The *t*-test, resource pooling, and psi: why the minimum sample size counts towards theoretical progress in behavioral science

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Abstract: A theoretical construct that subsumes an empirical phenomenon should rest on statistically significant test-results with high replication probably. To statistically establish such test-results, behavioral science publications typically rely on a *t*-test, and researchers typically operate under limited data collection resources. To publish more test-results, or to publish an individual result sooner, questionable strategies are commonly used to reduce data collection cost. One strategy is to increase the β -error rate from 0.05 to 0.20; a second strategy is to treat the control group as a constant, collapsing a two sample *t*-test into a one sample *t*-test. Both strategies happen to underlie Bem's (2011) rightly controversial results on human precognition ("psi hypothesis" [*Journal of Personality and Social Psychology*, 100, 3, 407-425]). Since both strategies undermine theoretical research in behavioral science, their ubiquity partially explains the rarity of well-corroborated theoretical constructs there. We advocate collaboration between individual labs as a viable route to enabling theoretical research by collecting a large enough sample jointly.

Keywords: Fisher; Neyman-Pearson; psi research; power; *t*-test, theory construction

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1. Introduction

In behavioral science, the probably most frequently used statistical test to compare the independent means in a controlled experimental study is the t -test. To reduce the minimum sample size (N_{MIN}) for a t -test, two strategies are commonly applied to save data collection cost. The first strategy lowers test-power from $(1-\beta)=0.95$ to $(1-\beta)=0.80$, lowering the probability that the original test-result is independently replicated. The second strategy collapses a Neyman Pearson-test into a Fisher-test, making test-results useless to develop an empirically adequate theoretical construct that is more informative than a non-random effect. Both strategies happen to underlie Bem's (2011) rightly controversial and misleading results on human precognition ("psi hypothesis").

Researchers seemingly deploy both strategies to publish test-results although a study's actual sample (N) fails to meet N_{MIN} , itself an element of Neyman-Pearson test-theory (NPTT) (Neyman & Pearson, 1967). We argue that, in order to develop a theoretical construct, a theoretician needs well-powered test-results, $(1-\beta)\geq 0.95$. Conversely, spending less than what NPTT identifies as *necessary* data collection cost is guaranteed to undermine theoretical efforts. This claim extends beyond a t -test, to NPTT-based tests that compare multiple means, variances, correlations, or frequencies (Cohen, 1977; 1988).

That both strategies are normal in behavioral science today goes along with research here focusing not on the development of theoretical constructs, but on the discovery of non-random test-results in *individual* studies. This focus partially explains the replication crisis in behavioral science, whose root cause is the inability to independently repeat a non-random test-result (Pashler & Wagenmakers, 2012; Klein et al., 2018; Camerer et al., 2018). To support the development of informative theoretical constructs (more precise than a two-sided non-random hypothesis or even a one-sided directional non-random hypothesis), we advocate that behavioral scientists should bear the cost of data collection *collectively*.

2. Induction vs. corroboration

With rare exception, behavioral science today regularly operates with *small* samples. In psychology, for instance, the typical sample size is $N < 30$ (Bakker, van Dijk & Wicherts, 2012). But a “small N -study” can discover a statistically significant effect, only if the empirically observed effect is large. Yet, behavioral science studies typically find small to medium size empirical effects, the actual N regularly being much smaller than N_{MIN} . This implies that published statistically significant test-results tend to be *underpowered*, leaving their independent replication improbable.

A statistically significant but underpowered test-result generally is a poor instance of an effect that a theoretician would seek to subsume under a theoretical construct. If a good theory predicts a true effect, then a statistically significant but underpowered effect is what a theoretician should *ignore*. Conversely, only well-powered statistically significant test-result warrant the effort of developing a theoretical construct (Witte & Zenker, 2018). Therefore, if $N < N_{\text{MIN}}$, then researchers avoid what from a theoretical point of view are *necessary* data collection cost.

To fully appreciate this point, it helps to contrast *induction* with *deduction* as two central modes of statistical reasoning. Researchers invariably collect data to induce an empirically observed effect size, often estimating it as Cohen’s d , the difference between the means in experimental and control group, divided by the standard deviation, $d = (m_1 - m_2) / s$, where $0 \leq |d| \leq \infty$.¹ By convention, $d = 0.01$ is considered a *very small* effect, $d = 0.20$ is *small*, $d = 0.50$ *medium*, $d = 0.80$ *large*, $d = 1.20$ *very large*; and some call $d = 2.0$ *huge*. Crucially, inducing d in ways a theoretician can take seriously requires that empirical data satisfy two criteria:

(C1) *Statistical significance criterion*

¹ Preacher and Kelley (2012) report that d is probably the most widely used effect size measure today, while Schaefer et al. (2019) report that it probably is r . Another common measure is Hedges’ g . Regardless which measure is most common, one can safely transform d into r or g , if experimental and control groups have similar base rates, because this maximizes a binary variable’s variance (McGrath & Meyer, 2006). Compared to d , only extremely diverging base rates (e.g., 0.95 vs. 0.05) would reduce r artificially. For other transformations, see Schulze (2004, 30ff).

On the assumption that the (random) H_0 , itself postulating $d=0$, is the best-fitting description of data, D , the observed empirical effect, $d=x$, is sufficiently improbable (p), where the statistical significance threshold is conventionally set to 0.05. In form: $p(D, H_{0(d=0)}) < 0.05$.

(C2) *Replicability criterion*

Assuming that $H_{1(d=x)}$ is the true value, replicating the point-specified effect $d=x$ in *new* data is sufficiently probable, i.e., $p(D, H_1) > (1-\beta)$, where the conventional replication threshold is set to $(1-\beta)=0.80$.

Since ' $p(D, H_x)$ ' denotes neither a conditional nor a joint probability, it conveys a meaning that is distinct from $p(D|H_x)$ or $p(D \& H_x)$. If d is transformed into a statistic, e.g., a t -value, then $p(D, H_x)$ denotes the probability of observing this statistic, or more extreme values of it. As a natural language expression for $p(D, H_x)$, this suggests: 'the probability of D *in view of* H_x '.

Fisher's (1956) approach to statistical inference had merely introduce the p -value, which suffices to estimate $p(D, H_0)$. The p -value thus succeeds in addressing C1. But Fisher's approach ignores H_1 , and so fails to address C2. Fisher's approach therefore offers neither an *effect size measure*, nor a *replication probability measure* (Mayo, 1996).

These two measures were only introduced in NPTT, which estimates $p(D, H_0)$ and $p(D, H_1)$ by drawing on:

- (i) the α -error rate (aka p -value): the probability of mistakenly accepting that observed data deviate significantly from H_0 (*false positive error rate*);
- (ii) the β -error rate: the probability of mistakenly accepting that observed data deviate significantly from H_1 in the direction of H_0 (*false negative error rate*); respectively the derived magnitude $(1-\beta)$ -error, i.e., the probability that data replicate if H_1 is true (aka. *test-power*);
- (iii) the *effect size*, d : the theoretically predicted or the empirically observed (non-random) causal effect, or correlation, between independent and dependent variables;
- (iv) the *minimum sample size*, N_{MIN} , given both d and the α - and β -error rates.

While origins, uses, and interpretations of the p -value and the α -error rate differ, and although both concepts are never equivalent, the p -value nevertheless relates to the α -error rate. In fact, both concepts can be combined to arrive at a binary decision. Originating in NPTT, the α -error rate represents the *theoretical* long-run probability of committing a Type I-error (false positive error rate). The α -error rate thus serves as a decision criterion when choosing between the two sides of a simple hypothesis. NPTT requires two such hypotheses, at least one of which, the H_1 , differs from a data-description because, in NPTT, the H_1 derives from a theory.

Originating in Fisher's approach, by contrast, the p -value represents the *empirical* probability of observing data at least as extreme as data in fact sampled, if H_0 is true. The p -value thus describes an empirical test-result in view of a single hypothesis, typically a random version of the H_0 . Since Fisher's approach offers merely H_0 and the p -value, but no H_1 -hypothesis, his approach can only measure whether data deviate from a random distribution (H_0). The p -value thus is useful to decide *that* something non-random has occurred, yet without implying a substantial hypothesis as to *what* has occurred. Given these constraints, the commonly used t -test with two error-probabilities (α , β) is therefore perfectly unintended in Fisher statistics; it becomes possible only under the constraints of NPTT.

A researcher seeking to decide whether, given data, one should pursue the H_0 or the H_1 would consider the probability of a mistaken decision. The p -value bases this probability on data (*objective*), while the α -error-rate bases that probability on the researcher's personal expected error-rate (*subjective*). Since the decision *should* be based on data, a researcher would typically set the expected personal error-rate no higher than the p -value serving as the statistical significance criterion. In practice, therefore, the differences between both concepts are effectively "hidden."

Thus, although the p -value and the α -error rate are non-equivalent concepts, one can meaningfully treat the empirically observed p -value *as if* it were the theoretical α -error, and thus arrive at an empirically-based binary decision whether to accept or reject a hypothesis. Moreover, since NPTT has only three degrees of freedom, fixing any three of α , β , d and N_{MIN} lets the fourth magnitude "fall out." Hence, given specified α - and β -error rates, as well as a point-specific value for d motivated by theoretical reasoning, NPTT deduces N_{MIN} . This feature recommends NPTT as a planning tool to estimate N_{MIN} , and thus determine *necessary* data

collection cost, using an *a priori test-power* analysis (Cohen, 1977; 1988²) as follows:

$$N_{\text{MIN}}=2[z_{(1-\alpha)} + z_{(1-\beta)}]^2 / d^2 \quad (1)$$

Here, z is the value of d for which the area under a standard normal distribution has a probability density that corresponds to the α - and β -error rates. Given $\alpha=\beta=0.05$ and $d=0.50$, for instance, $N_{\text{MIN}}=2[1.65+1.65]^2/0.50^2=87.12$, or approximately $N_{\text{MIN}}=88$.

Interpreted as normative magnitudes, the α - and β -error rates state error-rates that researchers have determined by convention. The effect size, by contrast, can be determined in but one of three ways: (i) again by convention (e.g., following Cohen, as *very small*, *small*, *medium*, or *large*), (ii) by induction, as an *observed d-value*, or (iii) by deduction based on theoretical considerations, as a *theoretical d-value*. Thus, if data collection cost are calculated *before* data collection starts, then N_{MIN} is the only material resource a researcher has to determine.

Since the induction vs. deduction-distinction reflects the basic purpose of research, it also constrains the shape of the appropriate formal support-measure. It is by induction that the probability measure $p(D, H_0)$ estimates the probability of an observed test-result in view of the data-distribution postulated by H_0 . Here, ‘D’ denotes an *interval* comprising empirically observed data, as well as more extreme possible data. To corroborate a hypothesis, by contrast, is to measure the extent to which D—now interpreted as theory-deduced *point-statistic*—supports H_1 or H_0 . As a distinct measure, corroboration requires the *likelihood ratio* (LR):

$L(H_1|D)/L(H_0|D)=p(H_1) \times p(D, H_1)/p(H_0) \times p(D, H_0)$, a likelihood being a probability multiplied by a positive constant (Edwards, Lindman & Savage, 1963; Edwards, 1972). (Decisive reason against using the Bayes factor as a corroboration measure are given in Witte and Zenker (2017b), and Krefeld-Schwalb, Zenker, and Witte (2018).)

Unlike in the inductive case, ‘D’ now denotes a point-specified observation, for instance the point-difference between the expected mean effect in experimental and control group. In view of criteria C1 and C2, then, only data that are statistically significant and well-powered *can* offer sufficient empirical support to corroborate a

theoretical hypothesis. In behavioral science, however, researchers show little interest in inducing effect sizes that enable a statistical hypothesis corroboration. In fact, a recent survey of some 200 meta-analyses based on almost 8,000 publications in psychology journals reports median test-power of $(1-\beta)=0.36$ (Stanley, Carter & Doucouliagos, 2018). This analysis also notes that only some 8% (!) of studies here achieve $(1-\beta)=0.80$. Bakker et al. (2012) report similar results.

The main telos of research thus appears to be the publication of statistically significant test-results under *minimal* data collection cost, results that address C1 but fail to address C2. That this must play out negatively for theory construction is what we now show in the context of a *t*-test.

3. The *t*-test

To plan a proper control group study, where $N \geq N_{\text{MIN}}$, one must stipulate not only α , β , and d , but also two data distributions. For a small sample ($N < 30$) and an estimated standard deviation, the distributions are known as *t*-distributions (Fig. 1). Although a *t*-distribution can only approximate observed data, statistical inferences are possible by acting *as if* observed data mirrored a normally distributed variable with a corresponding small sample standard deviation, s . For example, we may hypothesize $d_{H0}=0$ as the mean value in the control group, and $d_{H1}=0.50$ in the experimental group. When transforming d_{H1} into the *t*-distribution's non-centrality parameter $\delta = d \times \sqrt{(N/2)}$, and provided the sample size is the same in both groups ($N_1=N_2$), we find that δ depends only on N .² Given $N_1=N_2=88$, it thus follows that $d_{H0}=0$ yields $\delta = d \times \sqrt{(N/2)}=0$, whereas the largest probability density for d_{H1} is found at the point $\delta=3.32$ (Fig. 1.) So, beyond the ordinate $t=1.65$ (aka 'critical *t*'), the probability density for H_1 exceeds that for H_0 . For any observed data point exceeding critical *t*, therefore, the probability of *falsely* rejecting H_0 is smaller than $\alpha=0.05$.

² If $N_1 \neq N_2$, a useful sample must be neither too small, nor too large. Compared to a *t*-test where $N_1=N_2$, the harmonic, geometric, and arithmetic mean deliver an ever-increasing value, and thus estimate the actual sample size ever less conservatively.

Fig. 1 One-sided, two sample t -test for $d=0.50$ given $N_1=N_2=88$ and $\alpha=\beta=0.05$. The hypothetical H_0 -distribution (solid line) is centered on $\delta=0$; the observed H_1 -distribution (dashed line) is centered on $\delta=3.32$. The abscissa (x -axis) states the t -value given N ; the ordinate (y -axis) states the probability density for a given t -value. Up to critical $t=1.65366$, each shaded area (α, β) covers 5% of the other distribution. (Figure generated with G*Power; Faul, Erdfelder, Lang & Buchner, 2007).

To ensure that a study meets N_{MIN} , computer software readily facilitates an a priori test-power analysis (e.g., G*Power; Faul, Erdfelder, Lang & Buchner, 2007). But if median test-power is in fact no larger than about $(1-\beta)=0.36$ (Stanley, Carter & Doucouliagos, 2018), then the majority of published behavioral science studies that report statistically significant results would have determined N either at liberty—e.g., by stopping data collection once a test-result is statistically significant, which Bayarri et al. (2016) rightly call “cheating”—or would have determined N strictly according to available resources (e.g., the number of students in a class).

This suggests that the aim of publishing an empirical test-result regularly dominates the aim of securing sufficient test-power. Of course, if a researcher focuses only on the *observed* effect in the first place, but ignores the *theoretically expected* effect, then test-power cannot be computed other than *post-hoc*. Absent any theoretical considerations, however, Bakan’s (1966) well-known objection applies. Given $N \geq 60,000$, even a very small effect can become statistically significant by splitting the sample randomly. So, absent theoretical considerations, statistical significance is guaranteed to be a trivial criterion once $N=60,000$.

Irrespective of the specific effect size a statistically significant study reports, to the extent that the α - and β -error rates both *increase*, the effect size becomes ever less useful for constructing a theory that predicts a true effect. *Ceteris paribus*, if $H_{1(d=x)}$ were true, then as the β -error-rate increases, the probability that a statistically significant effect replicates decreases. Conversely, *ceteris paribus*, if $\alpha=\beta$, then $d=x$ becomes more useful for the same purpose as N increases, because both error-rates decrease. (Formally, decreasing the error rates increases the non-centrality parameter δ , which reduces the t -distributions' overlap in Fig. 1.) So, if $\alpha=\beta$, then already in case a prediction postulates nothing stronger than a *directional* deviation from random, a statistically significant effect's use-value for theory construction stands or falls with the β -error-rate.

Cohen (1977; ²1988) had recommended *asymmetric* error rates ($\alpha\neq\beta$), viz. $\alpha=0.05$, $\beta=0.20$. He stated that, given $\alpha=0.05$ as the antecedently accepted error rate, “[w]hen the investigator has no other basis for setting the desired power value, the value 0.80 is used” (Cohen, 1977, 56). Rather than motivating the value of 0.80 independently, however, Cohen accepted—without further argument—that it is more important to avoid false positive test-results than to avoid false negative ones (ibid.). Despite being text-book example of circular reasoning, his recommendation became so entrenched that $\alpha=0.05$ and $\beta=0.20$ are widely accepted today as the “right” error-rates.

Asymmetric error rates imply that researchers *weigh* the risk of falsely accepting that an effect is statistically significant (α) against the risk of failing to replicate a true non-random effect (β) (Witte, 1994; Witte & Kaufman, 1997). Given $N_1=N_2=50$ ($N_{TOTAL}=100$), for instance, if $d=0.50$, then $\alpha=0.05$ one-sided yields $(1-\beta)=0.80$, whereas $\alpha=0.05$ two-sided yields $(1-\beta)=0.70$, and $\alpha=0.01$ one-sided even only yields $(1-\beta)=0.56$, i.e., close to chance level. Yet, to meaningfully develop a theoretical construct, a theoretician needs error rates of $\alpha=\beta<0.05$, or lower. What else should a theoretical construct subsume if not the *true* effect?

Worse yet, researchers in fact pay lip service to Cohen's recommendation, while undermining it regularly. As publishing such test-results renders them of very limited use for theory development, we proceed to critique two common strategies meant to reduce necessary data collection cost. That both strategies happen to

underlie Bem’s (2011) results on human precognition (aka “psi-effect”) entails strong reasons to doubt these results.

4. Two cost reduction strategies

4.1 From $N_{MIN}=176$ to $N_{MIN}=27$

Since the behavioral science literature typically reports small to medium sized effects (Cafri, Kromrey, & Brannick, 2010), and given theoretically motivated error-rates of $\alpha=\beta<0.05$, collecting all of N_{MIN} can quickly generate massive data collection cost, as shown in Table 1 for the significance level $\alpha=0.05$.

Table 1: The estimated minimum sample size (N_{MIN}) for a two sample t -test as a function of test-power ($1-\beta$) and effect size, d , given $\alpha=0.05$

$(1-\beta)$	d			
	0.01	0.20	0.50	0.80
	<i>very small</i>	<i>small</i>	<i>medium</i>	<i>large</i>
0.40	38726	97	15	6
0.50	54111	135	22	8
0.80	123651	309	49	19
0.95	216443	541	88	34

Necessary data collection cost thus are what many researchers today appear to shun. For test-results in empirical psychology, for instance, the largest behavioral science field, meta-analyses have consistently estimated median test-power as $0.35 \leq (1-\beta) \leq 0.50$ (Cohen, 1962; Bakker, van Dijk & Wicherts, 2012; Stanley et al., 2018). Given publication bias, moreover, the behavioral science literature typically underrepresents the actual proportion of statistically non-significant results (Cafri, Kromrey, & Brannick, 2010; Francis, 2012; Sedlmeier & Gigerenzer, 1989). Although preregistration can eliminate this bias, even the recent well-powered preregistered replication studies could replicate the original effect size in only 25% of all cases (Klein et al., 2018; Schäfer & Schwartz, 2019).

To understand how underpowered test-results arise, let us critically consider two strategies that seemingly help researchers “save” up to 85% of necessary data collection cost. We already saw that a one-sided two sample t -test for a medium

effect ($d=0.50$) under $\alpha=\beta=0.05$ requires $N_{\text{MIN}}=N_1=N_2=88$ ($N_{\text{TOTAL}}=176$). The *first strategy* maintains $\alpha=0.05$ but increases $\beta=0.05$ —the β -error value that theoretically considerations warrant—by a factor of 4, to $\beta=0.20$. This reduces N_{MIN} by some 42% to $N_1=N_2=51$ ($102\approx 0.58\times 176$). Thus, rather than obtaining only one in twenty *false positive* statistically significant test-results that probably fail to replicate, one obtains one in five such results.

Underlying the *second strategy* is the assumption that random influences among data cancel out mutually. A given causal influence or correlation of size x in one direction would thus be matched by a corresponding influence of size x in the *opposite* direction. This assumption underlies Bem's (2011) psi-effect research (aka precognition), allowing Bem to treat the hypothesis his experiment shall falsify, the H_0 , as postulating the *constant* zero, or when expressed as a binary variable as postulating a 50% chance of a fair coin landing heads up. Experimental participants' average empirical reactions that *fail* to display a psi-effect are thus assumed to describe a random distribution.

This assumption, or so the following quote shows, allows Bem to avoid an *actual* (empirical) control group, and thus allows him to deploy a *one* sample *t*-test against a theoretically postulated constant random effect in an *imaginary* control group (Bem et al., 2011; Bem, Tressoldi, Rabeyron, & Duggan, 2016).

“I set 100 as the minimum number [aka N_{MIN}] of participants/sessions for each of the experiments reported in this article because most effect sizes (d_{emp}) reported in the psi literature range between [$d_{\text{emp}}=$]0.20 and 0.30. If [one sets the theoretical effect size to the point-value] $d_{\text{theo}}=0.25$ and [the sample to] $N=100$, the power to detect an effect as significant at [$\alpha=$]0.05 by a one-tail, one sample *t*-test is [$(1-\beta)=$]0.80.” (Bem, 2011, 409, fn. 1, notation adapted)

As a consequence, data collection cost are massively reduced. A (one-sided) *two* sample *t*-test given $d=0.25$, $\alpha=0.05$, and $(1-\beta)=0.80$, requires $N_1+N_2=N_{\text{MIN}}=398$. Yet, given the same values of d , α , and β , a (one-sided) *one* sample *t*-test only requires the $N_{\text{MIN}}=100$ that Bem mentions. And, a *one* sample *t*-test given a medium-sized effect of $d_{\text{theo}}=0.50$, $\alpha=0.05$, and $(1-\beta)=0.80$ even only requires

$N_2=N_{\text{MIN}}=27$, close to the median sample size of published studies in psychology, $N=24$ (Wetzels et al., 2011). This makes a “median-study” with $N_{\text{MIN}}=24$ nearly equivalent to a one sample t -test given $\alpha=0.05$, $(1-\beta)=0.80$, and $d=0.50$ (Fig. 2). If data collection cost are a linear function of N_{MIN} , which often is safe to assume, then compared to $N_{\text{MIN}}=398$, this saves some 75% of cost ($398 \times 0.25 \approx 100$).

Fig. 2 One sample t -test for the difference from a constant of zero, given $\alpha=0.05$, $\beta=0.20$, $d=0.50$, $N=27$, $t=1.70562$. Despite the term ‘constant’, one must employ two distributions rather than one to calculate test-power, including a second, theoretically predicted distribution centered on a theoretically motivated constant. (Compare labels in Fig. 1.)

The successive application of both strategies—increasing the β -error-rate from 0.05 to 0.20; treating the control group as a constant—thus results in 25% of N_{MIN} . But it risks masking Bem’s own inference strategy. By citing α , β , d and N_{MIN} , after all, Bem presents the inference strategy *as if* it were based on NPTT. Empirical measurements, however, occur *only* in the experimental group. (There is no actual control group.) Bem’s point- H_0 thus effectively predicts his own empirical setting’s unknown influences. Such influences, however, cannot be measured systematically because they arise from factors *besides* the focal experimental effect. In fact, the control-group strategy was developed precisely because uncontrollable influences cannot be controlled. So, a genuine NPTT-test must use an *actual* control group.

4.2 One- vs. two sample t -tests

Unlike the theoretically expected effect size, only the empirically observed effect size can describe a *specific* experimental condition's sample size, as well as that sample's specific standard deviation. In a two sample *t*-test, as we saw, the *d*-value reports the difference between the independent mean effect sizes in the experimental group and control group, $m_1 - m_2$, divided by the standard deviation, s . Since independent samples correlate to degree zero ($r_{12}=0$), one can derive s by adding the square root of the variances from each sample: $s = s_{(x1-x2)}$, where $s^2_{(x1-x2)} = s^2_{(x1)} + s^2_{(x2)} - 2 \times r_{12} \times s_{(x1)} \times s_{(x2)}$. So, if $s^2_{(x1)} = s^2_{(x2)}$, then $d = (m_1 - m_2) / \sqrt{2}s$. By normalizing the standard deviation to $s=1$, one obtains $d = (m_1 - m_2) / \sqrt{2} = (m_1 - m_2) / 1.41$.

Compared to a two sample *t*-test, then, if only a single sample's standard deviation is used to define the effect size, then it is by the value 1.41 that the standard deviation of a one sample *t*-test necessarily *decreases*. After all, the *t*-test's one sample variant sheds m_1 , retaining only $m_2 - c$, the difference between the mean of the experimental group's data distribution and a constant random effect, c . Yet, the constant random effect c is a point-specified effect, rather than a distributed effect. To a constant effect, however, one cannot meaningfully associate a standard deviation. In $d^* = (m_2 - c) / s_{(x1-x2)}$, therefore, the independently estimated value of $s_{(x1-x2)}$ must be *lower* than in $d = (m_1 - m_2) / s_{(x1-x2)}$.

As explained above, the observed effect size is an *inductive* concept, while the theoretically expected effect size is a *deductive* concept. Test-power, however, is meaningfully associated only to the latter concept, serving to plan experiments with N_{MIN} . Based on NPTT, such planning necessitates associating two hypotheses and two error-distributions with each theoretically postulated mean. Reducing this to one error-distribution implicitly *increases* d , because the inference is now based on only one distributions, where α - and β -error are undefined (see Figs. 1 and 2). In brief, if a statistical inference shall be based on NPTT (featuring defined α - and β -errors), then the numerator will be increased, because two distributions with an estimated standard deviation, s , are needed. In all other cases, the inference simply is not "according to the art."

Specifically, d relates to the *t*-value as: $t = d / \sqrt{2} \cdot s$. A one sample *t*-test's *t*-value is therefore *more sensitive* to empirical data than a two sample *t*-test, namely by the factor 1.41, above. For instance, $d=0.50$ given two samples would increase to $d^*=0.705$ given one sample. Since test-power depends on d , it follows that, if one

holds N constant, then a one sample t -test has *greater* test-power than a two sample t -test. Put differently, if test-power is constant, then N_{MIN} decreases. This explains why a one sample t -test incurs lower data collection cost than a two sample t -test.

According to Cohen (1977), the shift from two samples to one is mandatory. The increase in effect size between d and d^* shall “compensate for [his own test-power] tables’ assumption of double error variance” (ibid., 46). Cohen motivates this assumption by having simplified $s_{(x1-x2)}$ to $s_{(x2)}$. The constant c itself, Cohen “conceive[s] as the mean of a normal distribution whose standard deviation is also s ” (ibid., 46). Thus, “values of power are sought in the power tables by means of $d^* \times \sqrt{2}$ ” (ibid., 47; notation adapted). We consider this an illegitimate transformation of a Fisher-test into an NPTT-test, which must feature *two* error-prone hypotheses.

4.3 A one sample t -test is a Fisher-test

Having assumed a medium effect ($d=0.50$), we saw how the second strategy lowers N_{MIN} for a two sample t -test by some 85% ($186 \times 0.15 = 27$), while incurring the assumption that H_0 can be modeled without an actual control group, i.e., without an *empirical* data distribution. This assumption is problematic, because if one now fails to obtain d as the difference between two means of a statistical t -distribution, and so fails to obtain a test-power value. Yet, without a test-power value, one cannot deploy a genuine NPTT t -test. One has rather altogether abandoned this statistical concept.

For both the experimental and the control group, moreover, the standard deviations are normally established *empirically*, from data. A theory, by contrast, predicts d as a point-specific difference between means. But a reasonable theoretician would not go as far as to predict the *exclusively empirical* magnitude that a point-specific standard deviation is.

Even if the standard deviation in the experimental group is identical to that in the control group, there remains a non-zero probability that the experimental group’s t -value ($H_1=d$) is an element of the t -distribution centered on $H_0=c$ (Figs. 1 and 2). Call this probability the p -value. But to properly deploy an NPTT-test, the constant c fails to represent H_0 properly. (One would additionally have to center a random distribution on c ; otherwise the β -error is undefined.) Since Bem’s t -test uses the improper representation of the H_0 that a “distributed constant” c is, his t -test treats experimental group data *as if* they originated from the *random* distribution

representing the group mean, c . The test-result, however, is meant to report the probability to which data deviate statistically significantly from the *distribution* placed around the constant c . But Bem's t -test *can* evaluate data only in view of $H_0=c$. The empirical decision criterion, therefore, is *not* the α -error-rate, as defined in NPTT, but the empirically derived p -value.

This entails that Bem deploys not an NPTT-test, but a Fisher-test. After all, the attempt to interpret a one sample t -test as an NPPT-test would incur the unacceptable assumption that the control group's mean can be specified as the theoretical point-value c , *without* any variation. But point-specification is unrealistic because any experimental condition is subject to uncontrolled empirical influences. That, indeed, is why researchers should use an actual control group, to which participants are *randomly* assigned vis-à-vis an experimental group.

As a Fisher-test, a one sample t -test requires three values: the random distribution around c , the observed mean of the experimental group, m_2 , and the estimated standard deviation placed around c , the latter obtained from the observed values of the experimental group, $s_{(x_2)}$. The standardized mean difference-measure thus is $d=m_2-c/s_{(x_2)}$. Transformed into a statistic, it becomes $t=d_{\text{emp}} \times \sqrt{N/2}$. So, if $d^*=0.50$, then given $N=27$, one finds $t=1.84$. Under a random distribution, therefore, if $t=1.84$, then a mere 3% of all t -values are more extreme than the empirically observed t -value. But nowhere is there a H_1 -hypothesis, and thus no test-power. All Bem's t -test allows to obtain is the p -value that, given a random distribution, potential empirical test-results are the same, or yet more extreme than, an observed result. This is all that a one sample t -test reports.

Starting with an NPTT-test under conventionally accepted error rates, but then to reducing N_{MIN} to 15% while continuing to present it as a NPTT t -test under constant error rates, appears highly questionable. Given $N_1=N_2=27$ ($N_{\text{TOTAL}}=54$), $\alpha=0.05$, and $d=0.50$, even a proper two (independent) sample t -test would at most have test-power of $(1-\beta)=0.57$. Close to chance, this nearly implies speculative decision-making. At any rate, already Cohen's own (lenient) standard $(1-\beta)=0.80$ entails $N_{\text{MIN}}=N_1=N_2=51$ ($N_{\text{TOTAL}}=102$).

4.4 Double testing

A proper t -test for $d=0.50$ (one-sided) given $\alpha=\beta=0.05$ thus needs $N_{\text{MIN}}=N_1=N_2=88$ ($N_{\text{TOTAL}}=176$). Given the typical effect size range of published observed effects in behavioral science ($0.10 < d < 0.50$), as the d -value shrinks, N_{MIN} quickly reaches beyond the data collection resources that individual research groups commands. This suggests that resource restrictions are a major reason for N to stay below N_{MIN} , while letting a t -test *seem* a genuine NPTT-test (with conventionally accepted error rates).

Even if resource restrictions keep researchers from collecting all of N_{MIN} , the one strategy that NPTT offers to legitimately reduce N_{MIN} is *double testing*. ‘Double testing’ refers to testing the same participants in random order, in both the control and the experimental group condition, and then calculating for each subject the difference between both conditions (*repeated measurement design*). Double testing obviously creates a *dependent* sample. And, compared to how the measurement error spreads in two independent samples ($N_{\text{TOTAL}}=176$), one can arguably reduce $s_{(x1-x2)}$ legitimately—and thereby increase the effect size—only if participants’ responses in both groups are sufficiently correlated, e.g., to degree $r_{12}=0.50$.

Notice that the following holds: $s^2_{(x1-x2)} = s^2_{(x1)} + s^2_{(x2)} - 2 \times r_{12} \times s_{(x1)} \times s_{(x2)}$. Given *independent* samples (which correlate to degree $r_{12}=0$), therefore, the product $2 \times r_{12} \times s_{(x1)} \times s_{(x2)}$ goes to zero. Generally, as the standard deviation of two groups, $s_{(x1-x2)}$, converges to that of a single group, $s_{(x)}$, the variance of the mean of the differences between both groups continuously grows smaller. If $s_{x1}=s_{x2}=1$, for instance, then given $r_{12}=0.50$, one finds $s^2_{(x1-x2)} = s^2_{(x1)} = 1$. So, on formal grounds, the study of experimental participants’ individual differences in behavior using *two* groups makes an empirical test-condition statistically comparatively more *sensitive* than when using but *one* group. This because the formal model is now less error-prone.

Whether the psychological processes in focus are thought to be identical or even influenced by double testing, any reduction of N_{MIN} using double testing thus requires the necessary assumption that the value of r_{12} is *positive*. (Absent contrary evidence, a compromise between not knowing the true correlation and one’s resource restrictions may be the initial assumption that $r_{12}=0.50$.) Compared to a two independent sample t -test, lowering the error variance by means of double testing

increases the effect size, and thereby lowers N_{MIN} , and so decreases data collection cost. Given $\alpha=\beta=0.05$, for instance, a matched pairs t -test for $d=0.50$ with dependent samples requires $N_{\text{MIN}}=N_1=N_2=45$. Deploying the test twice yields 90 data points yet requires only 45 participants. This legitimate way of reducing the minimum sample thus lowers N_{MIN} from 176 to 45, or to some 26%.

On this background, we turn to resource pooling as a way for labs to reach N_{MIN} jointly.

5. Resource pooling

5.1 Resource pooling is possible and meaningful

A literature review would show the ubiquity of both strategies critiqued in Sect 5. The strategies' negative consequences for theoretical knowledge, of course, may be widely unknown. Alternatively, these negative consequences, though known, may for the following reasons seem acceptable.

The *first* reason is practical, citing a research group's lack of sufficient resources to individually meet N_{MIN} , as well as obstacles encountered when seeking to increase N to N_{MIN} by pooling several labs' resources (e.g., lack of experience, infrastructure, or peer-support). The *second* reason is motivational, stating that statistically significant, well-powered test-results are a goal that actual resource restrictions simply make unachievable. The *third* reason cites the impetus to avoid the hypocrisy of desiring what one should want (significant, well-powered test-results), but because of insufficient resources also cannot want.

The cogency of the third reason stands or falls with that of the second reason. As for that second reason, since no law of nature keeps from coordinating resources, several labs *can* in principle perform the same study under similar conditions to reach N_{MIN} jointly.³ They also *should*, because even underpowered test-results

³ See Landy et al. (2020), who report the replication efforts of 15 research teams investigating five social-psychological hypotheses with more than 15.000 experimental participants. They show that the typically large unexplained heterogeneity in results plausibly relates to researchers' subjective choices in designing experimental stimuli. The authors observe, "one way to reduce the role of subjective researcher choices in research outcomes may be to more fully flesh out the underlying theory at the outset" (ibid., 22).

remain theoretically useful to corroborate/falsify a hypothesis if one aggregates their likelihood ratios. Simulation results show that, if the corroboration threshold is sensitive to both error-rates—as holds for $LR_{H1/H0} > (1-\beta)/\alpha$ (Wald, 1947)—then log-likelihood addition combines underpowered test-results safely (Krefeld-Schwalb, Witte & Zenker, 2018). For exemplary analyses of combined test-results, see Witte and Zenker (2016) on macro-social stress (Chou, Parmar & Galinsky, 2016) and Witte and Zenker (2017a) on ego-depletion (Hagger & Chatzisarantis, 2016). Related proposals on how to limit the publication of underpowered data include Nosek and Bar-Anan (2012), and Nosek, Spies and Motyl (2012). Resource pooling thus compensates *fully* for an individual lab’s limited resources. The second and third reasons thus are non-cogent.

The first reason exclusively cites practical obstacles, which often require innovative solutions to old problems (e.g., how to share academic credit fairly; Rehg, 2009). These practical obstacles, however, certainly cannot be overcome by sticking to the approaches of Fisher or Neyman-Pearson, because their statistical concepts only enable decisions for an *individual* study. In fact, a method to combine test-results of a *series* of studies had long remained absent (Birnbaum, 1954). To our best knowledge, the only fair and firm combined statistical test for a series of studies is log-likelihood-ratio addition (Krefeld-Schwalb, Witte & Zenker, 2018; Witte & Zenker, 2017b).

5.2 Research diversity is a weak con-reason

The one reason *against* resource pooling cites research diversity (Schönbrodt & Wagenmakers, 2018). Given constant resources, if labs pool individual resources to achieve sufficient test-power, *ceteris paribus*, then each lab can “discover” only a comparatively smaller number of statistically significant effects. Yet, assigning value to diverse but *underpowered* statistically significant test-results, as opposed to less diverse but well-powered significant test-results, does not only subordinate theoretical progress (Witte & Strohmeier, 2013). An overly strong focus on research diversity without aggregation and subsequent theory development indeed *kills* theory. After all, the aggregation of diverse yet dissimilar experimental test-results is theoretically as pointless as the comparison of apples and oranges is proverbial (cf. Landy et al., 2020). Not pointless, by contrast, is a differentiation (with respect to

the same theory) of empirical test-results obtained under *dissimilar* experimental conditions, But this already implies undertaking at least initial efforts in theory construction. In brief, research diversity can be *at most as* important as obtaining well-powered significant test-results.

5.3 Resource pooling is necessary

Few researchers today manage resource pooling within an integrated research program in service of theoretical goals, even if several recent publications report preregistered *replications* that look *as if* an integrated research program were pursued (e.g., Hagger & Chatzisarantis, 2016; Landy, et al., 2020). Although preregistered replications aggregate the effect sizes of individual studies, no construct is specified as a point-alternative hypothesis. Researchers thus take a first step towards analyzing test-results of similar experimental conditions from different labs, but leave the full theoretical potential unused (Witte & Zenker, 2017a).

If the theoretical motivation for resource pooling were better understood, and properly incentivized, then training and infrastructure would probably develop accordingly. Test-power would then complement the *p*-value, both in job interviews and on review checklists. And, as journals predominantly published well-powered significant test-results, with the use of likelihood-ratios (rather than probability measures) becoming normal, too, *substantial hypothesis verifications* could inform theoretical research (Witte & Zenker, 2017b).

Instead, what researchers in behavioral science do today is seek to replicate originally underpowered effects at a test-power level of around $(1-\beta)=0.80$, the very value Cohen recommended (e.g., Cova et al., 2018). Counter-intuitively, however, once resource pooling leads to test-results under $\alpha=\beta=0.01$, for instance, then the typically effort-intensive replication attempts undertaken today would become *superfluous* (Witte & Zenker, 2018). In terms of statistical test-power, after all, independently replicating the statistically significant (non-zero) observed effect size of an already well-powered *original* study ($\alpha=\beta=0.05$) yields merely a warrant to *restate* confidence in the originally observed effect. But unless the error-rates are yet further decreased ($\alpha=\beta<<0.05$), independently replicating this original effect would fail to offer a warrant to *increase* confidence in the originally observed effect.

This is so because the *joint* replication probability of n independent studies (aka ‘aggregate test power’) is the *product* of these studies’ test-power, or $(1-\beta)^n$. If a first successful replication yields $(1-\beta)=0.80 \times 0.80=0.64$, for instance, then already the second successful replication yields only $(1-\beta)=0.512$. But this is too close to chance-level to use this observed effect size in theory construction. Unlike log-likelihood aggregation paired with $LR_{H1/H0} > (1-\beta)/\alpha$ as the corroboration threshold (Krefeld-Schwalb, Witte & Zenker, 2018), therefore, to determine whether a test-result is informative for theory construction, aggregate test-power simply is a *misleading* formal measure.

5.4 Inadequate samples sizes incur negative consequences

Hiring decisions regularly assign great importance to a job candidate’s number of a publications, using it as a proxy measure for research productivity. Gervais, Jewell, Najle, and Ng (2015) operationalize research productivity as the sum of two sample control group studies (N_1, N_2) that two hypothetical candidates publish over five years. *Ceteris paribus*, given $d=0.40$ and $N_1=N_2=100$, the candidate whose studies consistently achieve $(1-\beta)=0.95$ can in principle publish 21 studies with statistically significant results, compared to 33 studies if a candidate makes do with $N_1=N_2=25$. Given $\alpha=0.05$ (one-sided), however, $N_1=N_2=25$ implies $(1-\beta)=0.40$, which is below chance level. Even $N_1=N_2=100$ yields only $(1-\beta)=0.88$. Thus, avoiding an erroneous discovery of a non-random effect under $\alpha=0.05$ would thus be evaluated as being 2.4 times *more important* than avoiding an erroneous rejection of a true alternative hypothesis ($\beta=0.12=0.05 \times 2.4$).

From a theoretical viewpoint, this evaluation is unreasonable. An under-sized sample, after all, necessarily leaves vague what $\alpha=\beta=0.05$, or even $\alpha=\beta=0.01$, would instead turn into the “well-hardened” (Lakatos, 1978) empirical knowledge of an effect size, knowledge that a theoretician needs to develop a theoretical construct. Indeed, if we assume a *medium* effect ($d=0.50$) as typical, then *most* published effects in behavioral science publications would be underpowered. Behavioral science would thus generally underestimate the negative *theoretical* consequences of conducting underpowered studies. Indeed, “[r]unning underpowered studies [...] might constitute a type of *performance-enhancing design* that inflates an individual researcher’s productivity while having deleterious consequences for the collective

enterprise of science” (Gervais et al., 2015, 848). Though a hiring committee would reportedly reverse their decision upon learning that little of theoretical value speaks for underpowered test-results (Gervais et al., 2015), inadequate samples size clearly incur negative consequences for theory construction.

6. Discussion

As several recent guidelines make clear, reliable scientific knowledge requires independently replicated test-results (e.g., “Reproducibility and Replicability in Science” (NAS, 2019)). Replicable results in turn are more important than novel results, and the ability to *theoretically predict* replicable results is more important yet. The replication crisis in behavioral science, as we saw, can be explained as a consequence of typically low $(1-\beta)$ -error-rates, that is, an overly low probability of replicating an original test-result. The root cause of this crisis, we submit, is a wide misunderstanding or misapplication of statistical methods in contexts of limited data collection resources, because reducing N to a value smaller than N_{MIN} must be “paid” for in terms of test-power. And, both strategies critiqued here—lowering the $(1-\beta)$ -error-rate from 0.95 to 0.80; treating the control group mean as a constant—are typical examples of questionable research practices to reduce N_{MIN} .

As most behavioral science research today is data-driven, not only do many researchers misunderstand/misapply the best statistical methods (Gigerenzer, 2018), they also tend to over-interpret or overstate a test-result’s significance. An indicator of this is the verbal transition—without warrant—from ‘is (statistically) significant’ to ‘is (scientifically) important’. A related issue arises from assuming “exactly zero effect of site, experimenter, stimuli, task, instructions, and every other factor except subject” (Yarkoni, 2020), resulting in a mismatch between general verbal statements of theoretical hypotheses and their statistical expressions (aka ‘generalizability crisis’). Finally, the preference for novel results in behavioral science has long kept from appreciating the full value of replication research (Witte & Strohmeier, 2013; Ioannidis, 2005; Pasher & Wagenmakers, 2012; Open Science Collaboration, 2015). In fact, these causes were already discussed in the 1970s (e.g., Meehl, 1967; 1978).

A theoretically progressive behavioral science requires the ability to derive point-specific predictions from theories that subsume well-replicated experimental effects, thereby contributing to explaining, predicting, and intervening upon these effects. Indeed, the practical value of theoretical knowledge rests squarely on

successful interventions. Though the philosophy of science has long recognized *induction* and *deduction* as distinct modes of reasoning, behavioral science regularly conflates these modes, here taking it for granted that learning from already observed (‘old’) data would provide a substitute for a theory-based prediction of new data. Particularly Bayesian methods embrace this idea.

But predictions derive from theoretical knowledge, whereas induction can practically succeed without any theoretical accountability. While induction describes the process of arriving at a parameter estimate given old data (aka ‘retrodiction’), a point-specific version of this estimate is what a theoretical construct must predict for a specific empirical condition, and then test this prediction against new data. A confirmation by data, therefore, can be nothing other than a confirmation of a theory-derived point-prediction. Conversely, theories failing to point-predict cannot be confirmed by data. Indeed, absent point-predicting theories that retrodict old data and predict new data, theoretical progress is hard to define and hard to achieve.

Since the construction of empirically adequate theoretical constructs is impossible without collecting much larger samples (that guaranteed acceptable replication probability), collaboration between labs in the form of resource pooling remains a promising and viable route to avoid the negative consequences for theoretical knowledge in behavioral science. Even Nobel prize winner Daniel Kahneman admitted, in 2017 (Schimmack et al., 2017), that his bestselling “Thinking fast and slow” (Kahneman, 2011) relied on too many non-replicated empirical effects. Of course, it does not help that “published papers in top psychology, economics, and general interest journals that fail to replicate are cited more than those that replicate” (Serra-Garcia & Gneezy, 2021, 1).

7. Conclusion

The functional dependence between the α - and β -error-rates, d , and N_{MIN} allows researchers to plan empirical studies with sufficiently large samples to achieve test-results under $\alpha=\beta=0.05$, test-results that alone *can* warrant the effort of subsuming the corresponding observations under a theoretical construct. Presently, however, these very error-rates would *filter out* the bulk of published test-results in behavioral science as broadly useless for theory construction.

We have here critiqued two common strategies—lowering the $(1-\beta)$ -error-rate from 0.95 to 0.80; treating the control group mean as a constant—as typical

examples of questionable research practices to reduce N_{MIN} . We saw that, even if $\alpha=\beta=0.05$, a proper two sample t -test of the means in experimental and control group always requires a point-specification of the H_0 and the H_1 . Provided that r_{12} is positive, moreover, double testing is the *only* admissible strategy to reduce N_{MIN} .

A one sample t -test, by contrast, collapses a two sample Neyman-Person test into a Fisher test, which merely reports the p -value of data in view of H_0 . Although the p -value can have heuristic value, a Fisher test cannot estimate test-power, hence cannot statistically corroborate/falsify a theoretical hypothesis, so fails to inform theory construction. As we have argued, collaboration between individual labs in the form of resource pooling remains a viable route to increasing the sample sizes in behavioral science.

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