Calibration in experimental psychology: designing optimal calibration experiments

Dominik R Bach

University College London, London, UK.

Max Planck UCL Centre for Computational Psychiatry and Ageing Research & Wellcome
Centre for Human Neuroimaging

Author Note

The author thanks Dr. Filip Melinscak for discussion and insightful comments on a first draft of this manuscript. DRB receives funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (Grant agreement No. ERC-2018 CoG-816564 ActionContraThreat), and from the from the National Institute for Health Research (NIHR) UCLH Biomedical Research Centre. The Wellcome Centre for Human Neuroimaging is funded by core funding from the Wellcome (203147/Z/16/Z).

Abstract

Accurate measurement requires maximising the correlation between true scores and measured scores. Classical psychometric concepts such as construct validity and reliability are often difficult to apply in experimental contexts. To overcome this challenge, calibration has recently been suggested as generic framework for experimental research. In this approach, a calibration experiment is performed to impact the latent attribute in question. The a priori intended true scores can then serve as criterion, and their correlation with measured scores, termed retrodictive validity, is used to evaluate a measurement method. It has been shown that under plausible assumptions, increasing retrodictive validity is guaranteed to increase measurement accuracy. Since calibration experiments will be performed in finite samples, it is desirable to design them in a way that minimises the sample variance of retrodictive validity estimators. This is the topic of the current note. For arbitrary distributions of true and measured scores, we analytically derive the asymptotic variance of the sample estimator of retrodictive validity. We analyse qualitatively how different distribution features impact on estimator variance. Then, we numerically simulate asymptotic and finite-sample estimator variance for various distributions with combinations of feature values. We find that it is preferable to use uniformly distributed (if possible discrete) experimental treatments in calibration experiments. Secondly, inverse sigmoid systematic aberration has a large impact on estimator variance. Finally, reducing imprecision aberration decreases estimator variance in many but not all scenarios. From these findings, we derive recommendations for the design and for resource investment in calibration experiments.

Keywords: Calibration, retrodictive validity, measurement accuracy, measurement uncertainty

Calibration in experimental psychology: designing optimal calibration experiments

Introduction

Across experimental psychology, researchers are interested in measuring latent attributes with behavioural, physiological, or self-report instruments. Optimising these instruments engenders challenges that are substantially different from those encountered in classical psychometrics, for example in the measurement of cognitive abilities, attitudes, or personality traits (Bach, Melinscak, Fleming, & Voelke, 2020). First, measured attributes in experimental psychology are often volatile - their scores can change very rapidly. Secondly and relatedly, concurrent assessment of the same or of other attributes is often difficult. For both of these reasons, attributes are often inferred from only one observation, rather than a collection of items. Consequently, concepts like construct validity, criterion validity, internal consistency, or test-retest reliability, are often not applicable. Indeed, in many fields of experimental psychology, the selection of measurement strategies for a given attribute rests on tradition, expertise, or plausibility, but differs substantially between and even within communities (see for reviews, Lonsdorf et al. 2019, 2017; Ojala and Bach 2020; Silberzahn et al. 2018; Simmons, Nelson, and Simonsohn 2011; Steegen, Tuerlinckx, Gelman, and Vanpaemel 2016). Furthermore, the lack of quality metrics precludes a more rigorous evaluation of research results, their uncertainty, and generalisability, across laboratories and research fields (Rigdon, Sarstedt, & Becker, 2020).

As an alternative for experimental research, it has been suggested to create intended values of an attribute in calibration experiments that build on well-known experimental manipulations (Bach & Friston, 2013; Bach & Melinscak, 2020; Bach et al., 2020). These intended values can serve as external validity criterion. The correlation between intended and measured scores has been termed retrodictive validity. We have previously shown that under three plausible assumptions, increasing retrodictive validity is guaranteed to increase measurement accuracy, which we define here as the correlation between true score and measured score, in line with previous work (Cronbach & Meehl, 1955). The three criteria

are: (a) a positive (possibly small) correlation between intended scores and true scores, (b) a positive (possibly small) correlation between true scores and measured scores, and (c) no correlation between experimental aberration (the difference between intended and true scores) and measurement error (the difference between true and measured scores) (Bach et al., 2020).

To put this concept into practice, it is necessary to design calibration experiments. Many of the associated design questions may be specific to the studied attribute; but there are at least two domain-general aspects: the distribution of intended values, and the allocation of resources, for example whether to prioritise increasing the sample size or reducing the experimental aberration, which we define here as the difference between intended and true scores. This is the topic of the present work. We approach it from the angle of statistical estimation theory. Development of experimental measurement methods is often incremental, and previous research has suggested that the achievable improvements can be relatively small (Bach & Melinscak, 2020). In this situation, it is necessary to distinguish between two relatively similar population values for retrodictive validity from sample estimators. Consequently, retrodictive validity estimates themselves should be accurate. While the bivariate correlation coefficient is approximately unbiased for large samples, its variance depends not only on sample size but on features of the parent distributions, which are usually non-normal and can possibly be influenced by experimental design. Here, we ask what are the design factors that determine the variance of a retrodictive validity estimator. The distribution of true scores and of measured scores is not generally known a priori. Our approach is to analytically identify relevant distribution features, and then to numerically simulate distributions with combinations of these features. To render our derivation analytically tractable, we make two simplifications. First, we consider calibration experiments in which intended values are independently randomised for each measurement. Future work will consider more common balanced designs, including standard within-subject designs. Secondly, our analytical results and

thus, the choice of qualitative distribution features used in our numerical simulations, are based on asymptotic limits which are valid for large samples. The numerical simulations themselves are performed for the asymptotic case as well as for modestly sized samples.

Retrodiction model

To put the sequel on a probability-theoretic foundation, we use this section to recapitulate our previous retrodiction model (Bach et al., 2020) and to generalise it to continuous distributions of intended values. Readers not interested in the formalism may skip this section and proceed to the derivation of the main results. With subscripted $\mathbb{P}$, we always denote probability measures. We formalise conditional probability measures as probability kernels, denoted with $\mathbb{K}$. Recall that for two probability spaces $(\Omega_1, \mathcal{A}_1)$ and $(\Omega_2, \mathcal{A}_2)$, $\mathbb{K}_2^1 : \Omega_1 \times \mathcal{A}_2 \rightarrow [0, 1]$ is a probability kernel if it has the two following properties: (1) for fixed $\omega_1 \in \Omega_1$, $\mathbb{K}(\omega_1, \cdot)$ is a probability measure (that is, the probability of a set in $\mathcal{A}_2$ conditional on ω_1); (2) for fixed $A_2 \in \mathcal{A}_2$, $\mathbb{K}(\cdot, A_2)$ is a measurable function. We denote expectation and variance with $\mathbb{E}$ and $\mathbb{V}$, respectively. The model is illustrated in figure 1.

We consider a calibration experiment in which each participant is independently and randomly assigned to an experimental manipulation with associated intended attribute values $e \in \Omega_E \subseteq \mathbb{R}$, according to a probability measure $\mathbb{P}_E$. The set of intended values Ω_E , and their probability, can be freely determined by the experimenter. Once e is fixed, the experimental manipulation results in a true attribute value $t \in \mathbb{R}$ according to a probability kernel $\mathbb{K}_T^E(e, t)$, which depends on the experimental design in a way that cannot be fully known to the experimenter. The experimenter then takes one or several observations, which are transformed into a measured score $y \in \mathbb{R}$. Given a true score t , the measured score follows a kernel $\mathbb{K}_Y^T(t, y)$, which is unknown to the experimenter and, crucially, can be changed after the experiment by using novel transformations on existing observables. We are only interested in correlations between the variables e, t, y ; since correlations are invariant to translation and scale, we can arbitrarily define mean and variance of these

variables. This corresponds to the intuition that psychological attributes do not have a natural scale.

Now let $(\Omega, \mathcal{A}, \mathbb{P})$ be a probability space with the properties

$$\Omega := \Omega_E \times \mathbb{R} \times \mathbb{R}, \quad (1)$$

$$\mathcal{A} := \sigma(\Omega_E) \otimes \mathcal{B} \otimes \mathcal{B}, \quad (2)$$

where $\mathcal{B}$ is the Borel algebra, and with joint probability measure

$$\mathbb{P}(A) := \int_{\Omega_E} d\mathbb{P}_E \int_{\mathbb{R}} d\mathbb{K}_T^E \int_{\mathbb{R}} d\mathbb{K}_Y^T 1_A, \quad (3)$$

where 1_A is the characteristic (indicator) function of the set $A \in \mathcal{A}$.

Next, let $E : \Omega \rightarrow \Omega_E$ be a random variable with image measure $\mathbb{P} \circ E^{-1} = \mathbb{P}_E$, according to eq. (3). Without loss of generality, we assume that

$$\mathbb{E}(E) := 0, \quad \mathbb{E}(E^2) = \mathbb{V}(E) := 1. \quad (4)$$

Let $T : \Omega \rightarrow \mathbb{R}$ be a random variable that describes the standardised true scores with image measure $\mathbb{P}_T(t) := \mathbb{P} \circ T^{-1}(t) = \int_{\Omega_E} d\mathbb{P}_E \int_{\mathbb{R}} d\mathbb{K}_T^E 1_t$, according to eq. (3). We take “standardised” to mean that

$$\frac{Cov(E, T)}{\mathbb{V}(E)} = Cov(E, T) := 1, \quad (5)$$

without loss of generality. In the terminology of our previous model Bach et al. (2020), this corresponds to $\gamma := Cov(E, T) := 1$, but this parameter will not be necessary for the present work as we have already standardised T . Furthermore, we assume, without loss of generality

$$\mathbb{E}(T) := 0. \quad (6)$$

Let $Y : \Omega \rightarrow \mathbb{R}$ be a random variable that describes the standardised measurements. Again, “standardised” is taken to mean that

$$\frac{\text{Cov}(T, Y)}{\mathbb{V}(T)} := 1, \quad (7)$$

which in the terminology of our previous model (Bach et al., 2020) corresponds to $\beta := 1$, but we will not use this parameter in the present work. Furthermore, we assume, without loss of generality

$$\mathbb{E}(Y) := 0. \quad (8)$$

Now we define two more random variables: the standardised experimental aberration

$$\omega := T - E, \quad (9)$$

and the standardised measurement error

$$\varepsilon := Y - T, \quad (10)$$

such that

$$Y = E + \omega + \varepsilon. \quad (11)$$

We assume that ε and ω are uncorrelated (but not necessarily independent). Under this assumption, we have previously proven Bach et al. (2020) that retrodictive validity ρ_{EY} , can be expressed as follows:

$$\rho_{EY} = \frac{1}{\sqrt{1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon)}} = \frac{1}{\sqrt{\mathbb{V}(Y)}}. \quad (12)$$

As in our previous work, we decompose ω into a stochastic imprecision (I) component $\omega_I : \Omega \rightarrow \mathbb{R}$, and a deterministic systematic component that describes a residual (R) non-linearity $f_R : \Omega_E \rightarrow \mathbb{R}$ and takes the form of a conditional expectation. Thus, we define

$$f_R(e) := \int_{\mathbb{R}} (t - e) \mathbb{K}_T^E(e, dt) \quad (e \in \Omega_E), \quad (13)$$

$$\omega_I := \omega - f_R(E), \quad (14)$$

such that

$$T = E + f_R(E) + \omega_I. \quad (15)$$

The next identities follow from this definition:

$$\mathbb{E}(f_R(E)) = 0. \quad (16)$$

$$\text{Cov}(E, f_R(E)) = 0. \quad (17)$$

Finally we define

$$\mathbb{K}_{\omega_I}^E(e, t) := \mathbb{K}_T^E(e, t) - e - f_R(e). \quad (18)$$

and we note that

$$\int_{\mathbb{R}} \omega_I d\mathbb{K}_{\omega_I}^E(e, dt) = 0. \quad (19)$$

Derivation of the main result

The following theorem is proven in Steiger and Hakstian (1982). Let ρ denote a population (bivariate) correlation coefficient, and r the corresponding sample correlation coefficient .

Theorem 1. For a sample with size N , let $n := N - 1$. For two sample correlation coefficients with arbitrary parent distributions, the joint distribution of $\sqrt{n}(r_{ij} - \rho_{ij})$ and $\sqrt{n}(r_{kh} - \rho_{kh})$ converges to a bivariate normal distribution with covariance matrix

$$\begin{bmatrix} \gamma_{ij,ij} & \gamma_{ij,kh} \\ \gamma_{ij,kh} & \gamma_{kh,kh} \end{bmatrix} \text{ and}$$

$$\gamma_{ij,kh} = \rho_{ijkh} + \frac{1}{4}\rho_{ij}\rho_{kh}(\rho_{iikk} + \rho_{jjkk} + \rho_{iiah} + \rho_{jjhh}) - \frac{1}{2}\rho_{ij}(\rho_{iikh} + \rho_{jjkh}) - \frac{1}{2}\rho_{kh}(\rho_{ijkk} + \rho_{ijhh})$$

with

$$\sigma_{ij} := Cov(X_i, X_j)$$

$$\sigma_{ijkh} := \mathbb{E}((X_i - \mu_i)(X_j - \mu_j)(X_k - \mu_k)(X_h - \mu_h))$$

$$\rho_{ij} := \frac{\sigma_{ij}}{\sqrt{\sigma_{ii}\sigma_{jj}}}$$

$$\rho_{ijkh} := \frac{\sigma_{ijkh}}{\sqrt{\sigma_{ii}\sigma_{jj}\sigma_{kk}\sigma_{hh}}}.$$

For the variance, this reduces to:

$$\gamma_{ij,ij} = \rho_{ijij} + \frac{1}{4}\rho_{ij}^2 (\rho_{iiii} + 2\rho_{iijj} + \rho_{jjjj}) - \rho_{ij} (\rho_{iiij} + \rho_{ijjj}) \quad (20)$$

$$= \frac{\sigma_{iijj}}{\sigma_{ii}\sigma_{jj}} + \frac{1}{4}\rho_{ij}^2 \left(\frac{\sigma_{iiii}}{\sigma_{ii}^2} + 2\frac{\sigma_{iijj}}{\sigma_{ii}\sigma_{jj}} + \frac{\sigma_{jjjj}}{\sigma_{jj}^2} \right) - \rho_{ij} \left(\frac{\sigma_{iiij}}{\sqrt{\sigma_{ii}^3\sigma_{jj}}} + \frac{\sigma_{ijjj}}{\sqrt{\sigma_{ii}\sigma_{jj}^3}} \right). \quad (21)$$

For sample retrodictive validity $r_{EY} := \text{Cor}(\mathbf{e}, \mathbf{y})$, we have:

$$\begin{aligned} \lim_{n \rightarrow \infty} n\mathbb{V}(r_{ij}) &= \gamma_{ij,ij} = \frac{\mathbb{E}((EY)^2)}{\mathbb{V}(E)\mathbb{V}(Y)} + \frac{1}{4}\rho_{EY}^2 \left(\frac{\mathbb{E}(E^4)}{\mathbb{V}^2(E)} + 2\frac{\mathbb{E}((EY)^2)}{\mathbb{V}(E)\mathbb{V}(Y)} + \frac{\mathbb{E}(Y^4)}{\mathbb{V}^2(Y)} \right) \\ &\quad - \rho_{EY} \left(\frac{\mathbb{E}(E^3Y)}{\sqrt{\mathbb{V}^3(E)\mathbb{V}(Y)}} + \frac{\mathbb{E}(EY^3)}{\sqrt{\mathbb{V}(E)\mathbb{V}^3(Y)}} \right) \end{aligned} \quad (22)$$

$$\begin{aligned} &= \frac{\mathbb{E}((EY)^2)}{\mathbb{V}(Y)} + \frac{1}{4}\rho_{EY}^2 \left(\mathbb{E}(E^4) + 2\frac{\mathbb{E}((EY)^2)}{\mathbb{V}(Y)} + \frac{\mathbb{E}(Y^4)}{\mathbb{V}^2(Y)} \right) \\ &\quad - \rho_{EY} \left(\frac{\mathbb{E}(E^3Y)}{\sqrt{\mathbb{V}(Y)}} + \frac{\mathbb{E}(EY^3)}{\sqrt{\mathbb{V}^3(Y)}} \right) \end{aligned} \quad (23)$$

where the second equality used $\mathbb{E}(E) = \mathbb{E}(Y) = 0$ and $\mathbb{V}(E) = 1$.

Now we expand the expectations involving $Y = E + \omega + \varepsilon$:

$$\mathbb{E}((EY)^2) = \mathbb{E}((E(E + \omega + \varepsilon))^2) \quad (24)$$

$$= \mathbb{E}((E^2 + E\omega + E\varepsilon)^2) \quad (25)$$

$$= \mathbb{E} \left(E^4 + (E\omega)^2 + (E\varepsilon)^2 + 2E^3\omega + 2E^3\varepsilon + 2E^2\omega\varepsilon \right) \quad (26)$$

$$\mathbb{E} \left(E^3 Y \right) = \mathbb{E} \left(E^3 (E + \omega + \varepsilon) \right) \quad (27)$$

$$= \mathbb{E} \left(E^4 + E^3\omega + E^3\varepsilon \right) \quad (28)$$

$$\mathbb{E} \left(E Y^3 \right) = \mathbb{E} \left(E (E + \omega + \varepsilon)^3 \right) \quad (29)$$

$$= \mathbb{E} \left(E^4 + E\omega^3 + E\varepsilon^3 + 3E^3\omega + 3E^3\varepsilon + 3E^2\omega^2 + 3E\omega^2\varepsilon + 3E^2\varepsilon^2 + 3E\omega\varepsilon^2 + 6E^2\omega\varepsilon \right) \quad (30)$$

$$\mathbb{E} \left(Y^4 \right) = \mathbb{E} \left((E + \omega + \varepsilon)^4 \right) \quad (31)$$

$$= \mathbb{E} \left(E^4 + \omega^4 + \varepsilon^4 + 4E^3\omega + 4E^3\varepsilon + 4E\omega^3 + 4E\varepsilon^3 + 4\omega^3\varepsilon + 4\omega\varepsilon^3 + \right. \quad (32)$$

$$\left. 6E^2\omega^2 + 6E^2\varepsilon^2 + 6\omega^2\varepsilon^2 + 12E^2\omega\varepsilon + 12E\omega^2\varepsilon + 12E\omega\varepsilon^2 \right)$$

Taking all of this together yields

$$\begin{aligned} \lim_{n \rightarrow \infty} n \mathbb{V} (r_{ij}) &= \\ &= \frac{\mathbb{E} \left(E^4 + (E\omega)^2 + (E\varepsilon)^2 + 2E^3\omega + 2E^3\varepsilon + 2E^2\omega\varepsilon \right)}{\mathbb{V} (Y)} + \end{aligned}$$

$$\begin{aligned}
& \frac{1}{4}\rho_{EY}^2 \left(\mathbb{E}(E^4) + 2 \frac{\mathbb{E}(E^4 + (E\omega)^2 + (E\varepsilon)^2 + 2E^3\omega + 2E^3\varepsilon + 2E^2\omega\varepsilon)}{\mathbb{V}(Y)} + \right. \\
& \frac{\mathbb{E}(E^4 + \omega^4 + \varepsilon^4 + 4E^3\omega + 4E^3\varepsilon + 4E\omega^3 + 4E\varepsilon^3 + 4\omega^3\varepsilon + 4\omega\varepsilon^3)}{\mathbb{V}^2(Y)} + \\
& \left. \frac{\mathbb{E}(6E^2\omega^2 + 6E^2\varepsilon^2 + 6\omega^2\varepsilon^2 + 12E^2\omega\varepsilon + 12E\omega^2\varepsilon + 12E\omega\varepsilon^2)}{\mathbb{V}^2(Y)} \right) - \\
& \rho_{EY} \left(\frac{\mathbb{E}(E^4 + E^3\omega + E^3\varepsilon)}{\sqrt{\mathbb{V}(Y)}} + \right. \\
& \frac{\mathbb{E}(E^4 + E\omega^3 + E\varepsilon^3 + 3E^3\omega + 3E^3\varepsilon + 3E^2\omega^2)}{\sqrt{\mathbb{V}^3(Y)}} + \\
& \left. \frac{\mathbb{E}(3E\omega^2\varepsilon + 3E^2\varepsilon^2 + 3E\omega\varepsilon^2 + 6E^2\omega\varepsilon)}{\sqrt{\mathbb{V}^3(Y)}} \right). \tag{33}
\end{aligned}$$

Simplifying yields

$$\begin{aligned}
& \lim_{n \rightarrow \infty} n\mathbb{V}(r_{ij}) = \\
& = \rho_{EY}^2 \mathbb{E}(E^4 + (E\omega)^2 + (E\varepsilon)^2 + 2E^3\omega + 2E^3\varepsilon + 2E^2\omega\varepsilon) + \\
& \frac{1}{4}\rho_{EY}^2 \left(\mathbb{E}(E^4) + 2\rho_{EY}^2 \mathbb{E}(E^4 + (E\omega)^2 + (E\varepsilon)^2 + 2E^3\omega + 2E^3\varepsilon + 2E^2\omega\varepsilon) + \right. \\
& \left. \rho_{EY}^4 \mathbb{E}(E^4 + \omega^4 + \varepsilon^4 + 4E^3\omega + 4E^3\varepsilon + 4E\omega^3 + 4E\varepsilon^3 + 4\omega^3\varepsilon + 4\omega\varepsilon^3) + \right.
\end{aligned}$$

$$\rho_{EY}^4 \mathbb{E} \left(6E^2\omega^2 + 6E^2\varepsilon^2 + 6\omega^2\varepsilon^2 + 12E^2\omega\varepsilon + 12E\omega^2\varepsilon + 12E\omega\varepsilon^2 \right) -$$

$$\rho_{EY} \left(\rho_{EY} \mathbb{E} \left(E^4 + E^3\omega + E^3\varepsilon \right) + \right.$$

$$\rho_{EY}^3 \mathbb{E} \left(E^4 + E\omega^3 + E\varepsilon^3 + 3E^3\omega + 3E^3\varepsilon + 3E^2\omega^2 \right) +$$

$$\rho_{EY}^3 \mathbb{E} \left(3E\omega^2\varepsilon + 3E^2\varepsilon^2 + 3E\omega\varepsilon^2 + 6E^2\omega\varepsilon \right). \quad (34)$$

Now we collapse all terms involving ε , as they are outside the experimenter's control, depend on the particular measurement model and thus cannot be determined a priori. This yields

$$\lim_{n \rightarrow \infty} n\mathbb{V}(r_{ij}) =$$

$$= \rho_{EY}^2 \mathbb{E} \left(E^4 + (E\omega)^2 + 2E^3\omega \right) +$$

$$\frac{1}{4} \rho_{EY}^2 \left(\mathbb{E} \left(E^4 \right) + 2\rho_{EY}^2 \mathbb{E} \left(E^4 + (E\omega)^2 + 2E^3\omega \right) + \right.$$

$$\rho_{EY}^4 \mathbb{E} \left(E^4 + \omega^4 + 4E^3\omega + 4E\omega^3 \right) +$$

$$\rho_{EY}^4 \mathbb{E} \left(6E^2\omega^2 \right) -$$

$$\rho_{EY} \left(\rho_{EY} \mathbb{E} \left(E^4 + E^3\omega \right) + \right.$$

$$\begin{aligned}
& \rho_{EY}^3 \mathbb{E} \left(E^4 + E\omega^3 + 3E^3\omega + 3E^2\omega^2 \right) \\
& + f_\varepsilon(\varepsilon).
\end{aligned} \tag{35}$$

Re-arranging gives:

$$\begin{aligned}
& \lim_{n \rightarrow \infty} n \mathbb{V}(r_{ij}) = \\
& \mathbb{E} \left(E^4 \right) \frac{1}{4} \left(\rho_{EY}^2 - 2\rho_{EY}^4 + \rho_{EY}^6 \right) + \\
& \mathbb{E} \left(\omega^4 \right) \left(\frac{1}{4} \rho_{EY}^6 \right) + \\
& \mathbb{E} \left((E\omega)^2 \right) \left(\rho_{EY}^2 - \frac{5}{2} \rho_{EY}^4 + \frac{3}{2} \rho_{EY}^6 \right) + \\
& \mathbb{E} \left(E^3 \omega \right) \left(\rho_{EY}^2 - 2\rho_{EY}^4 + \rho_{EY}^6 \right) + \\
& \mathbb{E} \left(E\omega^3 \right) \left(-\rho_{EY}^4 + \rho_{EY}^6 \right) + \\
& + f_\varepsilon(\varepsilon).
\end{aligned} \tag{36}$$

We note that if $\varepsilon = 0$ then $f_\varepsilon(\varepsilon) = 0$, and so it follows that the sum of the other terms is non-negative.

Controllable model quantities

Intended values

Our goal is to minimise $\mathbb{V}(\rho_{EY})$. The intended values are usually under experimental control. In our previous work, we have argued that for some psychological attributes, the effect of an experimental manipulation can only be specified on two levels, because of a lack of quantitative theories. In other cases, however, theory specifies intended values on interval scale, such that an experimenter is free to choose any intended values. There may be conceptual reasons for choosing a wide range of values; here we exclusively focus on how the distribution of E impacts on $\mathbb{V}(\rho_{EY})$.

Scale and distribution of the experimental aberration

The distribution of the experimental aberration is not under direct experimental control. However it may be possible to unspecifically reduce the aberration with experimental measures; for example, testing in quiet rooms instead of ad-hoc testing in a shopping mall. Such procedures may unspecifically reduce the scale of the ω_I but could also change its shape. It is instructive to distinguish qualitative features of the distribution separate from its scale, and analyse their impact on $\mathbb{V}(\rho_{EY})$. In our derivation and simulations, we separate scale and shape. We assume $\mathbb{V}(\omega_I) < \infty$ and define $\omega_0 := \omega / \sqrt{\mathbb{V}(\omega)}$, such that $\mathbb{V}(\omega_0) = 1$. To simplify presentation of the simulations, we define $\zeta_I^2 := \mathbb{V}(\omega_I)$.

Measurement error

Measurement error is not under direct experimental control, although it may be possible to unspecifically influence its scale. Crucially, data transformations after the experiment influence the measurement error. Hence, it may not be known a priori when conducting a calibration experiment because new methods may be developed after the experiment has been completed. Therefore, we do not focus in this article on the role of

measurement error. In our numerical simulations, we assume throughout that aberration and measurement error are not only uncorrelated but also stochastically independent, and that measurement error follows a normal distribution, where we explore several levels of error variance.

Plausible ranges of model quantities

To prepare for the numerical expressions and examples in the sequel, we give an intuition of typical retrodictive validity values and their associated standardised aberrations and errors. We note that the examples given here come from within-subject manipulations and may not generalise to between-subjects manipulations; also the numerical values may be rather different between subdisciplines of experimental psychology. Systematic research using retrodictive validity as a metric has mainly been conducted in the field of psychophysiology, and more specifically, in the measurement of associative strength during or after threat conditioning (also termed fear conditioning). In published papers, retrodictive validity is typically expressed as (Cohen's) d for the within-subjects difference between two experimental treatments. Published values range between $d = 0.4$ for some skin conductance analysis methods, and $d = 1.2$ for fear-potentiated startle eye-blink (e.g. Bach and Melinscak (2020)). This corresponds to retrodictive validity estimates between (Pearson's) $r = .20$ and $r = .51$. Using eq. (12), we have

$$\mathbb{V}(\omega) + \mathbb{V}(\varepsilon) = \frac{1}{\rho_{EY}^2} - 1. \quad (37)$$

Thus, the two variances combined will take values between 24 for $r = .20$, and 2.8 for $r = .51$. As there are only two possible values of e , it is easy to show that $f_R = 0$. If aberration and error variance were equal, this would correspond to $\zeta_I = 3.46$ and $\zeta_I = 1.18$, respectively.

Numerical simulations

In our simulations, we evaluate the following distributions. These examples are based on the analytical results and represent distributions with extreme values for some relevant features.

Intended values

1. Two equiprobable values: $\Omega_E = \{-1, 1\}$, $\mathbb{P}_E(e) = 1/2$ ($e \in \Omega_E$).
2. Four equiprobable values:
 $\Omega_E = \{-3/\sqrt{5}, -1/\sqrt{5}, 1/\sqrt{5}, 3/\sqrt{5}\}$, $\mathbb{P}_E(e) = 1/4$ ($e \in \Omega_E$).
3. Continuous uniform distribution: $\Omega_E = [-\sqrt{3}, \sqrt{3}]$, $E \sim \mathcal{U}(-\sqrt{3}, \sqrt{3})$.
4. Standard normal distribution: $\Omega_E = \mathbb{R}$, $E \sim \mathcal{N}(0, 1)$.
5. Truncated standard normal distribution with parameters σ and ξ numerically determined to ensure $\mathbb{V}(E) = 1$ and $\mathbb{P}_E(\Omega_E) = 1$:
 $\Omega_E = [-2\sigma, 2\sigma]$, $E \sim \xi \mathcal{N}(0, \sigma^2)$ ($E(\omega) \in \Omega_E$). This is experimentally more plausible than a full normal distribution.

Imprecision aberration

For all distributions, ζ_I is parametrically varied between $\zeta_I = 0.1$ and $\zeta_I = 4$. The parameter ξ in some of these examples is numerically determined to ensure that $\mathbb{V}(\omega_0) = 1$.

1. Continuous uniform distribution (platykurtic example): $\omega_0 \sim \mathcal{U}(-\sqrt{3}, \sqrt{3})$.
2. Standard normal distribution (mesokurtic example): $\omega_0 \sim \mathcal{N}(0, 1)$.
3. Student's t-distribution with $\nu = 5$ degrees of freedom (leptokurtic example):
 $\omega_0 \sim t_5$
4. Normal distribution with larger σ for smaller absolute values of E : for fixed $e \in \Omega_E$, $\omega_0 \sim \mathcal{N}(0, \xi \exp(-|e|))$
5. Normal distribution with larger σ for larger absolute values of E : for fixed $e \in \Omega_E$,
 $\omega_0 \sim \mathcal{N}(0, \xi |e|)$

6. Gamma distribution with positive skew for larger E : for fixed $e \in \Omega_E$,
 $\omega_0 \sim e \left(\Gamma \left(2, 1/\sqrt{2} \right) \right) - \mathbb{E} \left(\Gamma \left(2, 1/\sqrt{2} \right) \right)$
7. Gamma distribution with positive skew for smaller E : for fixed $e \in \Omega_E$,
 $\omega_0 \sim -e \left(\Gamma \left(2, 1/\sqrt{2} \right) \right) - \mathbb{E} \left(\Gamma \left(2, 1/\sqrt{2} \right) \right)$

Residual non-linearity

For the following mappings, the parameter ξ was numerically determined to ensure that $Cov(E, f_R(E)) = 0$. For each of the two types of non-linear mappings, we implemented a version with smaller and with larger deviation from linearity, i.e. $\mathbb{V}(f_R(E))$.

1. No non-linearity: $f(e) = 0$
2. Resulting sigmoid mapping: $f(e) = \xi e^{1/3} - e$, and $f(e) = \xi e^{1/5} - e$
3. Resulting inverse sigmoid mapping: $f_0(e) = \xi e^3 - e$, and $f_0(e) = \xi e^5 - e$

Measurement error

Because measurement error is not usually known a priori, and can depend on the other model quantities in complicated ways, all simulations use a placeholder error distribution that represents i.i.d. Gaussian noise: $\varepsilon \sim \mathcal{N}(0, \sigma^2)$ with $\sigma \in \{0.1, 1, 4\}$.

Simulation settings

All simulations used Matlab's default Mersenne twister to generate random numbers. We simulated distributions by drawing 10^7 samples. Asymptotic results are based on all samples. For finite-sample results, we randomly selected (with replacement) from these simulated distributions 10^6 samples of size $n = 50$ and computed the resulting variance of the sample retrodictive validity estimator.

Results

Analytical results

In the following we analyse the five terms from eq. (36), keeping in mind our goal of minimising $\mathbb{V}(r_{EY})$. Figure 2 illustrates the different quantities.

Weighted kurtosis of the intended value distribution.

$$A_1 := \mathbb{E}(E^4) \frac{1}{4} (\rho_{EY}^2 - 2\rho_{EY}^4 + \rho_{EY}^6) \quad (38)$$

$$= \frac{1}{4} Kurt(E) (\rho_{EY}^2 - 2\rho_{EY}^4 + \rho_{EY}^6) \quad (39)$$

Both factors in this term are non-negative on $\rho_{EY} \in [0, 1]$. The first factor is the kurtosis of the intended value distribution, which is under experimental control, and reducing it will always reduce A_1 . The second factor has minima at $\rho_{EY} = 0$ and $\rho_{EY} = 1$, and a maximum at $\rho_{EY} = 1/\sqrt{3} \approx 0.577$, corresponding to $\mathbb{V}(\omega) + \mathbb{V}(\varepsilon) = 2$. This means that reducing $\mathbb{V}(\omega)$ can increase or reduce this term. The maximum value of A_1 on the interval $\rho_{EY} \in [0, 1]$ is $\frac{1}{27}\mathbb{E}(E^4)$. While kurtosis is theoretically unbounded, high-kurtosis distributions of E appear unnecessary in practice, and for a plausible normal distribution of E , $A_1 \leq \frac{1}{27} \approx 0.11$. Thus, this term makes a relatively modest contribution to $\mathbb{V}(r_{EY})$. Figure 2A illustrates different distributions of E and shows the associated kurtosis values.

Weighted kurtosis of the aberration distribution.

$$A_2 := \mathbb{E}(\omega^4) \left(\frac{1}{4} \rho_{EY}^6 \right) \quad (40)$$

$$= \frac{1}{4} Kurt(\omega) \left(\frac{\mathbb{V}^2(\omega)}{(1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon))^3} \right) \quad (41)$$

Both factors in this term are non-negative on $\rho_{EY} \in [0, 1]$. Reducing aberration

kurtosis will thus always reduce A_2 , although in practice this may not be under experimental control (Figure 2BC). The second factor has a minimum at $\rho_{EY} = 0$, and, when considered as a function of $\mathbb{V}(\omega)$, a maximum at $\mathbb{V}(\omega) - 2\mathbb{V}(\varepsilon) = 2$. In words, if the aberration is relatively large compared to the measurement error, then decreasing the aberration increases A_2 . If $\mathbb{V}(\omega)$ is below this bound, then decreasing the aberration decreases A_2 . This is the case for example if $\mathbb{V}(\varepsilon) = \mathbb{V}(\omega)$. Because $\mathbb{V}(\omega)$ also impacts on the other terms, its effect is analysed in detail in the numerical simulations below.

Weighted covariance of squared intended values and squared aberration.

$$A_3 := \mathbb{E} \left((E\omega)^2 \right) \left(\rho_{EY}^2 - \frac{5}{2}\rho_{EY}^4 + \frac{3}{2}\rho_{EY}^6 \right) \quad (42)$$

$$= \mathbb{E} \left(E^2 \omega_0^2 \right) \left(\frac{\mathbb{V}(\omega)}{1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon)} - \frac{5\mathbb{V}(\omega)}{2(1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon))^2} + \frac{3\mathbb{V}(\omega)}{2(1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon))^3} \right). \quad (43)$$

The first factor is non-negative. If E and ω are stochastically independent, or if there are just two equiprobable values of E , then $\mathbb{E}(E^2\omega^2) = \mathbb{V}(\omega)$ and the first factor equals 1. In other cases, the first factor is large when ω takes larger absolute values for more extreme values of E (Figure 2DE). The second factor is non-negative on $\mathbb{V}(\omega) \in \mathbb{R}^+$ and its derivative is positive if $\mathbb{V}(\varepsilon) > 1/2$. If $\mathbb{V}(\varepsilon) \leq 1/2$, then this factor is negative for $\mathbb{V}(\omega) < 1/2 - \mathbb{V}(\varepsilon)$. Furthermore, it has a negative derivative on an interval $[0, \mathbb{V}_0(\omega)]$; the maximum value of $\mathbb{V}_0(\omega)$ is achieved when $\mathbb{V}(\varepsilon) = 0$ which yields $\mathbb{V}_0(\omega) \approx 0.29$. On this interval, decreasing $\mathbb{V}(\omega)$ increases A_2 . To see whether this latter situation is practically relevant, assume that $\mathbb{V}(\varepsilon) = \mathbb{V}(\omega)$. In this case, $\mathbb{V}(\varepsilon) = 0.5$ corresponds to an effect size of $\rho_{EY} = 1/\sqrt{2} \approx 0.71$, or Cohen's $d \approx 2.0$. Until now, such effect sizes have not yet been achieved in calibration experiments. The impact of changing $\mathbb{V}(\omega)$ is analysed in detail in the numerical simulations below.

Weighted systematic aberration.

$$A_4 := \mathbb{E} \left(E^3 \omega \right) \left(\rho_{EY}^2 - 2\rho_{EY}^4 + \rho_{EY}^6 \right) \quad (44)$$

$$= \mathbb{E} \left(E^3 \omega_0 \right) \left(\frac{\sqrt{\mathbb{V}(\omega)}}{1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon)} - \frac{2\sqrt{\mathbb{V}(\omega)}}{(1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon))^2} + \frac{\sqrt{\mathbb{V}(\omega)}}{(1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon))^3} \right) \quad (45)$$

The first factor can be zero, positive or negative. It is zero if there are two or three equidistant values of E , or if $f_R = 0$. In the first case, $E \in \{-1, +1\}$ and thus $E^3 = E$. Hence, $\mathbb{E}(E^3 \omega) = \mathbb{E}(E \omega) = 0$. In the second case, $E \in \{-\sqrt{3/2}, 0, +\sqrt{3/2}\}$ and thus $E^3 = 3/2 E$. Hence, $\mathbb{E}(E^3 \omega) = 3/2 \mathbb{E}(E \omega) = 0$. In the third case,

$$\mathbb{E} \left(E^3 \omega \right) = \mathbb{E} \left(E^3 \omega_I \right) = \int_{\Omega_E} d\mathbb{P}_E E^3 \int_{\mathbb{R}} d\mathbb{K}_{\omega_I}^E(e, dt) \omega_I = 0. \quad (46)$$

Otherwise, the first factor is positive when ω takes more extreme values for more extreme values of E (e.g. inverse sigmoid mapping from E to T , figure 2F), and negative in the opposite case (e.g. sigmoid mapping from E to T , figure 2G). The second factor is non-negative and its derivative is positive on an interval $[0, \mathbb{V}_0(\omega)]$; the minimum value of $\mathbb{V}_0(\omega)$ is achieved when $\mathbb{V}(\varepsilon) = 0$ which yields $\mathbb{V}_0(\omega) = 5$. It is plausible to assume that $\mathbb{V}(\omega)$ will rarely be larger than this value. The impact of changing $\mathbb{V}(\omega)$ then only depends on the first factor and is analysed in detail in the numerical simulations below.

Weighted skewness of the aberration.

$$A_5 := \mathbb{E} \left(E \omega^3 \right) \left(-\rho_{EY}^4 + \rho_{EY}^6 \right) \quad (47)$$

$$= \mathbb{E} \left(E \omega_0^3 \right) \left(-\frac{\mathbb{V}^{3/2}(\omega)}{(1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon))^2} + \frac{\mathbb{V}^{3/2}(\omega)}{(1 + \mathbb{V}(\omega) + \mathbb{V}(\varepsilon))^3} \right). \quad (48)$$

The first factor is positive if ω is positively skewed for large(r) values of E and negatively skewed for small(er) values of E (see figure 2HI). For two values of E , this would mean that the distribution of ω has long tails into both directions, but fewer values in the middle. For the opposite situation, the first factor will be negative. If ω has no skew for any value of E , then this term will be zero. The second factor is negative, and its derivative is negative on an interval $[0, \mathbb{V}_0(\omega)]$; the minimum value of $\mathbb{V}_0(\omega)$ is achieved when $\mathbb{V}(\varepsilon) = 0$ which yields $\mathbb{V}_0(\omega) = 5$. It is plausible to assume that $\mathbb{V}(\omega)$ will rarely be larger than this value. The impact of changing $\mathbb{V}(\omega)$ then only depends on the first factor and is analysed in detail in the numerical simulations below.

Numerical results

Because several of the analytically identified terms depend on aberration in different ways, we numerically simulated various scenarios to quantify their impact on estimator variance. Figure 3 shows simulated asymptotic results, where rows refer to different distributions of imprecision aberration ω_I , and columns to different systematic aberrations, and levels of measurement error ε . Using the same layout, Figure 4 shows simulated results from finite samples with size $n = 50$.

We first consider estimator variance in the absence of systematic aberration (left three columns in figures 3 and 4). When ω_I is independent of the intended values E (top three rows), the tested distributions of E yield relatively similar estimator variance under different distributions of ω_I . This confirms the analytically derived result that although the kurtosis of E does affect A_1 , the numerical impact on estimator variance is relatively small. Furthermore, reducing the scale of imprecision aberration can reduce estimator variance considerably, but only for low to medium levels of measurement error, i.e. when the error variance is on a lower or same order of magnitude as the variance of E . In high-noise scenarios, when measurement error variance is on a larger order than E , reducing aberration only has a small effect on estimator variance.

Next, we consider conditional distributions of ω_I that do depend on E . Here, different choices of E yield more dissimilar estimator variance. In most situations, binary E yields the smallest, and normally distributed E the largest estimator variance, with a negligible impact of truncating E . In most circumstances, reducing the scale of ω_I reduces estimator variance. However, in the specific case that ω_I inversely scales with the absolute values of E (fourth row), these results are reversed: binary E yields the largest estimator variance, and reducing the scale of ω_I can increase estimator variance, in particular if measurement error is large. For small and medium levels of measurement noise, this effect is mainly due to term A_2 , as anticipated in the analytical results. However, A_2 has hardly an impact at high aberration. Here, the increase in estimator variance is due to an interplay of several factors, including the term f_ε . At high levels of measurement noise, this term generally increases with decreasing ω_I . We note that in the absence of prior knowledge on the distribution of measurement error, we assumed i.i.d. normally distributed error for the simulations. Insofar as some of these results depend on f_ε , they may not generalise.

In the presence of sigmoid aberration, the results look very similar (middle three columns), and there is almost no impact of the variance of the systematic aberration. However, inverse sigmoid aberration changes this picture (right three columns) in several important ways. First, asymptotic estimator variance becomes very large for large systematic aberration when E is a normal distribution and the scale of ω_I is small. This is due to term A_2 . Our implementation of inverse sigmoid aberration is a cubic function, and the kurtosis of the cube of a standard normally distributed variable corresponds to the value of its 12th moment which is $11!! = 2079$. This excessive kurtosis is reduced by adding random noise to ω ; hence the phenomenon is particularly pronounced for small scale of ω_I and ε . However, kurtosis is not as excessive for any truncated E or in small samples (see figure 4) and this phenomenon therefore be irrelevant for practical purposes. The other three differences apply both for asymptotic and finite-sample cases. Secondly, estimator variance is generally much larger for (truncated) normal distribution of E than for the

discrete and continuous uniform distributions, due to an increase of the term A_3 . Third, the scale of systematic aberration has a larger impact of estimator variance than for sigmoid aberration, again due to an impact on term A_3 . Fourth, there are more scenarios in which decreasing the scale of ω_I increases estimator variance.

Discussion

In this note, we investigated factors that determine the sample variance of retrodictive validity estimators (Bach et al., 2020). To make our analytical calculations tractable, we constrained our focus to independently randomised (rather than balanced) experimental treatments, which is a plausible experimental approach for between-subjects studies. We considered the typical case of a discrete number of treatments, as well as parametric treatments that are uniformly sampled from a given interval. We also considered parametric treatments drawn from a normal distribution, as well as the experimentally more plausible case of a truncated normal distribution. These treatments define intended true score values E , which were analysed under various scenarios involving different distributions of imprecision and systematic aberration, and different levels of measurement error. Our analytical results are based on previous work of asymptotic limits for sample covariance estimators (Steiger & Hakstian, 1982), and thus are valid for large samples. Based on these analytical results, we choose distributions that are numerically simulated to derive an estimate of observed estimator variance in finite samples.

Our first result is that in the absence of systematic aberration and when imprecision aberration is i.i.d., then the choice of E has no large impact on estimator variance. Secondly, however, in many other situations estimator variance increases from binary to discrete to continuous uniform to normally distributed E . Third, sigmoid systematic aberration has no large impact on estimator variance. Fourth, inverse sigmoid aberration (in the simulated case, a cubic transform), had a very large impact on estimator variance. Finally, reducing imprecision aberration reduces estimator variance when imprecision

aberration is i.i.d. and in the absence of inverse sigmoid aberration, and also in many other but not all scenarios.

From these results, several recommendations for the design of calibration experiments can be derived. First, it may be preferable to use a relatively small number of discrete experimental treatments, as is usually the case in many fields of experimental psychology. Binary treatments fare particularly well, but in advanced experimental fields, a goal may be to calibrate a measurement across a range of values and to include a possible impact of systematic aberration, which would motivate the use of several discrete values. In case a continuous distribution is desired, then normal distributions however, desirable as they are for simplified statistical computations, should be avoided in favor of a continuous uniform distribution. A second recommendation relates to aberration and the use of resources. Reducing aberration is obviously desirable in its own right, but it may be costly if it involves specific experimental efforts such as shielded testing rooms, prolonged resting periods, monitoring subjects over extended periods of time, etc. Our results suggest that reducing systematic aberration may be particularly worthy of investment when it is inversely sigmoid, i.e. when systematic aberration is particularly large for extreme values of E . In all scenarios considered, reducing inverse sigmoid systematic aberration reduced estimator variance across the board. Additionally, reducing imprecision aberration is useful in most scenarios but not when measurement noise is relatively high, and not under some particular aberration distributions. Since these distributions are not a priori known to the calibration planner, if there are limited resources available they may be better invested into increasing sample size, which uniformly and linearly decreases estimator variance.

Our results are necessarily limited to the choice of scenarios we analysed. To motivate these scenarios, we analytically derived distribution features that have a large impact on estimator variance, and then simulated distributions with extreme values of these features. In doing so, we may have missed out on some other distributions that are possibly more relevant in particular fields of experimental psychology. Our simulations are

openly available and can easily be expanded to encompass other situations. Furthermore, our analysis does not cover the case of balanced experimental treatments, which is particularly important for within-subjects designs, but analytically intractable. Future work will explore this experimental design in a purely numerical approach.

In order to analyse continuous distributions of E , we extended our previous calibration model (Bach et al., 2020) and put it on a sound probability-theoretic footing. We hope that this generalisation provides a useful tool for further analysis of calibration experiments, beyond the specific questions asked here.

As a general limitation of the calibration approach, and similar to the concept of construct validity (Cronbach & Meehl, 1955), it does not allow to empirically separate trueness (bias) and precision of the measurement (Bach et al., 2020). Although these can be distinguished in theory - just as we have distinguished systematic aberration (bias) from imprecision aberration - retrodictive validity collapses both into one accuracy metric. It would be desirable to distinguish these terms. One possibility is to additionally evaluate reliability, as a surrogate metric of precision once bias is established (Brandmaier et al., 2018). We note, however, that assessing reliability for experimental measurement can be non-trivial, as discussed previously (see supplemental information in Bach et al., 2020). Future work will address this experimental limitation and hopefully contribute to the derivation of uncertainty metrics in experimental measurement (Rigdon et al., 2020).

To summarise, estimator variance in calibration experiments can be reduced by using uniformly distributed, preferably discrete, experimental treatments; by reducing inverse sigmoid experimental aberration; and in many but not all circumstances by reducing imprecision aberration. If resources are limited, then the most predictable route to reducing estimator variance, under all experimental circumstances, remains increasing sample size.

References

- Bach, D. R., & Friston, K. J. (2013). Model-based analysis of skin conductance responses: Towards causal models in psychophysiology [Journal Article]. *Psychophysiology*, 50(1), 15-22. Retrieved from <http://www.ncbi.nlm.nih.gov/pubmed/23094650> doi: 10.1111/j.1469-8986.2012.01483.x
- Bach, D. R., & Melinscak, F. (2020). Psychophysiological modelling and the measurement of fear conditioning [Journal Article]. *Behav Res Ther*, 127, 103576. Retrieved from <https://www.ncbi.nlm.nih.gov/pubmed/32087391> doi: 10.1016/j.brat.2020.103576
- Bach, D. R., Melinscak, F., Fleming, S. M., & Voelkle, M. C. (2020). Calibrating the experimental measurement of psychological attributes [Journal Article]. *Nature human behaviour*.
- Brandmaier, A. M., Wenger, E., Bodammer, N. C., Kuhn, S., Raz, N., & Lindenberger, U. (2018). Assessing reliability in neuroimaging research through intra-class effect decomposition (iced) [Journal Article]. *Elife*, 7. Retrieved from <https://www.ncbi.nlm.nih.gov/pubmed/29963984> doi: 10.7554/eLife.35718
- Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests [Journal Article]. *Psychol Bull*, 52(4), 281-302. Retrieved from <https://www.ncbi.nlm.nih.gov/pubmed/13245896> doi: 10.1037/h0040957
- Lonsdorf, T. B., Klingelhofer-Jens, M., Andreatta, M., Beckers, T., Chalkia, A., Gerlicher, A., ... Merz, C. J. (2019). Navigating the garden of forking paths for data exclusions in fear conditioning research [Journal Article]. *Elife*, 8. Retrieved from <https://www.ncbi.nlm.nih.gov/pubmed/31841112> doi: 10.7554/eLife.52465
- Lonsdorf, T. B., Menz, M. M., Andreatta, M., Fullana, M. A., Golkar, A., Haaker, J., ... Merz, C. J. (2017). Don't fear 'fear conditioning': Methodological considerations for the design and analysis of studies on human fear acquisition, extinction, and return of fear [Journal Article]. *Neurosci Biobehav Rev*, 77, 247-285. Retrieved from

- <http://www.ncbi.nlm.nih.gov/pubmed/28263758> doi: 10.1016/j.neubiorev.2017.02.026
- Ojala, K. E., & Bach, D. R. (2020). Measuring learning in human classical threat conditioning: Translational, cognitive and methodological considerations [Journal Article]. *Neurosci Biobehav Rev*, 114, 96-112. Retrieved from <https://www.ncbi.nlm.nih.gov/pubmed/32343982> doi: 10.1016/j.neubiorev.2020.04.019
- Rigdon, E. E., Sarstedt, M., & Becker, J. M. (2020). Quantify uncertainty in behavioral research [Journal Article]. *Nat Hum Behav*, 4(4), 329-331. Retrieved from <https://www.ncbi.nlm.nih.gov/pubmed/31915371> doi: 10.1038/s41562-019-0806-0
- Silberzahn, R., Uhlmann, E. L., Martin, D. P., Anselmi, P., Aust, F., Awtrey, E., . . . Nosek, B. A. (2018). Many analysts, one data set: Making transparent how variations in analytic choices affect results [Journal Article]. *Advances in Methods and Practices in Psychological Science*, 1(3), 337-356. Retrieved from <https://doi.org/10.1177/2515245917747646> doi: 10.1177/2515245917747646
- Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant [Journal Article]. *Psychological Science*, 22(11), 1359-1366. Retrieved from <https://doi.org/10.1177/0956797611417632> doi: 10.1177/0956797611417632
- Steenen, S., Tuerlinckx, F., Gelman, A., & Vanpaemel, W. (2016). Increasing transparency through a multiverse analysis [Journal Article]. *Perspect Psychol Sci*, 11(5), 702-712. Retrieved from <https://www.ncbi.nlm.nih.gov/pubmed/27694465> doi: 10.1177/1745691616658637
- Steiger, J. H., & Hakstian, A. R. (1982). The asymptotic distribution of elements of a correlation matrix: Theory and application. *British Journal of Mathematical and Statistical Psychology*.

Figure 1. Illustration of the probabilistic set-up. Intended values e are randomly assigned according to a probability measure $\mathbb{P}_E$. In an experiment, a fixed intended value results in true score t according to a conditional probability measure $\mathbb{K}_T^E$. Given this true score, observation and data transformation lead to a measured score y according to another conditional probability $\mathbb{K}_Y^T$. The goal of the present note is to explore routes to reducing the sample variance of retrodictive validity, ρ_{EY} .

Figure 2. Illustration of the calibration-specific terms that impact on variance of retrodictive validity estimators. A: Kurtosis of the different intended value distributions used for numeric simulations. BC: Two aberration distributions with different kurtosis. B: Continuous uniform distribution (platykurtic example). C: Student's t-distribution with $\nu = 5$ (leptokurtic example). DE: Two conditional aberration distribution with different covariance of intended values and aberration. D: Normal distribution with larger σ for smaller absolute values of E . E: Normal distribution with smaller σ for larger absolute values of E . FG: Sigmoid and inverse sigmoid systematic aberration. HI: Two conditional aberration distribution with different skewness of the aberration. H: Gamma distribution with positive skew for larger E . I: Gamma distribution with negative skew for larger E .

Figure 3. Simulated asymptotic results, showing asymptotic estimator variance for 63 scenarios with different intended value distributions (colours), different imprecision aberration distributions (rows), different scale of imprecision aberration (x-axis), different systematic aberration (columns) and different levels of measurement error (columns).

Figure 4. Simulated finite sample results, showing observed estimator variance across 10^5 samples of size $n = 50$ for 63 scenarios with different intended value distributions (colours), different imprecision aberration distributions (rows), different scale of imprecision aberration (x-axis), different systematic aberration (columns) and different levels of measurement error (columns).