



**Université Libre de Bruxelles**  
**Faculté des Sciences Psychologiques et de l'Education**

**TITRE,**

**TITRE SUITE**

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En vue de l'obtention du grade de docteur

Septembre 2021

## **Abstract**

Abstract (voir nombre de mots requis)

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## **Remerciements**

Thank you for following this tutorial!

I hope you'll find it useful to write a very professional dissertation.

# Chapitre 1: Introduction

On attend des chercheurs en psychologie, et des psychologues en général, qu'ils soient capables de produire des connaissances fondées sur des preuves scientifiques (et non sur des croyances et opinions), et également de comprendre et évaluer les recherches menées par d'autres (Haslam & McGarty, 2014). Or, dans un domaine dominé par les analyses quantitatives<sup>1</sup> (Counsell & Harlow, 2017), les connaissances statistiques s'avèrent fondamentales pour comprendre, planifier et analyser une recherche (Everitt, 2001 ; Howitt & Cramer, 2017). Les statistiques font dès lors partie intégrante du cursus de formation des psychologues et jouent un rôle très important dans leur parcours (Hoekstra, Kiers, & Johnson, 2012).

Traditionnellement, depuis plus de 50 ans, les tests-*t* et les ANOVA se trouvent au chœur de la grande majorité des programmes dans les domaines des Sciences Psychologiques et de l'Education (Aiken, West, & Millsap, 2008 ; Curtis & Harwell, 1998 ; Golinski & Cribbie, 2009) et des livres d'introduction aux statistiques pour psychologues (Field, 2013 ; Judd, McClelland, & Ryan, 2011). Cela pourrait vraisemblablement expliquer pourquoi ils sont si persistants dans la recherche en psychologie (Counsell & Harlow, 2017). Ces tests sont les plus fréquemment cités dans la littérature scientifique depuis plus de 60 ans (Byrne, 1996 ; Golinski & Cribbie, 2009 ; Nunnally, 1960). Dans une revue de 486 articles publiés en 2000 dans des journaux populaires en psychologie<sup>2</sup>, Golinski & Cribbie (2009) avaient relevé 140 articles ( $\approx 29\%$ ) au sein desquels les auteurs avaient mené au moins une ANOVA à un ou plusieurs facteurs. Plus récemment, Counsell & Harlow (2017) mentionnaient que parmi un ensemble de 151 études soumises dans 4 revues canadiennes en 2013, environ 40% incluaient une comparaison de moyennes. Peut-être est-ce en raison de leur grande fréquence d'usage, ajoutée à leur apparente simplicité, qu'on tend à croire que la plupart des chercheurs, si pas tous, ont une bonne maîtrise des tests de comparaisons de moyennes (Aiken, West, & Millsap, 2008 ; Hoekstra, Kiers, & Johnson, 2012). Pourtant, certains indices semblent contredire cette conviction.

Bien qu'il existe plusieurs types de tests *t* et d'ANOVA, les chercheurs en psychologie privilégient

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1. parmi 68 articles analysés en 2013 par Counsell et ses collaborateurs (2017) dans 4 revues canadiennes, 92.7% incluaient au moins une analyse quantitative (contre 7.3% incluant une analyse qualitative)

2. Les revues analysées étaient les suivantes: "Child Development", "Journal of Abnormal Psychology", "Journal of Consulting and Clinical Psychology", "Journal of Experimental Psychology: General", "Journal of Personality" et "Social Psychology"

souvent par défaut le test *t* de Student et l'ANOVA de Fisher<sup>3</sup>. Ces tests consistent à comparer les scores moyens de deux (ou plusieurs) groupes indépendants de sujets, et reposent sur les hypothèses que les résidus, indépendants et identiquement distribués soient extraits d'une population qui se distribue normalement et qui a la même variance au sein de chaque groupe (c'est ce qu'on appelle la condition d'homogénéité des variances). Pourtant, on constate que dans les articles publiés, il n'est que rarement fait mention de ces conditions. Osborne & Christianson (2001), par exemple, avaient trouvé que seulement 8% des auteurs reportaient des informations sur les conditions d'application des tests, soit à peine 1% de plus qu'en 1969. Plus récemment, Hoekstra, Kiers, & Johnson (2012) ont montré que sur 50 articles publiés en 2011 dans *Psychological Science* utilisant au moins une ANOVA, test-*t* ou régression, seulement trois discutaient des questions de normalité et d'homogénéité des variances. Par ailleurs, les informations reportées sont souvent non exhaustives (Counsell & Harlow, 2017), et la condition d'homogénéité des variances est encore moins fréquemment citée que celle de normalité. Parmi les 61 articles analysés par Keselman et al. (1998), seulement 5% des articles mentionnaient simultanément les conditions de normalité et d'homogénéité des variances (et en tout, la condition de normalité était mentionnée dans 11% des cas, contre seulement 8% pour la condition d'homogénéité des variances). Golinski & Cribbie (2009) ont fait un constat similaire: parmi les 140 articles qu'ils ont analysé, seulement 11 mentionnaient explicitement la condition de normalité, contre 3 qui mentionnaient celle d'homogénéité des variances.

La non mention des conditions d'application dans les articles ne veut pas forcément dire qu'elles n'ont pas été prises en compte dans les analyses. On pourrait imaginer que les auteurs vérifient les conditions d'application des tests mais ne le mentionnent la plupart du temps que lorsqu'elles sont violées (Counsell & Harlow, 2017). Golinski & Cribbie (2009), par exemple, ont constaté à travers leurs revue de littérature que parmi les 11 articles qui mentionnaient la condition de normalité, 10 montraient une violation de cette dernière. Il est possible que motivés par le désir de rentabiliser l'espace disponible dans les manuscrits (Counsell & Harlow, 2017), les auteurs soient tentés de se limiter aux informations explicitement demandées par les éditeurs

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3. Parfois, ils le font de manière implicite, en indiquant réaliser un test *t* (ou une ANOVA) mais sans préciser duquel (ou de laquelle) il s'agit [retrouver référence]. Cela arrive même avec des méthodologistes! Dans l'article de Tomczak & Tomczak (2014), par exemple, ils parlent de l'ANOVA et du test *t*, sans précision, et ce n'est qu'en lisant l'ensemble de l'article qu'on comprend qu'en réalité, ils font allusion exclusivement au test *t* de Student et à l'ANOVA de Fisher, entre autres, parce qu'ils proposent d'associer ces tests à des mesures de taille d'effet qui impliquent l'usage du terme de variance poolée, qui sera décrit juste après.

et les reviewers des journaux (Counsell & Harlow, 2017). Or, les informations relatives aux conditions d'application des tests en font rarement partie. Par exemple, leur report n'est pas explicitement demandé dans le manuel des normes APA (Hoekstra, Kiers, & Johnson, 2012)<sup>4</sup>. Dans un tel contexte, il n'y a que peu d'intérêt pour les chercheurs à en discuter, si ce n'est pour discuter des violations des conditions (et éventuellement, se servir de cette information pour justifier une décision qui en découle). Il est néanmoins surprenant de constater que de telles discussions apparaissent dans un pourcentage si faible d'articles, puisqu'il a été argumenté à de nombreuses reprises que le respect des conditions de normalité et d'homogénéité des variances est plus l'exception que la norme dans de nombreux domaines de la psychologie (Cain, Zhang, & Yuan, 2017 ; Erceg-Hurn & Mirosevich, 2008 ; Grissom, 2000 ; Micceri, 1989 ; Yuan, Bentler, & Chan, 2004). Bien que l'on ne puisse totalement écarter la possibilité que certains chercheurs prennent des décisions inhérentes aux violations des conditions d'application sans le mentionner dans leur article, l'hypothèse privilégiée par Keselman et al. (1998) est que la majorité des chercheurs applique des tests paramétriques indépendamment du fait que leurs conditions soient ou non respectées. Cette hypothèse semble confirmée par une expérience de Hoekstra, Kiers, & Johnson (2012): afin d'étudier les pratiques des chercheurs lorsqu'ils étaient confrontés à un scénario qui impliquait la réalisation d'un test *t*, d'une ANOVA ou d'une régression linéaire, ces chercheurs ont observé 30 doctorants qui travaillaient depuis au moins deux ans dans des départements de psychologie aux Pays-Bas et qui avaient dû pratiquer tous ces tests au moins une fois. Alors que *tous* ont opté pour un test paramétrique, les conditions d'application de ces tests n'ont été testées que dans un faible pourcentage de cas. Après l'expérience, les 30 doctorants ont été soumis à un questionnaire. Celui-ci a révélé que la non vérification des conditions d'application des tests était dûe à leur manque de familiarité avec les conditions d'application des tests, plutôt que par un choix délibéré de leur part. Il est à noter qu'en réalité, vérifier les conditions d'application des tests est bien plus complexe qu'il n'y paraît, et tout chercheur désireux d'améliorer la transparence dans la transmission des analyses de données resterait confronté à un problème majeur: les conditions d'homogénéité des variances et de normalité reposent sur les paramètres de *population* et non sur les paramètres d'*échantillon*. Comme ces paramètres de population ne sont pas connus (Hoekstra, Kiers, & Johnson, 2012),

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4. Depuis l'article de Hoekstra et al. (2012), la septième édition du manuel des normes APA est parues. La mention explicite des conditions d'application ne fait pas partie des mises à jours proposées dans cette nouvelle édition.

on doit utiliser les paramètres de l'échantillon pour tenter d'inférer sur le respect des conditions d'application. Souvent, les chercheurs font cette inférence en utilisant des tests d'hypothèses, mais il a été démontré que l'application d'un test conditionnellement aux résultats d'un test statistique préliminaire a pour effet d'augmenter l'erreur de type I (Schucany & Tony Ng, 2006). La difficulté que représente la vérification des conditions d'application ne constituerait pas réellement un problème, en soi, si les test *t* de Student et *F* de Fisher étaient susceptibles de fournir des conclusions non biaisées et fiables même en cas d'écart à ces conditions, or ce n'est malheureusement pas toujours le cas. Ces tests sont particulièrement sensibles aux violations de la condition d'homogénéité des variances, et cette sensibilité est accentuée lorsque les échantillons n'ont pas tous la même taille (Keselman et al., 1998).

Compte tenu de tous les éléments précités, il semblerait donc qu'une solution viable serait d'utiliser des tests qui ne reposent pas sur les conditions de normalité et d'homogénéité des variances. Il existe, par exemple, des tests qui reposent sur la comparaison d'autres indicateurs de tendance centrale que la moyenne (comme la moyenne trimmée), mais ces derniers font très souvent face à une forte résistance de la part des chercheurs, qui persistent à vouloir comparer les moyennes (Erceg-Hurn & Mirosevich, 2008 ; Keselman et al., 1998 ; Wilcox, 1998). Dans la mesure où une revue approfondie de la littérature démontre que les taux d'erreur de type I et II des tests *t* de Student et *F* de Fisher sont bien plus affectés par le non respect de la condition d'homogénéité des variances que par le non respect de la condition de normalité (Erceg-Hurn & Mirosevich, 2008 ; Grissom, 2000 ; Hoekstra, Kiers, & Johnson, 2012 ; Osborne & Waters, 2002), nous recommandons aux psychologues de remplacer les tests *t* de Student et *F* de Fisher par le test de Welch, un test de comparaison de moyennes qui ne requiert pas la condition d'homogénéité des variances. Cette solution a été suggérée par de nombreux auteurs avant nous (voir, par exemple Rasch, Kubinger, & Moder, 2011 ; Ruxton, 2006 ; Zimmerman, 2004), pourtant, cela semble avoir eu un impact limité sur les pratiques des chercheurs en psychologie. Pour tenter de les influencer, nous nous sommes particulièrement appliqués, au sein des articles présentés dans les chapitres 2 à 3, à nous adresser directement à ce public de chercheurs. Pour ce faire, nous avons tenté (1) d'expliquer concrètement pourquoi selon nous, la condition d'homoscédasticité n'est pas réaliste, en nous appuyant sur des exemples directement issus de la recherche en psychologie, (2) de définir certaines notions statistiques de la manière la plus simple possible, en limitant les explications mathématiques et (3) d'illustrer graphiquement

l'impact des violations de la condition d'homoscédasticité, plutôt que de fournir des tableaux de chiffres lourds et complexes. De plus, nous avons conclu ces articles par des recommandations concrètes, afin d'aider les chercheurs à extraire le message clé de ces articles. Ajoutons que les deux articles ont été soumis et publiés dans une revue Open Access (l'*International Review of Social Psychology*) afin d'assurer la diffusion la plus large possible de notre message.

Au delà des tests d'hypothèse, de nombreux journaux de psychologies encouragent (voire même requièrent) de quantifier la taille des effets étudiés et de fournir un intervalle de confiance autour des estimations de taille d'effet (Cumming, Fidler, Kalinowski, & Lai, 2012). L'année 1999 a joué un rôle clé dans la mise en oeuvre de ces recommandations, puisque l'*APA Task Force* a publié un rapport dans lequel elle soulignait l'importance de reporter des mesures de taille d'effet. Ce rapport a été suivi de recommandations précises de la part de l'American Psychological Association (APA) et de l'American Educational Research Association (AERA) quant à la manière de reporter ces mesures (Peng, Chen, Chiang, & Chiang, 2013). Or, il semblerait que ces diverses recommandations aient été associées à des modifications dans les pratiques des chercheurs. Peng, Chen, Chiang, & Chiang (2013) ont étudié l'évolution du taux moyen de report des mesures de taille d'effet en comparant ce taux moyen avant et après 1999, distinctement dans 19 revues consacrées à la recherche dans les domaines de la Psychologie et de l'Education. Ils ont noté une augmentation de ce taux variant de 5.2 % à 96.3 % dans chacun de ces journaux. Ils ont cependant également noté la persistance de pratiques inadéquates, telles que la dominance de la mesure du traditionnel  $d$  de Cohen. Le  $d$  de Cohen est une mesure de taille d'effet standardisée qui appartient à la famille  $d$  et qui entretient une relation mathématique directe avec le  $t$  de Student. Par conséquent, il dépend des mêmes conditions d'application que le test  $t$  de Student, c'est donc sans surprise qu'en cas de violation de ces conditions, son usage peut amener à une sous-représentation (ou au contraire à une sur-représentation) de la taille d'effet (Grissom & Kim, 2001). De même que pour le test  $t$ , il semblerait que ce soit essentiellement la violation de la condition d'homogénéité des variances qui soit problématique. De nombreux auteurs se sont demandés si le  $d$  de Cohen pourrait être remplacé par une autre mesure de la même famille lorsque les variances diffèrent d'une population à l'autre, mais ils n'ont pas trouvé de consensus quant à la solution la plus appropriée (Shieh, 2013). C'est pour cette raison que nous avons décidé de réaliser des simulations Monte Carlo pour comparer le traditionnel  $d$  de Cohen aux mesures de la famille  $d$  les plus communément proposées pour remplacer le

classique  $d$  de Cohen en cas d'hétéroscédasticité. Les résultats de ces simulations sont présentés au sein de l'article du chapitre 4. Si nous nous focalisons exclusivement sur les mesures de la famille  $d$  dans ce chapitre, c'est parce que les chercheurs utilisent très fréquemment le  $d$  de Cohen lorsqu'ils réalisent un test  $t$ . Nous tenons cependant à rappeler que la notion de taille d'effet est très vaste. Elle englobe toute mesure susceptible de fournir une information relative à l'ampleur d'un effet étudié, que ce soit à travers une mesure *non standardisée* (moyenne, médiane, coefficient de régression non standardisé...) ou à travers une mesure *standardisées* ( $R^2$ , coefficient de régression standardisé, différence de moyennes standardisée..., Counsell & Harlow, 2017). Les mesures non standardisées peuvent également s'avérer très utiles, et ce même si une emphase sur les tailles d'effet standardisées a pu donner l'impression que seules ces dernières étaient dignes d'intérêt (pour une discussion intéressante sur l'intérêt des mesures non standardisées, nous recommandons l'article de Pek & Flora, 2018). Au sein de ce chapitre, nous avons tenté de comparer l'efficacité de différents estimateurs sous des déviations réalistes de la condition de normalité, en nous appuyant sur l'investigation de Cain, Zhang, & Yuan (2017). Ces auteurs avaient en effet calculé les indicateurs d'asymétrie et d'aplatissement<sup>5</sup> de 1567 distributions univariées provenant de 194 articles publiés dans *Psychological Science* (de Janvier 2013 à juin 2014) et *American Education Research Journal* (de janvier 2010 à juin 2014). Les résultats de cette étude nous ont aidé à définir le degré d'asymétrie et d'aplatissement des distributions dont étaient extraits nos échantillons, au sein de nos simulations. De plus, au delà des simulations, nous proposons des outils afin d'aider les chercheurs à calculer différents estimateurs de taille d'effet ainsi que les bornes de l'intervalle de confiance autour de ces estimateurs. Cela nous a semblé être une nécessité puisque malgré les recommandations, il semblerait que les mesures de taille d'effet soient rarement accompagnées d'un intervalle de confiance dans la littérature (Counsell & Harlow, 2017 ; Peng, Chen, Chiang, & Chiang, 2013). C'est vrai même lorsque ces mesures sont utilisées seules (indépendamment d'un test d'hypothèse, Counsell & Harlow, 2017). Dans le cas des mesures standardisées, le calcul des intervalles de confiance est particulièrement complexe puisqu'il requiert l'usage des distributions non centrales (Balluerka, Gómez, & Hidalgo, 2005). Cela nous a motivé à créer un package R en vue d'aider les chercheurs, ainsi qu'une application Shiny pour ceux qui ne sont pas familiers avec R. Ces outils seront fournis dans le chapitre 4.

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5. Nous utilisons ce terme "aplatissement" avec quelques réserves puisqu'en réalité, il est plus question de la densité des extrémités de la distribution que réellement d'aplatissement.

Qu'elles soient standardisées ou non, les mesures de taille d'effet ainsi que leur intervalles de confiance sont parfois vues comme des outils qui permettent de combler certaines limites des tests d'hypothèse. Une critique fréquemment avancée à l'égard des tests d'hypothèse est le fait qu'un rejet de l'hypothèse nulle ne fournit qu'une idée de la direction de l'effet, sans information relative à son importance. Cette critique repose implicitement sur la conception d'après laquelle l'hypothèse nulle doit être définie comme l'absence d'effet (ou l'absence de différence entre les groupes). Il est vrai que c'est l'hypothèse nulle la plus couramment définie par les chercheurs (Lakens, Scheel, & Isager, 2018 ; Nickerson, 2000). Pourtant, lorsque c'est pertinent, il est possible d'incorporer la significativité pratique dans les tests d'hypothèse. Cela implique de réfléchir *a priori* aux effets qui présentent un intérêt pratique aux yeux des chercheurs et des praticiens (Fraas & Newman, 2000), ce qui peut se faire sur base de diverses considérations, tel que des comparaisons coûts/bénéfices, par exemple (Fraas & Newman, 2000). Dans ce contexte, l'hypothèse nulle n'est plus que l'effet soit nul, mais qu'il ne dépasse pas une certaine valeur ou autrement dit, dans le cadre d'un test de comparaison de moyenne, que la différence de moyennes entre les groupes ne dépasse pas une certaine valeur (Newman, Fraas, & Herbert, 2001). Un rejet de l'hypothèse nulle ne constituera alors plus un soutien en faveur de n'importe quel effet non nul, mais plutôt un soutien en soutien en faveur d'un effet jugé pertinent. Il est également possible de montrer un soutien en faveur de l'**absence** d'un effet jugé pertinent, en définissant comme hypothèse nulle que l'effet *dépasse* une certaine valeur (Lakens, Scheel, & Isager, 2018). C'est le principe des tests d'équivalence, qui feront l'objet du cinquième et dernier chapitre de cette thèse.

# Chapitre 2: utiliser le test *t* de Welch par défaut

## RESEARCH ARTICLE

### Why Psychologists Should by Default Use Welch's *t*-test Instead of Student's *t*-test

Marie Delacre\*, Daniël Lakens† and Christophe Leys\*

When comparing two independent groups, psychology researchers commonly use Student's *t*-tests. Assumptions of normality and homogeneity of variance underlie this test. More often than not, when these conditions are not met, Student's *t*-test can be severely biased and lead to invalid statistical inferences. Moreover, we argue that the assumption of equal variances will seldom hold in psychological research, and choosing between Student's *t*-test and Welch's *t*-test based on the outcomes of a test of the equality of variances often fails to provide an appropriate answer. We show that the Welch's *t*-test provides a better control of Type 1 error rates when the assumption of homogeneity of variance is not met, and it loses little robustness compared to Student's *t*-test when the assumptions are met. We argue that Welch's *t*-test should be used as a default strategy.

**Keywords:** Welch's *t*-test; Student's *t*-test; homogeneity of variance; Levene's test; Homoscedasticity; statistical power; type 1 error; type 2 error

Independent sample *t*-tests are commonly used in the psychological literature to statistically test differences between means. There are different types of *t*-tests, such as Student's *t*-test, Welch's *t*-test, Yuen's *t*-test, and a bootstrapped *t*-test. These variations differ in the underlying assumptions about whether data is normally distributed and whether variances in both groups are equal (see, e.g., Rasch, Kubinger, & Moder, 2011; Yuen, 1974). Student's *t*-test is the default method to compare two groups in psychology. The alternatives that are available are considerably less often reported. This is surprising, since Welch's *t*-test is often the preferred choice and is available in practically all statistical software packages.

In this article, we will review the differences between Welch's *t*-test, Student's *t*-test, and Yuen's *t*-test, and we suggest that Welch's *t*-test is a better default for the social sciences than Student's and Yuen's *t*-tests. We do not include the bootstrapped *t*-test because it is known to fail in specific situations, such as when there are unequal sample sizes and standard deviations differ moderately (Hayes & Cai, 2007).

When performing a *t*-test, several software packages (i.e., R and Minitab) present Welch's *t*-test by default. Users can request Student's *t*-test, but only after explicitly stating that the assumption of equal variances is

met. Student's *t*-test is a parametric test, which means it relies on assumptions about the data that are analyzed. Parametric tests are believed to be more powerful than non-parametric tests (i.e., tests that do not require assumptions about the population parameters; Sheskin, 2003). However, Student's *t*-test is generally only more powerful when the data are normally distributed (the assumption of normality) and the variances are equal in both groups (homoscedasticity; the assumption of homogeneity of variance; Carroll & Schneider, 1985; Erceg-Hurn & Mirosevich, 2008).

When sample sizes are equal between groups, Student's *t*-test is robust to violations of the assumption of equal variances as long as sample sizes are big enough to allow correct estimates of both means and standard deviations (i.e.,  $n \geq 5$ ).<sup>1</sup> except when distributions underlying the data have very high skewness and kurtosis, such as a chi-square distribution with 2 degrees of freedom. However, if variances are *not* equal across groups and the sample sizes differ across independent groups, Student's *t*-test can be severely biased and lead to invalid statistical inferences (Erceg-Hurn & Mirosevich, 2008).<sup>2,3</sup> Here, we argue that there are no strong reasons to assume equal variances in the psychological literature by default nor substantial costs in abandoning this assumption.

In this article, we will first discuss why we need a default test and why a two-step procedure where researchers decide whether or not to use Welch's *t*-test based on a check of the assumption of normality and equal variances is undesirable. Then, we will discuss whether the assumption of equal variances is plausible in psychology and point out research areas where this assumption is implausible.

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We will then review differences between Student's *t*-test, Welch's *t*-test, and Yuen's *t*-test and show through simulations that the bias in Type 1 error rates when Yuen's *t*-test is used is often severely inflated (above 0.075, which is "critical inflation", following Bradley, 1978) and that the bias in Type 1 error rates when Student's *t*-test is used has a larger impact on statistical inferences than the rather modest impact on the Type 2 error rate of always using Welch's *t*-test by default. Given our analysis and the availability of Welch's *t*-test in all statistical software, we recommend a procedure where Welch's *t*-test is used by default when sample sizes are unequal.

### **Limitations of Two-Step Procedures**

Readers may have learned that the assumptions of normality and of equal variances (or the homoscedasticity assumption) must be examined using assumption checks prior to performing any *t*-test. When data are not normally distributed, with small sample sizes, alternatives should be used. Classic nonparametric statistics are well-known, such as the Mann-Whitney U-test and Kruskal-Wallis. However, unlike a *t*-test, tests based on rank assume that the distributions are the same between groups. Any departure to this assumption, such as unequal variances, will therefore lead to the rejection of the assumption of equal distributions (Zimmerman, 2000). Alternatives exist, known as the "modern robust statistics" (Wilcox, Granger, & Clark, 2013). For example, data sets with low kurtosis (i.e., a distribution flatter than the normal distribution) should be analyzed with the two-sample trimmed *t*-test for unequal population variances, also called Yuen's *t*-test (Luh & Guo, 2007; Yuen, 1974). However, analyses in a later section will show that the normality assumption is not very important for Welch's *t*-test and that there are good reasons to, in general, prefer Welch's *t*-test over Yuen's *t*-test.

With respect to the assumption of homogeneity of variance, if the test of the equality of variance is non-significant and the assumption of equal variances cannot be rejected, homoscedastic methods such as the Student's *t*-test should be used (Wilcox et al., 2013). If the test of the equality of variances is significant, Welch's *t*-test should be used instead of Student's *t*-test because the assumption of equal variances is violated. However, testing the equality of variances before deciding which *t*-test is performed is problematic for several reasons, which will be explained after having described some of the most widely used tests of equality of variances.

### **Different Ways to Test for Equal Variances**

Researchers have proposed several tests for the assumption of equal variances. Levene's test and the F-ratio test are the most likely to be used by researchers because they are available in popular statistical software (Hayes & Cai, 2007). Levene's test is the default option in SPSS. Levene's test is the One-Way ANOVA computed on the terms  $|x_{ij} - \bar{\theta}_j|$ , where  $x_{ij}$  is the *i*th observation in the *j*th group, and  $\bar{\theta}_j$  is the "center" of the distribution for the *j*th group (Carroll & Schneider, 1985). In R, the "center" is by default the median, which is also called "Brown Forsythe test for equal variances". In SPSS, the "center" is by default the mean

(which is the most powerful choice when the underlying data are symmetrical).<sup>4</sup> The F-ratio statistic is obtained by computing  $SD2/SD1$  (standard deviation ratio, SDR). A generalization of the F-ratio test, to be used when there are more than two groups to compare, is known as the Bartlett's test.

The F-ratio test and the Bartlett test are powerful, but they are only valid under the assumption of normality and collapse as soon as one deviates even slightly from the normal distribution. They are therefore not recommended (Rakotomalala, 2008).

Levene's test is more robust than Bartlett's test and the F-ratio test, but there are three arguments against the use of Levene's test. First, there are several ways to compute Levene's test (i.e., using the median or mean as center), and the best version of the test for equal variances depends on how symmetrically the data is distributed, which is itself difficult to statistically quantify.

Second, performing two tests (Levene's test followed by a *t*-test) on the same data makes the alpha level and power of the *t*-test dependent upon the outcome of Levene's test. When we perform Student's or Welch's *t*-test conditionally on a significant Levene's test, the long-run Type 1 and Type 2 error rates will depend on the power of Levene's test. When the power of Levene's test is low, the error rates of the conditional choice will be very close to Student's error rates (because the probability of choosing Student's *t*-test is very high). On the other hand, when the power of Levene's test is very high, the error rates of the conditional choice will be very close to Welch's error rate (because the probability of choosing Welch's *t*-test is very high; see Rasch, Kubinger, & Moder, 2011). When the power of Levene's test is medium, the error rates of the conditional choice will be somewhere between Student's and Welch's error rates (see, e.g., Zimmerman, 2004). This is problematic when the test most often performed actually has incorrect error rates.

Third, and relatedly, Levene's test can have very low power, which leads to Type 2 errors when sample sizes are small and unequal (Nordstokke & Zumbo, 2007). As an illustration, to estimate the power of Levene's test, we simulated 1,000,000 simulations with balanced designs of different sample sizes (ranging from 10 to 80 in each condition, with a step of 5) under three SDR where the true variances are unequal, respectively, 1.1, 1.5, and 2, yielding 45,000,000 simulations in total. When  $SDR = 1$ , the equal variances assumption is true when  $SDR > 1$  the standard deviation of the second sample is bigger than the standard deviation of the first sample and when  $SDR < 1$  the standard deviation of the second sample is smaller than the standard deviation of the first sample. We ran Levene's test centered around the mean and Levene's test centered around the median and estimated the power (in %) to detect unequal variances with equal sample sizes (giving the best achievable power for a given total *N*; see **Figure 1**).<sup>5</sup>

As we can see in the graph, the further SDR is from 1, the smaller the sample size needed to detect a statistically significant difference in the SDR. Furthermore, for each SDR, power curves of the Levene's test based on the mean



**Figure 1:** Estimated power of Levene's test as a function of sample size, SDR and centering parameter.

are slightly above power curves of the Levene's test based on the median, meaning that it leads to slightly higher power than Levene's test based on the median. This can be due to the fact that data is extracted from normal distributions. With asymmetric data, the median would perform better. When SDR = 2, approximately 50 subjects are needed to have 80 percent power to detect differences, while approximately 70 subjects are needed to have 95 percent power to detect differences (for both versions of Levene's test). To detect an SDR of 1.5 with Levene's test, approximately 120 subjects are needed to reach a power of 0.80 and about 160 to reach a power of 0.95. Since such an SDR is already very problematic in terms of the type I error rate for the Student's *t*-test (Bradley, 1978), needing such a large sample size to detect it is a serious hurdle. This issue becomes even worse for lower SDR, since an SDR as small as 1.1 already calls for the use of Welch's *t*-test (See table A3.1 to A3.9 in the additional file). Detecting such a small SDR calls for a huge sample size (a sample size of 160 provides a power rate of 0.16).

Since Welch's *t*-test has practically the same power as Student's *t*-test, even when SDR = 1, as explained using simulations later, we should seriously consider using Welch's *t*-test by default.

The problems in using a two-step procedure (first testing for equality of variances, then deciding upon which test to use) have already been discussed in the field of statistics (see e.g., Rasch, Kubinger, & Moder, 2011; Ruxton, 2006; Wilcox, Granger, & Clark, 2013; Zimmerman, 2004), but these insights have not changed the current practices in psychology, as of yet. More importantly, researchers do not even seem to take the assumptions of Student's *t*-test into consideration before performing the test, or at least rarely discuss assumption checks.

We surveyed statistical tests reported in the journal *SPPS* (*Social Psychological and Personality Science*) between April 2015 and April 2016. From the total of 282 studies, 97 used a *t*-test (34.4%), and the homogeneity of variance was explicitly discussed in only 2 of them. Moreover, based on the reported degrees of freedom in the results section, it seems that Student's *t*-test is used most often and that alternatives are considerably less popular. For 7 studies,

there were decimals in the values of the degrees of freedom, which suggests Welch's *t*-test might have been used, although the use of Welch's *t*-test might be higher but not identifiable because some statisticians recommend rounding the degrees of freedom to round numbers.

To explain this lack of attention to assumption checks, some authors have argued that researchers might have a lack of knowledge (or a misunderstanding) of the parametric assumptions and consequences of their violations or that they might not know how to check assumptions or what to do when assumptions are violated (Hoekstra, Kiers, & Johnson, 2012).<sup>6</sup> Finally, many researchers don't even know there are options other than the Student's *t*-test for comparing two groups (Erceg-Hurn & Mirosevich, 2008). How problematic this is depends on how plausible the assumption of equal variances is in psychological research. We will discuss circumstances under which the equality of variances assumption is especially improbable and provide real-life examples where the assumption of equal variances is violated.

### Homogeneity of Variance Assumptions

The homogeneity of variances assumption is rarely true in real life and cannot be taken for granted when performing a statistical test (Erceg-Hurn & Mirosevich, 2008; Zumbo & Coulombe, 1997). Many authors have examined real data and noted that SDR is often different from the 1:1 ratio (see, e.g., Grissom, 2000; Erceg-Hurn & Mirosevich, 2008). This shows that the presence of unequal variances is a realistic assumption in psychological research.<sup>7</sup> We will discuss three different origins of unequal standard deviations across two groups of observations.

A first reason for unequal variances across groups is that psychologists often use *measured variables* (such as age, gender, educational level, ethnic origin, depression level, etc.) instead of random assignment to condition. In their review of comparing psychological findings from all fields of the behavioral sciences across cultures, Henrich, Heine, and Norenzayan (2010) suggest that parameters vary largely from one population to another. In other words, variance is not systematically the same in every pre-existing group. For example, Feingold (1992) has

shown that intellectual abilities of males were more variable than intellectual abilities of females when looking at several standardized test batteries measuring general knowledge, mechanical reasoning, spatial visualization, quantitative ability, and spelling. Indeed, the variability hypothesis (that men demonstrate greater variability than women) is more than a century old (for a review, see Shields, 1975). In many research domains, such as mathematics performance, there are strong indicators that variances ratios differ between 1.1 and 1.2, although variances ratios do not differ in all countries, and the causes for these differences are not yet clear. Nevertheless, it is an empirical fact that variances ratios can differ among pre-existing groups.

Furthermore, some pre-existing groups have different variability by definition. An example from the field of education is the comparison of selective school systems (where students are accepted on the basis of selection criterions) versus comprehensive school systems (where all students are accepted, whatever their aptitudes; see, e.g., Hanushek & Wößmann, 2006). At the moment that a school accepts its students, variability in terms of aptitude will be greater in a comprehensive school than in a selective school, by definition.

Finally, a quasi-experimental treatment can have a different impact on variances between groups. Hanushek and Wößmann (2006) suggest that there is an impact of the educational system on variability in achievement. Even if variability, in terms of aptitude, is greater in a comprehensive school than in a selective school at first, a selective school system at primary school increases inequality (and then variability) in achievement in secondary school. Another example is variability in moods. Cowdry, Gardner, O'Leary, Leibenluft, & Rubinow (1991) noted that intra-individual variability is larger in patients suffering from premenstrual syndrome (PMS) than in normal patients and larger in normal patients than in depressive patients. Researchers studying the impact of an experimental treatment on mood changes can expect a bigger variability of mood changes in patients with PMS than in normal or depressive patients and thus a higher standard deviation in mood measurements.

A second reason for unequal variances across groups is that while variances of two groups are the same when group assignment is completely randomized, deviation from equality of variances can occur later, as a consequence of an experimental treatment (Cumming, 2013; Erceg-Hurn & Mirosevich, 2008; Keppel, 1991). For example, psychotherapy for depression can increase the variability in depressive symptoms, in comparison with a control group, because the effectiveness of the therapy will depend on individual differences (Bryk & Raudenbush, 1988; Erceg-Hurn & Mirosevich, 2008). Similarly, Kester (1969) compared the IQs of students from a control group with the IQs of students when high expectancies about students were induced in the teacher. While no effect of teacher expectancy on IQ was found, the variance was bigger in the treatment group than in the control group (56.52 vs. 32.59, that is, SDR ≈ 1.32). As proposed by Bryk

and Raudenbush (1988), this can result from the interaction between the treatment and the students' reactions: students can react differently to the induced expectations. More generally, whenever a manipulation has individual moderators, variability should increase compared to a control condition.

Knowing whether standard deviations differ across conditions is important information, but in many fields, we have no accurate estimates of the standard deviation in the population. Whereas we collect population effect sizes in meta-analyses, these meta-analyses often do not include the standard deviations from the literature. As a consequence, we regrettably do not have easy access to aggregated information about standard deviations across research areas, despite the importance of this information. It would be useful if meta-analysts start to code information about standard deviations when performing meta-analyses (Lakens, Hilgard, & Staaks, 2016), such that we can accurately quantify whether standard deviations differ between groups, and how large the SDR is.

### The Mathematical Differences Between Student's *t*-test, Welch's *t*-test, and Yuen's *t*-test

So far, we have simply mentioned that Welch's *t*-test differs from Student's *t*-test in that it does not rely on the equality of variances assumption. In this section, we will explain why this is the case. The Student's *t* statistic is calculated by dividing the mean difference between group  $\bar{x}_1 - \bar{x}_2$  by a pooled error term, where  $s_1^2$  and  $s_2^2$  are variance estimates from each independent group, and where  $n_1$  and  $n_2$  are the respective sample sizes for each independent group (Student, 1908):

$$t = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{\frac{(n_1 - 1)s_1^2 + (n_2 - 1)s_2^2}{n_1 + n_2 - 2} * \left( \frac{1}{n_1} + \frac{1}{n_2} \right)}} \quad (1)$$

The degrees of freedom are computed as follows (Student, 1908):

$$df = n_1 + n_2 - 2 \quad (2)$$

Student's *t*-test is calculated based on a *pooled* error term, which implies that both samples' variances are estimates of a common population variance. Whenever the variances of the two normal distributions are not similar and the sample sizes in each group are not equal, Student's *t*-test results are biased (Zimmerman, 1996). The more unbalanced the distribution of participants across both independent groups, the more Student's *t*-test is based on the incorrect standard error (Wilcox et al., 2013) and, consequently, the less accurate the computation of the *p*-value will be.

When the larger variance is associated with the *larger* sample size, there is a decrease in the nominal Type 1 error rate (Nimon, 2012; Overall, Atlas, & Gibson, 1995). The reason for this is that the error term increases, and,

as a consequence, the Student's *t*-value decreases, leading to fewer significant findings than expected with a specific alpha level. When the larger variance is associated with the *smaller* sample size, the Type 1 error rate is inflated (Nimon, 2012; Overall, Atlas, & Gibson, 1995). This inflation is caused by the under-evaluation of the error term, which increases Student's *t* value and thus leads to more significant results than are expected based on the alpha level.

As discussed earlier, Student's *t*-test is robust to unequal variances as long as the sample sizes of each group are similar (Nimon, 2012; Ruxton, 2006; Wallenstein, Zucker, & Fleiss, 1980), but, in practice, researchers often have different sample sizes in each of the independent groups (Ruxton, 2006). Unequal sample sizes are particularly common when examining measured variables, where it is not always possible to determine *a priori* how many of the collected subjects will fall in each category (e.g., sex, nationality, or marital status). However, even with complete randomized assignment to conditions, where the same number of subjects are assigned to each condition, unequal sample sizes can emerge when participants have to be removed from the data analysis due to being outliers because the experimental protocol was not followed when collecting the data (Shaw & Mitchell-Olds, 1993) or due to missing values (Wang et al., 2012).

Previous work by many researchers has shown that Student's *t*-test performs surprisingly poorly when variances are unequal and sample sizes are unequal (Glass, Peckham, & Sanders, 1972; Overall, Atlas, & Gibson, 1995; Zimmerman, 1996), especially with small sample sizes and low alpha levels (e.g., alpha = 1%; Zimmerman, 1996). The poor performance of Student's *t*-test when variances are unequal becomes visible when we look at the error rates of the test and the influence of both Type 1 errors and Type 2 errors. An increase in the Type 1 error rate leads to an inflation of the number of false positives in the literature, while an increase in the Type 2 error rate leads to a loss of statistical power (Banerjee et al., 2009).

To address these limitations of Student's *t*-test, Welch (1947) proposed a separate-variance *t*-test computed by dividing the mean difference between group  $\bar{x}_1 - \bar{x}_2$  by an unpooled error term, where  $s_1^2$  and  $s_2^2$  are variance estimates from each independent group, and where  $n_1$  and  $n_2$  are the respective sample sizes for each independent group:<sup>8</sup>

$$t = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{\frac{s_1^2}{n_1} + \frac{s_2^2}{n_2}}} \quad (3)$$

The degrees of freedom are computed as follows:

$$df = \frac{\left( \frac{s_1^2}{n_1} + \frac{s_2^2}{n_2} \right)^2}{\frac{\left( s_1^2 \right)^2}{n_1} + \frac{\left( s_2^2 \right)^2}{n_2}} \quad (4)$$

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**). When the variance is the same in both independent groups but the sample sizes differ, the *t*-value remains identical, but the degrees of freedom differ (and, as a consequence, the *p*-value differs). Similarly, when the variances differ between independent groups but the sample sizes in each group are the same, the *t*-value is identical in both tests, but the degrees of freedom differ (and, thus, the *p*-value differs). The most important difference between Student's *t*-test and Welch's *t*-test, and indeed the main reason Welch's *t*-test was developed, is when both the variances and the sample sizes differ between groups, the *t*-value, degrees of freedom, and *p*-value all differ between Student's *t*-test and Welch's *t*-test. Note that, in practice, samples practically never show exactly the same pattern of variance as populations, especially with small sample sizes (Baguley, 2012; also see table A2 in the additional file).

Yuen's *t*-test, also called "20 percent trimmed means test", is an extension of Welch's *t*-test and is allegedly more robust in case of non-normal distributions (Wilcox & Keselman, 2003). Yuen's *t*-test consists of removing the lowest and highest 20 percent of the data and applying Welch's *t*-test on the remaining values. The procedure is explained and well-illustrated in a paper by Erceg-Hurn and Mirosevich (2008).

#### Simulations: Error Rates for Student's *t*-test versus Welch's *t*-test

When we are working with a balanced design, the statistical power (the probability of finding a significant effect, when there is a true effect in the population, or 1 minus the Type 2 error rate) is very similar for Student's *t*-test

	Equal variances	Unequal variances
Balanced design	$t_{\text{Welch}} = t_{\text{Student}}$ $df_{\text{Welch}} = df_{\text{Student}}$ $p_{\text{Welch}} = p_{\text{Student}}$	$t_{\text{Welch}} = t_{\text{Student}}$ $df_{\text{Welch}} \neq df_{\text{Student}}$ $p_{\text{Welch}} \neq p_{\text{Student}}$
Unbalanced design	$t_{\text{Welch}} = t_{\text{Student}}$ $df_{\text{Welch}} \neq df_{\text{Student}}$ $p_{\text{Welch}} \neq p_{\text{Student}}$	$t_{\text{Welch}} \neq t_{\text{Student}}$ $df_{\text{Welch}} \neq df_{\text{Student}}$ $p_{\text{Welch}} \neq p_{\text{Student}}$

**Table 1:** Comparison of *t*-value and Degrees of Freedom of Welch's and Student's *t*-test.

and Welch's *t*-test. Even with extremely large SDR (respectively, 0.01, 0.1, 10, and 100) and small sample sizes (10 subjects per group), the biggest increase in power of Student's *t*-test compared to Welch's *t*-test is approximately 5 percent when the test is applied on two normal skewed distributions with unequal shapes. In all other cases, the difference in power between both tests is smaller (See table A1.1 to A1.9 in the additional file).

Considering the cases where sample sizes are unequal and SDR = 1, Student's *t*-test is sometimes better than Welch's *t*-test, and sometimes the reverse is true. The difference is small, except in three scenarios (See table A5.2, A5.5, and A5.6 in the additional file). However, because there is no correct test to perform that assures SDR = 1, and because variances are likely not to be equal in certain research areas, our recommendation is to always use Welch's *t*-test instead of Student's *t*-test.

To illustrate the differences in Type 1 error rates between Student's *t*-test and Welch's *t*-test, we simulated 1,000,000 studies under the null hypothesis (no difference between

the means in each group) under four scenarios. We chose a small sample ratio ( $n_1 = 40$  vs.  $n_2 = 60$ ) to show that when the equal variances assumption was not met and SDR = 2, biased error rates are observed in Student's *t*-test. We compared Scenario 1, where the variance is the same in each group (SDR = 1; homoscedasticity assumption met) and sample sizes are unequal (See **Figure 2a**), with Scenario 2, where the variance differs between groups (SDR = 2) but sample sizes are equal ( $n_1 = n_2 = 50$ ; see **Figure 2b**). Furthermore, we simulated Scenario 3, where both sample sizes and variances were unequal between groups and the larger variance is associated with the larger sample size (SDR = 2; see **Figure 2c**), and a similar Scenario 4, where the larger variance is associated with the smaller sample size (SDR = 0.5; see **Figure 2d**). *P*-value distributions for both Student's and Welch's *t*-tests were then plotted. When there is no true effect, *p*-values are distributed uniformly.

As long as the variances are equal between groups or sample sizes are equal, the distribution of Student's *p*-values is uniform, as expected (see **Figures 2a** and **2b**),



**Figure 2:** *P*-value distributions for Student's and Welch's *t*-test under the null as a function of SDR, and sample size.

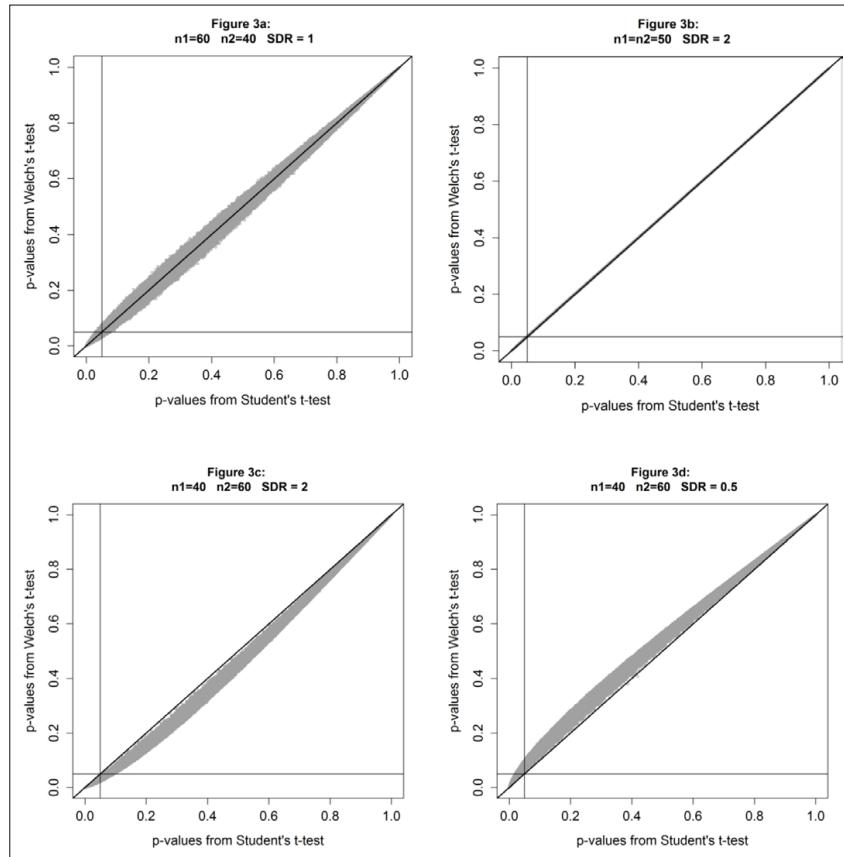
which implies that the probability of rejecting a true null hypothesis equals the alpha level for any value of alpha. On the other hand, when the larger variance is associated with the larger sample size, the frequency of *p*-values less than 5 percent decreases to 0.028 (see **Figure 2c**), and when the larger variance is associated with the smaller sample size, the frequency of *p*-values less than 5 percent increases to 0.083 (see **Figure 2d**). Welch's *t*-test has a more stable Type 1 error rate (see Keselman et al., 1998; Keselman, Othman, Wilcox, & Fradette, 2004; Moser & Stevens, 1992; Zimmerman, 2004) Additional simulations, presented in the additional file, show that these scenarios are similar for several shapes of distributions (see tables A3.1 to A3.9 and table A4 in the additional file).

Moreover, as discussed previously, with very small SDRs, Welch's *t*-test still has a better control of Type 1 error rates than Student's *t*-test, even if neither of them give critical values (i.e., values under 0.025 or above 0.075, according to the definition of Bradley, 1975). With SDR = 1.1, when the larger variance is associated with the larger sample size, the frequency of Student's *p*-value being less than 5 percent decreases to 0.046, and when the larger variance is associated with the smaller sample size, the frequency of Student's *p*-value being less than 5 percent increases to

0.054. On the other side, the frequency of Welch's *p*-values being below 0.05 is exactly 5 percent in both cases.

Yuen's *t*-test is not a good unconditional alternative because we observe an unacceptable departure from the nominal alpha risk of 5 percent for several shapes of distributions (see tables A3.1, A3.4, A3.7, A3.8, and A3.9 in the additional file), particularly when we are studying asymmetric distributions of unequal shapes (see tables A3.8 and A3.9 in the additional file). Moreover, even when Yuen's Type 1 error does not show a critical departure from the nominal alpha risk (i.e., values above 0.075), Welch's *t*-test more accurately controls the Type 1 error rate (see tables A3.2, A3.3, A3.5, and A3.6 in the additional file). The Type 1 error rate of Welch's *t*-test remains closer to the nominal size (i.e., 5%) in all the previously discussed cases and also performs better with very extreme SDRs and unbalanced designs, as long as there are at least 10 subjects per groups (See table A4 in the additional file).

In **Figure 3**, *p*-values from Welch's *t*-test and Student's *t*-test tests, shown separately in **Figure 2** (through histograms), are now plotted against each other. **Figure 3a** shows Student's *p*-values plotted against Welch's *p*-values of Scenario 1, where the variance is the same in each group (SDR = 1) and sample sizes are unequal. **Figure 3b** displays Student's *p*-values plotted against Welch's *p*-values of Scenario 2, where the



**Figure 3:** *P*-values from Student's *t*-test against *p*-values from Welch's *t*-test under the null.

variance differs between groups ( $SDR = 2$ ) but sample sizes are equal ( $n_1 = n_2 = 50$ ). **Figure 3c** shows Student's *p*-values plotted against Welch's *p*-values of Scenario 3, where both sample sizes and variances are unequal between groups and the larger variance is associated with the larger sample size ( $SDR = 2$ ). And, finally, **figure 3d** plots Student's *p*-values against Welch's *p*-values of Scenario 4, where the greater variance is associated with the smaller sample size ( $SDR = 0.5$ ).

Dots are marked on the black diagonal line when both tests return the same *p*-value. The top left quadrant contains all *p*-values less than 0.05 according to a Student's *t*-test, but greater than 0.05 according to Welch's *t*-test. The bottom right quadrant reports all *p*-values less than 0.05 according to Welch's *t*-test, but greater than 0.05 according to Student's *t*-test. The larger the standard deviations ratio and the greater the sample sizes ratio, the larger the difference between *p*-values from Welch's *t*-test and Student's *t*-test.

### Conclusion

When the assumption of equal variances is not met, Student's *t*-test yields unreliable results, while Welch's *t*-test controls Type 1 error rates as expected. The widely recommended two-step approach, where the assumption of equal variances is tested using Levene's test and, based on the outcome of this test, a choice of Student's *t*-test or Welch's *t*-test is made, should not be used. Because the statistical power for this test is often low, researchers will inappropriately choose Student's *t*-test instead of more robust alternatives. Furthermore, as we have argued, it is reasonable to assume that variances are unequal in many studies in psychology, either because measured variables are used (e.g., age, culture, gender) or because, after random assignment to conditions, variance is increased in the experimental condition compared to the control condition due to the experimental manipulation. As it is explained in the additional file, Yuen's *t*-test is not a better test than Welch's *t*-test, since it often suffers high departure from the alpha risk of 5 percent. Therefore, we argue that Welch's *t*-test should always be used instead of Student's *t*-test.

When using Welch's *t*-test, a very small loss in statistical power can occur, depending on the shape of the distributions. However, the Type 1 error rate is more stable when using Welch's *t*-test compared to Student's *t*-test, and Welch's *t*-test is less dependent on assumptions that cannot be easily tested. Welch's *t*-test is available in practically all statistical software packages (and already the default in R and Minitab) and is easy to use and report. We recommend that researchers make clear which test they use by specifying the analysis approach in the result section.

Convention is a weak justification for the current practice of using Student's *t*-test by default. Psychologists should pay more attention to the assumptions underlying the tests they perform. The default use of Welch's *t*-test is a straightforward way to improve statistical practice.

### Notes

<sup>1</sup> There is a Type 1 error rate inflation in a few cases where sample sizes are extremely small and SDR is big (e.g., when  $n_1 = n_2 = 3$  are sampled from uniform distributions and  $SDR = 2$ , the Type 1 error rate = 0.083;

or when  $n_1 = 3$  is sampled from a uniform distribution and  $n_2 = 3$  is sampled from a double exponential distribution). However, with extremely small sample sizes ( $N \leq 5$ ), the estimate of means and standard deviations is extremely inaccurate anyway. As we mentioned in table A2 (see the additional file), the smaller the sample size, the further the average standard deviation is from the population standard deviation, and the larger the dispersion around this average.

<sup>2</sup> This is called the Behren-Fisher problem (Hayes & Cai, 2007).

<sup>3</sup> In a simulation that explored Type 1 error rates, we varied the size of the first sample from 10 to 40 in steps of 10 and the sample sizes ratio and the standard deviation ratio from 0.5 to 2 in steps of 0.5, resulting in 64 simulations designs. Each design was tested 1,000,000 times. Considering these parameter values, we found that the alpha level can be inflated up to 0.11 or deflated down to 0.02 (see the additional file).

<sup>4</sup> Other variants have been proposed, such as the percent trimmed mean (Lim & Loh, 1996).

<sup>5</sup> Because sample sizes are equal for each pair of samples, which sample has the bigger standard deviation is not applicable. In this way,  $SDR = X$  will return the same answer in terms of percent power of Levene's test as  $SDR = 1/X$ . For example,  $SDR = 2$  will return the same answer as  $SDR = 1/2 = 0.5$ .

<sup>6</sup> For example, many statistical users believe that the Mann-Whitney non-parametric test can cope with both normality and homoscedasticity issues (Ruxton, 2006). This assumption is false, since the Mann-Whitney test remains sensitive to heteroscedasticity (Grissom, 2000; Nachar, 2008; Neuhäuser & Ruxton, 2009).

<sup>7</sup> Like Bryk and Raudenbush (1988), we note that unequal variances between groups does not systematically mean that population variances are different: standard deviation ratios are more or less biased estimates of population variance (see table A2 in the additional file). Differences can be a consequence of bias in measurement, such as response styles (Baumgartner & Steenkamp, 2001). However, there is no way to determine what part of the variability is due to error rather than the true population value.

<sup>8</sup> Also known as the Satterwaite's test, the Smith/Welch/Satterwaite test, the Aspin-Welch test, or the unequal variances *t*-test.

### Competing Interests

The authors have no competing interests to declare.

### Additional File

The additional file for this article can be found as follows:

- DOI: <https://doi.org/10.5334/irsp.82.s1>

### Author's Note

All code needed to recreate the simulations resulting in the figures and appendices is available at <https://osf.io/bver8/files/>, as are the .txt files containing the results of all simulations.

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**How to cite this article:** Delacre, M., Lakens, D. and Leys, C. (2017). Why Psychologists Should by Default Use Welch's *t*-test Instead of Student's *t*-test. *International Review of Social Psychology*, 30(1), 92–101, DOI: <https://doi.org/10.5334/irsp.82>

**Published:** 05 April 2017

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*International Review of Social Psychology* is a peer-reviewed open access journal published by Ubiquity Press.

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# Chapitre 3: utiliser l'ANOVA *F* de Welch par défaut

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## RESEARCH ARTICLE

### Taking Parametric Assumptions Seriously: Arguments for the Use of Welch's *F*-test instead of the Classical *F*-test in One-Way ANOVA

Marie Delacre\*, Christophe Leys\*, Youri L. Mora\* and Daniël Lakens†

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Student's *t*-test and classical *F*-test rely on the assumptions that two or more samples are independent, and that independent and identically distributed residuals are normal and have equal variances between groups. We focus on the assumptions of normality and equality of variances, and argue that these assumptions are often unrealistic in the field of psychology. We underline the current lack of attention to these assumptions through an analysis of researchers' practices. Through Monte Carlo simulations, we illustrate the consequences of performing the classic parametric *F*-test for ANOVA when the test assumptions are not met on the Type I error rate and statistical power. Under realistic deviations from the assumption of equal variances, the classic *F*-test can yield severely biased results and lead to invalid statistical inferences. We examine two common alternatives to the *F*-test, namely the Welch's ANOVA (*W*-test) and the Brown-Forsythe test (*F*\*-test). Our simulations show that under a range of realistic scenarios, the *W*-test is a better alternative and we therefore recommend using the *W*-test by default when comparing means. We provide a detailed example explaining how to perform the *W*-test in SPSS and R. We summarize our conclusions in practical recommendations that researchers can use to improve their statistical practices.

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**Keywords:** ANOVA; Welch test; parametric test; parametric assumptions; replicability crisis

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When comparing independent groups researchers often analyze the means by performing a Student's *t*-test or classical Analysis of Variance (ANOVA) *F*-test (Erceg-Hurn & Mirosevich, 2008; Keselman et al., 1998; Tomarken & Serlin, 1986). Both tests rely on the assumptions that independent and identically distributed residuals (1) are sampled from a normal distribution and (2) have equal variances between groups (or homoscedasticity; see Lix, Keselman, & Keselman, 1996). While a deviation from the normality assumption generally does not strongly affect either the Type I error rates (Glass, Peckham, & Sanders, 1972; Harwell, Rubinstein, Hayes, & Olds, 1992; Tiku, 1971) or the power of the *F*-test (David & Johnson, 1951; Harwell et al., 1992; Srivastava, 1959; Tiku, 1971), the *F*-test is not robust against unequal variances (Grissom, 2000). Unequal variances can alter both the Type I error rate (David & Johnson, 1951; Harwell et al., 1992) and statistical power (Nimon, 2012; Overall, Atlas, & Gibson, 1995) of the *F*-test.

Although it is important to make sure test assumptions are met before a statistical test is performed, researchers rarely provide information about test assumptions when they report an *F*-test. We examined statistical tests reported in 116 articles in the *Journal of Personality and Social Psychology* published in 2016. Fourteen percent of these articles reported a one-way *F*-test, but only one article indicated that the homogeneity of variances assumption was taken into account. They reported corrected degrees of freedom for unequal variances, which could signal the use of the *W*-test instead of the classical *F*-test. A similar investigation (Hoekstra, Kiers & Johnson, 2012) yielded conclusions about the lack of attention to both the homoscedasticity and the normality assumptions. Despite the fact that the *F*-test is currently used by default, better alternatives exist, such as the Welch's *W* ANOVA (*W*-test), the Alexander-Govern test, James' second order test, and the Brown-Forsythe ANOVA (*F*\*-test). Although not the focus of the current article, additional tests exist that allow researchers to compare groups either based on other estimators of central tendency than the mean (see for example Erceg-Hurn & Mirosevich, 2008; Wilcox, 1998), or based on other relevant parameters of distribution than the central tendency, such as standard deviations and the shape of the distribution (Grissom, 2000; Tomarken & Serlin, 1986). However, since most

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researchers currently generate hypotheses about differences between means (Erceg-Hurn & Mirosevich, 2008; Keselman et al., 1998), we think that a first realistic first step towards progress would be to get researchers to correctly test the hypothesis they are used to.

Although the debate surrounding the assumptions of the *F*-test has been widely explored (see for example the meta-analysis of Harwell et al., 1992), applied researchers still largely ignore the consequences of assumption violations. Non-mathematical pedagogical papers summarizing the arguments seem to be lacking from the literature, and the current paper aims to fill this gap. We will discuss the pertinence of the assumptions of the *F*-test, and focus on the question of heteroscedasticity (that is, as we will see, can have major consequences on error rates). We will provide a non-mathematical explanation of how alternatives to the classical *F*-test cope with heteroscedasticity violations. We conducted simulations in which we compare the *F*-test with the most promising alternatives. We argue that when variances are equal between groups, the *W*-test has nearly the same empirical Type I error rate and power as the *F*-test, but when variances are unequal, it provides empirical Type I and Type II error rates that are closer to the expected levels compared to the *F*-test. Since the *W*-test is available in practically all statistical software packages, researchers can immediately improve their statistical inferences by replacing the *F*-test by the *W*-test.

### **Normality and Homogeneity of Variances under Ecological Conditions**

For several reasons, assumptions of homogeneity of variances and normality are always more or less violated (Glass et al., 1972). In this section we will summarize the specificity of the methods used in our discipline that can account for this situation.

#### **Normality Assumption**

It has been argued that there are many fields in psychology where the assumption of normality does not hold (Cain, Zhang & Yuan, 2017; Micceri, 1989; Yuan, Bentler & Chan, 2004). As argued by Micceri (1989), there are several factors that could explain departures from the normality assumption, and we will focus on three of them: treatment effects, the presence of subpopulations, and the bounded measures underlying residuals.

First, although the mean can be influenced by the treatment effects, experimental treatment can also change the shape of a distribution, either by influencing the *skewness*, quantifying the asymmetry of the shape of the distribution, and *kurtosis*, a measure of the tendency to produce extreme values. A distribution with positive kurtosis will have heavier tails than the normal distribution, which means that extreme values will be more likely, while a distribution with negative kurtosis will have lighter tails than the normal distribution, meaning that extreme values will be less likely (Westfall, 2014; Wilcox, 2005). For example, a training aiming at reducing a bias perception of threat when being exposed to ambiguous words will not uniformly impact the perception of all participants, depending on their level of anxiety (Grey & Mathews, 2000). This

could influence the kurtosis of the distribution of bias score.

Second, prior to any experimental treatment, the presence of several subpopulations may lead to departures from the normality assumptions. A subgroup might exist that is unequal on some characteristics relevant to the measurements, that are not controlled within the studied group, which results in mixed distributions. This unavoidable lack of control is inherent of our field given its complexity. As an illustration, Wilcox (2005) writes that pooling two normally-distributed populations that have the same mean but different variances (e.g. normally distributed scores for schizophrenic and not schizophrenic participants) could result in distributions that are very similar to the normal curve, but with thicker tails. As another example, when assessing a wellness score for the general population, data may be sampled from a left-skewed distribution, because most people are probably not depressed (see Heun et al., 1999). In this case, people who suffer from depression and people who do not suffer from depression are part of the same population, which can leads to asymmetry in the distribution.

Third, bounded measures can also explain non-normal distributions. For example, response time can be very large, but never below zero, which results in right-skewed distributions. In sum, there are many common situations in which normally distributed data is an unlikely assumption.

#### **Homogeneity of Variances Assumption**

Homogeneity of variances (or homoscedasticity) is a mathematical requirement that is also ecologically unlikely (Erceg-Hurn & Mirosevich, 2008; Grissom, 2000). In a previous paper (Delacre, Lakens & Leys, 2017), we identified three different causes of heteroscedasticity: 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. One additional source of variability is the presence of unidentified moderators (Cohen et al., 2013).

First, psychologists, as many scholars from various fields in human sciences, often use measured variables (e.g. age, gender, educational level, ethnic origin, depression level, etc.) instead of random assignment to conditions. Prior to any treatment, parameters of pre-existing groups can vary largely from one population to another, as suggested by Henrich, Heine, and Norenzayan (2010). For example, Green, Deschamps, and Páez (2005) have shown that the scores of competitiveness, self-reliance and interdependence are more variable in some ethnic groups than in others. This stands true for many pre-existing groups such as gender, cultures, or religions and for various outcomes (see for example Adams et al., 2014; Beilmann et al., 2014; Church et al., 2012; Cohen & Hill, 2007; Haar et al., 2014; Montoya & Briggs, 2013). Moreover, groups are sometimes defined with the intention to have different variabilities. For example, as soon as a selective school admits its students based on the results of aptitude tests,

the variability will be smaller compared to a school that accepts all students.

Second, a quasi-experimental treatment can have different impacts on variances between pre-existing groups, that can even be of theoretical interest. For example, in the field of linguistics and social psychology, Wasserman and Weseley (2009) investigated the impact of language gender structure on sexist attitudes of women and men. They tested differences between sexist attitude scores of subjects who read a text in English (i.e. a language without grammatical gender) or in Spanish (i.e. a language with grammatical gender). The results showed that (for a reason not explained by the authors), the women's score on the sexism dimension was more variable when the text was read in Spanish than in English ( $SD_{\text{spanish}} = .80 > SD_{\text{english}} = .50$ ). For men, the reverse was true ( $SD_{\text{spanish}} = .97 < SD_{\text{english}} = 1.33$ ).<sup>1</sup>

Third, even when the variances of groups are the same before treatment (due to a complete successful randomization in group assignment), unequal variances can emerge later, as a consequence of an experimental treatment (Box, 1954; Bryk & Raudenbush, 1988; Cumming, 2005; Erceg-Hurn & Mirosevich, 2008; Keppel & Wickens, 2004). For example, Koeser and Sczesny (2014) have compared arguments advocating either masculine generic or gender-fair language with control messages in order to test the impact of these conditions on the use of gender-fair wording (measured as a frequency). They report that the standard deviations increase after treatment in all experimental conditions.

#### Consequences of Assumption Violations

Assumptions violations would not be a matter per se, if the *F*-test was perfectly robust against departures from them (Glass et al., 1972). When performing a test, two types of errors can be made: Type I errors and Type II errors. A Type I error consists of falsely rejecting the null hypothesis in favour of an alternative hypothesis, and the Type I error rate ( $\alpha$ ) is the proportion of tests that, when sampling many times from the same population, reject the null hypothesis when there is no true effect in the population. A Type II error consists of failing to reject the null hypothesis, and the Type II error rate ( $\beta$ ) is the proportion of tests, when sampling many times from the same population, that fail to reject the null hypothesis when there is a true effect. Finally, the statistical power ( $1 - \beta$ ) is the proportion of tests, when sampling many times from the same population, that correctly reject the null hypothesis when there is a true effect in the population.

#### Violation of the Normality Assumption

Regarding the Type I error rate, the shape of the distribution has very little impact on the *F*-test (Harwell et al., 1992). When departures are very small (i.e. a kurtosis between 1.2 and 3 or a skewness between -0.4 and 0.4), the Type I error rate of the *F*-test is very close to expectations, even with sample sizes as small as 11 subjects per group (Hsu & Feldt, 1969).

Regarding the Type II error rate, many authors underlined that departures from normality do not seriously

affect the power (Boneau, 1960; David & Johnson, 1951; Glass et al., 1972; Harwell et al., 1992; Srivastava, 1959; Tiku, 1971). However, we can conclude from Srivastava (1959) and Boneau (1960) that kurtosis has a slightly larger impact on the power than skewness. The effect of non-normality on power increases when sample sizes are unequal between groups (Glass et al., 1972). Lastly the effect of non-normality decreases when sample sizes increase (Srivastava, 1959).

#### Violation of Homogeneity of Variances Assumption

Regarding the Type I error rate, the *F*-test is sensitive to unequal variances (Harwell et al., 1992). More specifically, the more unequal the *SD* of the population's samples are extracted from, the higher the impact. When there are only two groups, the impact is smaller than when there are more than two groups (Harwell et al., 1992). When there are more than two groups, the *F*-test becomes more liberal, meaning that the Type I error rate is larger than the nominal alpha level, even when sample sizes are equal across groups (Tomarken & Serlin, 1986). Moreover, when sample sizes are unequal, there is a strong effect of the sample size and variance pairing. In case of a positive pairing (i.e. the group with the larger sample size also has the larger variance), the test is too conservative, meaning that the Type I error rate of the test is lower than the nominal alpha level, whereas in case of a negative pairing (i.e. the group with the larger sample size has the smaller variance), the test is too liberal (Glass et al., 1972; Nimon, 2012; Overall et al., 1995; Tomarken & Serlin, 1986).

Regarding the Type II error rate, there is a small impact of unequal variances when sample sizes are equal (Harwell et al., 1992), but there is a strong effect of the sample size and variance pairing (Nimon, 2012; Overall et al., 1995). In case of a positive pairing, the Type II error rate increases (i.e. the power decreases), and in case of a negative pairing, the Type II error decreases (i.e. the power increases).

#### Cumulative Violation of Normality and Homogeneity of Variance

Regarding both Type I and Type II error rates, following Harwell et al. (1992), there is no interaction between normality violations and unequal variances. Indeed, the effect of heteroscedasticity is relatively constant regardless of the shape of the distribution.

Based on mathematical explanations and Monte Carlo simulations we chose to compare the *F*-test with the *W*-test and *F*<sup>\*</sup>-test and to exclude the James' second-order and Alexander-Govern's test because the latter two yield very similar results to the *W*-test, but are less readily available in statistical software packages. Tomarken and Serlin (1986) have shown that from the available alternatives, the *F*<sup>\*</sup>-test and the *W*-test perform best, and both tests are available in SPSS, which is widely used software in the psychological sciences (Hoekstra et al., 2012). For a more extended description of the James' second-order and Alexander-Govern's test, see Schneider and Penfield (1997).

### The Mathematical Differences Between the *F*-test, *W*-test, and *F*<sup>\*</sup>-test

The mathematical differences between the *F*-test, *W*-test and *F*<sup>\*</sup>-test can be explained by focusing on how standard deviations are pooled across groups. As shown in (1) the *F* statistic is calculated by dividing the inter-group variance by a pooled error term, where  $s_j^2$  and  $n_j$  are respectively the variance estimates and the sample sizes from each independent group, and where  $k$  is the number of independent groups:

$$F = \frac{\frac{1}{k-1} \sum_{j=1}^k [n_j (\bar{x}_j - \bar{x}_{..})^2]}{\frac{1}{N-k} \sum_{j=1}^k (n_j - 1) s_j^2} \quad (1)$$

The degrees of freedom in the numerator (2) and in the denominator (3) of the *F*-test are computed as follows:

$$df_n = k - 1 \quad (2)$$

$$df_d = N - k, \quad (3)$$

With  $N = \sum_{j=1}^k n_j$ . As a generalization of the Student's *t*-test, the *F*-test is calculated based on a pooled error term. This implies that all samples are considered as issued from a common population variance (hence the assumption of homoscedasticity). When there is heteroscedasticity, and if the larger variance is associated with the larger sample size, the error term, which is the denominator in (1), is overestimated. The *F*-value is therefore smaller, leading to fewer significant findings than expected, and the *F*-test is too conservative. When the larger variance is associated with the smaller sample size the denominator in (1) is underestimated. The *F*-value is then inflated, which yields more significant results than expected.

The *F*<sup>\*</sup> statistic proposed by Brown and Forsythe (1974) is computed as follows:

$$F^* = \frac{\sum_{j=1}^k [n_j (\bar{x}_j - \bar{x}_{..})^2]}{\sum_{j=1}^k \left[ \left(1 - \frac{n_j}{N}\right) s_j^2 \right]} \quad (4)$$

Where  $\bar{x}_j$  and  $s_j^2$  are respectively the group mean and the group variance, and  $\bar{x}_{..}$  is the overall mean. As it can be seen in (4) the numerator of the *F*<sup>\*</sup> statistic is equal to the sum of squares between groups (which is equal to the numerator of the *F* statistic when one compares two groups). In the denominator, the variance of each group is weighted by 1 minus the relative frequency of each group. This adjustment implies that the variance associated with the group with the smallest sample size is given more weight compared to the *F*-test. As a result, when the larger variance is associated with the larger sample size, *F*<sup>\*</sup> is larger than *F*, because the denominator decreases, leading to more significant findings compared to the *F*-test. On the other hand, when the larger variance is associated with the smaller sample size, *F*<sup>\*</sup> is smaller than *F*, because the denominator increases, lead-

ing to fewer significant findings compared to the *F*-test. The degrees of freedom in the numerator and in the denominator of *F*<sup>\*</sup>-test are computed as follows (with the same principle as the denominator computation of the *F*<sup>\*</sup> statistic):

$$df_n = k - 1 \quad (5)$$

$$df_d = \frac{1}{\left( \frac{\sum_{j=1}^k \left[ \left(1 - \frac{n_j}{N}\right) s_j^2 \right]}{\sum_{j=1}^k [n_j - 1]} \right)^2} \quad (6)$$

Formula (7) provides the computation of the *W*-test, or Welch's *F*-test. In the numerator of the *W*-test the squared deviation between group means and the general mean are weighted by  $\frac{n_j}{s_j^2}$  instead of  $n_j$  (Brown & Forsythe, 1974). As a consequence, for equal sample sizes, the group with the highest variance will have smaller weight (Liu, 2015).

$$W = \frac{\frac{1}{k-1} \sum_{j=1}^k \left[ w_j (\bar{x}_j - \bar{x}')^2 \right]}{1 + \frac{2(k-2)}{k^2-1} \sum_{j=1}^k \left[ \left( \frac{1}{n_j - 1} \right) \left( 1 - \frac{w_j}{w} \right)^2 \right]} \quad (7)$$

where:

$$w_j = \frac{n_j}{s_j^2}$$

$$w = \sum_{j=1}^k \left( \frac{n_j}{s_j^2} \right)$$

$$\bar{x}' = \frac{\sum_{j=1}^k (w_j \bar{x}_j)}{w}$$

The degrees of freedom of the *W*-test are approximated as follows:

$$df_n = k - 1 \quad (8)$$

$$df_d = \frac{k^2 - 1}{3 \sum_{j=1}^k \left[ \frac{(1 - \frac{w_j}{w})^2}{n_j - 1} \right]} \quad (9)$$

When there are only two groups to compare, the *F*<sup>\*</sup>-test and *W*-test are identical (i.e., they have exactly the same statistical value, degrees of freedom and significance). However, when there are more than two groups to compare, the tests differ. In the appendix we illustrate the calculation of all three statistics in detail for a fictional three-group design for educational purposes.

### Monte Carlo simulations: *F*-test versus *W*-test versus *F*<sup>\*</sup>-test

We performed Monte Carlo simulations using R (version 3.5.0) to assess the Type I and Type II error rates for the three tests. One million datasets were generated for 3840 scenarios that address the arguments present in the literature. In 2560 scenarios, means were equal across all groups (i.e. the null hypothesis is true), in order to assess the Type I error rate of the tests. In 1280 scenarios, there were differences between means (i.e. the alternative hypothesis is true) in order to assess the power of the tests. In all scenarios, when using more than 2 samples, all samples but one was generated from the same population, and only one group had a different population mean.

Population parameter values were chosen in order to illustrate the consequences of factors known to play a key role on both the Type I error rate and the statistical power when performing an ANOVA. Based on the literature review presented above, we manipulated the number of groups, the sample sizes, the sample size ratio ( $n$ -ratio =  $\frac{n_k}{n_1}$ ), the  $SD$ -ratio ( $SD$ -ratio =  $\frac{\sigma_k}{\sigma_1}$ ), and the sample size and variance pairing. In our scenarios, the number of compared groups ( $k$ ) varied from 2 to 5. Sample sizes of  $k-1$  groups ( $n_j$ ) were 20, 30, 40, 50, or 100. The sample size of the last group was a function of the  $n$ -ratio, ranging from 0.5 to 2, in steps of 0.5. The simulations for which the  $n$ -ratio equals 1 are known as a balanced design (i.e. sample sizes are equal across all groups). The  $SD$  of the population from which was extracted last group was a function of the  $SD$ -ratio, with values of 0.5, 1, 2 or 4. The simulations for which the  $SD$ -ratio equals 1 are the particular case of homoscedasticity (i.e. equal variances across groups).

All possible combinations of  $n$ -ratio and  $SD$ -ratio were performed in order to distinguish positive pairings (the group with the largest sample size is extracted from the population with the largest  $SD$ ), negative pairings (the group with the smallest sample size is extracted from the population with the smallest  $SD$ ), and no pairing (sample sizes and/or population  $SD$  are equal across all groups). All of those conditions were tested with normal and non-normal distributions. When two groups are compared, conclusions for the three ANOVA tests ( $F$ ,  $F^*$ ,  $W$ ) should yield identical error rates when compared to their equivalent  $t$ -tests (the  $F$ -test is equivalent to Student's  $t$ -test, and the  $F^*$ -test and  $W$ -test are equivalent to Welch's  $t$ -test; Delacre et al., 2017). When there are more than three groups, the  $F$ -test becomes increasingly liberal as soon as the variances of the distributions in each group are not similar, even when sample sizes are equal between groups (Harwell et al., 1992; Quensel, 1947).

For didactic reasons, we will report only the results where we compared three groups ( $k = 3$ ). Increasing the number of groups increases how liberal all tests are. For interested readers, all figures for cases where we compare more than three groups are available here: <https://osf.io/h4ks8/>. Overall, the larger the sample sizes, the less the distributions of the population underlying the samples impact the robustness of the tests (Srivastava, 1959). However, increasing the sample sizes does not improve the robustness of the test when there is heteroscedasticity.

Interested reader can see all details in the following Excel spreadsheet, available on github: « Type I error rate.xlsx ».

In sum, the simulations grouped over different sample sizes yield 9 conditions based on the  $n$ -ratio,  $SD$ -ratio, and sample size and variance pairing, as summarized in **Table 1**.

In all Figures presented below, averaged results for each sub-condition are presented under seven different configurations of distributions, using the following legend.

### Type I Error Rate of the *F*-test, *W*-test, and *F*<sup>\*</sup>-test

As previously mentioned, the Type I error rate ( $\alpha$ ) is the long-run frequency of observing significant results when the null-hypothesis is true. When means are equal across all groups the Type I error rate of all test should be equal to the nominal alpha level. We assessed the Type I error rate of the  $F$ -test,  $W$ -test and  $F^*$ -test under 2560 scenarios using a nominal alpha level of 5%.

When there is no difference between means, the nine cells of **Table 1** simplify into five sub-conditions:

- Equal  $n$  and  $SD$  across groups (a)
- Unequal  $n$  but equal  $SD$  across groups (b and c)
- Unequal  $SD$  but equal  $n$  across groups (d and g)
- Unequal  $n$  and  $SD$  across groups, with positive correlation between  $n$  and  $SD$  (e and i)
- Unequal  $n$  and  $SD$  across groups, with negative correlation between  $n$  and  $SD$  (f and h)

**Table 1:** 9 conditions based on the  $n$ -ratio,  $SD$ -ratio, and sample size and variance pairing.

	<i>n</i> -ratio			
	1	>1	<1	
<i>SD</i> -ratio	1	a	b	c
	>1	d	e	f
	<1	g	h	i

*Note:* The  $n$ -ratio is the sample size of the last group divided by the sample size of the first group. When all sample sizes are equal across groups, the  $n$ -ratio equals 1. When the sample size of the last group is higher than the sample size of the first group,  $n$ -ratio >1, and when the sample size of the last group is smaller than the sample size of the first group,  $n$ -ratio <1.  $SD$ -ratio is the population  $SD$  of the last group divided by the population  $SD$  of the first group. When all samples are extracted from populations with the same  $SD$ , the  $SD$ -ratio equals 1. When the last group is extracted from a population with a larger  $SD$  than all other groups, the  $SD$ -ratio >1. When the last group is extracted from a population with a smaller  $SD$  than all other groups, the  $SD$ -ratio <1.



**Figure 1:** Legend.

In **Figures 2 to 6** (see **Figure 1** for the legend), we computed the average Type I error rate of the three tests under these five subcategories. The light grey area corresponds to the liberal criterion from Bradley (1978), who regards a departure from the nominal alpha level as acceptable whenever the Type I error rate falls within the interval  $[0.5 \times \alpha; 1.5 \times \alpha]$ . The dark grey area corresponds to the more conservative criterion from which departures from the nominal alpha is considered negligible as long as the Type I error rate falls within the interval  $[0.9 \times \alpha; 1.1 \times \alpha]$ .

In **Figures 2 and 3** (cells a, b, and c in **Table 1**), the population variance is equal between all groups, so the homoscedasticity assumption is met. The  $F$ -test and  $F^*$ -test only marginally deviate from the nominal 5%, regardless of the underlying distribution and the  $SD$ -ratio. The  $W$ -test also only marginally deviates from the nominal 5%, except under asymmetry (the test becomes a little more liberal) or extremely heavy tails (the test becomes a bit more conservative), consistently with observations in Harwell et al. (1992). However, deviations don't exceed the liberal criterion of Bradley (1978).

In **Figures 4, 5 and 6** (cells d to i, **Table 1**) the population variance is unequal between groups, so that the homoscedasticity assumption is not met. When sample sizes are equal across groups (**Figure 4**) and when there is a positive correlation between sample sizes and  $SDs$  (**Figure 5**), the Type I error rate of the  $W$ -test is closer to the nominal 5% than the Type I error rate of the  $F^*$ -test and the  $F$ -test, the latter which is consistently at the lower limit of the liberal interval suggested by Bradley, in line with Harwell et al. (1992), Glass et al. (1972), Nimon (2012) and Overall et al. (1995). Heteroscedasticity does not impact the Type I error rate of the  $W$ -test, regardless of the distribution (the order of the distribution shape remains the same in all conditions).

When there is a negative correlation between sample sizes and  $SDs$  (**Figure 6**), the Type I error rate of the  $F^*$ -test is slightly closer of the nominal 5% than the Type I error rate of the  $W$ -test, for which the distributions (more specifically, the skewness) has a larger impact on the Type I error rate than when there is homoscedasticity. This is consistent with conclusions of Lix et al. (1996) about



**Figure 2:** Type I error rate of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are equal  $SDs$  across groups and equal sample sizes (cell a in Table 1).



**Figure 4:** Type I error rate of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are unequal  $SDs$  across groups and equal sample sizes (cells d and g in Table 1).



**Figure 3:** Type I error rate of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are equal  $SDs$  across groups and unequal sample sizes (cells b and c in Table 1).



**Figure 5:** Type I error rate of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are unequal  $SDs$  across groups, and positive correlation between sample sizes and  $SDs$  (cells e and i in Table 1).



**Figure 6:** Type I error rate of the *F*-test, *W*-test and  $F^*$ -test when there are unequal SDs across groups, and negative correlation between sample sizes and SDs (cells f and g in Table 1).

the Alexander-Govern and the James' second order tests (which return very similar results as the *W*-test, as we already mentioned). However, both tests still perform relatively well, contrary to the *F*-test that is much too liberal, in line with observations by Harwell et al. (1992), Glass et al. (1972), Nimon (2012) and Overall et al. (1995).

### Conclusions

We can draw the following conclusions for the Type I error rate:

- 1) When all assumptions are met, all tests perform adequately.
- 2) When variances are equal between groups and distributions are not normal, the *W*-test is a little less efficient than both the *F*-test and the  $F^*$ -test, but departures from the nominal 5% Type I error rate never exceed the liberal criterion of Bradley (1978).
- 3) When the assumption of equal variances is violated, the *W*-test clearly outperforms both the  $F^*$ -test (which is more liberal) and the *F*-test (which is either more liberal or more conservative, depending on the SDs and *SD* pairing).
- 4) The last conclusion generally remains true when both the assumptions of equal variances and normality are not met.

### Statistical power for the *F*-test, *W*-test, and $F^*$ -test

As previously mentioned, the statistical power ( $1 - \beta$ ) of a test is the long-run probability of observing a statistically significant result when there is a true effect in the population. We assessed the power of the *F*-test, *W*-test and  $F^*$ -test under 1280 scenarios, while using the nominal alpha level of 5%. In all scenarios, the last group was extracted from a population that had a higher mean than the population from where were extracted all other groups ( $\mu_k = \mu_j + 1$ ). Because of that, in some scenarios there is a positive correlation between the *SD* and the mean (i.e. the last group has the largest *SD* and the largest mean) and in other scenarios, there is a negative correlation between *SD* and the mean (i.e. the last group has the smallest *SD*

and the largest mean). As we know that the correlation between the *SD* and the mean matters for the *W*-test (see Liu, 2015), the 9 sub-conditions in **Table 1** were analyzed separately.

We computed two main outcomes: the consistency and the power. The consistency refers to the relative difference between the observed power and the nominal power, divided by the expected power:

$$\text{Consistency} = \frac{O - E}{E} \quad (10)$$

When consistency equals zero, the observed power is consistent with the nominal power (under the parametric assumptions of normality and homoscedasticity); a negative consistency shows that the observed power is lower than the expected power; and a positive consistency shows that the observed power is higher than the expected power.

In **Figures 7, 8** and **9** (cells a, b, and c in **Table 1** see **Figure 1** for the legend), the population variance is equal between all groups, meaning that the homoscedasticity assumption is met. When distributions are normal,



**Figure 7:** Power and consistency of the *F*-test, *W*-test and  $F^*$ -test when there are equal SDs across groups and equal sample sizes (cell a in Table 1).



**Figure 8:** Power and consistency of the *F*-test, *W*-test and  $F^*$ -test when there are equal SDs across groups, and positive correlation between sample sizes and means (cell b in Table 1).



**Figure 9:** Power and consistency of the *F*-test, *W*-test and  $F^*$ -test when there are equal SDs across groups, and negative correlation between sample sizes and means (cell c in Table 1).

the *W*-test is slightly less powerful than the *F*-test and  $F^*$ -test, even though differences are very small. With all other distributions, the *W*-test is generally more powerful than the  $F^*$ -test and *F*-test, even with heavy-tailed distributions, which is in contrast with previous findings (Wilcox, 1998). Wilcox (1998) concluded that there is a loss of power when means from heavy-tailed distributions (e.g. double exponential or a mixed normal distribution) are compared to means from normal distributions. This finding is based on the argument that heavy-tailed distributions are associated with bigger standard deviations than normal distributions, and that the effect size for such distributions is therefore smaller (Wilcox, 2011). However, this conclusion is based on a common conflation of kurtosis and the standard deviation, which are completely independent (DeCarlo, 1997). One can find distributions that have similar SD but different kurtosis (see Appendix 2). However, while the *W*-test is more powerful than the *F*-test and the  $F^*$ -test in many situations, it is a bit less consistent with theoretical expectations than both other tests in the sense that the *W*-test is generally more powerful than expected (especially with high kurtosis, or when asymmetries go in opposite directions). This is due to the fact that the *W*-test is more impacted by the distribution shape, in line with observations by Harwell et al. (1992). Note that differences between *W*-test and other tests, in terms of consistency, are very small.

In **Figures 10 to 15** (cells d to i in **Table 1** see **Figure 1** for the legend), the population variance is unequal between groups, meaning that the homoscedasticity assumption is not met. When sample sizes are equal across groups (**Figures 10 and 11**), the *F*-test and the  $F^*$ -tests are equally powerful, and have the same consistency, whatever the correlation between the SD and the mean. On the other hand, the power of the *W*-test depends on the correlation between the SD and the mean (in line with Liu, 2015). When the group with the largest mean has the largest variance (**Figure 10**), the largest deviation between group means and the general mean is given less weight, and as a consequence the *W*-test is less powerful than both other tests. At the same time, the test is slightly less consistent than both other tests. When the group with the largest mean has the smallest variance



**Figure 10:** Power and consistency of the *F*-test, *W*-test and  $F^*$ -test when there are unequal SDs across groups, positive correlation between SDs and means, and equal sample sizes across groups (cell d in Table 1).



**Figure 11:** Power and consistency of the *F*-test, *W*-test and  $F^*$ -test when there are unequal SDs across groups, negative correlation between SDs and means, and equal sample sizes across groups (cell g in Table 1).

(**Figure 11**), the largest deviation between group means and the general mean is given more weight, and therefore the *W*-test is more powerful than both other tests. The test is also slightly more consistent than both other tests.

When sample sizes are unequal across groups, the power of the  $F^*$ -test and the *F*-test are a function of the correlation between sample sizes and SDs. When there is a negative correlation between sample sizes and SDs (**Figures 12 and 13**), the *F*-test is always more powerful than the  $F^*$ -test. Indeed, as was explained in the previous mathematical section, the *F*-test gives more weight to the smallest variance (the statistic is therefore increased) while the  $F^*$ -test gives more weight to the largest variance (the statistic is therefore decreased). Conversely, when there is a positive correlation between sample sizes and SDs (**Figures 14 and 15**), the *F*-test is always more conservative than the  $F^*$ -test, because the *F*-test gives more weight to the largest variance while the  $F^*$ -test gives more weight to the smallest variance.

The power of the *W*-test is not a function of the correlation between sample sizes and SDs, but rather a function of the correlation between SDs and means. The test is more powerful when there is a negative correlation between SDs and means, and less powerful when there is a positive



**Figure 12:** Power and consistency of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are unequal SDs across groups, negative correlation between sample sizes and SDs, and positive correlation between SDs and means (cell f in Table 1).



**Figure 15:** Power and consistency of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are unequal SDs across groups, positive correlation between sample sizes and SDs, and negative correlation between SDs and means (cell i in Table 1).



**Figure 13:** Power and consistency of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are unequal SDs across groups, negative correlation between sample sizes and SDs, and negative correlation between SDs and means (cell h in Table 1).



**Figure 14:** Power and consistency of the  $F$ -test,  $W$ -test and  $F^*$ -test when there are unequal SDs across groups, positive correlation between sample sizes and SDs, and positive correlation between SDs and means (cell e in Table 1).

correlation between  $SDs$  and means. Note that for all tests, the effect of heteroscedasticity is approximately the same regardless of the shape of the distribution. Moreover,

there is one constant observation in our simulations: whatever the configuration of the  $n$ -ratio, the consistency of the three tests is closer to zero when there is a negative correlation between the  $SD$  and the mean (meaning that the group with the highest mean has the lower variance).

We can draw the following conclusions about the statistical power of the three tests:

- 1) When all assumptions are met, the  $W$ -test falls slightly behind the  $F$ -test and the  $F^*$ -test, both in terms of power and consistency.
- 2) When variances are equal between groups and distributions are not normal, the  $W$ -test is slightly more powerful than both the  $F$ -test and the  $F^*$ -test, even with heavy-tailed distributions.
- 3) When the assumption of equal variances is violated, the  $F$ -test is either too liberal or too conservative, depending on the correlation between sample sizes and  $SDs$ . On the other side, the  $W$ -test is not influenced by the sample sizes and  $SDs$  pairing. However, it is influenced by the  $SD$  and means pairing.
- 4) The last conclusion generally remains true when both assumptions of equal variances and normality are not met.

### Recommendations

Taking both the effects of the assumption violations on the alpha risk and on the power, we recommend using the  $W$ -test instead of the  $F$ -test to compare groups means. The  $F$ -test and  $F^*$ -test should be avoided, because a) the equal variances assumption is often unrealistic, b) tests of the equal variances assumption will often fail to detect differences when these are present, c) the loss of power when using the  $W$ -test is very small (and often even negligible), and d) the gain in Type I error control is considerable under a wide range of realistic conditions. Also, we recommend the use of balanced designs (i.e. same sample sizes in each group) whenever possible. When using the  $W$ -test, the Type I error rate is a function of criteria such as the skewness of the distributions, and whether skewness is combined with unequal variances and unequal samples sizes between groups. Our simulations show that the Type

I error rate control is in general slightly better with balanced designs.

Note that the *W*-test suffers from limitations and cannot be used in all situations. First, as previously mentioned, *W*-test, as all tests based on means, does not allow researchers to compare other relevant parameters of a distribution than the mean. For these reason, we recommend to never neglect the descriptive analysis of the data. A complete description of the shape and characteristics of the data (e.g. histograms and boxplots) is important. When at least one statistical parameter relating to the shape of the distribution (e.g. variance, skewness, kurtosis) seems to vary between groups, comparing results of the *W*-test with results of a nonparametric procedure is useful in order to better understand the data. Second, with small sample sizes (i.e. less than 50 observations per group when comparing at most four groups, 100 observations when comparing more than four groups), the *W*-test will not control Type I error rate when skewness is present and detecting departures for normality is therefore especially important in small samples. Unless you have good reasons to believe that distributions underlying the data have small kurtosis and skewness, we recommend to avoid alternative tests that are based on means comparison, in favour of alternatives such as the trimmed means test (Erceg-Hurn & Mirosevich, 2008)<sup>2</sup> or nonparametric tests. For more information about robust alternatives that are based on other parameters than the mean, see Erceg-Hurn and Mirosevich (2008).

#### Notes

<sup>1</sup> Note that this is a didactic example, the differences have not been tested and might not differ statistically.

<sup>2</sup> The null hypothesis of the trimmed means test assumes that trimmed means are the same between groups. A trimmed mean is a mean computed on data after removing the lowest and highest values of the distribution. Trimmed means and means are equal when data are symmetric. On the other hand, when data are asymmetric, trimmed means and means differ.

#### Additional File

The additional file for this article can be found as follows:

- **Supplemental Materials.** A numerical example of the mathematical development of the *F*-test, *W*-test, and *F*\*-test (Appendix 1) and justification for the choice of distributions in simulation (Appendix 2). DOI: <https://doi.org/10.5334/irsp.198.s1>

#### Competing Interests

The authors have no competing interests to declare.

#### Author Contribution

The first author performed simulations. The first, second and fourth authors contributed to the design. All authors contributed to the writing and the review of the literature. The Supplemental Material, including the full

R code for the simulations and plots can be obtained from <https://github.com/mdelacre/W-ANOVA>. This work was supported by the Netherlands Organization for Scientific Research (NWO) VIDI grant 452-17-013. The authors declare that they have no conflicts of interest with respect to the authorship or the publication of this article.

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**How to cite this article:** Delacre, M., Leys, C., Mora, Y. L., & Lakens, D. (2019). Taking Parametric Assumptions Seriously: Arguments for the Use of Welch's *F*-test instead of the Classical *F*-test in One-Way ANOVA. *International Review of Social Psychology*, 32(1): 13, 1–12. DOI: <https://doi.org/10.5334/irsp.198>

**Submitted:** 05 June 2018

**Accepted:** 20 May 2019

**Published:** 01 August 2019

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**Chapitre 4: reporter le  $g_s^*$  de Hedges basé sur  
l'écart-type non poolé avec le test  $t$  de Welch**

## Chapitre 5: les tests d'équivalence

Lorsqu'on applique un test d'hypothèse, l'hypothèse nulle la plus couramment définie est celle d'absence d'effet ou de différence entre les groupes (Nickerson, 2000). Il arrive également parfois que les chercheurs définissent un intervalle de valeur comme hypothèse nulle, mais le plus souvent, cet intervalle est borné par la valeur 0 (Nickerson, 2000), on parle alors d'hypothèse unilatérale. Avec cette stratégie, le rejet de l'hypothèse nulle constitue un soutien en faveur de la présence d'un effet non nul, par contre, le non rejet de l'hypothèse nulle ne peut être interprété comme un soutien en faveur de l'absence d'effet. Pourtant, il arrive souvent que des chercheurs l'interprètent de la sorte (Anderson & Maxwell, 2016). Finch, Cumming, & Thomason (2001), par exemple, ont reporté que parmi 150 articles publiés entre 1940 et 1999 dans le *JAP* (*Journal of Applied Psychology*), 38% interprétaient un résultat non significatif comme une acceptation de l'hypothèse nulle. Plus récemment, Lakens (2017) a noté que l'expression “pas d'effet” a été utilisée dans 108 articles publié dans *Social Psychological and Personality Science* avant août 2016 et que dans presque tous les cas, c'était sur base du non rejet de l'hypothèse nulle que cette conclusion était tirée. Cette erreur d'interprétation est également fréquemment commise dans le cadre des études de réPLICATION. Anderson & Maxwell (2016), par exemple, ont analysé 50 réPLICATIONS d'études publiées en 2013 dans PsycINFO. Ils ont noté que 14 études affirmaient avoir obtenu des effets “nuls” (interprété comme un échec à la réPLICATION), et tous l'ont fait sur base de l'acceptation d'une hypothèse nulle d'absence d'effet. C'est par exemple de cette manière qu'on été réalisées la plupart des tentatives de réPLICATIONS de la célèbre étude de Bem (Ritchie, Wiseman & French, 2012, cités par Anderson & Maxwell, 2016).

A travers ce chapitre, notre premier objectif sera d'expliquer pourquoi interpréter le non rejet de l'hypothèse d'absence d'effet comme un soutien en faveur d'une absence d'effet n'est pas une bonne stratégie. Nous introduirons ensuite les tests d'équivalence qui permettent d'obtenir un soutien en faveur d'un effet jugé non pertinent, et plus particulièrement le TOST (Two One-sided test). Nous verrons que l'aspect le plus compliqué de la réalisation du TOST est la définition des bornes d'équivalence. Pour cette raison, notre troisième objectif sera de fournir quelques pistes en vue de définir ces bornes. Pour finir, nous présenterons un article dans lequel nous comparons le TOST à la SGpv (Second Generation P-Value), une stratégie récemment développée par Blume, D'Agostino McGowan, Dupont, & Greevy Jr (2018).

## Limites de l'approche traditionnelle

Lorsqu'on teste une hypothèse nulle, il y a deux conclusions possibles: soit on la rejette, soit on ne la rejette pas. Si rejeter l'hypothèse nulle amène à conclure en faveur de l'hypothèse alternative, ne pas la rejeter ne permet pas de conclure en faveur de l'hypothèse nulle. Au mieux, cela nous montre que les données ne sont pas compatibles avec l'hypothèse nulle, mais cela ne veut en aucun cas dire qu'elles ne sont compatibles avec aucune autre hypothèse. Afin de l'illustrer, la Table 1 résume les résultats de simulations Monte Carlo pour un ensemble de 42 scénarios qui varient en fonction de la taille des échantillons ( $n_j$ ) et de la différence entre les moyennes des deux populations dont sont extraits les échantillons ( $\mu_1 - \mu_2$ ). Pour chaque scénario, à 100,000 reprises, nous avons généré aléatoirement une paire d'échantillons indépendants, réalisé un test  $t$  de Student pour échantillons indépendants et extrait la  $p$ -valeur du test. Ensuite, nous avons calculé la proportion d'itérations associées à une  $p$ -valeurs supérieures à .05, nous amenant à ne pas rejeter l'hypothèse nulle lorsqu'on travaille avec un risque alpha de 5% (ce risque alpha étant communément accepté par la majorité des chercheurs, Meyners, 2012). Lorsque l'hypothèse nulle est fausse (toutes les colonnes de la Table 1, à l'exception de la première), cette proportion correspond au taux d'erreur de type II (communément appelé  $\beta$ ).

Table 1.

*Proportion de  $p$ -valeurs supérieures à .05 en fonction de la taille des échantillons ( $n_j$ ) et de la différence entre les moyennes de chaque population ( $\mu_1 - \mu_2$ ).*

$n_j$	Différence de moyennes dans la population ( $\mu_1 - \mu_2$ )					
	0	.1	.2	.3	.4	.5
100	0.949	0.891	0.709	0.441	0.196	0.060
200	0.949	0.832	0.483	0.151	0.021	0.001
300	0.950	0.768	0.312	0.043	0.001	< .001
400	0.949	0.708	0.194	0.011	< .001	< .001
500	0.949	0.646	0.116	0.003	< .001	< .001
600	0.950	0.591	0.066	0.001	< .001	< .001
700	0.948	0.536	0.037	< .001	< .001	< .001

*Note.* Pour chaque scénario, les deux échantillons sont toujours de même taille ( $n_1 = n_2 = n$ ) et sont extraits de populations se distribuant normalement et ayant la même variance ( $\sigma_1 = \sigma_2 = \sigma$ ). La moyenne de la première population ( $\mu_1$ ) vaut systématiquement 0, et celle de la deuxième population ( $\mu_2$ ) varie de sorte à obtenir la différence de moyenne  $\mu_1 - \mu_2$  désirée. Par ailleurs,  $\sigma$  vaut systématiquement 1, si bien que la différence de moyenne brute est égale au  $\delta$  de Cohen.

Pour les scénarios de la première colonne, l'hypothèse nulle est vraie: il n'y a pas de différence entre les moyennes. Puisque les conditions d'application du test  $t$  de Student sont toutes rencontrées, on est amené à rejeter l'hypothèse nulle, c'est-à-dire à commettre une erreur de type  $I$  dans une proportion d'itération égale à  $\alpha = 5\%$ . Par conséquent, on est amené à *ne pas* rejeter l'hypothèse nulle 95% du temps (ce qui correspond à  $(1-\alpha)\%$ ). Pour les scénarios envisagés dans toutes les autres colonnes de la Table 1, une vraie différence entre les moyennes de population existe, si bien que nous espérerions être en mesure de rejeter l'hypothèse nulle le plus souvent possible. Pourtant, pour plusieurs scénarios, le nombre d'itérations amenant à conclure au non rejet de l'hypothèse nulle est bien supérieur au nombre d'itérations amenant à conclure au rejet de l'hypothèse nulle, comme on peut le voir à travers les valeurs  $\beta$ . Par exemple, avec 100 sujets par groupes et considérant  $\sigma_1 = \sigma_2 = 1$ , on ne détectera pas une différence de moyenne de .1 dans près de 90% des cas. Avec 700 sujets par groupe, cette différence ne sera toujours pas détectée plus d'une fois sur deux ( $\approx 54\%$  des itérations). En présence d'un effet non nul, cela se justifie par un manque de puissance des tests réalisés, ce qui démontre bien qu'un non rejet de l'hypothèse nulle peut en fait signifier deux choses: soit qu'il n'y a vraiment pas de différence entre les moyennes des populations (ou autrement dit, que les différences observées sont dues au hasard), soit que le test n'est pas suffisamment puissant pour détecter la différence. Or, le manque de puissance des tests est récurrent dans la littérature, comme tendent à le montrer diverses méta-analyses (Bakker, Van Dijk, & Wicherts, 2012 ; Button et al., 2013 ; Funder et al., 2014).

Pour éviter d'interpréter un test peu puissant comme un soutien en faveur de l'hypothèse nulle, l'approche de la puissance est devenue l'approche par défaut dans les années 80 pour tester l'équivalence (Meyners, 2012). À travers cette approche qui est restée très populaire (Quertemont, 2011), dans un premier temps, on définit ce qu'on considère comme étant la plus petite valeur d'intérêt (en anglais, le “SESOI” pour “Smaller Effect Size of Interest”), c'est-à-dire la taille d'effet minimale requise pour considérer qu'un effet est pertinent. Ensuite, on estime la puissance de notre test à détecter un effet de cette taille<sup>6</sup>, et si cette estimation atteind une valeur jugée satisfaisante (en général, 80%), alors on considère que l'on peut interpréter le non rejet de l'hypothèse nulle d'absence d'effet comme soutien en faveur de l'équivalence (Meyners, 2012 ; Quertemont, 2011 ; Schuirmann, 1987). L'idée sous-jacente est que si l'effet est au moins

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6. On parle d'estimation et non de mesure, car la puissance du test dépend de  $\sigma$ , l'écart-type de la population, qu'on ne connaît pas et devra donc estimer sur base de  $S$ , l'écart-type de l'échantillon (Schuirmann, 1987).

aussi grand que les bornes de la zone d'équivalence, on devrait rejeter l'hypothèse nulle dans la majorité des cas. Par conséquent, un non rejet de l'hypothèse nulle devrait vraisemblablement signifier que l'effet n'atteint pas le SESOI et donc, que l'effet observé n'est pas pertinent. Bien que ce raisonnement puisse sembler tentant, de prime abord, il présente d'importantes limites.

Premièrement, le test n'a pas de bonnes propriétés asymptotiques. Lorsque les échantillons sont suffisamment grands pour atteindre la puissance minimale requise, la probabilité de détecter une absence d'effet va diminuer lorsque la taille des échantillons va augmenter (Seaman & Serlin, 1998). Afin de l'illustrer, nous avons créé la Table 2, dans laquelle nous envisageons les mêmes scénarios que dans la Table 1 et ajoutons une contrainte de puissance: nous décidons qu'on ne peut conclure à l'équivalence que si l'on atteind une puissance de 80% pour détecter une différence de moyenne de .3. Ce faisant, aucune itération n'amènera à conclure à l'équivalence avec 100 sujets par groupes, quelle que soit la différence entre les moyennes de population envisagée (même lorsque cette différence est nulle), car on n'atteint pas la puissance minimale requise. Une fois la puissance minimale requise atteinte, par contre, on constate que la proportion d'itérations qui amènent à conclure à l'équivalence diminue à mesure que la taille des échantillons augmente. Par exemple, si la probabilité de conclure à l'équivalence est d'environ 81% lorsque la vraie différence de moyenne vaut .1 et qu'il y a 200 sujets par groupe, cette probabilité tombe à approximativement 54% avec 700 sujets par groupe.

Table 2.

*Proportion d'itérations qui amènent à conclure à l'équivalence en fonction de la taille des échantillons ( $n_j$ ) et de la différence entre les moyennes de chaque population ( $\mu_1 - \mu_2$ ), quand on exige une puissance minimale de 80% pour détecter une différence de moyenne de .3.*

$n_j$	Différence de moyennes dans la population ( $\mu_1 - \mu_2$ )					
	0	.1	.2	.3	.4	.5
100	< .001	< .001	< .001	< .001	< .001	< .001
200	0.923	0.809	0.469	0.146	0.020	0.001
300	0.950	0.768	0.312	0.043	0.001	< .001
400	0.949	0.708	0.194	0.011	< .001	< .001
500	0.949	0.646	0.116	0.003	< .001	< .001
600	0.950	0.591	0.066	0.001	< .001	< .001
700	0.948	0.536	0.037	< .001	< .001	< .001

*Note.* Pour chaque scénario, les deux échantillons sont toujours de même taille ( $n_1 = n_2 = n$ ) et sont extraits de populations se distribuant normalement et ayant la même variance ( $\sigma_1 = \sigma_2 = \sigma$ ). La moyenne de la première population ( $\mu_1$ ) vaut systématiquement 0, et celle de la deuxième population ( $\mu_2$ ) varie de sorte à obtenir la différence de moyenne  $\mu_1 - \mu_2$  désirée. Par ailleurs,  $\sigma$  vaut systématiquement 1, si bien que la différence de moyenne brute est égale au  $\delta$  de Cohen.

Deuxièmement, pour une taille d'échantillon donnée, plus l'erreur (la variabilité des scores au sein de chaque groupe) sera plus grande (Meyners, 2012 ; Schuirmann, 1987), plus la probabilité de conclure à l'équivalence augmentera. Ce dernier point est illustré au sein de la Figure 1, dans le contexte de la comparaison de deux moyennes. Sur l'axe des abscisses, on représente différentes estimations de la différence de moyenne ( $\bar{X}_1 - \bar{X}_2$ ) et sur l'axe des ordonnées, la précision des estimations  $\bar{X}_1 - \bar{X}_2$  ( $S\sqrt{\frac{2}{n}}$  correspond à l'estimation de l'erreur standard de  $\bar{X}_1 - \bar{X}_2$ , avec  $S$  étant l'écart-type poolé et  $n$  la taille de chaque échantillon, lorsque les échantillons ont tous les deux la même taille et sont extraits de population ayant la même variance)<sup>7</sup>. Le triangle grisé représente l'ensemble des combinaisons estimation/précision qui vont amener à conclure à l'équivalence, avec l'approche de la puissance, lorsqu'on travaille avec des échantillons de taille 50, en acceptant un risque  $\alpha$  de 5% et en exigeant une puissance minimale de 80% pour détecter une différence de 20 unités ( $|\theta_j| = 20$ ,  $j = 1, 2$ ). Dans cet exemple, pour toutes les valeurs de  $S\sqrt{\frac{2}{n}}$  supérieures à 7.07 aucune estimation de différence de moyennes ne permettra de conclure à l'équivalence (pas même 0) puisque la puissance du test à détecter une différence de 20 unités est inférieure à 80%. Pour toutes les valeurs de  $S\sqrt{\frac{2}{n}}$  inférieures à 7.07, on constate que plus notre estimation de  $\bar{X}_1 - \bar{X}_2$  est précise (lorsqu'on se déplace du haut vers le bas, sur l'axe des ordonnées), plus l'estimation doit être proche de 0 pour pouvoir conclure à l'équivalence. Comme on peut le voir à travers le triangle hachuré sur la Figure 1, cette propriété peu désirable n'est pas partagée par le TOST, un test d'équivalence que nous allons décrire ci-dessous (Schuirmann, 1987).

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7. Par facilité, à l'instar de Schuirmann (1987), on envisage le cas où les échantillons sont de même taille et que l'on suppose que la condition d'homogénéité des variances est respectée. Notons cependant que d'après Schuirmann, ce raisonnement peut être généralisé aux scénarios où les deux échantillons n'ont pas la même taille et sont extraits de population n'ayant pas la même variance.

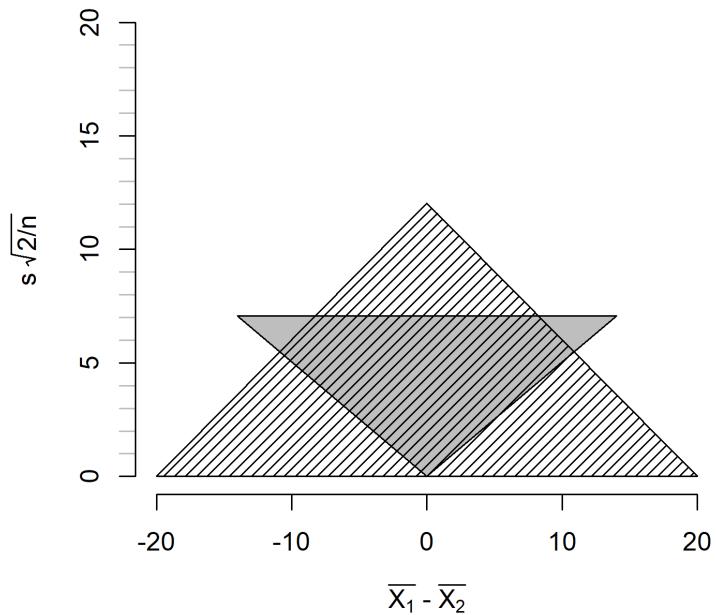


FIGURE 1 – Région d'équivalence pour l'approche de la puissance (zone grisée) et pour le TOST (zone hachurée), pour l'exemple où  $|\theta|=20$ ,  $n = 50$  et  $\alpha = .05$

## Les tests d'équivalence

Avec les tests d'équivalence, il n'est pas possible de démontrer qu'un effet vaille exactement zéro (Meyners, 2012). Il est par contre possible de montrer que l'effet observé est suffisamment petit pour être jugé non pertinent. Or, cela peut s'avérer précieux dans de nombreuses situations, par exemple pour justifier la décision de regrouper plusieurs groupes de sujets ensemble (Rogers, Howard, & Vessey, 1993), pour contrôler qu'il n'y ait pas de différence trop importante entre les groupes sur base de critères autres que le (ou les) facteur(s) d'intérêts en cas de quasi-expérience (Seaman & Serlin, 1998) ou encore pour falsifier une théorie qui prônerait en faveur d'un effet dépassant une certaine taille (Anderson & Maxwell, 2016 ; Lakens, 2017).

Le point de départ des tests d'équivalence est de définir  $\theta_1$  et  $\theta_2$ , les bornes inférieures et supérieures de la zone d'équivalence, cette dernière contenant l'ensemble des valeurs jugées trop petites pour être susceptibles de nous intéresser. Ces bornes peuvent être exprimées soit dans l'unité des données brutes, soit en terme standardisé, mais doivent être définies avant la récolte des données (Anderson & Maxwell, 2016 ; Lakens, Scheel, & Isager, 2018). Il existe ensuite

plusieurs approches pour démontrer que l'effet observé se situe dans la zone d'équivalence (voir Meyners, 2012, par exemple). Parmis celles-ci, une approche très simple est celle du “Two one-sided tests” (Lakens, 2017 ; Schuirmann, 1987), plus communément appelé le TOST<sup>8</sup>. Le principe est de définir deux hypothèses nulles. La première est que l'effet observé est inférieur à la borne inférieure de la zone d'équivalence:

$$H0_1 : \theta < \theta_1, \text{ avec } \theta_1 \neq 0$$

La deuxième est que l'effet observé est supérieur à la borne supérieure de la zone d'équivalence:

$$H0_2 : \theta > \theta_2, \text{ avec } \theta_2 \neq 0$$

Lorsque les deux hypothèses nulle peuvent être simultanément rejetées, on peut conclure à l'équivalence (Seaman & Serlin, 1998). Cela équivaut, statistiquement parlant, à montrer que l'intervalle de confiance à  $(1 - 2 \times \alpha)\%$  est entièrement inclus dans la zone d'équivalence (Lakens, 2017 ; Seaman & Serlin, 1998). Notons qu'il n'est pas nécessaire de reporter les résultats des deux tests unilatéraux, lorsqu'on réalise le TOST: il suffit de reporter les résultats du test associé à la plus petite valeur de statistique (et par conséquent, à la plus grande *p*-valeur). En effet, si ce test amène à conclure au rejet de l'hypothèse nulle, le second test amènera automatiquement à la même conclusion (Lakens, Scheel, & Isager, 2018 ; Rogers, Howard, & Vessey, 1993). Cette remarque reste vraie dans le cas particulier où les deux tests sont associés à la même valeur de statistique puisque dans ce cas, les deux tests mèneront à une conclusion identique (Rogers, Howard, & Vessey, 1993). Notons également qu'il n'est pas nécessaire de procéder à une correction du risque alpha dû à la réalisation simultanée de deux tests. En effet, une erreur de type *I* (rejeter à tort l'hypothèse nulle) ne peut être commise que si l'hypothèse nulle est vraie. Or, les deux hypothèses nulles testées sont mutuellement exclusives: il n'est pas possible que  $\theta$  soit simultanément inférieur à  $\theta_1$  (ce qui correspond à  $H0_1$ ) et supérieur à  $\theta_2$  (ce qui correspond à  $H0_2$ ).

Jusqu'il y a peu, le TOST n'était pas disponible dans la plupart des logiciels, à l'exception de Minitab, ce qui constituait un frein important à son usage. Pour cette raison, Lakens

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8. Il existe des alternatives au TOST qui sont très légèrement plus puissantes, mais le gain marginal en termes de puissance est contrebalancé par un niveau de complexité beaucoup plus élevé (Meyners, 2012).

(2016) a créé le package R “TOSTER” et plus récemment encore, ce même package a été implémenté dans Jamovi<sup>9</sup>. Tant dans R que dans Jamovi, le package compare simultanément l’effet observé à l’absence d’effet (cela correspond au test traditionnel) ainsi qu’aux deux bornes de la zone d’équivalence (cela correspond au TOST). Il en découle 4 conclusions distinctes possibles (Lakens, 2017), qui sont illustrées dans la figure 2 dans le contexte de la comparaison de deux moyennes indépendantes:

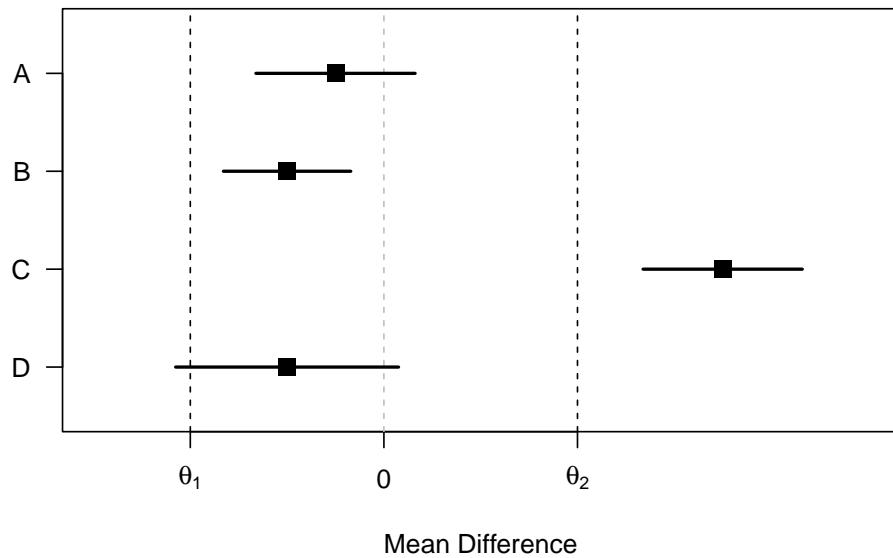


FIGURE 2 – Différence de moyennes ( $\bar{X}_1 - \bar{X}_2$ ) et IC à  $1 - 2\alpha\%$  autour de la différence de moyennes ( $\bar{X}_1 - \bar{X}_2$ ) pour 4 scénarios distincts.

- (1) La différence de moyenne observée diffère significativement des deux bornes d’équivalence, mais pas de 0 (scénario A, Figure 2): dans ce cas, on conclura à l’absence d’un effet au moins aussi grand que les bornes d’équivalence.
- (2) La différence de moyenne observée diffère significativement des deux bornes d’équivalence ainsi que de 0 (scénario B, Figure 2): on conclura alors qu’il existe un effet non nul, mais qui ne dépasse pas une certaine taille fixée par les bornes. C’est ce qui arrive typiquement lorsqu’on travaille avec de très grands échantillons, si bien que le test traditionnel est très

9. Jamovi est un logiciel clic-bouton entièrement gratuit qui gagne en popularité et qui présente, parmi ses nombreux avantages, le fait d’être particulièrement convivial. Dans la mesure où la plupart des chercheurs sont plus enclins à utiliser des procédures si elles sont implémentées dans ce type de logiciel (Fraas & Newman, 2000), cela constitue une excellente nouvelle pour le devenir du TOST dans la recherche en psychologie.

puissant, même pour détecter des effets très petits (Rogers, Howard, & Vessey, 1993).

- (3) La différence de moyenne observée diffère significativement de 0, mais ne diffère pas significativement d'au moins une des deux bornes d'équivalence (scénario C, Figure 2): on conclura alors à la présence d'un effet non nul (Rogers, Howard, & Vessey, 1993).
- (4) La différence de moyenne observée ne diffère significativement ni d'au moins une des deux bornes d'équivalence, ni de 0 (scénario D, Figure 2): c'est ce qui arrive lorsque les données sont si imprécises qu'on ne peut tirer aucune conclusion. Les données semblent compatibles tant avec un effet nul qu'avec un effet supérieur au SESOI.

## Définir les bornes de la zone d'équivalence

L'aspect le plus compliqué dans la réalisation du TOST est la définition des bornes d'équivalence. Dans certains cas, il est possible de définir un critère objectif qui permettra de déterminer à partir de quand un effet est jugé pertinent (Lakens, Scheel, & Isager, 2018). Dans ce cas, établir l'équivalence revient à rejeter la présence d'un effet ayant un quelconque intérêt pratique (Rogers, Howard, & Vessey, 1993). Par exemple, Burriss et al. (2015) avaient émis l'hypothèse qu'une augmentation de la rougeur de la peau chez femmes les rendraient plus attractives pour les hommes en période d'ovulation. Or, une telle hypothèse n'est crédible que si le changement facial est visible à l'oeil nu. Dans ce contexte, le SESOI serait la plus petite variation dans la rougeur de la peau qu'il est possible de détecter à l'oeil nu (Lakens, Scheel, & Isager, 2018). Il est également parfois possible pour des experts de déterminer expérimentalement ce qui constitue un changement important, pour certaines échelles de mesures fréquemment utilisées en psychologie, à l'instar de Button et al. (2015) qui se sont penchés sur le BDI<sup>10</sup>. Ces auteurs ont interrogé un grand nombre de patients quant à leur ressenti subjectif en termes d'amélioration de leur dépression dans un certain laps de temps, et ont comparé leurs réponses à la différence de score obtenu à l'aide du BDI dans ce même laps de temps (Lakens, Scheel, & Isager, 2018).

Malheureusement, il n'est pas toujours possible d'établir un critère objectif en vue de définir les bornes d'équivalence. Dans ce cas, il existe diverses stratégies, plus subjectives, en vue d'établir

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10. Le BDI (Beck Depression Inventory) est une échelle auto-rapportée évaluant les symptômes cognitifs courants de la dépression. Cette échelle est constituée de 21 items évalués à l'aide des échelles de Likert allant de 0 à 3, ce qui donne un score total compris entre 0 et 63 qui sera d'autant plus élevé que la dépression sera sévère (Button et al., 2015).

ces bornes. En les utilisant, il faut cependant avoir conscience du fait que la question à laquelle nous répondons varie en fonction de la stratégie utilisée.

Premièrement, il est possible de déterminer des bornes en s'inspirant de balises existantes, en vue d'exclure la présence d'un effet jugé petit, moyen ou grand par ces balises (Lakens, Scheel, & Isager, 2018). Notons que si cette stratégie est tentante de par sa simplicité, elle doit être utilisée avec prudence. D'abord, un effet ne devrait être qualifié de petit, moyen ou grand qu'en comparaison à d'autres effets connus, et non sur base d'impressions qualitatives (Gignac & Szodorai, 2016). Dit autrement, il est important d'avoir un cadre de référence pour juger de la taille d'un effet. Or, les balises de Cohen (en l'occurrence, les balises les plus célèbres et les plus largement utilisées) sont dépourvues de ce cadre de référence, puisqu'elles ont été établies à une époque où très peu de chercheurs se préoccupaient de la taille des effets étudiés (Funder & Ozer, 2019). Depuis Cohen, certains chercheurs ont déployé de gros efforts en vue d'établir de nouvelles normes sur base d'analyses systématiques quantitative de la littérature. Gignac & Szodorai (2016), par exemple, ont établi de nouvelles balises pour interpréter le  $r$  de Pearson, en définissant les quartiles d'une distribution de 708 mesures dérivées de méta-analyses issues de la psychologie sociale et de la personnalité. C'est de la sorte qu'ils ont proposé d'interpréter respectivement des mesures de 0.10, 0.20 et 0.30, dans ces domaines de la psychologie, comme représentant des effets relativement petits, typiques et relativement larges. Ces normes ont également été approuvées par Funder & Ozer (2019). Ensuite, les balises ne prennent pas en compte le contexte de l'étude si bien que statuer sur la taille d'un effet ne fournit pas nécessairement d'information sur sa valeur. *Un effet même de très petite taille peut faire une différence pour les personnes concernées. Yzerbit donne l'exemple suivant: l'effet de l'aspirine sur l'espérance de vie est minimale, mais pour les gens qui survivent, ça fait une différence.. Autre exemple: Imaginons un médicament contre la dépression qui permettrait de réduire les symptômes un tout petit peu mais qui ne coûte presque rien. Une faible différence d'un point de vue statistique peut devenir une grande différence pour les personnes concernées. Idem pour un médicament qui sauve des vies.* C'est pour cette raison que les balises devraient toujours être utilisées en dernier recours, lorsqu'on ne dispose d'aucune information contextuelle.

Deuxièmement, dans le contexte d'études de réPLICATION, il est possible de se baser sur les résultats d'études antérieures pour définir les bornes d'équivalence. 2.1. (Levine et al. 2007): se

baser sur les tailles d'effet suggérées dans la littérature (sur base de méta-analyses)<sup>11</sup>. Prenons l'exemple de l'augmentation de la pensée agressive quand on joue à des jeux violents. D'après une méta-analyse de Ferguson (2007), cette corrélation serait de  $r = .25$  (ce qui correspond à un  $d$  de Cohen de .51). Je peux utiliser cette valeur comme borne pour définir l'intervalle d'équivalence. Si je parviens à montrer qu'il y a équivalence, je montre que l'effet étudié serait vraisemblablement plus petit que suggéré dans la littérature. .

→ Remarque: la méta-analyse elle donne .25, OK, mais en réalité, il y a une distribution autour de l'effet dans la littérature. Une solution plus conservatrice est alors de se baser sur les bornes inférieures de l'IC autour de la valeur de la méta-analyse. → Comme dit Vincent, c'est la réalité de l'effet. C'est comme si tu faisais un test entre deux moyennes en prétendant que la myenne est une valeur et pas une distribution autorud e cette effet. Et donc c'est un peu délicat d'aller dire qu'on va tester un test d'équivalence pour contester l'effet de la violence en prenant la valeur obtenue dans la méta analyse comme une borne absolue et pas une distribution. Du coup, au minimum il essayerait d'intégrer le fait qu'il y a une distribution autour de la valeur, mais donc on ne peut pas décider que si on est en dessous de .50 ou au dessus de -.50.

→ Autre probleme, pourquoi prendre l'opposé comme borne inférieure? Alors que la méta analyse ne parle pas d'effet inverse. → Pour l'heure, je serais plus d'avis de confirmer la méta-analyse en montrer que l'effet est bien situé à l'intérieur des bornes de l'IC autour de cet effet dans la méta-analyse. 2.2. Solution proposée par Simonsohn (2015) pour remettre en question la pertinence de l'outil qui a ét utilisé pour démontrer un effet dans une étude antérieure. Il dit que si on a pour un effet d'intérêt une puissance inférieure à 33% de le détecter, on a vraiment un gros problème de puissance (pourquoi 33% ça reste arbitraire, of course). il part de cette optique là et il se dit que du coup, ça pourrait être intéressant de définir les bornes de la zone d'équivalence en considérant un effet que l'étude d'origine aurait pu détecter avec une puissance de 33%. Ce faisant, si on parvient à démontrer que l'effet est encore plus petit qu'un effet que l'étude ne base n'aurait pu détecter qu'avec une puissance de 33%, il y a peu de chance pour que l'effet originellement proposé par l'étude d'origine soit vraiment basé sur un outil pertinent. *exemple concrèt: imaginons un test t de Student et qu'on a 21 sujets par groupe. On peut déterminer, en faisant une analyse de sensibilité dans Gpower, qu'on a une puissance*

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11. Il est mieux de se baser sur des résultats de méta-analyse que d'étude isolée parce qu'à cause du biais de publication, les tailles d'effet observées sont souvent une sur-représentation de la réalité et donc si on a bcp de sujets dans notre étude, il y a vraiment bcp de chance qu'on démontre l'équivalence

*33% à détecter un effet d de COhen de .48. Du coup, on considererais .48 comme valeur pour notre zone d'équivalence.*

2.3. Lakens (2018): essayer de deviner implicitement ce que l'auteur de l'étude d'origine aurait pu considérer comme un effet pertinent (s'il n'a pas donné d'indication dans son article pour dire " je considère qu'un effet est pertinent à partir de telle valeur"). Cela peut se faire sur base de la taille d'échantillon utilisée par cette personne. *On ne pourra détecter un effet comme significatif que si la valeur de statistique observée dépasse une valeur seuil (la valeur critique). Il est possible, grâce à la relation qui existe entre la statistique t et la statistique d de Cohen, de déterminer à quelle "taille d'effet critique" correspond la statistique t critique. Par exemple, si le chercheur a utilisé 30 sujets par groupe: via gpower, on peut déterminer qu'il faudra une statistique observée t de minimum 2.045 pour pouvoir conclure au RH0. et compte tenu du lien entre la statistique t et la stat d, ça correspond à un d de Cohen de .373 ( $d_{crit} = t_{crit}/racine(n)$ ). Concrètement, si on observe une taille d'effet supérieure ou égale à .373, on pourra conclure au rejet de l'H0. Si la taille d'effet est plus petite, on ne sera pas capable de conclure au rejet de l'hypothèse nulle. L'idée ce serait de démontrer qu'il y a équivalence, la personne qui a écrit l'article d'origine a utilisé une taille d'échantillon insuffisante pour étudier l'effet suggéré et donc, si on veut étudier ce même sujet d'étude, il faudrait nécessairement récolter des échantillons plus grands pour être capable de le faire correctement.*

- 3) de se baser sur les ressources dont on dispose (analyse de sensibilité). Si moi je ne suis pas capable d'avoir un échantillon de plus de 2000 personnes, il y a certains effets que je ne serai pas capable de calculer. Et donc, je peux utiliser cette taille d'échantillon pour déterminer la taille d'effet dont je suis certain que je pourrai raisonnablement conclure au rejet de l'hypothèse nulle. Et donc là, si on démontre qu'il y a équivalence, on ne tire pas la ccl que l'effet n'est pas pertinent, mais que cet effet qu'on a envie d'étudier ne peut être l'être sur base des tailles d'échantillon qu'on a l'habitude d'utiliser.

Il est important de bien comprendre qu'en fonction de la stratégie utilisée, on ne se posera pas nécessairement la même question de recherche (et la réponse obtenue sera nécessairement liée à cette question de recherche).

- 1) définir comme limites la plus petite taille d'effet pour laquelle on peut atteindre une puissance de détection suffisante (déterminé par les ressources disponibles pour étudier

l'effet, Lakens (2017)) -> voir la section “Setting equivalence bounds” p. 359 mais je crois que j'en parle aussi dans la vidéo SOCLAB.

- 2) le SESOI peut parfois être fixé objectivement
- 3) Idéalement basé sur une analyse coût-bénéfice). Attention: bien sûr une dimension subjective dans la définition des coûts et des bénéfices. Attention: le SESOI doit être déterminé AVANT et INDEPENDAMMENT des données.

# Comparaison du TOST et du SGPV

Meta-Psychology, 2020, vol 4, MP.2018.933  
<https://doi.org/10.15626/MP.2018.933>  
Article type: Original Article  
Published under the CC-BY4.0 license



Open data: N/A  
Open materials: Yes  
Open and reproducible analysis: Yes  
Open reviews and editorial process: Yes  
Preregistration: N/A

Edited by: S. R. Martin  
Reviewed by: J. D. Blume, O. L. Olvera Astivia  
Analysis reproduced by: André Kalnendal  
All supplementary files can be accessed at OSF:  
<https://doi.org/10.17605/OSF.IO/ZP3KF>

## Equivalence Testing and the Second Generation P-Value.

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### Abstract

To move beyond the limitations of null-hypothesis tests, statistical approaches have been developed where the observed data are compared against a range of values that are equivalent to the absence of a meaningful effect. Specifying a range of values around zero allows researchers to statistically reject the presence of effects large enough to matter, and prevents practically insignificant effects from being interpreted as a statistically significant difference. We compare the behavior of the recently proposed second generation *p*-value (Blume, D'Agostino McGowan, Dupont, & Greevy, 2018) with the more established Two One-Sided Tests (TOST) equivalence testing procedure (Schuirmann, 1987). We show that the two approaches yield almost identical results under optimal conditions. Under suboptimal conditions (e.g., when the confidence interval is wider than the equivalence range, or when confidence intervals are asymmetric) the second generation *p*-value becomes difficult to interpret. The second generation *p*-value is interpretable in a dichotomous manner (i.e., when the SGPV equals 0 or 1 because the confidence intervals lies completely within or outside of the equivalence range), but this dichotomous interpretation does not require calculations. We conclude that equivalence tests yield more consistent *p*-values, distinguish between datasets that yield the same second generation *p*-value, and allow for easier control of Type I and Type II error rates.

**Keywords:** equivalence testing, second generation *p*-values, hypothesis testing, TOST, statistical inference

To test predictions researchers predominantly rely on null-hypothesis tests. This statistical approach can be used to examine whether observed data are sufficiently surprising under the null hypothesis to reject an effect that equals exactly zero. Null-hypothesis tests have an important limitation, in that this procedure can only reject the hypothesis that there is no effect, while scientists should also be able to provide statistical support for *equivalence*. When testing for equivalence researchers aim to examine whether an observed effect is too small to be considered meaningful, and therefore is practi-

cally equivalent to zero. By specifying a range around the null hypothesis of values that are deemed practically equivalent to the absence of an effect (i.e., 0 + - 0.3) the observed data can be compared against an *equivalence range* and researchers can test if a meaningful effect is absent (Hauck & Anderson, 1984; Kruschke, 2018; Rogers, Howard, & Vessey, 1993; Serlin & Lap-sley, 1985; Spiegelhalter, Freedman, & Parmar, 1994; Wellek, 2010; Westlake, 1972).

Second generation *p*-values (SGPV) were recently proposed as a statistic that represents "the proportion of

data-supported hypotheses that are also null hypotheses" (Blume et al., 2018). The researcher specifies an equivalence range around a null hypothesis of values that are considered practically equivalent to the null hypothesis. The SGPV measures the degree to which a set of data-supported parameter values falls within the interval null hypothesis. If the estimation interval falls completely within the equivalence range, the SGPV is 1. If the confidence interval falls completely outside of the equivalence range, the SGPV is 0. Otherwise the SGPV is a value between 0 and 1 that expresses the overlap of data-supported hypotheses and the equivalence range. When calculating the SGPV the set of data-supported parameter values can be represented by a confidence interval (CI), although one could also choose to use credible intervals or Likelihood support intervals (SI). When a confidence interval is used, the SGPV and equivalence tests such as the Two One-Sided Tests (TOST) procedure (Lakens, 2017; Meyners, 2012; Quertemont, 2011; Schuirmann, 1987) appear to have close ties, because both tests compare a confidence interval against an equivalence range. Here, we aim to examine the similarities and differences between the TOST procedure and the SGPV. We limit our analysis to continuous data sampled from a bivariate normal distribution. The TOST procedure also relies on the confidence interval around the effect. In the TOST procedure the data are tested against the lower equivalence bound in the first one-sided test, and against the upper equivalence bound in the second one-sided test (Lakens, Scheel, & Isager, 2018). For an excellent discussion of the strengths and weaknesses of different frequentist equivalence tests, including alternatives to the TOST procedure, see Meyners (2012). If both tests statistically reject an effect as extreme or more extreme than the equivalence bound, you can conclude the observed effect is practically equivalent to zero from a Neyman-Pearson approach to statistical inferences. Because one-sided tests are performed, one can also conclude equivalence by checking whether the  $1-2\times\alpha$  confidence interval (e.g., when the alpha level is 0.05, a 90% CI) falls completely within the equivalence bounds. Because both equivalence tests as the SGPV are based on whether and how much a confidence interval overlaps with equivalence bounds, it seems worthwhile to compare the behavior of the newly proposed SGPV to equivalence tests to examine the unique contribution of the SGPV to the statistical toolbox.

### The relationship between *p*-values from TOST and SGPV when confidence intervals are symmetrical

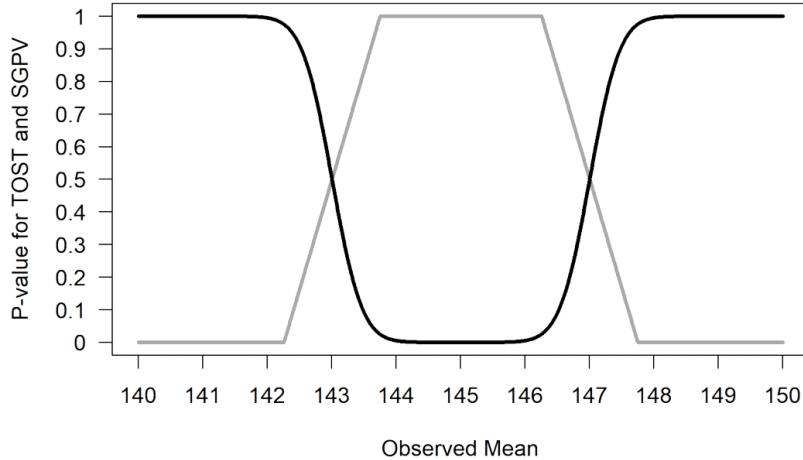
The second generation *p*-value (SGPV) is calculated as:

$$p_\delta = \frac{|I \cap H_0|}{|I|} \times \max \left\{ \frac{|I|}{2|H_0|}, 1 \right\}$$

where  $I$  is the interval based on the data (e.g., a 95% confidence interval) and  $H_0$  is the equivalence range. The first term of this formula implies that the second generation *p*-value is the width of the confidence interval that overlaps with the equivalence range, divided by the total width of the confidence interval. The second term is a "small sample correction" (which will be discussed later) that comes into play whenever the confidence interval is more than twice as wide as the equivalence range. To examine the relation between the TOST *p*-value and the SGPV we can calculate both statistics across a range of observed effect sizes. Building on the example by Blume et al. (2018), in Figure 1 *p*-values are plotted for the TOST procedure and the SGPV. The statistics are calculated for hypothetical one-sample *t*-tests for observed means ranging from 140 to 150 (on the x-axis). The equivalence range is set to 145 ± 2 (i.e., an equivalence range from 143 to 147), the observed standard deviation is assumed to be 2, and the sample size is 30. For example, for the left-most point in Figure 1 the SGPV and the TOST *p*-value is calculated for a hypothetical study with a sample size of 30, an observed standard deviation of 2, and an observed mean of 140, where the *p*-value for the equivalence test is 1, and the SGPV is 0.

Our conclusions about the relationship between TOST *p*-values and SGPV hold for second generation *p*-values calculated from confidence intervals, and assuming data is sampled from a bivariate normal distribution. Readers can explore the relationship between TOST *p*-values and SGPV for themselves in an online Shiny app: [http://shiny.ieis.tue.nl/TOST\\_vs\\_SGPV/](http://shiny.ieis.tue.nl/TOST_vs_SGPV/).

The SGPV treats the equivalence range as the null-hypothesis, while the TOST procedure treats the values outside of the equivalence range as the null-hypothesis. For ease of comparison we can plot 1-SGPV (see Figure 2) to make the values more easily comparable. We see that the *p*-value from the TOST procedure and the SGPV follow each other closely. When we discuss the relationship between the *p*-values from TOST and the SGPV, we focus on their correspondence at three values, namely where the TOST  $p = 0.025$  and SGPV is 1, where the TOST  $p = 0.5$  and SGPV = 0.5, and where the TOST  $p = 0.975$  and SGPV = 1. These three values are important for the SGPV because they indicate the values at which the SGPV indicates the data should be interpreted as compatible with the null hypothesis (SGPV =



*Figure 1.* Comparison of  $p$ -values from TOST (black line) and SGPV (grey line) across a range of observed sample means (x-axis) tested against a mean of 145 in a one-sample  $t$ -test with a sample size of 30 and a standard deviation of 2, illustrating that when the TOST  $p$ -value = 0.5, the SGPV = 0.5, when the TOST  $p$ -value is 0.975, 1-SGPV = 1, and when the TOST  $p$ -value = 0.025, 1-SGPV = 0.

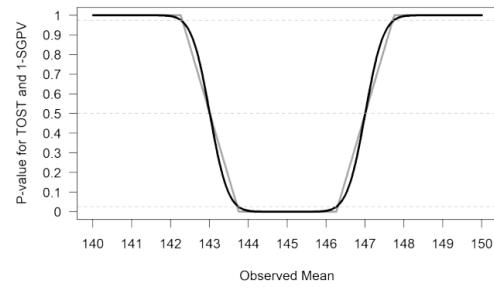
1), or with the alternative hypothesis ( $SGPV = 0$ ), or when the data are strictly inconclusive ( $SGPV = 0.5$ ).

These three points of overlap are indicated by the horizontal dotted lines in Figure 2 at TOST  $p$ -values of 0.975, 0.5, and 0.025.

When the observed sample mean is 145, the sample size is 30, and the standard deviation is 2, and we are testing against equivalence bounds of 143 and 147 using the TOST procedure for a one-sample  $t$ -test, the equivalence test is significant,  $t(29) = 5.48$ ,  $p < .001$ . Because the 95% CI falls completely within the equivalence bounds, the SGPV is 1 (see Figure 1). On the other hand, when the observed mean is 140, the equivalence test is not significant (the observed mean is far outside the equivalence range of 143 to 147),  $t(29) = -8.22$ ,  $p = 1$  (or more accurately,  $p > .999$  as  $p$ -values are bounded between 0 and 1). Because the 95% CI falls completely outside the equivalence bounds, the SGPV is 0 (see Figure 1).

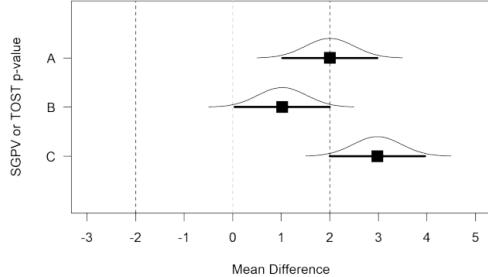
#### SGPV as a uniform measure of overlap

It is clear the SGPV and the  $p$ -value from TOST are closely related. When confidence intervals are symmetric we can think of the SGPV as a straight line that is



*Figure 2.* Comparison of  $p$ -values from TOST (black line) and 1-SGPV (grey line) across a range of observed sample means (x-axis) tested against a mean of 145 in a one-sample  $t$ -test with a sample size of 30 and a standard deviation of 2.

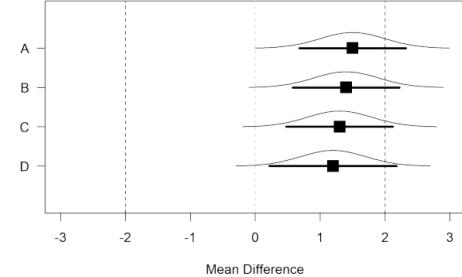
directly related to the  $p$ -value from an equivalence test for three values. When the TOST  $p$ -value is 0.5, the SGPV is also 0.5 (note that the reverse is not true). The



*Figure 3.* Means, normal distribution, and 95% CI for three example datasets that illustrate the relationship between p-values from TOST and SGPV.

SGPV is 50% when the observed mean falls exactly on the lower or upper equivalence bound, because 50% of the symmetrical confidence interval overlaps with the equivalence range. When the observed mean equals the equivalence bound, the difference between the mean in the data and the equivalence bound is 0, the *t*-value for the equivalence test is also 0, and thus the *p*-value is 0.5 (situation A, Figure 3).

Two other points always have to overlap. When the 95% CI falls completely inside the equivalence region, and one endpoint of the confidence interval is exactly equal to one of the equivalence bounds (see situation B in Figure 3) the TOST *p*-value (which relies on a one-sided test) is always 0.025, and the SGPV is 1. Note that when sample sizes are small or equivalence bounds are narrow, small *p*-values for the TOST or a SGPV = 1 might not be observed in practice if too few observations are collected. The third point where the SGPV and the *p*-value from the TOST procedure should overlap is where the 95% CI falls completely outside of the equivalence range, but one endpoint of the confidence interval is equal to the equivalence bound (see situation C in Figure 3), when the *p*-value will always be 0.975, and the SGPV is 0. Note that this situation is in essence a minimum-effect test (Murphy, Myors, & Wolach, 2014). The goal of a minimum-effect is not just to reject a difference of zero, but to reject the smallest effect size of interest (i.e., the equivalence bounds). An equivalence test and minimum effect test against the same equivalence bound are complementary, and when a TOST *p*-value is larger than 0.975, the *p*-value for the minimum effect test is smaller than 0.05 (and therefore the minimum effect test provides no additional information that can not be derived from the *p*-value from the equiva-



*Figure 4.* Means, normal distribution, and 95% CI for samples where the observed population mean is 1.5, 1.4, 1.3, and 1.2.

lence test). The SGPV summarizes the information from an equivalence test (and the complementary minimum-effect test). These can be two relevant questions to ask, although it often makes sense to combine an equivalence test and a null-hypothesis test instead (Lakens et al., 2018).

For example, in Figure 4 we have plotted four SGPVs. From A to D the SGPV is 0.76, 0.81, 0.86, and 0.91. The difference in the percentage of overlap between A and B (-0.05) is identical to the difference in the percentage of overlap between C and D as the mean gets 0.1 closer to the test value (-0.05). As the observed mean in a one-sample *t*-test lies closer to the test value, from situation A to D, the difference in the overlap changes uniformly. As we move the observed mean closer to the test value in steps of 0.1 across A to D the *p*-value calculated for normally distributed data are not uniformly distributed. The probability of observing data more extreme than the upper bound of 2 is (from A to D) 0.16, 0.12, 0.08, and 0.05. As we can see, the difference between A and B (0.04) is not the same as the difference between C and D (0.03). Indeed, the difference in *p*-values is the largest as you start at *p* = 0.5 (when the observed mean falls on the test value), which is why the line in Figure 1 is the steepest at *p* = 0.5. Note that where the SGPV reaches 1 or 0, *p*-values closely approximate 0 and 1, but never reach these values.

#### When different *p*-values for equivalence tests yield the same SGPV

There are three situations where *p*-values for TOST differentiate between observed results, while the SGPV does not differentiate. The first two situations were discussed before and can be seen in Figure 1. When

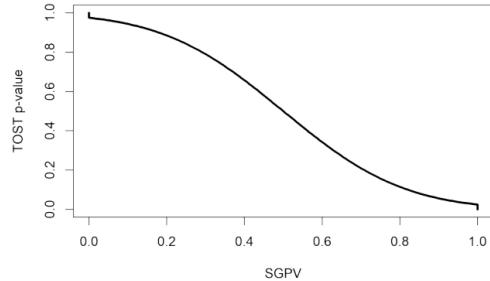


Figure 5. The relationship between  $p$ -values from the TOST procedure and the SGPV for the same scenario as in Figure 1.

the SGPV is either 0 or 1,  $p$ -values from the equivalence test fall between 0.975 and 1 or between 0 and 0.025. Where the SGPV is 1 as long as the confidence interval falls completely within the equivalence bounds, the  $p$ -value for the TOST continues to differentiate between results as a function of how far the confidence interval lies within the equivalence bounds (the further the confidence interval is from both bounds, the lower the  $p$ -value). The easiest way to see this is by plotting the SGPV against the  $p$ -value from the TOST procedure. The situations where the  $p$ -values from the TOST procedure continue to differentiate based on how extreme the results are, but the SGPV is a fixed value are indicated by the parts of the curve where there are vertical straight lines at second generation  $p$ -values of 0 and 1.

A third situation in which the SGPV remains stable across a range of observed effects, while the TOST  $p$ -value continues to differentiate, is whenever the CI is wider than the equivalence range, and the CI overlaps with the upper *and* lower equivalence bound. When the confidence interval is more than twice as wide as the equivalence range the SGPV is set to 0.5. Blume et al. (2018) call this the “small sample correction factor”. However, it is not a correction in the typical sense of the word, since the SGPV is not adjusted to any “correct” value. When the normal calculation would be “misleading” (i.e., the SGPV would be small, which normally would suggest support for the alternative hypothesis, but at the same time all values in the equivalence range are supported), the SGPV is set to 0.5 which according to Blume and colleagues signals that the SGPV is “uninformative”. Note that the CI can be twice as wide as the equivalence range whenever the sample size is small (and the confidence interval width is large) *or* when

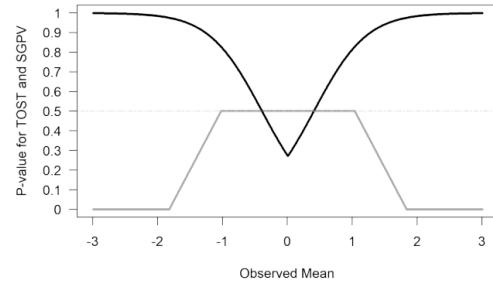


Figure 6. Comparison of  $p$ -values from TOST (black line) and SGPV (grey line) across a range of observed sample means (x-axis). Because the sample size is small ( $n = 10$ ) and with a standard deviation of 2 the CI is more than twice as wide as the equivalence range (set to -0.4 to 0.4), the SGPV is set to 0.5 (horizontal light-grey line) across a range of observed means.

then equivalence range is narrow. It is therefore not so much a “small sample correction” as it is an exception to the typical calculation of the SGPV whenever the ratio of the confidence interval width to the equivalence range exceeds 2:1 and the CI overlaps with the upper and lower bounds.

We can examine this situation by calculating the SGPV and performing the TOST for a situation where sample sizes are small and the equivalence range is narrow, such that the CI is more than twice as large as the equivalence range (see Figure 6). When the two statistics are plotted against each other we can see where the SGPV is the same while the TOST  $p$ -value still differentiates different observed means (indicated by straight lines in the curve, see Figure 7). We see the SGPV is 0.5 for a range of observed means where the  $p$ -value from the equivalence test still varies. It should be noted that in these calculations the  $p$ -values for the TOST procedure are *never* smaller than 0.05 (i.e., they do not get below 0.05 on the y-axis). In other words, we cannot conclude equivalence based on any of the observed means. This happens because we are examining a scenario where the 90% CI is so wide that it never falls completely within the two equivalence bounds.

As Lakens (2017) notes: “in small samples (where CIs are wide), a study might have no statistical power (i.e., the CI will always be so wide that it is necessarily wider than the equivalence bounds).” None of the  $p$ -values based on the TOST procedure are below 0.05, and thus, in the long run we have 0% power.

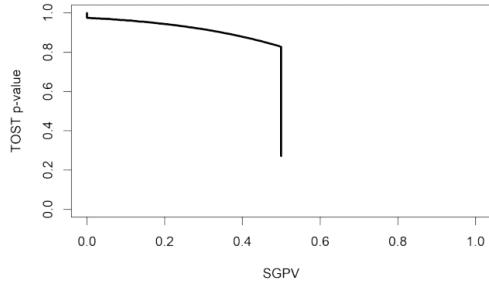


Figure 7. The relationship between  $p$ -values from the TOST procedure and the SGPV for the same scenario as in Figure 6.

The  $p$ -value from the TOST procedure still differentiates observed means, while the SGPV does not, when the CI is wider than the equivalence range (so the precision is low) and overlaps with the upper and lower equivalence bound, but the CI is not twice as wide as the equivalence range. In the example below, we see that the CI is only 1.79 times as wide as the equivalence bounds, but the CI overlaps with the lower and upper equivalence bounds (Figure 8). This means the SGPV is not set to 0.5, but it is constant across a range of observed means, while the TOST  $p$ -value is not constant across this range.

If the observed mean would be somewhat closer to 0, or further away from 0, the SGPV remains constant (the CI width does not change, and it completely over-

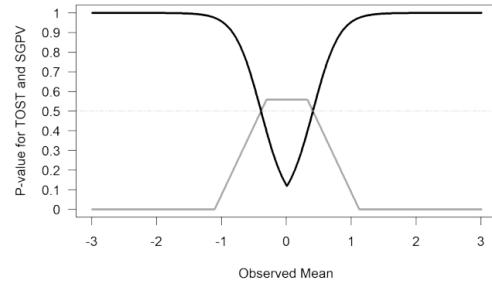


Figure 9. Comparison of  $p$ -values from TOST (black line) and SGPV (grey line) across a range of observed sample means (x-axis). The sample size is small ( $n = 10$ ), but because the  $sd$  is half as big as in Figure 7 (1 instead of 2) the CI is less than twice as wide as the equivalence range (set to  $-0.4$  to  $0.4$ ). The SGPV is not set to 0.5 (horizontal light grey line) but reaches a maximum slightly above 0.5 across a range of observed means.

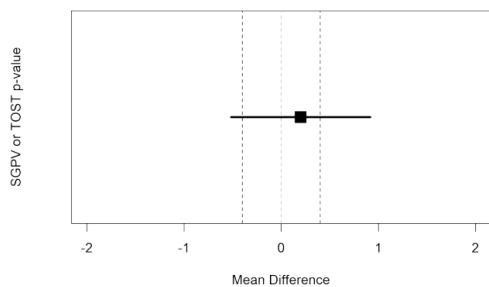


Figure 8. Example of a 95% CI that overlaps with the lower and upper equivalence bound (indicated by the vertical dotted lines).

laps with the equivalence range) while the  $p$ -value for the TOST procedure does vary. We can see this in Figure 9 below. The SGPV is not set to 0.5, but is slightly higher than 0.5 across a range of means. How high the SGPV will be for a CI that is not twice as wide as the equivalence range, but overlaps with the lower and upper equivalence bounds, depends on the width of the CI and the equivalence range.

If we once more plot the two statistics against each other we see the SGPV is 0.56 for a range of observed means where the  $p$ -value from the equivalence test still varies, as indicated by the straight section of the line (Figure 10).

To conclude this section, there are situations where the  $p$ -value from the TOST procedure continues to differentiate, while the SGPV does not. Therefore, interpreted as a continuous statistic, the SGPV is more limited than the  $p$ -value from the TOST procedure.

#### The relation between equivalence tests and SGPV for asymmetrical confidence intervals around correlations

So far we have only looked at the relation between equivalence tests and the SGPV when confidence intervals are symmetric (e.g., for confidence intervals around mean differences). For correlations, which are bound between  $-1$  and  $1$ , confidence intervals are only symmetric for a correlation of exactly 0. The confidence interval for a correlation becomes increasingly asymmetric

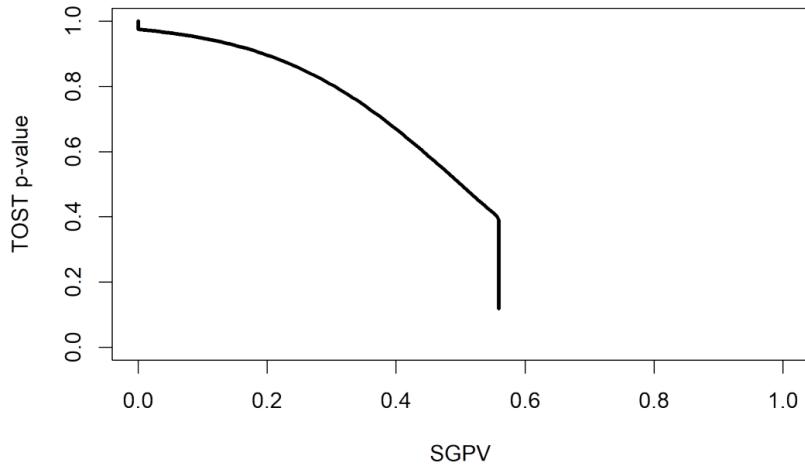


Figure 10. The relationship between  $p$ -values from the TOST procedure and the SGPV for the same scenario as in Figure 9.

as the observed correlation nears -1 or 1. For example, with ten observations, an observed correlation of 0 has a symmetric 95% confidence interval ranging from -0.63 to 0.63, while an observed correlation of 0.7 has an asymmetric 95% confidence interval ranging from 0.13 to 0.92. Note that calculating confidence intervals for a correlation involves a Fisher's z-transformation, which transforms values such that they are approximately normally z-distributed, which allows one to compute symmetric confidence intervals. These confidence intervals are then retransformed into a correlation, where the confidence intervals are asymmetric if the correlation is not exactly zero.

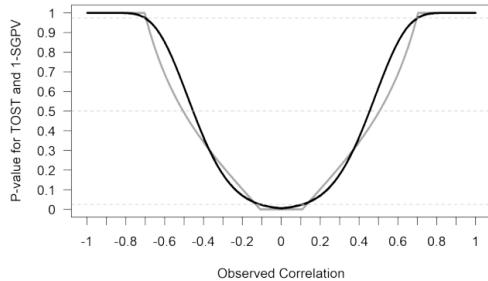
The effect of asymmetric confidence intervals around correlations is most noticeable at smaller sample sizes. In Figure 11 we plot the  $p$ -values from equivalence tests and the SGPV (again plotted as 1-SGPV for ease of comparison) for correlations. The sample size is 30 pairs of observations, and the lower and upper equivalence bounds are set to -0.45 and 0.45, with an alpha of 0.05. As the observed correlation in the sample moves from -.99 to 0 the  $p$ -value from the equivalence test becomes smaller, as does 1-SGPV. The pattern is quite similar to that in Figure 2. The  $p$ -value for the TOST procedure and 1-SGPV are still related as discussed above, with TOST  $p$ -values of 0.975 and 0.025 corresponding to a 1-

SGPV of 1 and 0, respectively. There are two important differences, however. First of all, the SGPV is no longer a straight line, but a curve, due to the asymmetry in the 95% CI. Second, and most importantly, the  $p$ -value for the equivalence test and the SGPV do no longer overlap at  $p = 0.5$ .

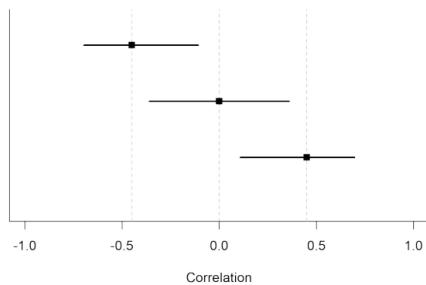
The reason that the equivalence test and SGPV no longer overlap is due to asymmetric confidence intervals. If the observed correlation falls exactly on the equivalence bound the  $p$ -value for the equivalence test is 0.5. In the equivalence test for correlations the  $p$ -value is computed based on a z-transformation which better controls error rates (Goertzen & Cribbie, 2010). This transformation is computed as follows, where  $r$  is the observed correlation and  $\rho$  is the theoretical correlation under the null:

$$z = \frac{\log(\frac{1+r}{1-r})}{2} - \frac{\log(\frac{1+\rho}{1-\rho})}{2} \sqrt{\frac{1}{n-3}}$$

Because the z-distribution is symmetric, the probability of observing the observed or more extreme z-score, assuming the equivalence bound is the true effect size, is 50%. However, because the  $r$  distribution is not symmetric, this does not mean that there is always a 50% probability of observing a correlation smaller or larger



*Figure 11.* Comparison of  $p$ -values from TOST (black line) and 1-SGPV (grey curve) across a range of observed sample correlations (x-axis) tested against equivalence bounds of  $r = -0.45$  and  $r = 0.45$  with  $n = 30$  and an alpha of 0.05.



*Figure 12.* Three 95% confidence intervals for observed effect sizes of  $r = -0.45$ ,  $r = 0$ , and  $r = 0.45$  for  $n = 30$ . Only the confidence interval for  $r = 0$  is symmetric.

than the true correlation. As can be seen in Figure 12, the proportion of the confidence interval that overlaps with the equivalence range is larger than 50% when the observed correlations are  $r = -.45$  and  $r = .45$ , meaning that the two second generation  $p$ -values associated with these correlations are larger than 50%. Because the confidence intervals are asymmetric around the observed effect size of 0.45 (ranging from 0.11 to 0.70) according to Blume et al. (2018) 58.11% of the data-supported hypotheses are null hypotheses, and therefore 58.11% of the data-supported hypotheses are compatible with the null premise.

The further away from 0, the larger the SGPV when the observed mean falls on the equivalence bound. The

SGPV is the proportion of values in a 95% confidence interval that overlap with the equivalence range, but not the probability that these values will be observed. In the most extreme case (i.e., a sample size of 4, and equivalence bounds set to  $r = -0.99$  and  $0.99$ , with a true correlation of  $0.99$ ) 97.60% of the confidence interval overlaps with the equivalence range, even though in the long run only 36% of the correlations observed in the future will fall in this range.

It should be noted that in larger sample sizes the SGPV is closer to 0.5 whenever the observed correlation falls on the equivalence bound, but this extreme example nevertheless clearly illustrates the difference between question the SGPV answers, and the question a  $p$ -value answers. The conclusion of this section on asymmetric confidence intervals is that a SGPV of 1 or 0 can still be interpreted as a  $p < 0.025$  or  $p > 0.975$  in an equivalence test, since the SGPV and  $p$ -value for the TOST procedure are always directly related at the values  $p = 0.025$  and  $p = 0.975$ . Although Blume et al. (2018) state that “the degree of overlap conveys how compatible the data are with the null premise” this definition of what the SGPV provides does not hold for asymmetric confidence intervals. Although a SGPV of 1 or 0 can be directly interpreted, a SGPV between 0 and 1 is not interpretable as “compatibility with the null hypothesis” under the assumption of a bivariate normal distribution, and the generalizability of this statement needs to be examined beyond normal bivariate distributions. Indeed, Blume and colleagues write in the supplemental material that “The magnitude of an inconclusive second-generation  $p$ -value can vary slightly when the effect size scale is transformed. However definitive findings, i.e. a  $p$ -value of 0 or 1 are *not* affected by the scale changes.”

#### What are the Relative Strengths and Weaknesses of Equivalence Testing and the SGPV?

When introducing a new statistical method, it is important to compare it to existing approaches and specify its relative strengths and weaknesses. Here, we aimed to compare the SGPV against equivalence tests based on the TOST procedure. First of all, even though a SGPV of 1 or 0 has a clear interpretation (we can reject effects outside or inside the equivalence range), intermediate values are not as easy to interpret (especially for effects that have asymmetric confidence intervals). In one sense, they are what they are (the proportion of overlap), but it can be unclear what this number tells us about the data we have collected. This is not too problematic, since the main use of the SGPV (e.g., in all examples provided by Blume and colleagues) seems to be to examine whether the SGPV is 0, 1, or inconclusive.

As already mentioned, this interpretation of a SGpv is very similar to the Neyman-Pearson interpretation of an equivalence test and a minimum effect tests (which are complementary). The difference is that where a SGpv of 1 can be interpreted as  $p < .025$ , equivalence tests provide exact  $p$ -values, and they continue to differentiate between for example  $p = 0.024$  and  $p = 0.002$ . Whether this is desirable depends on the perspective that is used. From a Neyman-Pearson perspective on statistical inferences the main conclusion is based on whether or not  $p < \alpha$ , and thus an equivalence test and SGpv can be performed by simply checking whether the confidence interval falls within the equivalence range, just as a null-hypothesis test can be performed by checking whether the confidence interval contains zero or not. At the same time, it is recommended to report exact  $p$ -values (American Psychological Association, 2010), and exact  $p$ -values might provide information of interest to readers about how precisely how surprising the data, or more extreme data, is under the null model. Some researchers might be interested in combining an equivalence test with a null-hypothesis significance test. This allows a researcher to ask whether there is an effect that is statistically different from zero, and whether effect sizes that are considered meaningful can be rejected. Equivalence tests combined with null-hypothesis tests classify results into four possible categories, and for example allow researchers to conclude an effect is significant *and* equivalent (i.e., statistically different from zero, but also too small to be considered meaningful; see Lakens et al., 2018).

An important issue when calculating the SGpv is its reliance on the “small sample correction”, where the SGpv is set to 0.5 whenever the ratio of the confidence interval width to the equivalence range exceeds 2:1 and the CI overlaps with the upper and lower bounds. This exception to the normal calculation of the SGpv is introduced to prevent misleading values. Without this correction it is possible that a confidence interval is extremely wide, and an equivalence range is extremely narrow, which without the correction would lead to a very low value for the SGpv. Blume et al. (2018) suggest that under such a scenario “the data favor alternative hypotheses”, even when a better interpretation would be that there is not enough data to accurately estimate the true effect compared to the width of the equivalence range. Although it is necessary to set the SGpv to 0.5 whenever the ratio of the confidence interval width to the equivalence range exceeds 2:1, it leads to a range of situations where the SGpv is set to 0.5, while the  $p$ -value from the TOST procedure continues to differentiate (see for example Figure 6). An important benefit of equivalence tests is that it does not need

such a correction to prevent misleading results.

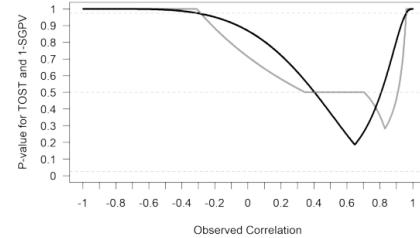


Figure 13. Comparison of  $p$ -values from TOST (black line) and 1-SGPV (grey curve) across a range of observed sample correlations (x-axis) tested against equivalence bounds of  $r = 0.4$  and  $r = 0.8$  with  $n = 10$  and an alpha of 0.05.

As a more extreme example of the peculiar behavior of the “small sample correction” as currently implemented in the calculation of the SGpv, see Figure 13. In this figure observed correlations (from a sample size of 10) from -.99 to .99 are tested against an equivalence range from  $r = 0.4$  to  $r = 0.8$ . We can see the SGpv has a peculiar shape because it is set to 0.5 for certain observed correlations, even though there is no risk of a “misleading” SGpv in this range. This example suggests that the current implementation of the “small sample correction” could be improved. If, on the other hand, the SGpv is mainly meant to be interpreted when it is 0 or 1, it might be preferable to simply never apply the “small sample correction”.

Blume et al. (2018) claim that when using the SGpv “Adjustments for multiple comparisons are obviated” (p. 15). However, this is not correct. Given the direct relationship between TOST and SGpv highlighted in this manuscript (where a TOST  $p = 0.025$  equals SGpv = 1, as long as the SGpv is calculated based on confidence intervals, and assuming data are sampled from a continuous bivariate normal distribution), not correcting for multiple comparisons will inflate the probability of concluding the absence of a meaningful effect based on the SGpv in exactly the same way as it will for equivalence tests. Whenever statistical tests are interpreted as support for a hypothesis (e.g., SGpv = 0 or SGpv = 1), it is possible to do so erroneously, and if researchers want to control error rates, they need to correct for multiple comparisons.

## Conclusion

We believe that our explanation of the similarities between the TOST procedure and the SGpv provides context to interpret the contribution of second generation *p*-values to the statistical toolbox. The novelty of the SGpv can be limited when confidence intervals are asymmetrical or wider than the equivalence range. There are strong similarities with *p*-values from the TOST procedure, and in all situations where the statistics yield different results, the behavior of the *p*-value from the TOST procedure is more consistent. We hope this overview of the relationship between the SGpv and equivalence tests will help researchers to make an informed decision about which statistical approach provides the best answer to their question. Our comparisons show that when proposing alternatives to null-hypothesis tests, it is important to compare new proposals to already existing procedures. We believe equivalence tests achieve the goals of the second generation *p*-value while allowing users to easily control error rates, and while yielding more consistent statistical outcomes.

## Authors Note

All code associated with this article, including the reproducible manuscript, is available from [https://github.com/Lakens/TOST\\_vs\\_SGPV](https://github.com/Lakens/TOST_vs_SGPV) and <https://osf.io/8crkg/>. The preprint can be found at <https://psyarxiv.com/7k6ay/>.

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## Conflict of Interest and Funding

No conflict of interest and no external funding. This work was supported by the Netherlands Organization for Scientific Research (NWO) VIDI grant 452-17-013.

## Author Contributions

DL conceptualized the idea, both authors wrote and revised this manuscript.

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# Conclusion

The conclusion section should specify the key findings of your study, explain their wider significance in the context of the research field and explain how you have filled the knowledge gap that you have identified in the introduction. This is your chance to present to your reader the major take-home messages of your dissertation research. It should be similar in content to the last sentence of your summary abstract. It should not be a repetition of the first paragraph of the discussion. They can be distinguished in their connection to broader issues. The first paragraph of the discussion will tend to focus on the direct scientific implications of your work (i.e. basic science, fundamental knowledge) while the conclusion will tend to focus more on the implications of the results for society, conservation, etc.

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Ces derniers semblent assez peu utilisés par les chercheurs. En tout cas s'ils les utilisent, ils les citent très peu dans leurs références pour justifier leurs choix (cf. article de Mills, Abdulla, & Cribbie (2010): le mode du nombre de citation méthodo dans les articles de recherche appliqués est 0, et la médiane vaut 1...). Dans l'autre sens, on constate que les articles méthodos sont généralement peu cités, et ils le sont encore 3 fois moins par les chercheurs appliqués que par les autres méthodologistes (Mills, Abdulla, & Cribbie, 2010, p. 56).

On est en droit de questionner l'impact réel des publications méthodologiques, pour 2 raisons, d'après Mills, Abdulla, & Cribbie (2010):

- (1) Les chercheurs appliqués sont noyés sous les articles dans leur domaine d'expertise si bien que cela limite le temps dont ils disposent pour se consacrer aux articles méthodologiques.
- (2) malgré que des nouvelles méthodes sont disponibles, les chercheurs continuent à opter pour des tests traditionnels et familiaux (mais souvent inappropriés).

-> Qu'est-ce qui va pousser les chercheurs à lire des articles méthodos? -> Si je trouve la réponse à ceci, j'ai mon intro.

## **Importance des simulations et des logiciels modernes pour enseigner les statistiques fréquentistes**

On sait que les chercheurs tendent à privilégier les méthodes qui sont proposées par défaut dans des logiciels de clique bouton (comme SPSS). C'est en tout cas ce que dit Counsell & Harlow (2017) dans le contexte de la gestion des données manquantes (mais je crois que c'est vrai pour tout). Une manière d'améliorer les pratiques serait d'améliorer les options proposées par défaut dans les logiciels de clic-bouton. C'est à ce genre de choses que j'aspire à travers mes articles.

Malgré tout, un logiciel ne fait pas tout et après avoir utilisé le test adéquat, il est important d'être capable de l'interpréter correctement. Les tests font appel à des notions faussement simples telles que les p-valeurs et les distributions d'échantillonnage. A mon sens, le seul moyen d'enseigner correctement ces notions, c'est à travers des simulations.

D'après Thompson (1999a, cité par Fraas & Newman (2000)), les chercheurs continuent à utiliser la nil nul hypothesis pour 2 raisons: (1) la plupart des logiciels partent du postulat que c'est l'hypothèse nulle qu'utilisent les chercheurs et ne donnent pas la possibilité de faire autre chose (2) les non nil-nul hypotheses incluent un niveau de complexité pas toujours possible à atteindre dans bcp de designs. Fraas et Newman (2001) admettent que les chercheurs sont probablement plus enclins à utiliser des procédures si elles sont implémentées dans des logiciels “user friendly”.

-> Concernant la raison (1), ce n'est plus tellement vrai en 2021. Jamovi, par exemple, contient un package “TOSTER” qui permet de faire des tests d'effets minimaux ET des tests d'équivalence. Il est très important que des logiciels le fassent, car comme disaient Fraas & Newman (2000), “unless researchers are able to test non-nil null hypotheses with readily available computer software, they may continue to exclusively use nil null hypothesis” (p.4).

## **Comment écrire/transmettre l'info aux psy's**

Un consultant doit pouvoir parler de langage des psy's, c'est-à-dire décrire et expliquer les méthodes requises d'une manière compréhensible pour les clients (Golinski & Cribbie, 2009). Est-ce bien de demander à des mathématiciens/Statisticiens d'enseigner les stat aux psy's? Par forcément, car un psychologue spécialisé en méthodo quanti sera plus à même de comprendre les procédures et méthodes requises par les psy's (ex. de la question de la taille d'effet qui

n'intéresse pas vraiment les statisticiens; Golinski & Cribbie (2009)).

## Recommandations générales

Mills, Abdulla, & Cribbie (2010):

- au moins un reviewer compétant pour analyser le caractère approprié des méthodes stat
  - que les éditeurs/reviewers encouragent l'usage d'article de méthodo dans leur recherche -> interesting, mais réaliste? La proportion de méthodologistes parmis les psychologues n'est pas assez élevée... Ou alors il faut vraiment de l'interdisciplinarité!
- 2) Par manque de connaissances, les chercheurs se contentent souvent des informations fournies dans les logiciels clic/bouton. *for example, if software does not report a CI on Cohen's d, it is unlikely that a researcher will calculate one his or herself* (Counsell & Harlow (2017)). *Une chance qu'on a, c'est Jamovi* (regarder si Jamovi me cite)

Anecdote, pour quand je parlerai des logiciels et de leur intérêt: les chercheurs font souvent l'erreur de croire qu'il faut vérifier la normalité de la VD en faisant une régression. Dans SPSS, il est assez complexe de le faire car il faut d'abord calculer les résidus, ce qui implique de comprendre que les tests t et ANOVA sont des cas particuliers de régression, puis ensuite a posteriori représenter graphiquement les résidus. C'est chronophage et complexe. Dans Jamovi, par contre, la vérification de la normalité des résidus est automatiquement réalisée lorsqu'on fait un test t. Le rôle des méthodologistes, à mon sens, est de préparer le travail, pour permettre à d'autres de créer des outils conçus pour améliorer les pratiques de recherche. à partir du moment où c'est automatiquement fait correctement, il devient moins problématique que les psychologues maîtrisent le détail. Débarassés de ces questions, ils pourront peut-être alors plus se focaliser sur l'important pour mieux comprendre et interpréter les résultats de leur tests: càd comprendre la distribution d'échantillonnage, dont pratiquement tt découle.

## Limites

**pour le chapitre 4** Dans le chapitre 4, on compare essentiellement les estimateurs sur base de leurs propriétés inférentielles. Nous avons tenté de prendre la dimension interprétative en compte, mais c'est parfois très compliqué. Cette dimension est d'ailleurs rarement prises en

compte par les chercheurs. On constate que même si les mesures de taille d'effet sont de plus en plus fréquemment reportées, elles ne sont que rarement interprétées et incluses dans les discussions (Funder & Ozer, 2019 ; Thompson & Snyder, 1997) par les chercheurs. Dans un tel contexte, il est particulièrement important d'ouvrir les débats sur cette question.

**pour le chapitre 5** Déterminer un effet pratiquement significatif reste super compliqué. C'est un fait admis même par les chercheurs qui prônent ces méthodes. "With respect to determining the practical significance of results, Cohen's definitions of small, medium , and large effects represent a good beginning. However, much more systematic research is needed to extend his work... If practical significance is to be a useful concept, its determination must not be ritualized" (Fraas & Newman, 2000). Note: je pense que je pourrais parler là des bornes plus normatives que celles de Cohen (cf document word).

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# Annexe(s)

## Annexe A: erratum

### Why psychologists Should by Default Use Welch's *t*-test Instead of Student's *t*-test

#### Erreurs conceptuelles

- Erreur (p. [99]): "As it is explained in the additional file, Yuen's *t*-test is not a better test than Welch's *t*-test, since it often suffers high departure from the alpha risk of 5 percent".

Explication: d'un point de vue purement statistique, à travers le test de Yuen, on ne compare plus les moyennes de chaque groupe, mais les moyennes *trimmées* (soit les moyennes calculées sur les données après avoir écarté les 20% des scores les plus bas ainsi que les 20% des scores les plus élevés). Or, à travers nos simulations, les scénarios créés en vue de tester le taux d'erreur de type I (risque alpha) étaient systématiquement des scénarios dans lesquels les moyennes de chaque groupe étaient identiques. Lorsque les distributions sont symétriques, les moyennes de populations sont identiques aux moyennes trimmées des populations. Quand les distributions sont asymétriques, par contre, la moyenne trimmée sera plus proche du mode de la distribution que la moyenne. Cela explique pourquoi nous avions noté ceci: "Yuen's *t*-test is not a good unconditional alternative because we observe an unacceptable departure from the nominal alpha risk of 5 percent for several shapes of distributions [...] particularly when we are studying asymmetric distributions of unequal shapes."

A partir du moment où cela ne correspond *pas* à l'hypothèse nulle du test de Yuen, il est erroné de conclure à une inflation du risque alpha pour ce test sur base de nos scénarios. \*D'un point de vue méthodologique, par contre, nous pensons qu'il n'est pas clair pour beaucoup de gens que l'hypothèse nulle du test de Yuen n'est pas la même que celle du test *t* de Student, ce qui pourrait être source d'ambiguïté. En conclusion, ce test ne devrait être utilisé que par des personnes ayant pleinement conscience du fait que le test de Student et celui de Yuen ne testent pas la même hypothèse. \*\*Relire l'article de Wilcox et celui de Erceg-Hurn pour voir si c'est vraiment bien clair dedans, histoire de me défendre un peu\*

When both the normality and equal variances assumptions are violated, we can use a combination of the Trimmed Means *t*-Test and Welch's *t*-Test, called the Yuen-Welch Test.

Using the notation for the Trimmed Means t-test, the Yuen-Welch Test is

**Mise en forme et Notations** En termes de mises en forme, nous avons omis à plusieurs endroit d'italiser les notations mathématiques. Par exemple, à la page [93], nous avons indiqué "F-ratio test" au lieu de "F-ratio test" à plusieurs reprises. A la même page, nous avons également noté " $x_{ij}$ " au lieu de " $x_{ij}$ ", et  $|x_{ij} - \hat{\theta}_j|$  au lieu de  $|x_{ij} - \hat{\theta}_j|$ .

En termes de notation, nous avons relevé des inconsistances dans la notation de la variance de chaque groupes. Par exemple, dans l'équation 1, nous utilisons  $S_j^2$ , alors que nous utilisons  $s_j^2$  dans l'équation 4 ou encore SDj lorsque nous définission la statistique du F-ratio à la page [93].

### Taking parametric assumptions very seriously

#### Erreurs conceptuelles

**Mise en forme et Notations** 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. S'assurer qu'une lettre apparaissent en majuscule, via latex, implique de l'entourer des symboles {}, ce qui n'a pas été fait. Par exemple, dans le titre de l'article de Tiku(1971), il aurait fallu indiquer "Power function of the {F}-test.." au lieu de "Power function of the F-test...". Cela ne serait pas arrivé, si j'avais utilisé un outil comme Zotero, afin d'exporter directement un fichier au format Bibtex (puisque via ces outils, ce genre de détail est automatiquement inclu), mais je n'ai découvert cette possibilité que très récemment.

#### Effect sizes

#### Equivalence tests

## **Annexe B**

Insert code (if any) used during your dissertation work here.