# Measurement Practices in Large-Scale Replications: Insights from Many Labs 2

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Abstract

Validity of measurement is integral to the interpretability of research endeavours and any subsequent replication attempts. To assess current measurement practices and the construct validity of measures in large-scale replication studies, we conducted a systematic review of measures used in *Many Labs 2: Investigating Variation in Replicability Across Samples and Settings* (Klein et al., 2018). To evaluate the psychometric properties of the scales used in *Many Labs 2* we conducted factor and reliability analyses on the publicly-available data. We report that measures in *Many Labs 2* were often short with little validity evidence reported in the original study, that measures with more validity evidence in the original study had stronger psychometric properties in the replication sample, and that translated versions of scales had lower reliability. We discuss the implications of these findings for interpreting replication results, and make recommendations to improve measurement practices in future replications.

*Keywords:* measurement, replication, construct validity, measurement invariance

Public significance statement: Valid measurement is a key aspect of conducting robust, reproducible research. Our review indicates that current measurement practices in original and replication studies are lacking rigour. We discuss the resulting implications and recommend steps that the field can take to improve the validity of measurement in original and replication research.

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## Introduction

Replication is a key tenet of the scientific process; it facilitates the accumulation of evidence for an effect (or lack thereof) across different contexts, and can thus curtail the proliferation of false results and prevent the waste of resources associated with pursuing "null fields" (Nosek, Spies, & Motyl, 2012; Sijtsma, 2016).

In the early 2010s, a series of high-profile events brought the replicability of psychological science under scrutiny: fraud cases like that of Diederik Stapel (Bhattacharjee, 2013), outlandish claims like the existence of extrasensory perception (Bem, 2011), concerns about the prevalence of false positives (Ioannidis, 2005; Simmons, Nelson, & Simonsohn, 2011) and questionable research practices (John, Loewenstein, & Prelec, 2012), and failed replications (e.g., Doyen, Klein, Pichon, & Cleeremans, 2012; Open Science Collaboration, 2015). This period of scrutiny prompted methodological and statistical introspection and reforms (Nelson, Simmons, & Simonsohn, 2018) like increasing use of pre-registration (Humphreys, Sanchez de la Sierra, & van der Windt, 2017), p-curving to assess the likelihood that results reflect selective reporting (Simonsohn, Nelson, & Simmons, 2014), adjusting significance standards (e.g., Benjamin et al., 2018), multiverse analyses (Steegen, Tuerlinckx, Gelman, & Vanpaemel, 2016), and improving reporting standards and data sharing (Sijtsma, 2016). Additionally, a number of large-scale replications are underway or have been published (e.g., Klein et al., 2014; Klein et al., 2018; Moshontz et al., 2018; Open Science Collaboration, 2015), allowing for tests of effects with large sample sizes and across heterogeneous contexts (e.g., language, country, time).

These reforms have potential to improve the state of psychological science, but the importance of measurement practices has been neglected, particularly within replications (Finkel, Eastwick, & Reis, 2017). Recent reviews of measurement properties in psychological research (e.g., Slaney, 2017) detail the neglect of measurement: authors over-emphasize Cronbach's alpha, create or adapt scales without providing or updating validity evidence, and report little evidence for factor structure and measurement invariance.

The goal of the current work is to assess measurement practices in large-scale replication studies by conducting a systematic review of the measures used in "Many Labs 2: Investigating Variation in Replicability Across Samples and Settings" (ML2; Klein et al., 2018). Prior to the review of measures, we highlight the importance of measurement practices in psychological research generally and replication studies specifically. Following the review of measures and their psychometric properties, we summarize our main findings and recommend measurement considerations for future replication studies.

**Measurement Matters**

Imagine that Alex, a physician, is interested in the relationship between bodyweight and heart rate amongst their patients. Alex assesses each patient's resting heart rate and then weighs the patient using a digital scale. After collecting data for dozens of patients, Alex realizes that the scale is broken: it shows random numbers that do not correspond to patients' weights. Alex needs to calibrate the scale to measure bodyweight accurately before modelling the relationship between bodyweight and heart rate. Said another way, Alex cannot investigate the relationship between bodyweight and heart rate without a proper measure of bodyweight. This example illustrates that measurement validity is integral to the quality of any given results. Without valid

scores from measures, it is unclear what exactly is being measured. A researcher cannot test their hypotheses involving a measured construct when there is uncertainty about what is being measured, because that uncertainty persists to affect results and conclusions.

Psychological research involves latent constructs (e.g., motivation, attitudes) which, unlike heart rate and bodyweight in the above scenario, are typically unobservable phenomena. Researchers develop measures to assess these phenomena, and construct validation is like the process of building and calibrating the weight scale: it provides evidence that justifies (or nullifies) the use of a given measure to assess a construct (Messick, 1995). The process of construct validation includes a diverse set of theoretical and methodological approaches that can be categorized into three stages: substantive, structural, and external (Loevinger, 1957; Messick, 1995). The substantive stage of construct validation concerns the underlying theory of a measure; based on previous research, what is the definition of the construct, and what must the measure include to accurately reflect the construct's dimensions? The structural stage concerns whether the structure of the measure reflects the structure of the construct, using psychometric tests like item and/or subscale intercorrelations and factor analyses. The external stage concerns whether scores on the construct covary with other constructs as expected. The three stages of construct validation should proceed in order, such that each stage must be established before the next to consolidate evidence in favor of using (scores of) a measure (Benson, 1998). We should not, for example, consider the covariance between a target construct and other constructs (external stage) without solid structural evidence, and cannot establish structural evidence before defining the construct and its structure in the substantive stage.

In psychology, we tend to use scales to measure a latent construct by observable indicators, such as items from which scores on variables are derived. Before investigating any substantive research questions, it is necessary to decide how the construct of interest will be scored and provide evidence – via construct validation – supporting that scoring. Moreover, construct validation is an ongoing process of accruing evidence supporting (or contesting) the use and interpretation of scores on a measure to represent a latent construct (Messick, 1995). If the operationalization of the measure in a study is altered, as can occur between original and replication studies, researchers should provide evidence that scores obtained with the new method still reflect the construct of interest.

The ML2 data provides a rich source of information about the psychometric properties of scales, and to our knowledge no secondary analyses on the structural validity of these scales have been conducted. To assess current measurement practices in large-scale replication studies and the psychometric properties of these scales, we will (a) assess the number and type of measures used in ML2, (b) review the psychometric (reliability and validity) information provided for scales, (c) use the publicly-available ML2 data to analyze reliability and validity of scales considering two potential sources of measurement variance (i.e., labs and languages), and (d) use these findings to recommend measurement practices for future replication studies.

## Method

In the following sub-sections, we detail the coding protocol used to assess the number and type of measures used in ML2 and then describe the procedures used to conduct factor and reliability analyses for scales. We define *scales* as measures for which items were used to capture a construct of interest in the same manner as Flake, Pek, and Hehman (2017). We focus on scales

because there is a rich literature documenting their psychometric evaluation with factor and reliability analyses, which we can easily evaluate here.

**Data Source and Documentation**

Our sample for this review was ML2 and its associated original articles (N = 26). ML2 tested the replicability of 28 classic and contemporary effects across 125 samples, 82 of which were outside of the United States and Canada, in 16 different languages. Our team of researchers read each replication study, recorded how many and what type of measures were used, and extracted any validity information reported in the original or replication study. We used the ML2 data available on the OSF (https://osf.io/fanre/) to conduct factor and reliability analyses. Our team's final coding sheet, data cleaning and analysis code, output, and other pertinent materials (e.g., a detailed coding guide and key) are hosted on the OSF (https://osf.io/v4wth/).

**Coding**

For each study in ML2, we coded the original and replication report for the number and type of measures, in addition to other information like reliability and validity evidence reported. We focused on measures used and described in the final replication report.

We instituted quality control mechanisms throughout the coding process to facilitate transparency and ameliorate the subjective aspects of our review. First, the lead and senior authors on the paper (MS; JKF) adapted a coding key from previous research (Flake, Davidson, Wong, & Pek, 2019) and piloted it using a sample of seven effects. Once we finalized the coding sheet structure, two other researchers (SE; RL) were trained using three studies.

After the initial piloting and training sessions, we began our major coding effort. The lead author (MS) coded all 28 effects, while two others coded one half each (SE: 1–14; RL: 15–28).

We first coded independently, then met to discuss major aspects of the coding: number of measures per effect, measure types, scale type and scoring (for applicable measure types), replication sample size, and reliability and validity evidence reported. We flagged any discrepancies that we deemed too difficult to clearly interpret and code. The lead and senior authors later reviewed the discrepancies and logged the resolution in a document available on the OSF. In some cases, we established a new coding rule as a result of a discrepancy; to check for consistent application of these rules, two authors (MS; RL) re-coded a random sample of five studies after the first major coding effort. There were no discrepancies between our re-code and original coding sheet, so we considered the original sheet finalized. Descriptive statistics for the finalized coding sheet were generated in R 3.6.0 using *dplyr* 0.8.3.

**Factor Analyses**

We evaluated the applied measurement model of each scale using confirmatory factor analyses (CFAs), where we defined the factor structure of each construct based on the scoring procedure applied in the replication analyses. For scales measuring different sub-constructs, resulting in separate variable scores, we specified one CFA model for each construct using the respective items to load on the latent factor. We only selected scales that measured a latent construct using the three indicator rule (Flora, 2018).

We conducted these analyses in R version 3.6.1 using the *lavaan* 0.6-5 package (Rosseel, 2012). We determined the estimator function to fit the models depending on the measurement scale of the items. If items had more than five ordered response options, they were treated as continuous and CFA models were fit on their covariance matrix using Maximum Likelihood estimation (Flora, 2018; Rhemtulla, Brosseau-Liard, & Savalei, 2012). Scales including

ordered-categorical (less than 5 response options) or binary items were fitted using the diagonally weighted least squares DWLS estimation method (Muthén, 1984), setting the estimator to WLSM . For scales with both continuous and ordinal items we used DWLS specifying the ordinal items in the model.

The unidimensionality of each scale was evaluated based on a recommended basic set of fit indices currently available (Kline, 2015). First, we determined the $X^2$ statistic as a measure of exact fit, where a model is rejected if $X^2$ (df) is significant at the $\alpha = .05$ level. However, this convention should not be binding because high powered tests over-reject equivalence and decisions based on retaining the null hypothesis are not sufficient evidence for the hypothesis to be true (Steiger, 2007). Therefore, we obtained the following descriptive fit indices to evaluate and compare models: the Root Mean Square Error of Approximation (RMSEA) and its 90% CI, taking the complexity of each model into account; the Comparative Fit Index (CFI) as an incremental fit index comparing each model to a null model; and the Standardized Mean Square Error of Approximation (SRMR) estimating the residuals between the covariance matrices of the model and the sample. As the generality of thresholds to determine model fit have been questioned (Yuan, 2005), we tentatively evaluated good fit using RMSEA < .05 (Browne & Cudeck, 1993), CFI > .95, and SRMR < .08 (Hu & Bentler, 1998).

**Reliability Analyses**

Cronbach's alpha values and their associated 95% confidence intervals were computed for unidimensional scales, in line with best practices for interpreting alpha (Iacobucci & Duhachek, 2003). These values were computed in R 3.6.1 using the *psych* package (version 1.8.12) with the cleaned datasets provided for each individual scale. Reverse-coding was

specified as per the ML2 analysis scripts. To compare reliability differences between samples and translated questionnaires, we computed scale reliabilities and confidence intervals for both total samples and individual labs.

## Results

### Summary of Measures

In ML2, 60 measures were used to test 28 original claims, with a mean of 1.88, a median of 2, and a mode of 2 (*SD* = 1.08) per study. Item-based scales were the most common type of measure, representing 43 (72%) of all measures. We define *item-based scales* as measures for which an item or items were used to capture a construct of interest, as in Flake et al. (2017). Of item-based scales, 33 (77%) were single-item measures, and the other 10 (23%) were composed of two or more items. The remaining measures were six (10%) one-item measures that captured a predicted behavior, three (5%) demographic measures, two (3%) tasks, and six (10%) other types of measures that were not easily categorized.

We categorized all measures as primary or secondary. Primary measures were dependent variables, independent variables, and covariates used to estimate the targeted replication effect. Secondary measures were those not used to estimate the targeted replication effect, like manipulation checks and measures used for supplemental or exploratory analyses. Of the 60 total measures, 37 (62%) were primary and 23 (38%) were secondary. Of the 43 item-based scales, 24 (56%) were primary and 19 (44%) were secondary measures. For the remainder of this review, we will focus on these item-based scales.

For all 43 item-based scales, the most common response format was a Likert scale (33; 77%), followed by dichotomous choice (8; 19%). Of the 10 scales with two or more items, the

most common response format was also Likert scale (7; 70%), and the mean number of items was 7.5, the median was 5.5, and the mode was 5 (*SD* = 6.33).

**Summary of Reported Validity Evidence**

We documented reliability and validity evidence reported in original studies with a focus on three types of evidence: scale source (i.e., did the original authors reference a source for the scale?), factor structure, and reliability. The published ML2 paper did not include scale source, factor structure, or reliability information for scales, but we used the publicly available ML2 data to conduct factor analyses and, for unidimensional scales, calculate reliability coefficients, which we report after the summary.

**Reporting of validity evidence in original articles.** Of the 43 item-based scales used in the original studies, authors reported eight (19%) with a source: seven of these scales had two or more items, and one scale had one item. Of the eight item-based scales with sources provided, at least one other form of evidence (i.e., reliability or factor structure) was reported for two (25%). Of the 35 scales without a source, one scale (3%) had at least one other form of evidence reported. Of all 43 item-based scales, original authors provided at least one form of validity evidence (source and/or reliability coefficient and/or validity evidence) for nine (21%); no scale was supported by all three forms of evidence. Of the 24 primary scales used to estimate the targeted replication effect, at least one form of validity evidence was reported for 7 (29%), and no validity evidence was cited for 17 (71%) (i.e., no source, no evidence of factor structure, no reliability estimate).

**Structural validity evidence from ML2 data: CFAs.** Of the 43 item-based scales, 10 had two or more items. Of those 10, the following six were eligible for CFA (i.e., authors assumed unidimensionality of the scale). We briefly describe each measure below.

1. Syllogism Scale (ML2 Study 3): a cognitive assessment scale using six items to measure higher-order reasoning. The items on this scale stem from prior research on syllogistic reasoning (Johnson-Laird & Bara, 1984; Zielinski, Goodwin, & Halford, 2010); there were easy, moderate, and difficult items, with two items for each level of difficulty. All six items were administered to participants in the replication study, but for each sample only the moderately difficult items (i.e., those answered correctly by more than 25% and less than 75% of participants) were used in analyses. To combine scores from samples in an overall analysis, it is assumed that all items equally reflect the construct (Holland, 1990); we tested this assumption in our model where all six items were specified as reflective of the construct measured by syllogism scale.
2. Moral Foundations Questionnaire (MFQ; ML2 Study 4): a measure of the degree to which individuals endorse five different moral foundations: harm, fairness, ingroup, authority, and purity. Fifteen items were administered to measure the five foundations (3 items per foundation), and the foundations were then grouped to measure two higher-order constructs: individualizing and binding moral foundations. The individualizing construct was measured using the 6 items of the harm and fairness sub-scales; the binding construct was measured using the 9 items of the ingroup, authority, and purity sub-scales. Scores on these constructs were determined by averaging their respective items into a composite score.

3. Disgust Sensitivity Scale (ML2 Study 8): a measure of individual differences in the experience of disgust towards aversive stimuli. The replicators used the 5-item Contamination-Based Disgust subscale of a longer 25-item disgust scale (Olatunji et al., 2007). Disgust sensitivity scores were determined by taking the mean of the five items.
4. Scale of Subjective Well Being (ML2 Study 12): a composite scale of the 5-item Satisfaction With Life Scale (SWLS; Diener, Emmons, Larsen, & Griffin, 1985), and the 20-item Positive and Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988). Subjective Well Being is considered to be a unidimensional construct scored by averaging the SWLS scores with the PANAS positive affect and the reverse-scored PANAS negative affect scores. The scales differ in their response format, 5-point versus 7-point Likert, so the scoring of the scale was adapted by standardizing the three measures, adding the SWLS and PANAS positive affect items together, subtracting the PANAS negative affect items, and averaging the score.
5. Leader power scale (ML2 Study 15): a measure of leader power with five items rated on a 7-point Likert scale. The dependent measure is the average of participants' responses on the 5 items.
6. Cleaning desire scale (ML2 Study 23): a scale developed by the authors of the original study (Zhong & Liljenquist, 2006) asking participants to rate their desire of five cleaning products and five control products on a 7-point Likert scale. Desire for cleaning products and desire for control products are assumed to be two separate unidimensional constructs, each calculated by averaging the ratings of the five items in each product group.

We conducted analyses on the psychometric properties of these six eligible scales, measuring eight different constructs. Results from the CFAs testing the unidimensionality of the constructs, as assumed in the replication, on the replication sample are presented in Table 1. Furthermore, we report the reliability coefficients for the scales in the overall dataset.

No CFAs met all three criteria for good fit (i.e., RMSEA < .05, CFI > .95, SRMR < .08). No scales met the RMSEA criterion, the Individualizing Moral Foundation scale met the CFI criterion, and all scales except the scale of Subjective Well Being met the SRMR criterion.

**Reliability.**

Reliability estimates for each scale are summarized in Table 2 and shown by lab for one scale in Figure 1. Figures showing reliability estimates by lab for the remainder of the scales are available in the supplemental materials. Overall, reliability was lower for labs using translated versions of a scale than labs using the original version.

Table 1.

*Summary of Confirmatory Factor Analyses*

| Scale | Items | Type | α | *n* | $X^2$ | df | CFI | RMSEA/90% CI | | | SRMR |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | Lower | Upper | |
| Syllogism Scale | 6 | Binary | 0.423 | 6935 | 208.186 | 9 | 0.919 | 0.056 | 0.05 | 0.063 | 0.066 |
| *Moral Foundations Questionnaire* | | | | | | | | | | | |
| Individualizing | 6 | 6-Point | 0.822 | 6970 | 431.193 | 9 | 0.966 | 0.082 | 0.076 | 0.089 | 0.028 |
| Binding | 9 | 6-Point | 0.783 | 6966 | 1645.311 | 27 | 0.877 | 0.093 | 0.089 | 0.097 | 0.047 |
| Disgust | 5 | Mixed | 0.485 | 7041 | 126.215 | 5 | 0.946 | 0.059 | 0.05 | 0.068 | 0.045 |
| Well Being | 25 | Mixed | 0.838 | 6882 | 130518.092 | 275 | 0.616 | 0.262 | 0.261 | 0.264 | 0.267 |
| Leader Power | 5 | 7-Point | 0.857 | 7890 | 1444.501 | 5 | 0.92 | 0.191 | 0.183 | 0.199 | 0.04 |
| *Desire* | | | | | | | | | | | |
| Cleaning | 5 | 7-Point | 0.773 | 7001 | 1066.478 | 5 | 0.889 | 0.174 | 0.165 | 0.183 | 0.059 |
| Control | 5 | 7-Point | 0.502 | 7001 | 260.456 | 5 | 0.873 | 0.085 | 0.077 | 0.094 | 0.036 |

Table 2.

*Summary of Reliability Estimates*

| Scale | Items | Untranslated α | Translated α | α Range |
|---|---|---|---|---|
| Syllogism Scale | 6 | 0.44 [0.42, 0.47] | 0.39 [0.36, 0.42] | [0.00, 0.55] |
| MFQ - Individualizing | 6 | 0.83 [0.82, 0.84] | 0.81 [0.80, 0.82] | [0.45, 0.91] |
| MFQ - Binding | 9 | 0.80 [0.79, 0.81] | 0.75 [0.74, 0.77] | [0.51, 0.86] |
| Disgust | 5 | 0.51 [0.48, 0.53] | 0.44 [0.41, 0.47] | [0.19, 0.66] |
| Well Being | 25 | 0.85 [0.84, 0.86] | 0.85 [0.84, 0.86] | [0.63, 0.90] |
| Leader Power | 5 | 0.87 [0.86, 0.87] | 0.84 [0.82, 0.85] | [0.63, 0.97] |
| Desire - Cleaning | 5 | 0.77 [0.76, 0.78] | 0.74 [0.72, 0.76] | [0.00, 0.83] |
| Desire - Control | 5 | 0.49 [0.47, 0.51] | 0.52 [0.48, 0.55] | [0.15, 0.70] |

*Note.* Cronbach's α point estimates are accompanied by 95% CIs.

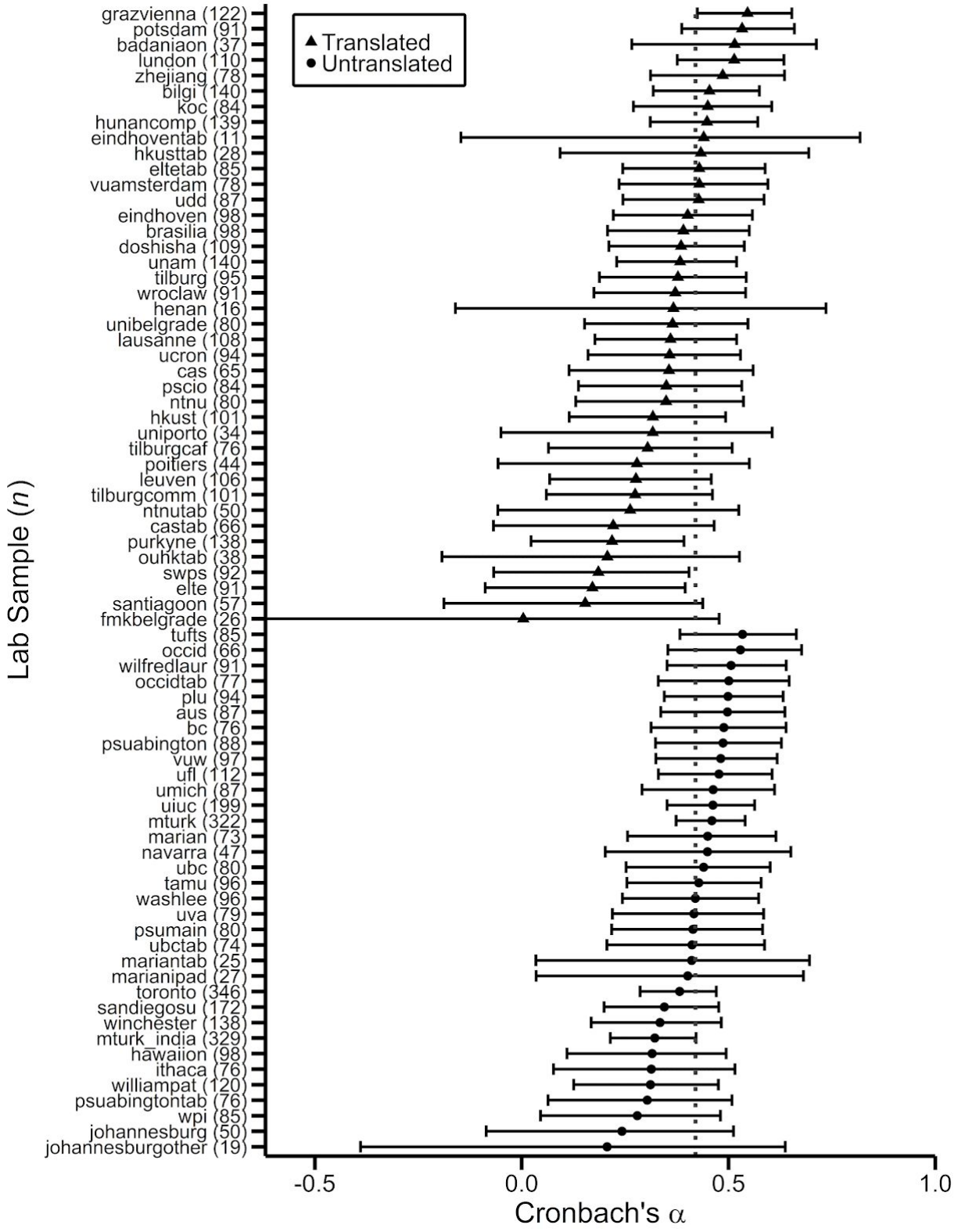


*Figure 1*. Lab-wise reliabilities for the syllogism scale. The dotted line shows overall sample α.

## Discussion

In this review, we reported the results of a systematic evaluation of the measurement properties of scales used in a large-scale replication study. The researchers of ML2 examined an array of effects in heterogeneous contexts and the resulting data are rife with measures for primary and follow-up analyses, with original and translated measures, and in some cases adapted for local contexts. Our results indicate that replicators need to consider measurement validity and invariance, from selection of studies through to scale use and interpretation. Many short scales were used with little validity evidence originally and – to our knowledge – no sample-specific validation. When we conducted CFAs using the public ML2 data, the models fit poorly and reliability varied by scale translation and lab sample size. In the sections that follow, we expand on the key findings from our review and their implications for interpreting replication results, comment on positive practices employed in ML2, and provide suggestions to improve measurement practices in future original and replication research.

### Preponderance of Short Scales

ML2 mostly consisted of short studies so that participants could feasibly complete many of them in a testing session. This design favoured brief measures; the majority of scales used in ML2 had one item (77%). However, one-item scales have limitations. Construct validity assumes that a measure captures all aspects of a proposed trait (Loevinger, 1957; Messick, 1995). For example, if you assume that personality is composed of the Big Five traits (openness to experience, conscientiousness, extroversion, agreeableness, neuroticism), then your measure of personality should assess all five of those aspects of personality. A single-item measure *may* accurately capture a construct with one dimension. However, whether a single-item measure

*does* accurately capture a construct – especially a complex, multidimensional construct – must be carefully considered (e.g., Diamantopoulos, Sarstedt, Fuchs, Wilczynski, & Kaiser, 2012; Grapentine, 2001; Robins, Hendin, & Trzesniewski, 2001). Construct validity needs to be ascertained, not assumed, and only one one-item measure in ML2 provided any validity evidence. As such, the use of so many one-item measures to calculate replication (and original) effect sizes leaves many questions unanswered regarding the adequacy of those measures and ultimately the validity of the replication conclusions.

We offer two recommendations to original authors and replicators with respect to scale length. First, construct complexity should inform scale complexity. We are sensitive to the time constraints in large-scale replications like ML2, and reiterate that single-item measures could be used to assess simple, unidimensional constructs. However, complex and multidimensional constructs will require longer scales to map the entire construct domain. Second, scale validation – or at least providing compelling pre-existing evidence for the validity of a scale in a given context – is mandatory for an original or a replication study, regardless of scale length.

**Factor Structure and Existing Validity Evidence**

Latent variable measurement accounted for many of the measures used in ML2; 72% of all measures were item-based scales. However, validity evidence supporting the use and interpretation of these latent variable measures was sparse. Among original articles, 19% of the scales had a cited source, with the other 81% ostensibly author-developed. Twenty-one percent reported at least one form of validity evidence. Of the scales used to estimate the targeted replication effect, 29% reported at least one form of evidence.

Evidence is essential to ensuring valid scale use; without evidence, one runs the risk of using scores in poorly-justified ways that do not represent the construct under study. For example, the poorest fitting CFA was that for the Subjective Well-Being scale. This was not surprising, because the composite scale comprised two questionnaires measuring separate constructs, namely satisfaction with life (SWL scale) and positive and negative affect (PANAS). Results provided evidence that items on this scale do not reflect one underlying construct and thus item responses should not be aggregated into a total score. Moreover, scales that were specifically developed for the original study but not validated by the original authors had lower fit indices when we evaluated them for the replication sample (Leader Power and Desire scales). In contrast, scales with more published validity evidence from the original study or elsewhere (MFQ and Disgust scales) had better fit indices. This is consistent with results of other reviews; scales with more published evidence perform better when evaluated in a new sample (Hussey & Hughes, 2019).

We reiterate our recommendation that future original authors and replicators evaluate and collect construct validity evidence for all scales for their sample and context. Original or replication studies without construct validity result in ambiguous evidence for whatever was under study. Given this, replicators should consider measurement when choosing studies to replicate, with a preference for studies with strong validity evidence. Original studies with weak validity evidence suggest that theories and measures need further development, whereas the goal of replicators may be to verify developed theories. If replicators opt for studies with little/no validation of measures, they may use replication resources to develop those measures and gather construct validity evidence. This broadens the scope of the replication, and results could suggest

poor validity evidence for measures and preclude conducting the replication in full. Even if replicators find strong validity evidence in the replication sample, the lack of validity evidence in the original study remains, limiting the comparison of the replication to the original study. Moreover, we note that we focused on item-based scales because of the existence of fairly straightforward and well-developed psychometric models for validating them. However, other types of measures (e.g., tasks, reaction times) also require validation. Focusing on these other measures is an important area for future review work and development.

**Variation and Interpretability**

In addition to original measures lacking validity evidence, the replication authors also did not report validity evidence supporting scale use in a variety of heterogeneous contexts. Construct validity is an ongoing process of accruing evidence supporting (or contesting) the use and interpretation of scores on a measure to represent the latent construct (American Educational Research Association, American Psychological Association, & National Council on Measurement in Education, 2014; Messick, 1995). Existing general evidence for the construct validity of a given measure may lack specific evidence for the relevance and utility of the measure in a new setting; said another way, construct validity may be sensitive to contextual factors (e.g., time, language, culture; Messick, 1995). The potential contextual sensitivity of a measure is consequential for replication studies, as failing to account for it may introduce measurement non-invariance (i.e., measurement heterogeneity) and complicate the interpretation of replicated effects. Measurement invariance is a property implying that an instrument measures the same construct the same way across various sub-groups of respondents (Horn & McArdle, 1992). It is integral to the interpretability of replication results; without measurement invariance,

the ability to compare different groups on a measure is limited (Horn & McArdle, 1992; Van De Schoot, Schmidt, De Beuckelaer, Lek, & Zondervan-Zwijnenburg, 2015). To conduct a replication and compare different samples (that of the original study and that of the replication) on a measure, you must consider measurement invariance (Fabrigar & Wegener, 2016). The extent to which measurement invariance affects the interpretability of replication results is ambiguous, and an important area for future research.

Issues of measurement invariance are especially pertinent for a large-scale replication like ML2, which used heterogeneous samples to estimate meta effect sizes. Two potential sources of measurement heterogeneity in ML2 are different testing contexts and modified procedures compared to original. Samples in ML2 varied from one another in a number of ways: Data were mostly collected online but occasionally in person, from labs in 36 different countries, in 16 different languages. Moreover, the procedure was changed in some way between original and replication study for 26 of 28 studies overall, though most of those changes involved moving an in-lab experiment online. Here we focus on scales, which were modified between the original and replication for two of the six studies for which we fit CFAs. We describe one modification here and we detail the other in our supplemental materials. The Disgust scale used by the original authors comprised eight items adapted from a larger 32-item scale reflecting seven domains of disgust (Haidt, McCauley, & Rozin, 1994). A 25-item scale reflecting three factors was developed in subsequent scale validation research (Disgust Scale–Revised [DS–R]; Olatunji et al., 2007). The original authors suggested the replicators use the DS–R, but given time constraints replicators used the 5-item Contamination-Based Disgust subscale from the DS–R. In the end, only two items measuring disgust sensitivity overlapped between the original and

replication studies. While it is possible that measurement invariance holds across such sources of potential variance, it is also possible that it does not hold. Without evidence of measurement invariance in the specific conditions a scale is used in, the ability to unambiguously interpret the replication results is undermined (Horn & McArdle, 1992). Measurement non-invariance can occur to different degrees. Moving forward, we need methodological research that ascertains how measurement heterogeneity compromises comparisons of effect sizes and the estimation of meta effects in studies like ML2.

We recommend that future original authors and replicators modify measures with caution. When a measure is modified throughout the original literature and then again in the replication study, we cannot disentangle effect heterogeneity from measurement heterogeneity. Replicators must at least acknowledge using different scales from original research when interpreting their results. Even when the same scale is used, it is important to test for measurement invariance, particularly when the scale is translated or used in a substantially different context for which it was developed. One downstream consequence of incorporating measurement invariance testing into replications is sample size planning, as larger samples are required for tests of measurement invariance than those for most tests of statistical significance (e.g., Mundfrom, Shaw, & Ke, 2005; Wolf, Harrington, Clark, & Miller, 2013).

**Reliability Evidence and Interpretation**

The original ML2 analysis did not report reliability evidence for overall scales, between translated and untranslated scales, or within labs. We conducted reliability analyses using the publicly available ML2 data. We noted reliability differences across translations: Reliability was lower and more variable in labs using translations than labs not using translations. This

corresponds to the lower sample sizes that were recruited in labs using translated scales, which may have resulted from resource limitations. As discussed above, most of the scales did not demonstrate evidence of unidimensionality, which may suggest unstable psychometric properties across contexts. The heterogeneity of reliability estimates may further suggest differences in interpretation of the scales across translations (i.e., measurement non-invariance), exacerbating concerns about aggregating and generalizing results across labs.

In the current data, reliability estimates for the overall samples could be perceived as acceptable for five of the eight scales by conventional rules of thumb (e.g., Cronbach's alpha exceeding 0.70; Nunally, 1978). However, the context sensitivity discussed above also applies to reliability. For every scale we examined, there were lab reliability estimates that were lower than the overall sample reliability, and at least one lab that showed reliability far below common rules of thumb. Acceptable reliability of an overall sample or scale does not necessarily imply that the scale is acceptably reliable for its specific application or that conclusions can be made across settings.

Many reliability estimates were also largely imprecise due to low lab sample sizes, which severely limited the evidence that could be acquired through reliability analysis. Some labs recruited a very small number of participants. As a result, the associated 95% C.I. for alpha were large and produced out of range negative lower bound values. For example, for the Syllogism Scale in Figure 1, multiple labs with sample sizes as low as 11 participants demonstrated large and out-of-range lower bound values. Indeed, most scales showed substantial variability in both point and interval estimates of reliability across labs due to variable sample sizes, regardless of translation. Furthermore, we could not conduct significance tests of Cronbach's alpha due to low

lab sample sizes, resulting in insufficient statistical power. Statistical error rates would be further inflated by making multiple comparisons across the large number of labs.

To address issues of precision and comprehension in reliability analyses, we recommend that replicators obtain larger sample sizes within labs and that both original and replication authors collect multiple forms of reliability evidence. Larger sample sizes are essential for both ensuring measurement invariance (in this case, across labs and languages) and conducting robust reliability analyses. As discussed above, ML2 used many short scales. Our analyses were limited to multi-item scales because data required to compute other forms of reliability (e.g., test-retest reliability) were not collected as part of ML2. Additionally, we reiterate our recommendation that replicators consider measurement when choosing studies to replicate, as many of the original studies had exhibited no or poor reliability evidence. Given the significant resources expended for large-scale replications, we should be mindful of the studies we select and either favour studies that employ measures with acceptable psychometric properties, including reliability, or plan to conduct the necessary construct validation to support the interpretation of the replications.

**Benefits of Open Data**

As discussed at length above, the ML2 replication project included many original studies that incorporated measures with little or no validity evidence, and the evidence of those measures was not further evaluated as a part of the replication effort. However, making all data openly available makes it possible for us to retroactively conduct construct validation. Using these data, we were able to assess preliminary reliability and validity evidence of multiple scales with larger samples than the original studies could achieve. Over time, as public data related to the same

scales accumulate, similar post-hoc validity testing can be conducted to gather evidence for scales in use. Furthermore, if studies are designed with large enough sample sizes, these public data can be used to conduct measurement invariance tests and large, rigorous, cross-cultural validation studies on a scale unprecedented in psychology.

**Solutions, Summarized**

Though our review points to a neglect of construct validity in original and replication studies, there are clear steps forward. We recommend the following measurement considerations be incorporated into original and replication projects to bolster the interpretability and robustness of findings. These considerations should be applied throughout the project, from selecting which studies to conduct and replicate, planning which analyses to execute, estimating replicated effects, to publishing the results:

(1) Scales should have validity evidence supporting their use and interpretation in the study, even if they are only one-item;

(2) Measurement invariance testing should be conducted when moving scales to new contexts. Otherwise, authors should justify why invariance testing is not required and acknowledge potential measurement differences in conclusions;

(3) Sample size planning must account for necessary psychometric analyses (e.g., tests of factor structure, measurement invariance, estimating reliability, etc.) because larger samples will be needed than just to estimate the effect size; and

(4) Data should continue to be made openly available, to facilitate the accumulation of validity evidence and large-scale psychometric testing.

In closing, we emphasize that measurement considerations are integral to the interpretability and robustness of both original and replication work. To return to the example of Alex, the physician with the broken scale interested in the relationship between bodyweight and heart health: If Alex does not realize that their scale is broken, their results will be meaningless. If another physician tries to replicate the results with the same broken scale, the results will also be meaningless, regardless of whether they find the same relationship that Alex did. So too in replications of psychological research: Before you can meaningfully assess the replicability of a finding, you need to ensure that your scales are functioning properly. Our recommendations serve to bolster the positive methodological reforms already underway in psychology, like large-scale projects with publicly available data, by ensuring that scales therein function properly.

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