# Erratum

## "Why psychologists Should by Default Use Welch's *t*-test Instead of Student's *t*-test" (Chapitre 2)

### Erreurs conceptuelles

1) **p.10**: *"the F-ratio statistic is obtained by computing SD2/SD1 (standard deviation ratio, SDR)"* :

- D'abord, le ratio entre les 2 écart-types d'échantillons ne correspond pas au SDR, mais à l'**estimation** du SDR (SDR = $\frac{\sigma_2}{\sigma_1}$).
- Ensuite, ce n'est pas nécessairement le deuxième écart-type d'échantillon qui se trouve au numérateur, mais le plus grand des deux échantillons.

**Proposition de reformulation :** *"The F-ratio statistic is obtained by computing* $\frac{max(S_1,S_2)}{min(S_1,S_2)}$ *where* $S_j$ *is the sample standard deviation of the* $j^{th}$ *group* ($j = 1, 2$)."

Cette nouvelle formulation implique de définir le SDR ultérieurement, ce qui est fait 4 paragraphes plus loin dans la version corrigée: *"under three population standard deviation ratio (SDR =* $\frac{\sigma_2}{\sigma_1}$ *where* $\sigma_j$ *is the population standard deviation of the* $j^{th}$ *group;* $j = 1, 2$*) : respectively 1.1, 1.5 and 2".*

2) **p.10**: "When SDR = 1, the equal variances assumption is true, when SDR > 1 the standard deviation of the second ~~sample~~ population is bigger than the standard deviation of the first ~~sample~~ population , and when SDR < 1 the standard deviation of the second ~~sample~~ population is smaller than the standard deviation of the first ~~sample~~ population".

3) **p.14** (partie "simulations") : *"As long as the variances are equal between ~~groups~~ populations or sample sizes are equal, the distribution of Student's p-values is uniform. . . ".*

4) **Partout**: toutes les mentions au test de Yuen, pour les raisons expliquées en limites. **Proposition de rectification** : supprimer tout ce pagraphe (et la table associée).

### Ambiguïtés possibles

1) **p.12** : *"When both variances and sample sizes are the same in each independent group, the t-values, degrees of freedom, and the p-values in Student's t-test and Welch's t-test are the same (see Table 1)."* Cette phrase peut donner l'impression que les deux statistiques mentionnées, ainsi que les degrés de liberté et *p*-valeurs qui leur sont associés sont identiques lorsqu'on travaille avec des échantillons de tailles identiques et que la condition d'homogénéité des variances est respectée, ou autrement dit, lorsque les variances de population ainsi que les tailles d'échantillon sont identiques, or, ce n'est nullement vrai. Pour calculer les statistiques *t* de Student et *t* de Welch ainsi que leurs degrés de liberté, on utilise les *estimations* des variances de chaque groupe, et non les variances de population. C'est donc chaque fois que l'on obtiendra des *estimations* identiques pour les variances de chaque groupe, sur base d'échantillons de taille égale, que les statistiques, leur degré de liberté et leur *p*-valeur seront identiques. Or, ceci n'est pas une information très pertinente en soi, puisqu'il arrive très fréquemment d'obtenir des estimations de variance différentes pour chaque groupe lorsque la condition d'homogénéité des variances est respectée, et qu'à l'inverse, il est possible (bien que peu probable) d'obtenir des estimations de variance identiques pour chaque groupe lorsque la condition d'homogénéité des variances n'est pas respectée.
**Proposition de rectification** : supprimer tout ce pagraphe (et la table associée).

2) **p.16** (à propos du test de Levene) : *"Because the statistical power for this test is often low, researchers will inappropriately choose Student's t-test instead of more robust alternatives."* Cette phrase peut amener à comprendre que si le test de Levene était toujours très puissant, il serait approprié de l'utiliser en vue de choisir entre les tests $t$ de Student et $t$ de Welch. Pourtant, privilégier le test $t$ de Student lorsque l'on ne peut rejeter l'hypothèse d'égalité des variances (autrement dit, lorsque les résultats du test de Levene sont non significatifs) reviendrait à confondre le non-rejet de l'hypothèse d'égalité des variances avec l'acceptation de l'hypothèse d'égalité des variances. Au sein du chapitre 5 dédié aux tests d'équivalence, nous avons montré que même lorsqu'on s'assure d'avoir une puissance suffisante pour détecter un effet de taille donnée, la stratégie qui consiste à interpréter le non-rejet de l'hypothèse nulle comme un soutien en faveur de l'hypothèse nulle n'est pas appropriée.

**Mise en forme et Notations**

1) Les lettres utilisées pour décrire les statistiques ($t$ ou $F$, par exemple) doivent toujours être inscrites en *italique*. Or, cela a été omis à plusieurs reprises dans l'article. Par exemple, il aurait fallu écrire :
**- p.10** : "... as the Mann-Whitney $U$ -test..." ;
**- p.10** : "$F$ -ratio test".

2) Certaines notations mathématiques auraient également dû être indiquées en italique. Par exemple, à la p.10, il aurait fallu écrire :

- "$x_{ij}$" au lieu de "$\mathrm{x_{ij}}$";
- $|x_{ij} - \hat{\theta_j}|$ au lieu de $|\mathrm{x_{ij}} - \hat{\theta}_\mathrm{j}|$.

3) Il est très important d'être consistant dans le choix des notations mathématiques, pour éviter toute confusion pour le lecteur. Or, nous ne l'avons pas toujours été. Par exemple, nous avons utilisé plusieurs notations différentes pour décrire l'écart-type et la variance :

- p.10 : nous utilisons respectivement SD1 et SD2 pour décrire l'écart-type de chaque groupe;
- p.12 (équation 1) : nous utilisons respectivement $S_1^2$ et $S_2^2$ pour décrire la variance de chaque groupe, alors que nous utilisons $s_1^2$ et $s_2^2$ (lettres minuscules) dans la légende de cette formule;
- p.13 (équations 3 et 4) : nous utilisons respectivement $s_1^2$ et $s_2^2$ (lettres minuscules) pour décrire la variance de chaque groupe.

4) On parle normalement d'erreur de type I et II (et non d'erreur de type 1 et 2): or dans tout l'article du chapitre 2, j'ai parlé des erreurs de type 1 et 2. Par exemple, p.13: *"An increase in the Type 1 error rate leads to an inflation of... while an increase in the Type 2 error rate..."*

5) **p.11**: *We will discuss three origins of unequal standard deviations across two groups of observations : the variability inherent to the use of measured variables, the variability induced by quasi-experimental treatments on measured variables, and the variability induced by different experimental treatments on randomly assigned subjects.[...]* Précision apportée car la découpe semblait montrer 2 causes ("A first reason..." "A second reason..").

**Faute(s) de frappe**

- p.13 : "see ~~v~~ **Figure** 2a".
- p.15 : "$p$-values from Welch's ~~$t$-test~~ and Student's ~~$t$-test~~ $t$-tests, shown separately. . . "
- p.16 : Note 4 : "other variants have been proposed such as the **20%** trimmed mean"

**Bibliographie**

Référence manquante: Bradley (1978).

## Annexe B: erratum de l'article "Taking parametric assumptions very seriously : Arguments for the Use of Welch's *F*-test instead of the Classical *F*-test in One-Way ANOVA" (Chapitre 3)

### Mise en forme et Notations

Une légende est manquante pour certaines notations mathématiques. Par exemple, en ce qui concerne l'équation (1), bien que $n_j$, $k$ et $s_j^2$ aient été correctement définis, les définitions pour $\bar{x_j}$, $\bar{x_{..}}$ et $N$ ne sont données que plus tard, en référence à d'autres équations. Cela peut rendre la lecture de l'article plus compliquée pour certaines personnes non familières avec ces notations.

Par ailleurs, comme dans l'article précédent sur le test $t$ de Welch, on constate certaines incohérences en termes de notation. Par exemple, si la moyenne de chaque groupe est définie par $\bar{x_j}$ dans l'équation (1), elle est définie par $\bar{X}_j$ dans l'équation (7).

**p.21** : ~~($SD_{spanish} = .80 > SD_{english} = .50$)~~ ($S_{spanish} = .80 > S_{english} = .50$, with S = sample standard deviation) [...] For men, the reverse was true ~~($SD_{spanish} = .97 < SD_{english} = 1.33$)~~ ($S_{spanish} = .97 < S_{english} = 1.33$)

Enfin, dû à un manque de connaissance de Latex lors de mes premières tentatives d'écritures d'articles via Rmarkdown, certaines majuscules sont manquantes dans les références bibliographiques.

### Faute(s) de frappe et grammaire

- p.18 : "Although it is important to make sure ~~test~~ **that** assumptions are met";
- p.19 : "... we think that a ~~first~~ realistic first step towards progress would be to get researchers..";
- p.20 : "Based on mathematical explanations and Monte~~o~~ Carlo simulations";
- p.21 : "~~With~~ **w**ith $N =$...";
- p.21 : "~~Where~~ **w**here ~~$x_j$~~ $\bar{x_j}$ and $s_j^2$ are respectively the group mean and the group variance...";
- p.22 : "... negative pairings (the group with the ~~smallest~~ **largest** sample size is extracted from the population with the smallest $SD$);
- p.22 : "the type I error rate of all test**s**";
- p.24 : "... which is either more liberal or more conservative, depending on the $SDs$ and ~~$SD$~~ **sample sizes** pairing";
- p.24 : "... ~~whatever~~ the correlation between the $SD$ and the mean **does not matter**";

## Annexe C : échanges avec Geoff Cumming, en vue d'améliorer l'article non publié "Why Hedges' $g_s^*$ based on the non-pooled standard deviation should be reported with Welch's $t$-test"

Le 4 juin 2021, suite à la soumission d'un preprint de notre article sur les tailles d'effet, nous avons eu le plaisir de recevoir un email de la part de Geoff Cumming. Cet email est présenté à la page suivante.

En pièce jointe de cet email, figurait un feedback long et détaillé de notre article. Celui-ci est entièrement retranscris juste après le mail. Le texte en bleu qui est inséré dans ce feedback correspond aux réponses que nous lui avons fournies, et le texte en brun correspond aux réactions de Cumming à nos réponses.

Pour finir, cet échange a donné lieu à un blog post disponible à l'adresse suivante : https://thenewstatistics.com/itns/2021/06/17/which-standardised-effect-size-measure-is-best-when-variances-are-unequal/.

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## your fine preprint

**Geoff Cumming** <g.cumming@latrobe.edu.au> 4 juin 2021 à 06:29
À : "marie.delacre@ulb.ac.be" <marie.delacre@ulb.ac.be>
Cc : "d.lakens@tue.nl" <d.lakens@tue.nl>, "Calin-Jageman, Robert" <rcalinjageman@dom.edu>

Dear Dr Delacre and colleagues,

I was delighted to see your fine preprint "Why Hedges' $g_s$* based on the non-pooled standard deviation should be reported with Welch's *t*-test". It strikes me as important and compelling, as well, I'm sure, as requiring a vast amount of work.

I plan to blog about it, but first I'd like, if I may, to ask some questions and offer comments, as attached. I'd greatly appreciate any comments you might care to make in reply.

Of course, I'm more than happy to discuss any of these issues--it's all fascinating as well as essential stuff!

With thanks, and best regards,

Geoff

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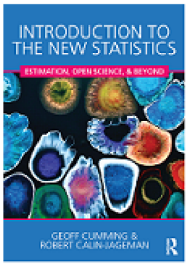
Intro textbook: Introduction to The New Statistics: Estimation, Open Science, and Beyond
www.thenewstatistics.com
First book: Understanding The New Statistics: Effect Sizes, Confidence Intervals, and Meta-Analysis
ww.thenewstatistics.com
Own page: http://www.latrobe.edu.au/she/contact-us/staff/profile?uname=GDCumming
ESCI (Exploratory Software for Confidence Intervals): www.thenewstatistics.com


**Delacre comments 4 Jun 21.docx**
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**Why Hedges' $g_s^*$ based on the non-pooled standard deviation should be reported with Welch's *t*-test**

https://psyarxiv.com/tu6mp/

Comments by Geoff Cumming
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4 June 2021

**Subscript *s***

I'm wondering why you use subscript $s$ for all eight ES measures. Yes, Cohen's original term for the estimate was $d_s$, but $d$ became the standard usage. Perhaps you use the subscript $s$ to indicate that the standardiser is an $SD$ estimated from data? By contrast $d_\delta$, for example, would indicate that a population value is available to use as standardiser. But in your paper there are no such cases, so you could simplify everything by simply omitting all those subscript $s$'s?

Also, that role for the subscript rules out use of it to distinguish, for example, between $d_p$ for pooled $s$, and $d_C$ for Control group $SD$ as standardiser. However, I know there are no well-established conventions for such subscripts, and you need to use 'Shieh' as a label for those ES measures, so perhaps using name labels (Cohen's, etc) for all measures—as you do—and dropping all the $s$ subscripts would be simplest.

I used the subscripts to indicate that the ES measure is estimated based on a sample, but I agree that it is maybe not necessary, so I will remove all subscripts and use labels instead.

**Typos?**

Thanks for pointing all these typos out!

p. 7, l. 139: Hedges' $d_s^*$ should be Cohen's $d_s^*$? That's right, it should be Cohen's $d^*$

p. 8, l. 152: Use $\sigma$ rather than $\sigma_{pooled}$ (twice) because here we're assuming homogeneity of variance, with $\sigma = \sigma_1 = \sigma_2$ as the common population $SD$? (Also Table 1, line 1 of Note.) Ok!

p. 9, l. 3 of footnote: Not 52 but .52? (By my calcs using $df = 3$, $N = 5$, and the approximate debiasing formula the value should be .529?) Yes, it's a typo, the right number is .52 instead of 52 (see the R console below).

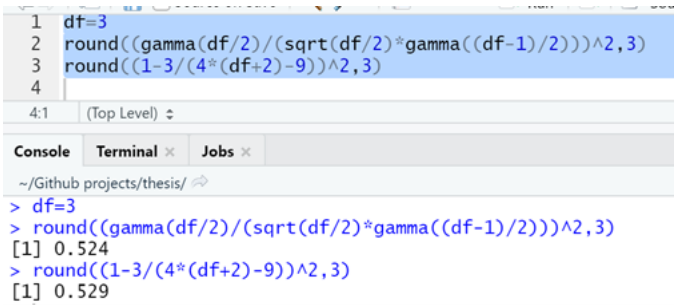
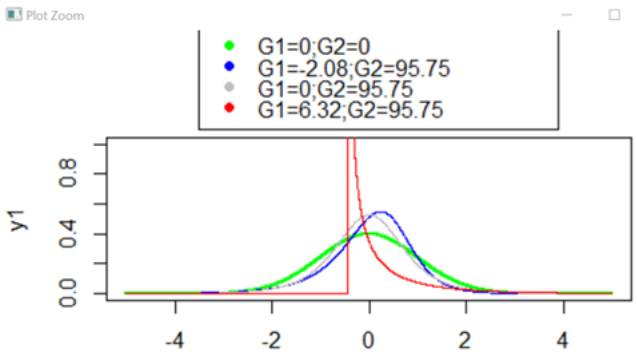



Table 3: Cohen's $g_s^*$ should be Hedges' $g_s^*$? Also on p. 20, l. 275 (without *), and p. 30, l. 485. Also true.

**Non-normality**

It's great that you explore non-normality, so that you are investigating the robustness of the various ES estimators. I'd love to see a figure showing the three example distributions you choose to study, alongside a normal curve. Then I could have some intuitions about how extreme they are, especially in comparison with distributions that I might see in data I'm analysing. Thanks.

In the plot below, here is what the four distributions look like when $\mu = 0$ and $\sigma = 1$.




It is a nice suggestion to add a Figure in the manuscript to show the compared distributions, I will take it into account.

Additional comment: in our simulations, we used only continuous distributions. I think it would be interesting to test the properties of estimators when distributions are discrete (e.g. Likert scales) but I have no idea how to generate such distributions in a realistic way. If you have any suggestion, I would be more than happy to think about it next time!

Yes, it would be valuable to have investigations with discrete distributions, but I can't think of good starting points. We'd hope for better than the normal approximation to the binomial. Maybe start with the investigations that have been made of that, especially with small N (Likert, as you mention).

**Bias**

I take your point (p. 23) about the value of focusing on relative, rather than absolute, bias (and variance) when there is a change in measurement units. But that doesn't happen in your investigations.

Well, I wrote about a "change of unit" because we use a different standardizer for each estimator. As a consequence, each estimator computation results in a different value in order to describe the same amount of difference. I will clarify in the text.

The one other reason you state for considering relative bias is a reference, on p. 5, bottom, that Tables 1 and 2 illustrate that "bias is directly related to the population effect size". That's true for Cohen's $d$ in Table 1, and for $d$ and $d^*$ in the top two rows of Table 2. (I'm omitting Shieh from my comments—see below for reasons.) The debiasing that gives Hedges' $g$, etc, is proportional, and results in zero bias for any population effect size. I think, therefore, that neither of the reasons you mention for using relative bias applies for any of the $g$ family measures in Figures 2-9 (omitting Shieh, as I will do unless specifically mentioned), and so I'm wondering whether seeing means (i. e., averaged over $\delta$ values) of relative or absolute bias would be more informative.

I agree that under the normality assumption, there is no bias for any unbiased estimator, of course. However, when the normality assumption is not met, biases occur. In Supplemental Material 1, we mention that when samples are extracted from heavy-tailed symmetric distributions, this bias depends on the same parameters as the bias of biased estimators when the normality assumption is met. It's true that we mainly based our decision to compute the relative bias on the comparison between Cohen's $d$ (Hedges' $g$) and Shieh's $d$ (Shieh's $g$). For example, when designs are balanced and population variances are equal across groups, the bias of Shieh's $g$ is always approximately twice smaller as the bias of Hedges' $g$, and its variance is always approximately four times smaller as the bias of Hedges' $g$. It gives the illusion that under this specific configuration, Shieh's $g$ is less biased and variable as Hedges' $g$, but it is only due to what we called "a change of unit".

Note: I explain below why I think it's important to consider Shieh's $d$ in our comparisons (even if I agree with the fact that interpreting this measure is very hard).

I guess I'd like to see the bias for each $\delta$ value. (On p. 24, line 2, you give a link to an illustration of raw bias and variance, but I can't find any tables or graphs of values at that site.)

The graphs for raw estimators of goodness are available on my Github account: https://github.com/mdelacre/Effect-sizes/.
You will find them in the following folder: "Scripts outputs/Quality of ES measures/Graphs/Unbiased estimators/Raw estimators of goodness/". The fact that you haven't found it probably means that I should reorganize the folder, in order to make it clearer, to indicate more clearly the Figures position in the folder or to add a read me file!

Thanks for the link for raw results. https://github.com/mdelacre/Effect-sizes/ That's different from the link in the preprint. [En effet, nous avions collé un mauvais lien par erreur dans le preprint].

You say that the main purpose of the figures is to allow comparisons between the different ES measures, rather than absolute values for bias (and variance). Even so, knowing when bias is likely to be absolutely small or large can inform judgments about the different ES measures in various contexts. In Figures 2 and 3, we have $\sigma_1 = \sigma_2$ and four versions of $g$, all debiased. If I'm following, the relative bias values pictured in the top rows of both figures are averaged over $\delta = 1$, 2, 3, and 4. (I'm assuming $\sigma_1 = \sigma_2 = 1$; correct?) Right! For the non-normal distributions, average relative bias in the best cases is around 1-2%, so not bad. But up to around 5-10% in some other cases, a bit more concerning.

Absolutely right. However, as mentioned in the manuscript, "we chose very extreme conditions, and we know that none of the parametric measures of ES will be robust against such extreme conditions". It is therefore not surprising that *all* estimators are very large under some conditions. We could insist more on the fact that the parametric estimators are robust only under moderate deviations from the normality assumptions.

It's interesting to see that almost all the relative bias values plotted in Figures 2-9 are positive, so in nearly all cases the ES estimates are on average too high? If so, this may be worth noting? (Apologies if I missed your comment on this.)

You're perfectly right. There is a mathematical reason for that (at least for symmetric distributions; it is explained in Supplemental Material 1, in the "Preliminary note"; see below). We did not include this note in the main manuscript because we were afraid that it would be too technical for many psychologists, and we did not want to complicate even more a paper that already contains many complex concepts, but maybe we could mention this in a footnote if you think it's missing.

**Preliminary note**

For all previously mentioned estimators (Cohen's $d_s$, Glass's $d_s$, Cohen's $d_s^*$ and Shieh's $d_s$), the theoretical expectation is computed by multiplying the population effect size (respectively $\delta_{Cohen}$, $\delta_{Glass}$, $\delta^*_{Cohen}$ and $\delta_{Shieh}$) by the following multiplier coefficient:

$$\gamma = \frac{\sqrt{\frac{df}{2}} \times \Gamma\frac{df-1}{2}}{\Gamma\frac{df}{2}} \quad (1)$$

where $df$ are the degrees of freedom (see the main article). $\gamma$ is *always* positive, meaning that when the population effect size is not zero, all estimators will overestimate the population effect size. Moreover, its limit tends to 1 when the degrees of freedom ($df$) tend to infinity, meaning that the larger the degrees of freedom, the lower the bias.

While we focus on the theoretical bias of biased estimators when the normality assumption is met, it is interesting to notice that our main conclusions seem to generalize to :

- biased estimators when samples are extracted from symmetric distributions;
- unbiased estimators when samples are extracted from heavy-tailed symmetric distributions.

### Variance

For bias, the goal is zero, an unbiased estimate. For relative variance, the smaller the better, but we don't expect zero because we'll always have sampling variability in estimates of $\delta$. I'm finding it hard to think of a good way to get intuitions about the empirical variances of the different ES measures, starting with the

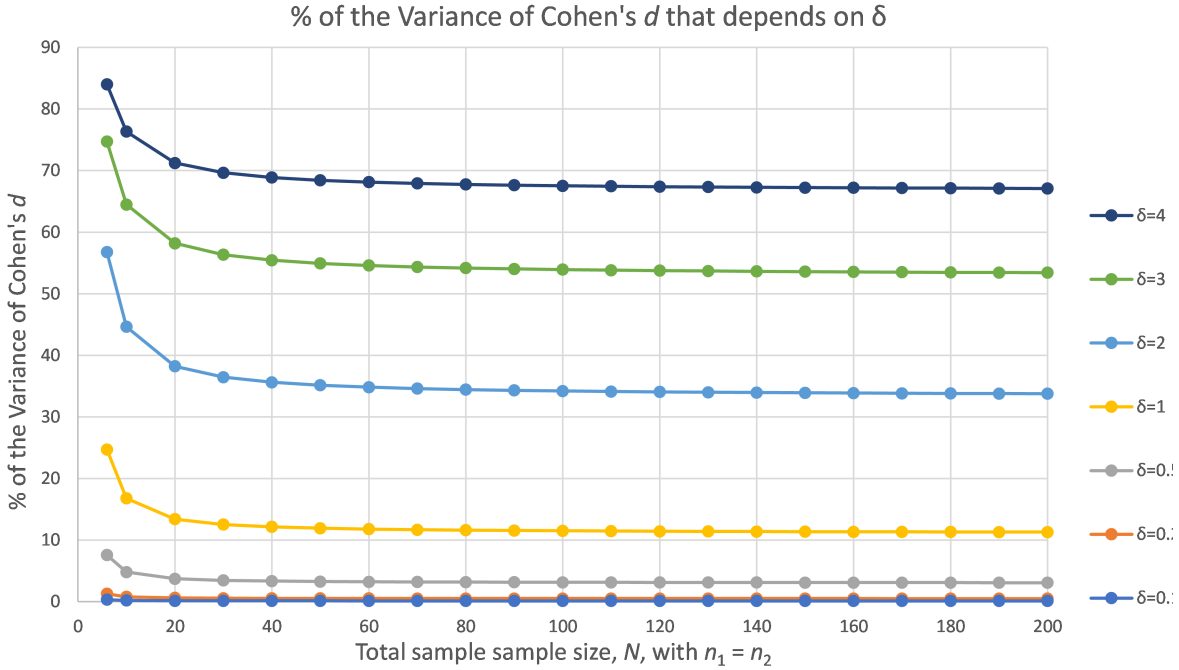
relative values reported in Figures 2 and 3. Part of my difficulty is that variance is a squared measure. As usual, I'd much prefer to be able to think in terms of CIs and their lengths.

Thinking in terms of CIs and their lengths require to run new simulations because we compute CIs based on the noncentral t distribution method (the method is explained in CI.pdf, here: https://github.com/mdelacre/Effect-sizes/tree/master/Supplemental%20Material%204/CI.pdf/ ; the R script is available here: https://github.com/mdelacre/Effect-sizes/blob/master/Scripts/Confidence%20intervals/CI.R.

I have made simulations in order to compute CIs around point estimators for all scenarios where $\mu_1 - \mu_2 = 0$ and $\mu_1 - \mu_2 = 1$.

Note that analyses on CIs would probably be very redundant with that of the variance. I agree that the length of CIs is probably more intuitive, but at the same time, everybody knows that the variance is a measure of dispersion, and the greater the variance, the greater the dispersion. We mention that our main goal is to *compare* the relative bias and variance of different estimators, and variance allows this comparison.

The variance formulas in Tables 1 and 2 show that the variance of $d$ and $g$, and the top two ES measures in Table 2, do indeed depend on $\delta$. To try to get a feel for the extent of this dependence I made a quick spreadsheet of the variance formula for Cohen's $d$, using the approximate formula in the second line of Table 1. The figure below shows the percentage of variance in $d$ that is given by the second, $\delta$-dependent, term in the formula for variance. I assumed two equal sized groups. The figure suggests that for $\delta < 1$ only a very small % of the variance is dependent on $\delta$, and for $\delta = 1$ (around 10%) and greater, the % increases rapidly with $\delta$—not surprisingly, given the $\delta^2$ term in the formula. The % hardly changes with sample size, for two samples each of at least 20, as you use.




For $\delta$ values of 1, 2, 3, and 4 (as are averaged in Figures 2, 3), the %ages are, in round numbers, about 11, 34, 54, and 67, respectively. I expect the %ages for the other ES measures would not be vastly different. Therefore the relative variance values shown in the lower row in Figures 2 and 3 are averages over cases in which dramatically different proportions of the variance are dependent on $\delta$. I think this makes it harder to get good intuitions. Values of the variance of the estimates, rather than relative values, may be more understandable?

This is a very interesting analysis. I was quite confused about this question, and I've been wondering for a long time what the best solution would be. We had many long discussions in order to decide whether we should compute the variance or the relative variance. The issue was indeed the fact that the variance only partly depends on delta. Finally, we decided to compute the relative variance because we had in mind that

when $n_1 = n_2$ and $\sigma_1 = \sigma_2$, the bias (variance) of Shieh's $d$ is twice smaller (four times smaller) as the bias for Cohen's $d$, only because Shieh's $d$ is twice smaller as Cohen's $d$ (see Appendix 1 in our paper). However, I keep in mind that you argue in favor of removing Shieh's $d$ from our analyses.

Taking a different approach, and considering the relative variance values plotted in Figures 2 and 3, would it be reasonable to say that relative $SD$, the square root of relative variance, is the $SE$ used to calculate the CI on that ES estimate, expressed as a fraction of $\delta$, the population ES? If so, such a 95% CI would be about $\pm 2 \times SE\ value \times \delta$. (Given that the smallest total $N$ being considered is 40, that approximation should be reasonable? Perhaps less so for much smaller group sizes. The asymmetry of CIs on $d$ is actually very small unless $N$ is very small and $\delta$ is large.) If that's at least roughly correct, then your reported values of average efficiency become indicators of the precision of the different ES estimates. An efficiency estimate (lower row in the figures) of, say, 0.04 (a typical low value) would correspond approximately to $SE = 0.2\delta$ and a 95% CI of about $\pm 0.4\delta$. However, that's very rough because the efficiency values plotted are means of four possibly quite different values. (The dependence of relative variance—and thus CI length—on $N$ is clear in Figure 2, and can also be seen in Figure 3.)

This is not very clear to me. If you think this is a key point, perhaps we could discuss it together when I will revise the article?
It looks complicated here but, I think, mainly because I'm trying to get approx CI info from averaged relative variance values. It's probably not worth considering this further, given that somewhere in future plans may be analyses that tell us directly about CI lengths and coverage %ages, and likely errors (biases) in these. I'm more than happy to discuss at revision time.

**Shieh's *d* and *g***

I can see that including these does give a comprehensive exploration of ES possibilities. Seeing them here led me to go back to Shieh's original article and my comment. You cite that comment (thank you) as reason to doubt the value of the Shieh approach. You also point out (p. 15, bottom) that in the base case the Shieh value is half $d$. This seems to me so crazy that it alone may provide sufficient reason to dump the Shieh approach. For example, a difference of 7.5 between two IQ means is $d = 0.5$, half an $SD$. That makes sense. But Shieh would calculate 0.25! Units of double the SD! How could we make sense of that? How could I even attempt to justify that to students as a meaningful standardised ES measure?

But as you say there's more. Calculating a pooled $SD$ weights by sample size, for good reasons. The Shieh calculation, however counter-weights, so the $SD$ of the *smaller* group is weighted more heavily. That's what we need to calculate the variance, but, surely, not a value that could justifiably be used as standardiser—the units in which to express our standardised ES.

Even more, again as you say: Simply change the $n$-ratio and the $SD$ estimate changes, so we can't see the ES value we get as an estimate of any readily understood population quantity.

All that seems to me more than enough reason simply to dump the Shieh approach. Imho, it's worth no more than a para or two to explain why it's not worth pursuing.

I get your point and I agree with all what you say. At the same time, Shieh's $d$ is based on the same statistical quantity as Welch's $t$-test. By "same quantity", I mean that the percentage of $p$-values associated with Welch's $t$-test below the alpha risk exactly equals the proportion of confidence intervals around Shieh's $d$ that does not include 0. Our first motivation to write this paper was to advice researchers with a measure they could use when performing Welch's $t$-test, in the continuity of our previously written articles on the subject ("Why Psychologists Should by Default Use Welch's $t$-test Instead of Student's $t$-test" and "Taking Parametric Assumptions Seriously: Arguments for the Use of Welch's $F$-test instead of the Classical $F$-test in One-Way ANOVA"). In that context, I think it's a bit weird to not include this effect size estimator in our comparisons. Moreover, before running all my simulations, I thought that this test was the best from an inferential point of view (again, due to this mathematical relation). The study of bias and variance reveals that even for inference, it has big flaws. For that alone, I find it interesting to include it in the comparisons. At the same time, we can (and we will) insist a little more on the fact that the mathematical relation between Welch's t-test and Shieh's d explains why we took this ES estimator into account, and specify at the outset that the simulations will reveal that this measure is not very defensible, even purely statistically speaking.

**Results section** p. 24, ll. 356-8: moving left to right, columns 2-4: in both figures the bias increases slightly then decreases considerably—not "moving left to right... the larger the bias"

Have you noticed that in column 4, we don't use the same scale as in column 2 and 3 (this is true for both Figures 2 and 3)? So indeed the bias is larger in column 3 than in column 2, and larger in column 4 than in column 3.

In some places I did note the differing vertical scales in your figures, esp. Column 4. But I slipped up in my comments on interpretation of those figures. Sorry.

p. 27, l. 403: Unequal variances. In Figure 4, what are the variances? A single pair of values in all cases, or are the pictured values averaged over a range of variance pairs?

Scenarios in Figure 4 are those where $\sigma_1$ always equals 1, and $\sigma_2$ = .1, .25, .5, 2, 4 or 10, so pictured values are averaged over a range of variance pairs. I'll mention it explicitly in the next version to avoid confusion.

p. 27, ll. 407-8: "when moving from left to right... the larger the bias". But for all Figures 4-9, except 7, bias increases slightly (or hardly) from column 2 to 3, then drops to be smallest in column 4.

Again, we don't use the same scale in column 4 vs. columns 2 and 3. It was mentioned p.24, l. 339.

p. 27, ll. 408-9: "the bias of Hedges' $g_s$ remains very small..." But it is often large, and in every case no smaller than that of Hedges' $g_s^*$.

I meant that the bias of Hedges' g remains very small when the normality assumption is met (but this needs to be clarified better in the sentence!).

I'm wondering what value you use for $\delta$ when assessing relative bias and variance for Hedges' $g$. Perhaps what you define for $\delta_{Cohen}$ back on p. 8, l. 152? If so, that value depends not only on the two variances, but on the two sample sizes. So we're estimating a population ES that doesn't reflect any relevant population in the world and, moreover, assuming a population ES that changes merely because we happen to use different sample sizes.

What would you recommend to use? Indeed I'm using $\delta_{Cohen}$. Mathematically speaking, the expectation of Hedges' $g$ always equals $\delta_{Cohen}$ when both assumptions of normality and equal population variances are met, so we compare mean(Hedges' g) with $\delta_{Cohen}$ in order to compute the raw bias. This explains why I also used $\delta_{Cohen}$ in the denominator when I computed the relative bias.

I had in mind the introduction of Cohen's d on pp. 7-8. We are assuming homogeneity of variance. I made a comment:
p. 8, l. 152: "Use $\sigma$ rather than $\sigma_{pooled}$ (twice) because here we're assuming homogeneity of variance, with $\sigma = \sigma_1 = \sigma_2$ as the common population $SD$? (Also Table 1, line 1 of Note.)" Ok!

→ you agreed with that. So the formula, line 152, with the square root (I'll call that 'the weird formula') reduces to $\sigma = \sigma$. I guess I'm assuming that Cohen's $d$, and also Hedges' $g$, are defined assuming homogeneity of variance and their formulas use $s_p$, the pooled value. They both estimate $\delta$, defined as in your (5).

Thinking further about this, I take your point. In a simulation we know $\sigma_1^2$ and $\sigma_2^2$, as well as the two sample sizes, but we know of no 'true' single $\sigma$ to use in defining the $\delta$ we would like to use as the benchmark to assess the estimates calculated in the simulation. Perhaps using the weird formula is the best we can do; if anything using this means we're bending over backwards to minimise likely estimation error.

Perhaps you could consider shifting the weird formula from the basic intro to Cohen's $d$ on pp. 7-8, where to me it doesn't seem to fit, and introducing it, as a sort of necessary evil, when explaining the simulation evaluation of $d$ and $g$?

However, on p. 27, from l. 411, you say that pooling variance estimates in this situation results in an [effect size] estimator that "will be smaller... or larger... than it should be." What should it be? Doesn't such an estimator match the definition of $\delta$ I refer to above? Then you make the same statement about population values, although that would not be true if you are using the definition of $\delta$ I refer to above—which is based on just such a pooled (and weighted by sample size) population $SD$? Even so, your conclusion at the top of p. 28, dismissing Hedges' $g$, may be justified—when variances differ.

I'm not sure that I understand your comment. Could you please clarify? P.27, we meant that when variances are unequal across populations, the pooled error term is not valid (because it assumes equal population variances). When we compute the bias of Cohen's $d$ in scenarios with unequal population variances, we compare an invalid estimate with an invalid population measure of effect size. Because both estimate and population value are invalid, the problem of Cohen's $d$ in case of heteroscedasticity is not revealed by the calculation of the bias. This is actually the same explanation as p.10:

172 [redacted] If we pool the estimates of two unequal population variances, the
173 estimator of effect size will be smaller as it should be in case of positive pairing (i.e. the
174 group with the larger sample size is extracted from the population with the larger variance)
175 and larger as it should be in case of negative pairing (i.e. the group with the larger sample
176 size is extracted from the population with the smaller variance).[redacted]

Given your explanation about using the weird formula, I think I now understand what you were getting at. Your statement that the estimate and the population measure (calculated using the weird formula) are both invalid expresses the problem that there's probably no good way to calculate the population measure when variances are unequal. No sensible way to specify what the estimate should be estimating. Indeed!

Of course, if we try to avoid the double invalidity you point to by simply averaging the two different $\sigma$ values, we get the $d^*$ and $g^*$ estimators. These may solve the unequal $n$ problem of the weird formula, but still have problems (in my view, severe problems) in terms of interpretability.

p. 28, l. 422: "parameters that we cannot control…". You include the $n$-ratio, which is true if, for example, we are meta-analysing past research. But in many cases a researcher chooses sample sizes, and your investigations are likely to lead to useful advice about choice of sample sizes.

I think it's partly true. About the bias, the larger the control group size, the lower the bias. About the variance, it's a bit more complex: in Supplemental Material 1 we write that when the normality assumption is met "The variance always decreases when the control and/or the experimental group increases. The benefit of adding subjects in the control, in the experimental, or in both groups, in order to reduce the variance, varies as a function of the $SD$-ratio and the population effect size. The only situation where it is optimal to maximize the experimental group is when $\sigma_e > \sigma_c$ and $\delta_{Glass} \approx 0$. Most of the time, it is more efficient to maximize the control groups (e.g. anytime $\sigma_e < \sigma_c$, and when $\delta_{Glass}$ is very large) or to uniformly add subjects in both groups (e.g. when $\sigma_e > \sigma_c$ and $\delta_{Glass}$ is neither null nor huge)" (This is a summary but you can see more details and figures here: https://github.com/mdelacre/Effect-sizes/blob/master/Supplemental$/%$20Material$/%$201/Theoretical$/%$20Variance$/%$20of$/%$20all$/%$20estimators$/%$20as$/%$20a$/%$20function$/%$20of$/%$20population$/%$20parameters.pdf ).

As we can see in this summary, recommendations about sample sizes would be different as a function of $SD$-ratio and most of the time, we don't know the $SD$-ratio in advance. But thanks for pointing this out, it probably deserves at least a footnote!

**Discussion**

I think your three purposes of ES estimators (pp. 3-4) are spot on. Your three desirable properties are indeed the desirable statistical properties, but I'd list interpretability in the research context as the first essential property. If we don't have interpretability for a particular standardised ES estimator then we shouldn't be using it.

Theoretically speaking, I agree with you. But practically, as I've mentioned before, I'm not sure if it's a good idea to rule out an estimator that has such a direct connection to Welch's $t$-test. It is probably possible to introduce things differently, to explain in advance that the Shieh's $d$ is not desirable (and to add that even from an inferential point of view, it has big flaws).

Apart for that, you're right, interpretability should be our first priority. For example, if we don't have an interpretable measure, we cannot use it in order to make informative null hypothesis (e.g. when performing equivalence test). However, I'm a bit concerned with the fact that interpretability and good inferential

properties seem so difficult to reconcile. How could we interpret an estimator correctly if it is very biased? This makes me very reserved about Glass's $d$.

I note these recommendations:

1. p. 10: "Because the assumption of equal variances... is rarely realistic... both Cohen's $d$ and Hedges' $g$ should be abandoned...". I think that's arguable. I'm not convinced that assumption is rarely realistic. The assumption is very often made, for example, I think, in medicine when calculating SMD. I suspect a large proportion of Cochrane systematic reviews include meta-analyses that make this assumption. Of course that doesn't make it justified in every situation, but I suggest the emphasis should be on informed judgment in context rather than simply abandoning these ES estimates.

Perhaps do we need to be more nuanced in asserting this. However, there are many authors who believe that the assumption of homogeneity of variances often does not hold (see for example Erceg-Hurn & Mirosevich, 2008; Zumbo & Coulombe, 1997). In a previous paper (Delacre et al. 2017) we develop many reasons why we think equal population variances are very rare in practice.

Moreover, it's very hard to check for the homogeneity of variances assumption, because:
* the assumption is about population parameters that we don't know ($\sigma_1$ and $\sigma_2$);
* inferential statements about the homogeneity of variances assumptions based on assumptions tests often lack power to detect assumption violations.

Finally, when we look at figures, we notice that when variances are equal across groups, Cohen's $g$ and Cohen's $g^*$ are either identical (Figure 2) or very close (Figure 3). The only exception is when both skewness and kurtosis are very large (as reminder, we used different scale in the fourth column in comparison with all other columns). Most of the time, there is therefore little cost in choosing Cohen's $d^*$ by default. On the contrary, Cohen's $d$ cannot be used in case of heterogeneity of variances. – Delacre, M., Lakens, D., & Leys, C. (2017). Why psychologists should by default use 521 Welch's t-test instead of Student's t-test. International Review of Social Psychology, 522 30 (1), 92–101. https://doi.org/10.5334/irsp.82

– Erceg-Hurn, D. M. & Mirosevich, V. M. (2008). Modern robust statistical methods: An easy way to maximize the accuracy and power of your research. American Psychologist, 63(7), 591. DOI: https://doi.org/10.1037/0003-066X.63.7.591

– Zumbo, B. D. & Coulombe, D. (1997). Investigation of the robust rank-order test for non-normal populations with unequal variances: The case of reaction time. Canadian Journal of Experimental Psychology/Revue Canadienne de Psychologie Expérimentale, 51(2), 139. DOI: https://doi. org/10.1037/1196-1961.51.2.139

2. pp. 17-18: You note the wide criticism of Cohen's $d^*$ (and by implication Hedges' $g^*$) because the standardiser is not the $SD$ of an existing relevant population. You state that, even so, these estimators have very good inferential properties. Grissom and Kim (2012), referring to this averaging of variances, state that we "are estimating the $\sigma$ of a hypothetical population... which is a concern for some ... but not others" (p. 72). I tend towards the concerned end of that range, but I know there are arguments in favour of using the square root of the average of two different variances as standardiser. Maybe it can sometimes be possible to give a reasonable interpretation using such a strange standardiser. Think of two overlapping normal curves with different $SD$s: What $SD$ unit is best for expressing the difference between the two means? Does it make sense to use a compromise between those two $SD$ values? Again, this should be recognised as a judgment call, and lack of interpretability in context should never be over-ruled by good statistical properties.

I find this convincing (especially the fact that interpretability should never be over-ruled by good statistical properties) and will have to think about a better way to introduce this estimator.

1. p. 28, l. 424: "We do not recommend using [Glass's $g$]." I suggest that Glass vs something else is the choice that most clearly should be based on the context. Does it make sense to use the $SD$ of one

group as the standardiser? If so, we should do so, unless there are very strong reasons against. We should use choice of sample sizes and perhaps other strategies to minimise any disadvantage of the Glass ES estimate. You make several observations that can help, mainly that increasing the control group sample size is desirable.

As previously mentioned, we need to increase the control group in order to reduce the bias, but sometimes, we need to increase the experimental group in order to reduce the variance. This makes recommendations about sample sizes very complicated.

2. p. 30, l. 473: "We recommend using Hedges' $g^*$..." You state some strong advantages of this ES estimator, but I remain hesitant because of the possible interpretive difficulty, given its standardiser.

Overall, you provide a wonderful range of detailed findings. I'd love to see you develop the discussion, drawing further on those findings, in a way that guides a researcher's likely full decision sequence. For example, first, do we have some relevant population $SD$ value? If so, use this as standardiser. If not, Glass or something else, considering the context? If not Glass, homogeneity of variance or not? Then, can we improve the precision of estimation of our chosen standardiser by, for example, pooling over studies, or meta-analysing our data with past research? Meaningfulness and interpretability in the context should always be the primary consideration.

The great value of your findings is then to help us be specific about the trade-offs. If I choose Glass, how large is the likely bias? If only a few % I might be prepared to tolerate that, or I might consider a data transformation to reduce the departure from normality. (Perhaps a log transform of RT data?) Of course, all such choices should where possible be made in advance of seeing the data, when formulating the data analysis plan to be preregistered.

This is a set of considerations that I will have to take into account when I rework my article in September. If you're ok with that, I'd love to show you our next draft before submitting it.

I'd be more than happy to comment on future drafts, in due course. Or to discuss any of these fascinating issues!

Earlier I made a very sketchy attempt to estimate roughly what CI length might be, given your relative variance results. You refer (e.g., p. 17, l. 253) to "very good inferential properties". That's highly relevant, but I suggest CI length and coverage should be central issues in assessing the inferential effectiveness of any estimator. Then, when considering some judgment call, I could weigh up any price there might be to pay in terms of a longer CI for one of the options under consideration, or a slight departure from 95% coverage. No doubt investigation of CI length and coverage would be a further project, although perhaps you could derive from your variance results some relevant guidance based on CIs?

Ideally, we'd like to see the performance of CIs based on non-central $t$ distributions, and also bootstrapped CIs, for the various estimators and conditions you investigate. Ideally, again, we could then weigh up the various choices, with some idea as to the cost in terms of bias or CI length or coverage error of a choice that offers superior interpretability—which should always be paramount.

As mentioned above, I already computed CIs for all the conditions, based on non-central $t$ distributions. However, I haven't analyzed them yet but I could use them in order to compute the coverage probability and CI length. Therefore these comments could be part of the "further studies" section, and be the subject of a second paper.

Overall, your investigations give us highly valuable findings to guide such discussions.

**Title**

Current title: "Why Hedges' $g_s^*$ based on the non-pooled standard deviation should be reported with Welch's $t$-test".

Why mention that $t$-test? Yes, it fits with your message that the assumption of homogeneity of variance should be avoided, but it's hardly mentioned in the paper, and if an ES estimate and CI are reported there's no need to report or even mention any $t$-test. I would argue that it's much better to avoid doing so.

More positively, your investigations warrant broader recognition and description. Perhaps something like: "$d$-family standardised effect size estimators: Interpretability, bias, efficiency, and robustness" "$d$-family standardised effect sizes: Bias, efficiency, and robustness" "Cohen's $d$ and related effect size estimators: Interpretability, bias, precision, and robustness" [my current favourite; assumes some discussion of CIs] "Cohen's $d$, Hedges' $g^*$, and related effect size estimators: Interpretability, bias, precision, and robustness" [includes the largest set of terms likely to be used in an online search]

We wanted to mention $t$-test because this answering the question many people asked us: Which effect size should I report for Welch's $t$-test. Yes, the paper discusses more, adding Interpretability, bias, precision, and robustness is a good addition. What do you think about: "Arguments to use the Hedges'$g_s^*$ with the Welch's $t$-test: an investigation of interpretability, bias, precision, and robustness of d-family effect sizes"?

As I said in the blog post, [Voir lien vers le blog post] I'm not convinced that Welch's $t$-test deserves mention, even though you have made a strong case for using that form of the $t$-test in your earlier paper. If people ask 'which $ES$?' I'd answer that they need to make a judgment in context, partly guided by your robustness results—but I've said all that in the post.

I know that you make a general recommendation that, when variances are or may be unequal, we should use Hedges' $g^*$, and, separately, Welch's $t$-test, but I don't see any other link between the two.

Your Equation (10), p. 15, expresses the very strong link between Welch's $t$ and Shieh's $d$. Somewhere I've read a statement that the CI on Shieh's $d$ corresponds with Welch's $t$ test in the sense that the two give the same $p$ values, tho' I can't find that statement in Shieh (2013) or in your preprint or supp material—sorry if I've missed it. However, I'd expect that the CI on your recommended Hedges' $g^*$ and Welch's $t$ would not give the same $p$ values. If that's correct, we have one more reason not to link the two together, for example in the title.

Grissom, R. J., & Kim, J. J. (2012). *Effect sizes for research*, 2*nd* ed. New York: Routledge.

It's nice to see a bunch of likes and retweets to the tweet about the blog post. [See https://twitter.com/TheNewStats/status/14

There's a useful comment (link below) from Brenton Wiernik about relevant work by Doug Bonnett. Very likely you know this, tho' it's new to me. I'd better have a squiz.

Cheers, Geoff

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