**Convenience Samples and Measurement Equivalence in Replication Research**

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**Abstract**

A great deal of research in psychology employs either university student or online crowdsourced convenience samples (Chandler & Shapiro, 2016; Strickland & Stoops, 2019) and there is evidence that these groups differ in meaningful ways (Behrend et al., 2011). This practice could result in the presence of unaccounted-for measurement differences across convenience sample sources, which may bias results when these groups are compared or the resulting data are pooled. In this registered report, we used the openly available data from the Many Labs replication projects to test for measurement equivalence across different convenience sample sources. We examined 8 measures that showed acceptable baseline model fit and tested them for non-equivalence across convenience samples from different sources, including university participant pools, MTurk, and Project Implicit. We then examined whether replication results are robust to non-equivalence by fitting partial invariance models and sensitivity analyses of replication results. Many of the measures examined were not equivalent across student and crowdsourced convenience samples, or across different types of convenience samples. Only two tests, comparing lab and online student samples, retained strict equivalence, while 14 of 30 tests rejected configural equivalence. However, correcting for non-equivalence changed the estimated effect sizes of the replication effects very little. Based on these results, we advise researchers to test for measurement equivalence when combining or comparing data from different convenience samples. At the same time, due to a lack of validity evidence for many of the measures and variable power of our tests, we interpret results with caution.

**Convenience Samples and Measurement Equivalence in Replication Research**

In recent years, concerns about replication have become a source of interest and anxiety in many scientific fields, including psychology, genetics, cancer research, neuroscience, and economics (Zwaan et al., 2017). This is due, at least in part, to large collaborative projects that have attempted to estimate the rate at which findings replicate. One series of collaborations, called the Many Labs projects, has pooled resources across hundreds of scientists to collect large datasets for dozens of replication studies. There are five completed Many Labs studies (Ebersole et al., 2016, 2020; Klein et al., 2019, 2014, 2018), all involving large-scale collaboration of scientists and the pooling of data. Across all 62 effects replicated as part of these projects, 30 (48%) showed statistically significant effects in the same direction as the original study. Many scientists feel that the replication rates found by Many Labs and other similar projects are lower than they ought to be (Baker, 2016), and several statistical reforms meant to increase the replicability of the scientific literature have been discussed as a result (Shrout & Rodgers, 2018).

However, there has also been debate about the meaning of failed replications and what evidence they provide about the existence of any particular effect, as there are many features of both replications and original studies that could impact results. Various causes of failed replications have been discussed in the literature: lack of statistical power (Maxwell et al., 2015; Shrout & Rodgers, 2018), deviations from original methods in replication attempts (Gilbert et al., 2016), issues of research design and sampling (Nosek et al., 2022; Shrout & Rodgers, 2018), and measurement challenges (Fabrigar et al., 2020; Loken & Gelman, 2017). Though not often discussed, aspects of measurement can complicate the interpretation of replication results, including measurement differences between the original study and the replication, low reliability, lack of validity evidence, and measurement differences across relevant groups (Flake et al., 2022; Markon, n.d.; Shaw et al., 2020). Measurement differences across groups often arise because people from varying backgrounds interpret items differently or use response scales in a dissimilar way. When this happens, the measure is said to be non-equivalent for those groups. The focus of this registered report is to consider the measurement equivalence (ME) of instruments collected as part of the Many Labs projects across two forms of convenience samples, specifically student and online crowdsourced samples. To introduce the study, we discuss measurement and replication, explain ME in more detail, and review the literature on measurement differences across convenience samples.

**Measurement and Replication Research**

In psychology, because the constructs we are interested in are not directly observable, researchers rely heavily on self-report scales, which aim to quantify unobservable psychological features, such as attitudes, moods, and personality traits. However, if researchers throw together a series of questions, they can’t merely have faith that adding up the responses will result in a meaningful measure of the intended construct: they need to verify the validity and reliability of the scores they create or use (American Educational Research Association et al., 2014). Reviews of the psychological literature have found that the validity evidence presented by researchers does not live up to the standards of best scientific practice (Flake et al., 2017; Hogan & Agnello, 2004; Slaney, 2017). Reflecting the state of the field in general, the measures used in replication projects tend to have little validity evidence (Flake et al., 2022), and the Many Labs projects are no exception (Shaw et al., 2020). For instance, a review of all the measures used in Many Labs 2 (Shaw et al., 2020) found that 30% reported no reliability coefficients or validity evidence whatsoever and only 19% had a cited source. Additionally, Shaw et al. (2020) examined psychometrics of the measures using the open data from Many Labs 2 and found that most measures performed poorly according to common disciplinary standards: of the six scales examined, none met all three fit index cut-offs selected (root mean square error of approximation [RMSEA] < .05, comparative fit index [CFI] > .95, standardized root mean squared residual [SRMR] < .08).

Large replication projects such as the Many Labs present a host of measurement challenges. The international and collaborative data collection is a strength (Henrich et al., 2010), but the pooling of data from heterogeneous samples can also introduce invalidity. When samples are drawn from different populations, there is the possibility that measures exhibit non-equivalence because the items do not hold the same meaning across populations. This poses a problem for replication projects, as ME is a prerequisite for valid group comparisons and the pooling of data across samples (Davidov et al., 2014).

Two types of data sources are pooled in four of the five completed Many Labs projects: student samples and crowdsourced online samples. Because there are notable differences between these populations (Weigold & Weigold, 2021), there is a possibility this could introduce measurement non-equivalence, which might subsequently impact replication results. Though not a focus of the Many Labs projects at the outset, the open data and materials make it possible to evaluate ME after the fact. In this registered report, we propose to use a multiple group confirmatory factor analytic (MG-CFA) approach to test whether the measures employed in the Many Labs studies are equivalent across student samples and crowdsourced online samples, such as Amazon Mechanical Turk (MTurk). Confirmatory factor analysis (CFA) is a statistical modelling approach which aims to represent “the causal relations between one or more unobserved, or latent, variables and a set of observed variables” (Flora, 2017), and MG-CFA is the extension of this approach to model multi-group data, allowing for the detection and modelling of differences due to group membership. Next, we will complete a sensitivity analysis to understand if correcting for non-equivalence changes the results of the replication studies. Though the Many Labs projects are already completed, our results will help future researchers who hope to conduct large-scale collaborative research to understand whether variation across convenience samples is likely to be a meaningful and impactful source of measurement non-equivalence, allowing researchers to account for this possibility in their analyses.

**What is Measurement Equivalence?**

Also called measurement invariance, measurement equivalence is concerned with whether a particular scale is measuring the same thing in the same way across different groups. Formally, this means that, for a given level of the latent trait, the conditional distribution of the items of the measure is the same across subpopulations (Meredith & Millsap, 1992). Thus, within a latent variable modelling framework, “measuring something in the same way” means that the items of the scale are related to the latent variable in the same manner across groups. There are different levels or degrees of ME, each of which has as its focus a different aspect of the item to latent variable relationship. These hierarchical, increasingly restrictive models can be tested using multiple group CFA, allowing researchers to understand to what degree the measures function in the same way across groups. Figure 1 shows an overview of the hierarchical levels of measurement equivalence; they are described in more detail below.

Figure 1.

Diagram

Description automatically generated

Note: Overview of the Four Levels of Measurement Equivalence. Reprinted from “Measurement Invariance Testing Using Confirmatory Factor Analysis and Alignment Optimization,” by R. Luong and J. K. Flake, 2022, *Psychological Methods, Advance online publication*, p. 3. Copyright 2022 by the American Psychological Association.

The least restrictive level of ME is referred to as configural equivalence (Horn et al., 1983). This level requires that the number of latent factors, and which items load onto which factors, are the same across groups. In the case of scales intended to tap a single construct, this means that a unidimensional model must show adequate fit in both groups. The next level, commonly known as metric or weak equivalence, concerns the equivalence of the factor loadings across groups. The factor loadings represent the strength of the relationship between the individual items and the latent variable (Bollen, 1989); thus, metric equivalence is achieved when the slope of the item’s regression on the latent variable is the same across groups. The third level of equivalence is concerned with the intercepts of the item in the latent variable regressions and is called scalar or strong equivalence. Scalar non-equivalence occurs when one group uses the response scale for a particular item differently than another group, yielding mean items responses that are systematically higher or lower though their levels of the latent trait are the same (Cheung, 2008). Finally, the equivalence of error variances, or strict equivalence, should be considered. This will indicate whether items relate to the construct with the same degree of precision across groups.

When both metric and scalar equivalence are achieved, this is called strong factorial invariance. This is considered by many to be a prerequisite for using observed scores to make valid group comparisons (Cheung, 2008). Though MG-CFA can correct non-equivalence by estimating factor scores that take into account measurement differences, it isn’t standard practice for researchers in the social sciences: even among studies that compared across cultures, where non-equivalence is highly plausible, a review conducted by Boer et al. (2018) found that only 13% of included studies tested for ME. Instead, researchers commonly calculate and compare sum scores (McNeish & Wolf, 2020). If there is non-equivalence across the groups measured and observed scores are used, these scores will be biased: intercept non-equivalence will bias group mean estimates and impact the results of t-tests (Steinmetz, 2013), while loading non-equivalence will impact regression coefficients and correlations (Chen, 2008).

Though ME is highly relevant to replication research, very little work has explored this intersection. As is the case with the Many Labs projects, many replications are conducted as part of large collaborative efforts where data from multiple populations are pooled. Even if replicators carry out the same research protocol and analyses, the conceptual interpretation of the items may be different across the different populations included in the study. If this is the case, the pooling of these data is not justified, and the presence of non-equivalence could bias results. Moreover, examination of the generalizability of the replication results across groups is compromised, as bias due to measurement non-equivalence may account for group differences regarding the effect of interest. In addition to being highly relevant to replication projects, these concerns apply to any “big team science” that pools data from many sources.

It would also be ideal for replication researchers to test for ME between original and replication studies (Fabrigar & Wegener, 2016), but this is difficult in practice: for the most part, the original studies that are replicated do not have publicly available data and have small sample sizes (Fraley & Vazire, 2014). This is a barrier to detecting measurement non-equivalence, as sample sizes of approximately 400 per group are recommended to detect meaningful effects (French & Finch, 2016; Koziol & Bovaird, 2018; Meade & Bauer, 2007). However, large replication projects such as Many Labs make their data publicly available, enabling the assessment of ME across groups within the replications, such as data collection labs, translated versions of measures, and different sample sources. In this registered report, we made use of the availability of these data to examine measure equivalence across student and crowdsourced convenience samples, two sample sources which are pooled in three of the five Many Labs projects.

**Comparing Convenience Samples**

University students and online crowdsourced samples are examples of different convenience samples. Baker et al. (2013) define convenience sampling as a non-probability data collection method that prioritizes “the ease with which potential participants can be located or recruited” (p. 94). The use of university student samples has been a popular form of convenience sampling in psychology for a long time, and the popularity of online crowdsourced samples is growing (Chandler & Shapiro, 2016; Strickland & Stoops, 2019). It is no wonder crowdsourcing research is becoming more popular: this approach offers many advantages, including cost-effectiveness, the ability to collect large samples quickly, and the potential to access diverse and hard to reach samples (Chandler & Shapiro, 2016; Strickland & Stoops, 2019). However, Strickland & Stoops (2019) point out that crowdsourced samples may differ from “the populations to which the results ideally would generalize” (p. 9), a type of selection bias. To deal with this limitation, they recommend that researchers collect samples through diverse methods and consider aggregate results. If this approach is to be effective, it’s important that aggregated samples demonstrate ME, or that researchers employ a statistical model that accounts for non-equivalence across samples. If selection bias and hidden measurement differences are both impacting the results of a study, it is important to correct for ME in order to disentangle these two sources of bias.

MTurk is one of the most popular platforms for crowdsourcing research participants, due to its large user base, affordability, and ease of use. As such, a great deal of the research comparing crowdsourced and student convenience samples focuses specifically on MTurk and has found that student and MTurk samples tend to differ in several ways. Demographically, MTurk samples are consistently older than student samples (Behrend et al., 2011; Roulin, 2015; Steelman et al., 2014), often more ethnically diverse (Behrend et al., 2011), and come from a lower socioeconomic background (Weigold & Weigold, 2021). Additionally, though college students can be recruited through MTurk, they tend to be farther along in their degrees and are more likely to be part time compared to those recruited through university participant pools (Weigold & Weigold, 2021). MTurk and student samples also show mean differences on measures of personality: student samples are reliably higher in extraversion (Behrend et al., 2011; Colman et al., 2018; Goodman et al., 2013; Weigold & Weigold, 2021), and MTurk samples tend to score higher on openness to experience (Behrend et al., 2011; Colman et al., 2018; Weigold & Weigold, 2021). MTurk is the most studied online crowdsourcing platform, but research on differences from student samples may not generalize to other, similar data-collection platforms. Peer et al. (2022) found that data from Prolific, CloudResearch, Qualtrics, and Dynata differed from MTurk in terms of demographics and data quality. While differences across samples do not necessarily indicate non-equivalence, differences in sample characteristics could potentially contribute to non-equivalent measurement for particular constructs, as respondents from groups that differ from each other may understand items differently. However, it is also possible that very different people interpret items in the same way, and, therefore, these groups could still be equivalent in terms of measurement properties for a given construct. It is important to examine the issue directly.

There is a small but growing body of research on ME between student and MTurk samples. One study investigating a measure of post-traumatic stress disorder symptomology concluded that strict ME held across these samples (Caldas et al., 2020). Other studies that found equivalence across these samples, examining a multi-faceted personality disorder measure and measures of openness and innovation respectively, only examined configural (Miller et al., 2017) or loading equivalence (Winton & Sabol, 2021), leaving the equivalence of intercepts and error variances untested. Additionally, Behrend et al. (2011) assessed the equivalence of measures of Big Five personality traits and goal orientation and found that, while a few items from these scales were non-equivalent across groups, the effect sizes were small enough that the scales were functionally equivalent.

Adding some complexity to the issue, no two MTurk samples are the same and can vary in terms of culture and English language ability, as MTurkers can be recruited from all over the world. For instance, Feitosa et al. (2015) found that a measure of Big Five personality traits was equivalent to the scalar level between a student and a US-only MTurk sample, but only configural equivalence held when students were compared with a non-US MTurk sample. As this non-US sample was composed largely of non-native English speakers from India, they conclude that equivalence may not hold when MTurkers first language is not English.

In this registered report, we extend and build on previous work in three important ways. First, we conduct a thorough investigation of ME for a set of untested scales. While previous work has tested the equivalence of a number of measures, this does not mean that the same conclusions will be reached for different measures. Equivalence is sensitive to the construct being measured and the specific wording of items, so what holds for one measure may not for others. Second, we examine a source of crowdsourced data other than MTurk, as Many Labs 1 also includes a sample collected through Project Implicit (see Table 1 for a breakdown of sample sizes by source). This extends the literature on this topic because MTurk samples are used almost exclusively to represent all online crowdsourced samples, but there is no guarantee that the results would generalize to other similar sources. Third, Many Labs 2 includes an MTurk sample from India and one from the US, which allows us to test whether prior work on the importance of language spoken (Feitosa et al., 2015) is found in a new set of measures. Overall, this study is the most comprehensive examination of ME between convenience samples to date, in terms of the number of measures examined, the variety of sample sources, and sample size.

|  |  |
| --- | --- |
| **ML1** | MTurk: 1000  Implicit: 1329  Student (lab): 2435  Student (online): 593 |
| **ML2 (slate 1)** | MTurk (India): 360  MTurk (US): 331  Student (lab): 2735  Student (online): 332 |
| **ML2 (slate 2)** | MTurk (India): 362  MTurk (US): 340  Student (lab): 2645  Student (online): 2372 |
| **ML3** | MTurk: 737  Student: 3022 |

**Table 1.** Sample sources in each Many Labs project and total sample size per source.

Our analyses are driven by two primary research questions:

RQ1. To what extent do measures function equivalently across different convenience samples in the Many Labs projects?

RQ2. When measures are non-equivalent, does correcting for this change the statistical significance or effect sizes of the replications?

Answering these questions will contribute to understanding and addressing methodological challenges that are present in replication projects and beyond. First, previous research has not explored the degree to which a lack of ME across samples in replications and other collaborative projects presents an issue, both in terms of prevalence (RQ1) and impact (RQ2). By examining the issue for convenience samples, we can begin to explore the scope of this problem for one possible source of non-equivalence. Second, to the extent that measurement non-equivalence presents a problem, the analyses that we present here may serve as a template for researchers to consider ME as a part of their analysis plan in future replications and collaborative research projects and, based on our experience completing these analyses, we make recommendations for best practices moving forward. Finally, the results of this project contribute to understanding whether different convenience sample sources tend to display measurement non-equivalence by examining multiple measures, which is useful more broadly than just replication research, especially given how common these sample sources are in psychology. Understanding whether different convenience samples are likely to display measurement non-equivalence aids in the interpretation of all studies that use these samples and contributes to building a cumulative psychological science. For an overview of the design of our study to answer each of our research questions, see the Study Design Table in our supplementary materials.

**Methods**

In the following section, we describe in detail the preliminary measure inclusion analyses and the analyses for the main questions of interest, the equivalence testing and sensitivity analysis.[[1]](#footnote-2) Code for all analyses can be found in the supplementary materials.

**Preliminary Measure Inclusion Analyses**

The primary proposed analysis is psychometric equivalence testing. We performed these tests using MG-CFA with maximum likelihood estimation, which requires that the data meet the assumptions of the estimation method (multivariate normal, sufficient response options to approximate continuous) and, additionally, that the baseline measurement model is adequately specified (French & Finch, 2011). To determine which scales were amenable to the analyses, we carried out Confirmatory Factor Analyses (CFAs) for all measures that met the following criteria: 4 or more items per factor, enough response options that the items may be treated as continuous (Rhemtulla et al., 2012), and completed by both student and online crowdsourced samples (see Table 2 for scale information and CFA results). Type I error rates for equivalence tests may be inflated when the baseline model is misspecified (French & Finch, 2011), resulting in a higher probability of incorrectly concluding that a scale is non-equivalent across groups. For example, if a measure is modelled as unidimensional, but the items in fact load onto two factors, an equivalence test for this incorrectly specified unidimensional model would be more likely to find non-equivalence across groups, even though the true, 2-factor model is equivalent. For this study, we must balance the importance of controlling Type I error rates with the importance of investigating as wide of a range of instruments as possible. Given those considerations, we selected fit index cut-offs consistent with mediocre, but not clearly terrible fit: RMSEA ≤ .10 (Browne & Cudeck, 1992), SRMR ≤ .10 (Kline, 2015), CFI ≥ .90 (Kline, 2015). We excluded models from further analyses which failed to meet two out of three of these cut-offs. Code for these analyses can be found in the supplementary materials (Inclusion Code).

Overall, 5 measures were eliminated, and 9 remained as candidates for equivalence testing (see Table 2). When accessing the data for full analysis, we realized that responses to the Argument Quality scale used in ML3 had only been collected in the student sample, and thus could not be tested for equivalence. The resulting 8 instruments were included for full analysis. These measures represent a diverse set of constructs, which increases the generalizability of our conclusions. These 8 measures are briefly described below.

1. Contact Intentions (ML1 Study 11): this 4-item measure of respondents’ future intentions to interact with Muslims was adapted by Husnu & Crisp (2010) from a measure of behavioural intentions (Ratcliff et al., 1999). Replicators changed the items to refer to Muslims more generally rather than British Muslims, as in the original study.

2. Explicit Math Attitudes (ML1 Study 13): measures the valence of respondents’ attitudes towards math using six Likert items and one 100-point feelings thermometer. This measure was developed by authors for a study of explicit and implicit attitudes towards math across genders (Nosek et al., 2002), replicators used a subset of items.

3. & 4. Moral Foundations Questionnaire - Individualizing and Binding (ML2 Study 4): developed by Graham et al. (2009) to measure the relevance to moral decision-making of their theorized five moral foundations: harm, fairness, ingroup, authority, and purity. These foundations were assessed using 15 Likert items, three per foundation, which were further grouped into the higher-order factors of individualizing and binding moral foundations: the harm and fairness foundations are grouped under individualizing, and the ingroup, authority, and purity foundations form the binding factor. For the replication, this measure was scored by averaging responses to the items that form the higher-order individualizing and binding factors; for this reason, we examined Individualizing and Binding as separate scales.

5. Leader Power Scale (ML2 Study 15): a scale for rating the perceived power of a leader or manager, created by Giessner and Schubert (2007). This measure consists of five Likert-type items that assess the perceived dominance, confidence, and level of control that the target leader displays.

6. Desire for Control Products (ML2 Study 23): two scales were developed for use in a study by Zhong & Liljenquist (2006), one where respondents rated their desire for five different cleaning products, and this scale, where respondents rated their desire for an assortment of five other products (“control products”). While we considered both scales for inclusion in this study, only the Desire for Control Products scale met our fit criteria.

7. Need for Cognition (ML3 Study 8/Individual difference measure 5): the original study (Cacioppo et al., 1983) employed a 34-item measure of the need for cognition construct. According to the developers, this scale examines “the tendency for an individual to engage in and enjoy thinking” (Cacioppo & Petty, 1982, p. 116). Replicators used a shortened version, consisting of the six items with the highest factor loadings in the validation literature.

8. Perceived Stress Scale (ML3 Individual difference measure 4): this scale was not part of any replicated effect but was employed to measure respondents’ perceptions of their stress over the past week. A short-form scale consisting of four items was used (Cohen et al., 1983).

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| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| *Scale* | *Items* | *Type* | *α* | *χ2* | *df* | *CFI* | *RMSEA - 90%CI* | *SRMR* |
| Political Attitudes (PA) | 8 | 7-Point | .68 | 1251.94 | 20 | 0.80 | 0.11 [0.10, 0.11] | 0.06\* |
| System Justification (SJ) | 8 | 7-Point | .78 | 1414.82 | 20 | 0.86 | 0.12 [0.11, 0.12] | 0.06\* |
| **Contact Intentions (CI)** | **4** | **9-Point** | **.83** | **198.01** | **2** | **0.98\*** | **0.14 [0.12, 0.15]** | **0.02\*** |
| **Explicit Math Attitudes (EMA)** | **7** | **Mixed** | **.95** | **1034.01** | **14** | **0.97\*** | **0.13 [0.12, 0.14]** | **0.02\*** |
| **Moral Foundations Questionnaire Individualizing (MFQ-I)** | **6** | **6-Point** | **.82** | **271.12** | **9** | **0.97\*** | **0.08\* [0.07, 0.09]** | **0.03\*** |
| **Moral Foundations Questionnaire Binding (MFQ-B)** | **9** | **6-Point** | **.78** | **1333.97** | **27** | **0.88** | **0.09\* [0.09, 0.10]** | **0.05\*** |
| Subjective Well Being (SWB) | 25 | Mixed | .79 | 18653.19 | 275 | 0.41 | 0.16 [0.15, 0.16] | 0.20 |
| **Leader Power (LP)** | **5** | **7-Point** | **.86** | **785.03** | **5** | **0.92\*** | **0.19 [0.18, 0.20]** | **0.04\*** |
| Desire for Cleaning Products (D-Clean) | 5 | 7-Point | .77 | 863.04 | 5 | 0.89 | 0.17 [0.16, 0.18] | 0.06\* |
| **Desire for Control Products (D-Cont)** | **5** | **7-Point** | **.49** | **249.94** | **5** | **0.87** | **0.09\* [0.08, 0.09]** | **0.04\*** |
| **Need for Cognition (NfC)** | **6** | **5-Point** | **.67** | **99.78** | **9** | **0.95\*** | **0.06\* [0.05, 0.07]** | **0.03\*** |
| **Perceived Stress Scale (PSS)** | **4** | **5-Point** | **.72** | **93.03** | **2** | **0.96\*** | **0.13 [0.11, 0.15]** | **0.03\*** |
| Intrinsic Motivation (IM) | 15 | 4-Point | .79 | 5816.42 | 90 | 0.57 | 0.14 [0.14, 0.15] | 0.12 |

**Table 2.** CFA results for all suitable measures, using total sample collected for each measure. \* fit index meets proposed cut-off. Scales that qualify for further analyses are bold.

**Analysis Plan**

Code for the following analyses can be found in the supplementary materials. There is a separate R file for each measure, and the files are named for the measure analyzed (i.e. Contact Intentions Analyses, Explicit Math Attitudes Analyses etc.). The code used to develop the analysis plan can also be found in the supplementary materials (Planned Analysis Code). Sections of code pertaining to the analyses described below are cited as (code x.x), and the sections are numbered in the same way for all code files.

***Demographics***

We examined the available demographic variables by sample group for each Many Labs project included in this paper (1, 2, and 3) in the appropriate way for each variable type (mean and standard deviation for continuous variables, percentages for categorical variables like gender, code 1.2). There is some variation as to which variables were collected for each project: Many Labs 2 reports only age and gender, while Many Labs 1 and 3 collected a number of other demographic variables, such as ethnicity and native language.

***Assumptions and Data Checks***

To minimize the impact of assumption violations such as the lack of multivariate normality or model misspecification, we employed maximum likelihood estimation with Huber-White robust standard errors (MLR estimator in lavaan). However, we still examined some item level information to check that our data were reasonable after processing. Specifically, we examined skew, kurtosis, and item histograms and correlation matrices (code 1.3). Additionally, we fit single sample CFAs in the full data, and separately in each group, and examined the fit statistics and reliability (code 1.4).

***Measurement Equivalence Analyses***

In order to avoid conflating the issue of non-equivalence due to instrument translation with non-equivalence due to sample source, we limited our analyses to participants who completed the studies in English (code 1.1). The analyses were completed for all measures that fit the selection criteria, 8 scales in total. For each measure, each sample group was compared separately to each other sample group available for that measure. For example, in Many Labs 1 there are four sample groups of interest, so equivalence was tested across six pairs of convenience sample types for every measure from that project: MTurk vs. Project Implicit, MTurk vs. student (lab), MTurk vs. student (online), Project Implicit vs. student (lab), Project Implicit vs. student (online), and student (lab) vs. student (online).

For ME testing, we used a hierarchical approach: we compared multiple group CFA models of increasing restrictiveness (equal factor structure, loadings, intercepts, residuals) and stopped when the additional restrictions were rejected (Byrne & van de Vijver, 2010; Luong & Flake, 2022) (code 2.2, 2.5). To set the scale of the latent variable, we fixed its mean to 0 and variance to 1 for one group and freely estimated these values for the other. To identify the model, it is also necessary to select an anchor item. This is an item which is presumed to be equal psychometrically across groups. By constraining the loading and intercept of this item to be equivalent across groups, this ensures that the scale of the latent variable is the same, which allows for the equivalence of other items to be tested. To determine the anchor item, we employed Likelihood ratio tests using the all-other-items-as-anchors approach (Woods, 2009): starting from a model with all loadings and intercepts constrained to be equal across groups, then freeing both parameters for one item at a time and comparing this to the constrained model. For each measure, the item with the smallest Likelihood ratio associated with this test was selected as the anchor item (code 2.1).

For the stage 2 submission, we developed the following criteria for retaining configural equivalence: each subsample CFA, as well as the configural MG-CFA with only the anchor item restricted, must meet the same registered fit criteria we used for and instruments to be included in the study (2 out of 3 of: RMSEA ≤ .10, SRMR ≤ .10, CFI ≥ .90)[[2]](#footnote-3) (Browne & Cudeck, 1992; Kline, 2015).

Many of the convenience sample groups we examined are of very different sizes, which can bias equivalence testing such that non-equivalence is more difficult to detect (Yoon & Lai, 2018). For any sample pairing which was substantially unbalanced (one sample 1.5 or more times the size of the other), we employed the subsampling method proposed by Yoon & Lai (2018) to force balance to the samples (code 2.2, 2.5).

In addition to unbalanced sample sizes, it is important to consider the impact of sample size on power, as results of statistical tests should be interpreted with caution in situations where the power to detect a meaningful effect is insufficient. Power for the χ2-difference test of the equivalence of loadings and intercepts across groups is complex, as it is influenced not only by sample size and the amount and degree of non-equivalence, but also by many other features of the data and model, including: the strength of the loadings for non-equivalent items (Meade & Bauer, 2007), whether the direction of the non-equivalence is uniform or mixed (i.e. some loadings higher and some lower in the focal group, versus all loadings lower in the focal group; Meade & Bauer, 2007), the number of factors (French & Finch, 2006; Meade & Bauer, 2007), and the number of items per factor (Finch & French, 2018; French & Finch, 2006).

Simulation research on the χ2-difference test of the equivalence of loadings has found that, for sample sizes of 150 to 200 per group, power varies substantially based on these features (as low as .29 or as high as .95; French & Finch, 2016, 2006; Koziol & Bovaird, 2018; Meade & Bauer, 2007). For sample sizes of 400 to 500 per group, power is generally high: while one study reported power of .57 in a condition with 500 per group (French & Finch, 2006), this was an anomaly, and every other study reported values of .89 or greater (French & Finch, 2016; Koziol & Bovaird, 2018; Meade & Bauer, 2007). Of the 14 sample groups that we plan to examine, 5 of them have a sample size less than 400, and one of these is below 300 (the online student sample in ML2 slate 1). As such, we interpret results involving these sample groups with caution.

To evaluate the tenability of each level of parameter restrictions, we compared each nested model to the next most restricted one using Satorra and Bentler's (2001) approach to calculating the scaled χ2-difference statistic. A non-significant χ2-difference test indicates that the addition of the restricted parameters does not add an unacceptable degree of misfit and it is plausible that the relevant parameters are equal across groups in the population. If one of the χ2-difference tests was significant at  = .05, this was taken to indicate non-equivalence at that level (code 2.2, 2.5). Due to the fact that we may find statistically significant, but not practically significant non-equivalence, we also report dMACS effect sizes (Nye & Drasgow, 2011), though these were not used for decision making (code 2.3). Based on simulation studies by (Nye et al., 2019), when less than 50% of the items are non-equivalent, we consider dMACS > .40 to be practically significant; and when 50% or more are non-equivalent, we consider dMACS > .20 to be practically significant.

If a particular measure was not equivalent between groups to the strict level, we stopped the hierarchical testing procedure at whichever level the additional restrictions were rejected and proceeded to test the equivalence of the items so that we could develop a partial equivalence model. This is necessary in order to complete the sensitivity analysis (RQ2) comparing results using sum scores to factor scores produce by the partial equivalence models. In order to identify which item parameters were non-equivalent, we employed univariate score tests (Bentler & Chou, 1992), also referred to as modification indices (code 2.4). We assessed the parameters iteratively, releasing the one with the largest χ2 value and then testing the items again to identify any additional non-equivalent parameters. We proceeded until all score tests were non-significant, or the relevant parameter was only constrained for two items in the final model (Byrne et al., 1989). We used a Bonferroni corrected alpha level of .05 divided by the number of parameters being tested in that block. For example, if testing the loadings of an 8-item measure, the critical  would be .007, or .05 divided by one less than the total number of items, due to the anchor item remaining fixed (code 2.4). We only completed this process for loadings and intercepts; if strict equivalence was rejected, we allowed all error variances to differ across groups.

***Sensitivity Analysis of Replication Effects***

To examine the impact of measurement non-equivalence on the replicated effects, we reproduced the analyses conducted in the Many Labs for any measure that displayed some level of non-equivalence across groups and is involved in a replication effect. We produced factor scores using the final partial equivalence MG-CFA model for that measure (code 3.1) and, using the openly available analysis code for each study, reproduced the replication analyses using these factor scores in the place of the sum or mean scores originally used (code 3.2). Because factor scores also correct for measurement error, using them could change the results of some analyses even in the absence of measurement non-equivalence. To isolate the specific effect of non-equivalence, we also reproduced the analyses using factor scores from single group CFAs (code 3.1, 3.2). Regression factor scores were used (Thurstone, 1935) because they exhibit less bias in the estimation of downstream effects compared to Bartlett’s factor scores (Devlieger, Mayer, and Rosseel, 2016), the other factor score estimation method implemented in lavaan for continuous data.

***Level of Bias Control***

We submitted this registered report as designated at Level 2 bias control. This is because the data were already available at the time of analysis planning, and we had accessed the data to perform other analyses but had not separated the data by convenience sample source or performed any of the ME analyses for these groups. To further control for the risk of bias, we developed a detailed analysis plan including code. The Planned Analysis Code contains all proposed analyses completed using the real data for one measure, the 8-item Political Attitudes (PA) measure from Many Labs 1. However, we created a fake, randomly generated grouping variable rather than separating the data by sample source (Planned Analysis Code 1.1) to reduce the risk that we would make choices in order to achieve interesting results in the planning stage. We chose this measure for the purpose of analysis planning because it was eliminated from inclusion in the final study due to poor model fit, so we did not need to interact further with the portions of the data that would be used for our primary analyses.

**Results**

**Demographics**

Table 3 reports a full summary of demographic characteristics by sample. The average age of crowdsourced samples tends to be older than student samples and the variation of age in crowdsourced samples is greater. Additionally, the sex of student samples tends to be majority female, while crowdsourced samples are variable: the MTurk (India) sample is majority male, MTurk (US) samples have a similar number of males and females, and the Project Implicit sample is majority female.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Many Labs 1** | **Age** | **Sex** | | **Native Language** |
| Project Implicit | Range: 13-85  Mean: 35.2 (14.2) | Female: 860 (65%)  Male: 465 (35%) | | English: 1224 (92%)  Spanish: 37 (3%)  Other: 59 (4%) |
| MTurk | Range: 18-79  Mean: 35 (12.5) | Female: 521 (52%)  Male: 479 (48%) | | English: 952 (95%)  Spanish: 5 (<1%)  Other: 39 (4%) |
| Student (lab) | Range: 16-62  Mean: 20.3 (4.1) | Female: 1755 (72%)  Male: 675 (28%) | | English: 1917 (79%)  Spanish: 85 (3%)  Other: 424 (17%) |
| Student (online) | Range: 17-52  Mean: 19 (2.4) | Female: 470 (79%)  Male: 122 (21%) | | English: 539 (91%)  Spanish: 36 (6%)  Other: 16 (3%) |
| **Many Labs 2 (slate 1)** | **Age** | | **Sex** | |
| MTurk (India) | NR | | NR | |
| MTurk (US) | NR | | NR | |
| Student (lab) | NR | | NR | |
| Student (online) | NR | | NR | |
| **Many Labs 2 (slate 2)** | **Age** | | **Sex** | |
| MTurk (India) | NR | | Female: 122 (34%)  Male: 235 (65%)  Other: 0  Prefer not to answer: 1 (<1%) | |
| MTurk (US) | NR | | Female: 175 (51%)  Male: 162 (48%)  Other: 0  Prefer not to answer: 1 (<1%) | |
| Student (lab) | NR | | Female: 1689 (64%)  Male: 924 (35%)  Other: 4 (<1%)  Prefer not to answer: 8 (<1%) | |
| Student (online) | NR | | Female: 1448 (61%)  Male: 865 (36%)  Other: 19 (<1%)  Prefer not to answer: 29 (1%) | |
| **Many Labs 3** | **Age** | | **Sex** | |
| MTurk | Range: 18-72  Mean: 35.1 (10.9) | | Female: 278 (37%)  Male: 294 (40%)  Other: 1 (<1%)  NA: 164 (22%) | |
| Student | Range: 13-54  Mean: 19.3 (2.7) | | Female: 1818 (60%)  Male: 780 (26%)  Other: 12 (<1%)  NA: 412 (22%) | |

**Table 3.** Demographics by sample group. [Note: For Many Labs 2, we could not locate the age and sex variables in the slate 1 data, or the age variable in the slate 2 data.]

**Measurement Equivalence**

We conducted equivalence tests on all pairwise group comparisons, resulting in 38 equivalence testing procedures. The results from the hierarchical equivalence testing are displayed in Table 4, with full statistical results available in the supplementary materials (osf.io/ht48z/). For 8 of these comparisons, no anchor item could be identified and, therefore, a valid test could not be conducted (marked in orange in Table 4). We selected anchor items using a likelihood ratio testing approach developed by Woods (2009). However, downstream of that analysis, when testing for partial equivalence on applicable scales, we found that freeing the loading or intercept of the anchor item sometimes resulted in the most improvement to model fit (i.e., largest chi-square of the available parameters to free). Rather than freeing the anchor item, we halted testing, as this indicates that the anchor item is not actually equivalent across groups and calls the results of any equivalence tests into question, because a true anchor item is an assumption of these tests.

Of the 30 equivalence testing procedures with a stable anchor item, 13 (43%) rejected configural equivalence, 4 (13%) rejected metric equivalence, 10 (33%) rejected scalar equivalence, 1 (3%) rejected strict equivalence only, and 2 (7%) retained all levels of equivalence. Rejection of configural equivalence occurred in tests of the MFQ Individualizing, Binding, and the Leader Power scale, and all cases involved the MTurk (US) or Student (online) samples. A lack of valid anchor item was observed in tests of the Contact Intentions scale, Explicit Math Attitudes, Desire for Control Products, and Perceived Stress Scale, indicating the instruments are not equivalent. Partial equivalence models and DMACS effect sizes are presented in the following sections for each Many Labs project and measure.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Many Labs 1** | **Contact Intentions** | | | | **Explicit Math Attitudes** | | | |
| Config | Metric | Scalar | Strict | Config | Metric | Scalar | Strict |
| Implicit vs MTurk [1000] |  |  | **X** | **X** |  |  | **X** |  |
| Implicit vs Student (lab) [1329] |  |  |  |  |  | **X** | **X** |  |
| Implicit vs Student (on.) [593] |  |  |  |  |  |  | **X** |  |
| MTurk vs Student (lab) [1000] |  |  | **X** | **X** |  | **X** | **X** |  |
| MTurk vs Student (on.) [593] |  |  |  |  |  |  |  |  |
| Student (lab) vs Student (on.) [593] |  |  |  |  |  |  | **X** | **X** |
| **Many Labs 2** | **MFQ Individualizing** | | | | **MFQ Binding** | | | |
| Config | Metric | Scalar | Strict | Config | Metric | Scalar | Strict |
| MTurk (India) vs MTurk (US) [331] | **X** |  |  |  | **X** |  |  |  |
| MTurk (India) vs Student (lab) [360] |  |  | **X** | **X** |  |  | **X** |  |
| MTurk (India) vs Student (on.) [332] |  |  |  | **X** | **X** |  |  |  |
| MTurk (US) vs Student (lab) [331] | **X** |  |  |  | **X** |  |  |  |
| MTurk (US) vs Student (on.) [331] | **X** |  |  |  | **X** |  |  |  |
| Student (lab) vs Student (on.) [332] |  |  |  |  | **X** |  |  |  |
| **Many Labs 2** | **Leader Power** | | | | **Desire for Control Products** | | | |
| Config | Metric | Scalar | Strict | Config | Metric | Scalar | Strict |
| MTurk (India) vs MTurk (US) [337] | **X** |  |  |  |  |  |  |  |
| MTurk (India) vs Student (lab) [359] |  |  | **X** | **X** |  |  | **X** | **X** |
| MTurk (India) vs Student (on.) [359] | **X** |  |  |  |  |  |  |  |
| MTurk (US) vs Student (lab) [337] | **X** |  |  |  |  |  |  |  |
| MTurk (US) vs Student (on.) [337] | **X** |  |  |  |  |  | **X** | **X** |
| Student (lab) vs Student (on.) [2335] | **X** |  |  |  |  | **X** | **X** | **X** |
| **Many Labs 3** | **Need for Cognition** | | | | **Perceived Stress Scale** | | | |
| Config | Metric | Scalar | Strict | Config | Metric | Scalar | Strict |
| MTurk vs Student [737] |  | **X** | **X** |  |  |  |  |  |

**Table 4.** Measurement equivalence test results: levels of equivalence retained for each sample are marked in green, X indicates the levels at which equivalence was rejected. Sample size for the smallest group involved in the analysis is indicated in brackets in the first column. Comparisons marked in orange had no valid anchor item, so a valid comparison was not possible. Full statistical results available in supplementary materials: osf.io/ht48z

Of the 19 tests that compared crowdsourced samples to student samples, 8 (42%) rejected configural equivalence, 3 (16%) rejected metric equivalence, 7 (37%) rejected scalar equivalence, and 1 (5%) rejected strict equivalence only. Of the 10 comparisons that retained configural but rejected metric or scalar equivalence, 3 displayed DMACs effect sizes that were below the cut-off for a small effect according to suggested cut-offs for interpretation (>.20 and <.40 small, >.40 and <.70 medium, >.70 large; Nye et al., 2019). This leaves 7 comparisons with both a statistically significant rejection of metric or scalar equivalence and DMACs effect sizes of potential practical significance.

Of the 5 tests that compared a crowdsourced sample to another crowdsourced sample, 3 (60%) rejected configural equivalence, and 2 (40%) rejected scalar equivalence. Of the 2 comparisons that rejected scalar equivalence, 1 displayed only DMACs effect sizes that were below the cut-off for a small effect. All the tests that rejected configural equivalence compared the MTurk samples from the US and India in ML2, while both of the tests that rejected scalar equivalence compared the MTurk and Project Implicit samples in ML1.

Of the 6 tests that compared a student sample collected in a lab to student sample collected online, 2 (33%) rejected configural equivalence, 1 (17%) rejected metric equivalence, 1 (17%) rejected scalar equivalence, and 2 (33%) retained all levels of equivalence tested. The comparison that rejected scalar equivalence displayed DMACs effect sizes that were below the cut-off for a small effect. Both tests that rejected configural equivalence compared the online and in lab student samples in ML2. Detailed description of the results by measure can be found in the supplementary materials (osf.io/ht48z).



**Figure 1.** Proportion of equivalence tests performed that were rejected at each level, grouped by type of samples compared. The categories of samples are as follows – Crowdsourced: MTurk, MTurk (US), MTurk (India), and Project Implicit; Student: Online and Lab.























































**Sensitivity Analysis**

To be included in the sensitivity analysis, a measure had to (a) be used in the estimation of a replication effect, (b) have evidence of a valid partial equivalence model, meaning that it met configural equivalence, but rejected either metric of scalar for some of the group comparisons, and (c) have data available on all variables necessary for the estimation of the effect in the groups examined. While the Need for Cognition scale was used in the estimation of a replication effect, other variables necessary for the estimation of this effect were not collected for the MTurk sample; therefore, this measure was not included in the sensitivity analysis. In total, 13 sample group pairs were included in the sensitivity analysis.

Overall, correcting for measurement non-equivalence changed the effect sizes by small amounts, but did not change the conclusions of any the replications. For Cohen’s d effect sizes, changing from mean scores to corrected factor scores changed the effect size estimate by less than .023 in all cases (mean change of .01). However, much of this change is likely due to using factor scores at all, as the maximum difference between corrected and uncorrected factor scores was only .013 (mean of .003). Switching from mean scores to factor scores made the effect size larger (farther from 0) in the majority of cases, while switching from uncorrected to corrected factor scores was equally likely to increase or decrease the effect size. For correlations, changing from mean scores to corrected factor scores changed the effect size estimate by less than .013 in all cases (mean change of .008). Once again, much of this change is likely due to using factor scores at all, as the maximum difference between corrected and uncorrected factor scores was only .005 (mean of .002). Switching from mean scores to factor scores made the effect size larger (farther from 0) in all cases, while switching from uncorrected to corrected factor scores made the effect size larger in most cases, but smaller (closer to 0) in one case. Detailed description of the results by measure can be found in the supplementary materials (osf.io/ht48z).



















**Discussion**

The purpose of this report was to investigate the assumption of measurement equivalence in large-scale replication studies that pool multiple convenience samples and generate recommendations for instrument use and replication practice. We examined measurement equivalence across convenience samples in the Many Labs projects and conducted a sensitivity analysis to explore whether correcting for measurement non-equivalence impacted the estimation of replication effects. Here we discuss the main conclusions from these analyses, the limitations of the work, and our recommendations for replication projects and other research that pools or compares student and crowdsourced samples.

**Equivalence Analyses**

We found that student and crowdsourced samples displayed measurement non-equivalence for some groups at the configural, metric, or scalar level for all measures that we examined. Overall, comparisons between crowdsourced and student samples and between different types of crowdsourced samples resulted in a higher proportion of statistically significant non-equivalence than comparisons involving only online and in lab student samples. This emphasizes the importance of testing for ME in projects that pool or compare crowdsourced samples and student samples. Our results replicate previous findings that MTurk samples from India are not equivalent to MTurk samples from the US (Feitosa et al., 2015). Additionally, ours is the first study to compare samples from Project Implicit and MTurk. We found that these samples were non-equivalent at the scalar level for the two measures examined, and caution researchers who employ these samples that comparisons of results may not be valid without correction for non-equivalence.

These results have implications for replication research, large-scale collaborations, and psychological research more broadly. Given our findings, the presence of undetected non-equivalence in research that pools data from crowdsourced and student convenience samples is certainly a possibility. This could impact the results of such work, as measurement non-equivalence can bias estimates of effects and population parameters. This is especially relevant to large-scale collaborative studies, such as the Many Labs projects, because this type of research is more likely to pool samples from diverse sources, including crowdsourced and student samples. The high proportion of comparisons that rejected configural equivalence in our study especially warrants attention. When configural equivalence does not hold, it is not merely the case that estimates may be biased; rather, the interpretation of the effect is not valid because the proposed factor structure does not hold in at least one group. This is a serious concern and echoes previous findings that measurement validity in replication research is lacking overall (Flake et al., 2022; Shaw et al., 2020).

**Sensitivity Analysis**

Of the 38 sample group pairs that we considered, we were able to include 13 in the sensitivity analysis. A substantial proportion of the comparisons considered could not be included because configural equivalence was rejected or no valid anchor item could be identified. For these group comparisons, particularly related to the rejection of configural equivalence, there is no evidence that the data should be pooled at all, as there is a qualitative difference in the structure that cannot be corrected with quantitative analyses.

Overall, the results of the replicated effects were robust to the presence of measurement non-equivalence of intercepts or loadings, when the configuration of items to factors was the same across groups. Correcting for non-equivalent items did not change the conclusion of any statistical tests, and changed the effect size estimates by only small amounts. While this indicates that results may be robust to the presence of measurement non-equivalence across groups, caution is still warranted, as the impact of non-equivalent parameters on effect size estimates is impacted by the strength and direction of non-equivalence. Additionally, the use of factor scores introduces a level of uncertainty into the model and resulting estimates, especially given the low reliability of some of the measures examined, so results of the sensitivity analysis must be interpreted with caution.

While correcting for item level non-equivalence does not change the conclusions of the ML replications, it did change the effect size estimates by up to .023 for Cohen’s d effect sizes and up to .013 for correlations. In other cases, a change to the effect size of this amount could alter the conclusions of the study, and researchers may care about estimating the size of an effect precisely in addition to testing its significance. Additionally, we were only able to include 13 sample pairs in our sensitivity analysis, and it is likely that other situations with a greater degree of non-equivalence would bias results to a greater extent. Therefore, while it is good news that analyses may be robust to undetected non-equivalence, we still advise researchers to test and correct for non-equivalence when it is feasible to do so.

**Limitations**

It is important to the transparency and credibility of research to acknowledge the most serious limitations of any project. One important threat to the validity and interpretability of our results is the overall poor validity evidence for the measures we examined. Many of the measures we examined had very little, if any, validity evidence for their use. Additionally, we used more relaxed model fit index cut-offs when selecting measures for inclusion, and most of these measures would not have met the more stringent cut-offs often recommended. This is a serious problem because ME testing examines whether a scale is measuring a construct in the same way across groups. If the scale is not, in fact, measuring any construct at all, this question ceases to make any sense. It is also the case that when a CFA model is misspecified, parameter estimates may be incorrect. As the ME tests are comparing loadings and intercepts across groups, if these parameter estimates are incorrect due to misspecification of the baseline model, the ME test results can also be inaccurate. For this reason, the pattern of our results should be interpreted with caution, as it may not reflect the pattern that would emerge using measures with stronger validity evidence and baseline model fit.

Baseline model misspecification may be one reason why we were unable to identify a stable anchor item for so many of our group comparisons. It is also possible that model misspecification contributed to the high rate of rejection of configural equivalence for the MFQ Individualizing and Binding and the Leader Power Scale. Notably, the creators of the MFQ recommend a five-factor model rather than the two-factor model we examined, suggesting that the baseline model may be misspecified.

Another threat to the validity of our results is statistical power. Many of the samples we examined were of very different sizes and as a result our tests, of which there were over a hundred, had varying levels of power to detect the same effect size of measurement non-equivalence. Any patterns of statistical significance in our results could be due to these power differences rather than actual differences in the amount of non-equivalence present. It is difficult to entirely account for this issue as, in addition to being impacted by sample size and effect size, power in ME testing is impacted by the strength of inter-item correlations and the type of equivalence being tested (Meade & Bauer, 2007). We caution readers when interpreting patterns of significance from this project and encourage them to consider effect sizes where provided.

In addition, to examine equivalence across convenience samples in this project, we had to make decisions about how to deal with other plausible sources of non-equivalence. We opted to collapse across many groups, such as experimental conditions, participant gender, and participant race, all of which can contribute to non-equivalence. We also eliminated translated instruments, which are known to be a source of non-equivalence (Davidov & De Beuckelaer, 2010), by only using English versions of measures. If we considered every possible subgrouping, and clustered respondents with only those exactly like them, the groups would be too multitudinous and fine-grained to proceed with any examination. If we had sufficient data to do so, we could consider more groups simultaneously using the alignment method (Asparouhov & Muthén, 2014) for equivalence testing. However, given the available subgroup sample sizes, the issue necessitates some simplifying decisions regarding which features are likely to be relevant. As a result, a limitation of this work is that we may have done what we ask researchers not to do: pool data that include non-equivalent groups. Other undetected sources of measurement non-equivalence are a type of model misspecification that could bias equivalence test results, as well as downstream results of replicated effects.

**Recommendations**

Based on the results of the measurement equivalence testing and sensitivity analysis, we have recommendations for researchers interested in pooling data from or comparing across different convenience samples. Especially given the high proportion of tests that rejected configural equivalence, we recommend that researchers always test for measurement equivalence across these samples. While our sensitivity analysis indicates that effect estimates are often robust to item level non-equivalence, and some non-equivalent parameters are unlikely to change the overall conclusions of a study, a lack of configural equivalence is a serious problem and calls into question the interpretation of results for any groups in which the proposed factor structure does not hold.

We recommend that researchers determine in advance: (1) how they will decide whether samples meet configural equivalence; (2) how they will proceed if configural equivalence is rejected for one or more samples. There are a number of possible ways to test for configural equivalence, such as requiring that all single sample and multiple group CFA models meet pre-specified fit index cut-offs, as we did in this study, or employing permutation tests (Jorgensen et al., 2017). Similarly, there are many options for how to proceed if configural equivalence is rejected: researchers could choose to focus analyses on only the samples that displayed the hypothesized factor structure, to perform an exploratory factor analysis on those that do not in order to find a reasonable model to proceed with, or to analyse groups that do not fit the hypothesized factor structure separately knowing that different constructs are being measured when interpreting the results. These are a few reasonable paths we could think of, but there will be others. This garden of forking paths is the reason we advise researchers to make these decisions in advance, as there are many options which could be subconsciously exploited if particular results are desired.

In 8 out of 38 (21%) of the comparisons made for this study, no stable anchor item was identifiable. This was determined either by the fact that all items were statistically significant using likelihood ratio tests to identify non-equivalent items, and/or the previously identified anchor item was flagged as non-equivalent in later partial equivalence testing. If testing indicates that there may not be an anchor item, typical equivalence testing procedures are not possible. If researchers are confident that no items are psychometrically equivalent, samples should be analysed separately. We also recommend exploring the issue with qualitative work.

Given that the pattern of results was not consistent, and the different types of crowdsourced and student samples were not equivalent to each other in every case, it is likely that measurement equivalence across convenience samples is dependent on the specific source, rather than being generalizable across crowdsourced and convenience samples as broad categories. For this reason, researchers should not assume that different crowdsourced samples will be equivalent to each other, or even student samples collected in different settings e.g., in the lab versus online. When multiple different convenience sample sources are being pooled or compared as part of a study, we advise researchers to examine measurement equivalence for all samples collected.

While large-scale collaboration in psychology address limitations often found in other research, such as low power and limited generalizability, measurement differences pose a methodological challenge. We feel that employing large samples in psychology with greater diversity is a positive move for the field, and the Many Labs and other big team science projects have made important contributions to this effort. However, it is because we believe in the value of this undertaking that we want to ensure that challenges threatening the validity of conclusions from such research are adequately addressed.For scores from measures to be validly interpretable in the context of these studies, more work is needed examining their validity in relevant groups. In advance of further large-scale replications and other collaborations using existing measures of unknown quality, we feel that similarly large-scale collaborative construct validation research would help put any future projects on more solid methodological footing.

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**Study Design Table**

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| **Question** | **Sampling plan** | **Analysis Plan** | **Rationale for deciding the sensitivity of the test for confirming or disconfirming the hypothesis** | **Interpretation given different outcomes** | **Theory that could be shown wrong by the outcomes** | **Interpretation of findings** |
| RQ1. To what extent do measures function equivalently across different convenience samples in the Many Labs projects? | Using the previously collected open data from the Many Labs projects, we will examine every measure that meets our criteria for baseline model fit.  We will use only data from participants collected in English. | We will test the equivalence of loadings (metric equivalence) and intercepts (scalar equivalence) using likelihood ratio tests for each measure and sample group pair examined at . If the equivalence of all loadings or intercepts is rejected, we will test the equivalence of parameters at the item level using univariate score tests at .05 / the number of items. We will also calculate and report dMACS effect sizes at the item level. | According to our review of the simulation literature on the likelihood ratio test for detecting measurement non-equivalence, we most likely have power of 80% or greater for tests involving only the 9 largest samples we are examining. Tests involving the 5 smaller samples may be underpowered and results will be discussed with caution. | If all measures are equivalent across all convenience samples: these samples are likely to display measurement equivalence. The pooling of samples in the ML was justified, and pooling or comparing measurements using others samples from these sources without correcting for non-equivalence is likely to be justified in future cases, though not guaranteed.  If some measures are equivalent across convenience samples but others are not: measurement equivalence for convenience samples is dependent upon the construct and/or the specific measure. It should be tested or accounted for if measures from these data sources will be pooled or compared.  If some crowdsourced samples are equivalent with student samples and others are not: measurement equivalence across convenience samples is dependent on the specific source, rather than being generalizable across crowdsourced and student samples more broadly. Interpretation will depend on the pattern of results. Given the sample from India, language and culture may be a more reliable source of non-equivalence than convenience sample type.  If all measures are non-equivalent across all convenience samples: data from these sample sources should not be pooled or compared without considering potential measurement differences, as they are likely to be a reliable source of non-equivalence. Pooling these samples was not justified in the ML and may have impacted results. | The theory that measurement properties are equivalent across convenience sample sources (student and crowdsourced). This theory is assumed by the pooling of these data sources using uncorrected sum scores in the ML projects. | The measures we examined were non-equivalent across crowdsourced and student samples. Additionally, measures were non-equivalent across different crowdsourced samples (i.e., MTurk and Project Implicit), and some measures were equivalent across student samples collected online vs in the lab while others were not. We recommend that researchers interested in pooling or combining these samples test for measurement equivalence. |
| RQ2. When measures are non-equivalent, does correcting for this change the statistical significance or effect sizes of the replications? | Based upon the analyses conducted for RQ1, we will examine for RQ2 only the measures and samples which demonstrate configural equivalence but display statistically significant metric or scalar non-equivalence. | We will develop a partial equivalence model for each measure and sample pair on the basis of the results of the univariate score tests from RQ1. This model will restrict parameters found to be equivalent so they are equal across groups and free parameters that display statistically significant non-equivalence. We will generate factor scores from this multiple group model, which will correct for the non-equivalent parameters. We will reproduce the replication effects using these factor scores and compare these results to the effects estimated using original scoring methods. To determine whether effect sizes are different, we will calculate 95% confidence intervals. | Answering this research question will itself constitute a sensitivity analysis. We are not attempting to make inferences to other cases with these analyses; rather, we are aiming to describe whether the presence of measurement non-equivalence has had an impact on the estimation of effects in the ML replications. | If the results of the replications are not changed by correcting for non-equivalence, then, while the pooling of the samples was not justified in the cases where they displayed non-equivalence, the results were robust to this.  If the results of the replications are changed by correcting for non-equivalence, then these findings are not robust to the presence of non-equivalence. This may serve as a cautionary note and impetus for changing research practices of researchers pooling or comparing samples from these sources, although the results will not necessarily generalize to other cases, as the robustness of findings depend on particular features of the data in each case. | This analysis is not attempting to disprove any theory, but rather explore the robustness of the ML findings to the presence of measurement non-equivalence. | Correcting for the non-equivalence of loadings and intercepts did not change the overall conclusions of any of the replication effects and changed the estimated effect sizes by only small amounts. While the pooling of uncorrected data from these samples is not justified, the results are robust to this practice. However, many measures displayed configural non-equivalence across samples, and data from these should not be combined, as conclusions will not be valid. |

1. Note: measure inclusion analyses were performed before the submission of the stage 1 manuscript. The other analyses were completed for stage 2. [↑](#footnote-ref-2)
2. Our stage 1 registration did not include clear criteria for determining configural equivalence. We acknowledge that determining model fit cut-off criteria after results are known can introduce bias. To mitigate this, we selected our previously registered cut-offs, and JKF made the decision with no knowledge of the results. [↑](#footnote-ref-3)