts be corrected if the methods were transparently reported, peer-reviewed, and accepted by a journal? And if so, for which time frames?
First problem	Our efforts to provide information and statistical arguments for our use of the Aguinis et al. (2008) formular were not sufficiently heard and integrated in the process leading to the correction.
Second problem	Correcting a single study using a certain methodological approach versus critiquing the general use of this approach in the field.
Third problem	Lacking pointers to the research literature describing why our approach may have been wrong.
Fourth problem	Lacking author involvement in the formulation of the correction.
Second question	What would be a good procedure to decide whether to issue a correction to a possible mistake in the scientific record?