Why Most Studies of Individual Differences With Inhibition Tasks Are Bound To Fail

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Abstract

Establishing correlations among common inhibition tasks such as Stroop or flanker tasks has been proven quite difficult despite many attempts. It remains unknown whether this difficulty occurs because inhibition is a disparate set of phenomena or whether the analytic techniques to uncover a unified inhibition phenomenon fail in real-world contexts. In this paper, we explore the field-wide inability to assess whether inhibition is unified or disparate. We do so by showing that ordinary methods of correlating performance including those with latent variable models are doomed to fail because of trial noise (or, as it is sometimes called, measurement error). We then develop hierarchical models that account for variation across trials, variation across individuals, and covariation across individuals and tasks. These hierarchical models also fail to uncover correlations in typical designs for the same reasons. While we can characterize the degree of trial noise, we cannot recover correlations in typical designs that enroll hundreds of people. We discuss possible improvements to study designs to help uncovering correlations, though we are not sure how feasible they are.

Keywords: Individual Differences, Cognitive Tasks, Hierarchical Models, Bayesian Inference

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In the past two decades, it has become popular to include experimental tasks in studies of individual differences. This is particularly salient in the study of individual differences in inhibition where studies often include experimental tasks such as the Stroop task (Stroop, 1935), the Simon task (Simon, 1968), and the Flanker task (Eriksen & Eriksen, 1974). On the face of it, individual-difference researchers should be sanguine about using such tasks for the following reasons: First, many of these tasks are designed to isolate a specific cognitive process, such as inhibitio,n by contrasting specific conditions. For example, in the Stroop task, the score is the contrast between performance for incongruent and congruent conditions. The subtraction inherent in the contrast controls for unrelated sources of variation such as overall speed. Second, many of these tasks are robust in that the effects are easy to obtain in a variety of circumstances. Take again, for example, the Stroop task. The Stroop effect is so robust that it is considered universal (MacLeod, 1991). Third, because these tasks are laboratory based and center on experimenter-controlled manipulations, they often have a high degree of internal validity. Fourth, because these tasks are used so often, there is usually a large literature about them to guide implementation and interpretation. Fifth, task scores are relatively easy to collect and analyze with latent-variable models.

Figure 1 shows the usual course of analysis in individual-difference research with cognitive tasks. There are raw data (Panel A), which are quite numerous, often on the order of hundreds of thousands of observations. These are cleaned, and to start the analysis, task scores for each participant are tabulated (Panel B). For example, if Task 1 is a Stroop task, then the task scores would be each individual's Stroop effect, that is, the difference between the mean RT for incongruent and congruent conditions. A typical task score is a difference of conditions, and might be in the 10s of milliseconds range. The table of individual task scores is treated as a multivariate distribution, and the covariation of this distribution (Panel C) is decomposed into meaningful sources of variation through latent variable models (Panel

D; e.g., Bollen, 1989; Skrondal & Rabe-Hesketh, 2004).

There is a wrench, however, in the setup. Unfortunately, scores from experimental tasks correlate with one another far less than one might think *a priori.* An example is the lack of correlation among the Stroop task and the flanker task. While Friedman and Miyake (2004) found a healthy correlation of .18 between the tasks, subsequent large-scale studies from Hedge, Powell, and Sumner (2018), Pettigrew and Martin (2014), Rey-Mermet, Gade, and Oberauer (2018) and Von Bastian, Souza, and Gade (2015) have found correlations that range from -.09 to .03, and average -.03 in value. The near-zero value of correlation between these two tasks is not an outlier. As a rule, effects in inhibition tasks show surprisingly low correlations (Rey-Mermet et al., 2018). And the low correlations are not limited to inhibition tasks. Ito et al. (2015) considered several implicit attitude tasks used for measuring implicit bias. Here again, there is surprisingly little correlation among tasks that purportedly measure the same concept. This lack of correlation may also be seen in latent variable analyses. Factor loadings from latent variables to tasks are often dominated by a single task indicating that there is little covariation to decompose (MacKillop et al., 2016).

The question of why these correlations are so low has been the subject of recent work by Draheim, Mashburn, Martin, and Engle (2019), Hedge et al. (2018) and Rey-Mermet et al. (2018) among others. On one hand, they could reflect underlying true task performance that is uncorrelated or weakly correlated. In this case, the low correlations indicate that performance on the tasks do not largely overlap, and that the tasks are indexing different mental processes. Indeed, this substantive interpretation is taken by Rey-Mermet et al. (2018), who argue that inhibition should be viewed as a disparate rather than a unified concept. By extension, different tasks rely on different and disparate inhibition processes.

On the other hand, the true correlations could be large but masked by measurement error. Several authors have noted the possibility of a large degree of measurement error. Hedge et al. (2018), for example, set out to empirically assess the reliability of task measures

by asking participants to perform a battery of tasks and to return three weeks later to repeat the battery. With these two measures, Hedge et al. (2018) computed the test-retest reliability of the tasks. The results were somewhat disheartening with test-retest reliabilities for popular tasks in the range from .2 to .7. Draheim et al. (2019) argue that commonly used response time difference scores are susceptible to low reliability and other artifacts such as speed-accuracy tradeoffs.

It has been well known for over a century that correlations among measures are attenuated in low reliability environments (Spearman, 1904). Yet, how much attenuation can we expect? If it is negligible, then the observed low correlations may be interpreted as true indicators that the tasks are largely measuring uncorrelated mental abilities. But if the attenuation is sizable, then the underlying true correlation remains unknown. One of our contributions in this paper is to document just how big this attenuation is in common designs.

Figure 2 provides an example of attenuation. Shown in Panel A are hypothetical *true difference scores* (or true effects) for 200 individuals on two tasks. The plot is a scatter plot—each point is for an individual; the x-axis value is the true score on one task, the y-axis value is the true score on the other task. As can be seen, there is a large correlation, in this case it is 0.78. Researchers do not observe these true scores; instead they analyze difference scores from noisy trial data with the tabulation shown in Figure 1. Figure 2B shows the scatterplot of these observed difference scores (or observed effects). Because these observed effects reflect trial noise, the correlation is attenuated. In this case it is 0.38. While this correlation is statistically detectable, the observed value is dramatically lower than the true one.

The amount of attenuation of the correlation is dependent on critical inputs such as the number of trials and the degree of trial variability. Therefore, to get a realistic picture of the effects of measurement error it is critical to obtain realistic values for these inputs. In

this paper, we survey 15 fairly large inhibition studies. From this survey, presented here subsequently, we derive typical values for the number of trials and the degree of trial variability. These typical values are used in Figure 2, and the amount of attenuation of the correlation therefore represents a typical rather than a worst-case scenario. As will be discussed, we believe that observed correlations in typical designs are about 1/2 of the true values.

Measurement Error and Trial Noise

The amount of attenuation shown in Figure 2, from 0.78 to 0.38, is striking. No wonder it has been so hard to find correlations! What can be done? Our view is that there is a productive path by considering what contributes to measurement error (Rouder & Haaf, 2019). Consider the workflow in Figure 1 where the first analytic step is forming participant-by-task-score tables (Panel B). It is these scores that are susceptible to measurement error. But where does this error come from? The score is the differences in sample means, and the measurement error of these sample means is a function of the number of trials in the task. If there are a great many trials, then the sample means precisely estimate true means, sample differences precisely estimate true differences, and the correlation reflects the true correlation among the tasks. If there are few trials, then the sample means are variable and the observed correlation is attenuated. Hence, the number of trials per task is a critical quantity as it determines the systematic downward bias in correlation.

There are two immediate consequences to noting that the main component of measurement error is trial noise (Rouder & Haaf, 2019). The first is that one cannot talk about the reliability of a task or the correlation among two tasks. These values are critically dependent on the number of trials. We cannot compare different values from different experiments without somehow accounting for differences in this design element. Simply put,

there is no such thing as the reliability of a task or a correlation between tasks without reference to sample sizes.

The second consequence is that the number of trials is far more important than the number of participants. The number of participants determines the unsystematic noise in the correlation; the number of trials determines the systematic downward bias. With few trials per task and many participants, researchers will have high confidence in a greatly biased estimate.

The realization that measurement error is primarily trial noise is wonderful news! It means that measurement error may be overcome by running many trials per participant per condition per task. Even more importantly, trial noise can be estimated and perhaps removed using statistical techniques. The hope is that with such techniques, it may be possible to obtain unbiased estimates of the correlations even in realistic designs with limited numbers of trials per person per task. For example, Behseta, Berdyyeva, Olson, and Kass (2009), Matzke et al. (2017), and Rouder and Haaf (2019) propose hierarchical statistical models to disattenuate correlations. The potential of such models is shown in Figure 2C. Here, a hierarchical model, to be discussed subsequently, was applied to the data in 2B, and the resulting posterior estimates of participants' effects reveal the true strong correlation.

Based on the demonstration in Figure 2C, we had come into this research with the hope of telling a *you-can-have-your-cake-and-eat-it* story. We thought that perhaps hierarchical models would allow for the accurate recovery of correlations in typical designs providing for an answer to whether inhibition is unified or disparate. Yet, the story we tell here is far more complicated. First, we study 15 previously-published experiments to characterize the amount of measurement noise, true variability, and sample sizes in typical designs in inhibition-task research with individual differences. With these inputs, we then study correlation recovery through simulation. To foreshadow, overall estimates from hierarchical models do disattenuate correlations. But, in the course, they suffer from a large

degree of imprecision. It seems that in typical designs, one can use sample statistics and suffer massive attenuation or use a modeling approach and accept a large degree of imprecision. And this difficulty is why we believe most studies of individual differences with tasks are doomed to fail. This story is not the one we had hoped for, but it is a critical story for the community of individual-differences scholars to digest.

Spearman's Correction for Attenuation

Before addressing the main question about recovery, we consider the Spearman (1904) correction for the attenuation of correlation from measurement error. In this brief detour, we assess whether Spearman's correction leads to the recovery of latent correlations among tasks in typical designs. The assessment provides guidance because the data generation in simulations match well with the assumptions in Spearman's correction. If Spearman's correction cannot recover the latent correlations in realistic designs, these correlations may indeed be unrecoverable.

Spearman's derivation comes from decomposing observed variation into true variation and measurement noise. When reliabilities are low, correlations may be upweighted to account for them. In Spearman's classic formula, the disattenuated correlation, denoted r'_{xy} between two variables x and y is

$$r'_{xy} = \frac{r_{xy}}{\sqrt{r_{xx}r_{yy}}},$$

where r_{xy} is the sample correlation and r_{xx} and r_{yy} are the sample reliabilities.[1]

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[1]The estimation of reliability in tasks is different than the estimation of reliability in a classical test because there are replicates within people and conditions in tasks. The presence of these replicates may be leveraged to produce better estimates of error variability than when they are not present. Let $\bar{Y}_{ik}$ and $s_{\bar{y}_{ik}}$ be the sample mean and sample standard error for the ith individual in the kth condition, $k = 1, 2$. Let $d_i = \bar{Y}_{i2} - \bar{Y}_{i1}$ be the effect for the ith individual, and let V_d be the sample variance of these effects. This sample variable is the total variance to be decomposed into true and error variances. Assuming an equal number of trials per condition, the error variance for the ith person, denoted V_{ei} is $s^2_{\bar{y}_{i1}} + s^2_{\bar{y}_{i2}}$. The estimate of error variance is simply the average of these individual error variances, or $V_e = \sum_i \sum_k s^2_{\bar{y}_{ik}}/I$. The reliability is $r = (V_d - V_e)/V_d$.

Spearman's correction, while well known, is not used often. The problem is that it is unstable. Panel D of Figure 2 shows the results of a small simulation based on realistic values from inhibition tasks discussed subsequently. The true correlation is .80. The Spearman-corrected correlations, however, are not only variable ranging from 0.37 to 1.50, but not restricted to valid ranges. In fact, 4.40% of the simulated values are greater than 1.0. We should take these problems with Spearman's correction seriously. The poor results in Figure 2D may indicate that in low-reliability environments, true correlations among tasks may not be recoverable. And this lack of recoverability may be fundamental—measurement noise may destroy the correlation signatures.

In the next section, we analyze existing data sets to find appropriate settings for simulations. These settings include sample sizes and estimates of the amount of variability we may reasonably expect across trials and across individuals. With these settings established, we simulate data and assess whether correlations are recoverable. The hierarchical latent correlation estimators, while far from perfect, are better than Spearman-corrected correlation estimators. Subsequently, we apply the same analysis to a large data set from Rey-Mermet et al. (2018) spanning four inhibition tasks to assess whether the observed low correlations reflect independent task performance or attenuation from trial noise. Yet, even with hierarchical modeling, we are unable to definitively answer this question.

Variability in Experimental Tasks

To explore whether it is possible to recover correlations in typical designs, it is important to understand not only typical sample sizes, but typical ranges of variability. To estimate within-trial and across-individual variabilities, we use an ordinary variance-components hierarchical model. To truly appreciate how variation can be assessed, the models need to be fully specified rather than left to short-hand. Let $Y_{ijk\ell}$ be the ℓth response for the ith individual in the jth task and kth condition. In this section we analyze

each task independently, so we may safely ignore j, the task subscript (we will use it subsequently, however). The model for one task is:

$$Y_{ik\ell} \sim \text{Normal}(\alpha_i + x_k\theta_i, \sigma^2),$$

where α_i is the ith individual's true response time in the congruent condition, $x_k = 0, 1$ codes for the incongruent condition, θ_i is the ith individual's true effect, and σ^2 is the trial noise within an individual-by-condition cell. The critical target are the θ_is, and these are modeled as random effects:

$$\theta_i \sim \text{Normal}(\mu_\theta, \sigma_\theta^2),$$

where μ_θ describes the overall mean effect and σ_θ^2 is the between-person variation in individuals' true effects. Our targets then are the within-cell trial noise, σ^2, and between-individual variance, σ_θ^2.

To analyze the model priors are needed for all parameters. Our strategy is to choose scientifically-informed priors (Dienes & Mclatchie, 2018; Etz, Haaf, Rouder, & Vandekerckhove, 2018; Rouder, Morey, & Wagenmakers, 2016; Vanpaemel & Lee, 2012) that anticipate the overall scale of the data. The parameters on baseline response times, in seconds, are $\alpha_i \sim \text{Normal}(.8, 1)$. These priors are quite broad and place no substantive constraints on the data other than baselines are somewhere around 800 ms plus or minus 2000 ms. The prior on variability is $\sigma^2 \sim \text{Inverse Gamma}(.1, .1)$, where the inverse gamma is parameterized with shape and scale parameters (Rouder & Lu, 2005). This prior, too, is broad and places no substantive constraint on data. Priors for μ_θ and σ_θ^2 were informed by the empirical observation that typical inhibition effects are in the range of 10 ms to 100 ms. They were $\mu_\theta \sim \text{Normal}(50, 100^2)$ and $\sigma_\theta^2 \sim \text{Inverse Gamma}(2, 30^2)$, where the values are in milliseconds rather than seconds. A graph of these prior settings for μ and $\sigma_\theta = \sqrt{\sigma_\theta^2}$ is shown in Figure 3. These priors make the substantive assumption that effects are relatively small and are not arbitrarily variable across people. The scale setting on σ_θ^2 is important as

it controls the amount of regularization in the model, and the choice of 30 (on the ms scale) is scientifically informed (see Haaf & Rouder, 2017).

We applied this model to a collection of 15 experimental tasks from a variety of authors. Brief descriptions of the tasks are provided in the Appendix. The experiments were chosen based on the following criteria: I. Raw trial-level data were available and adequately documented. This criterion is necessary because model analysis relies on the raw data and cannot be performed with the usual summary statistics. II. These raw data could be shared. This research is offered within a fully open and transparent mode (Rouder et al., 2019), and you may inspect all steps from raw data to conclusions. III. The data come from an experimental setup where there was a contrast between conditions; i.e., between congruent and incongruent conditions. We think that given our limited goals of getting a sense of values for simulations, these criteria are appropriate.

The results are shown in Table 1, and the specific values inform our subsequent simulations. The first three columns describe the sample sizes: The first column is the total number of observations across the two conditions after cleaning (see Appendix), the second column is the total number of individuals, and the third column is the average number of replicates per individual per condition. The fourth and fifth columns provide estimates of reliability. The column labeled "Full" is the sample reliability using all the observations in one group (see Footnote 1); the column labeled "Split" is the split-half reliability. Here, even and odd trials comprised two groups and the correlation of individuals' effects across these groups was upweighted by the Spearman-Brown prophecy formula. Note that the former estimate is more accurate than the split-half estimate because the former uses variability information across trials, much like in ANOVA, where the later does not. The next pair of columns shows the mean sample effect and the standard deviation of individuals' sample effects around this mean. These are sample statistics calculated in the usual way and do not reflect the model. The next two columns are standard deviation estimates from the

hierarchical model. The column $\hat{\sigma}$ is the posterior mean for residual variability and the column $\hat{\sigma}_\theta$ is the posterior mean for the true variability across individuals. The final column, labeled $\hat{\eta}$, is the ratio of these standard deviations. As discussed subsequently, this ratio reflects how reliable the task is and how much the naive correlations will be attenuated.

In hierarchical models, the estimate of true variability across people, σ_θ is smaller than the variability among sample effects (s_d in the table). The reason is straightforward—s_d contains contributions from both individual variability and trial noise. The phenomenon is sometimes called *hierarchical shrinkage* or *hierarchical regularization*, and a brilliant explanation is provided in Efron and Morris (1977). Rouder and Haaf (2019) extend this explanation to inhibition tasks, and the reader is referred to these sources for further discussion.

From the table, we derive the following critical values for the following simulations. We set the number of individuals to $I = 200$ and the number of trials per condition to $L = 100$. We set the trial-by-trial variation to $\sigma = 175$ ms, and the variation of individuals' true effects to $\sigma_\theta = 25$ ms.

Let's examine these choices in more detail. The choice of $I = 200$ people and $L = 100$ replicates per condition is made to emulate designs where many people are going to run in several inhibition tasks. For tasks with two conditions, there are 40,000 observations per task. In a typical battery with $J = 10$ tasks, the total number of observations is 400,000, which is quite large. Hence, our choices seem appropriate to typical large-scale individual-difference studies with experimental tasks.

Next, let's examine the choices of variabilities: $\sigma = 175$ ms and $\sigma_\theta = 25$ ms. The critical choice is the latter, and a reader may question its small size. Does it make sense, and why is the larger value s_d, the empirically observed standard deviation of individuals effect scores not used. The values s_d are larger because they necessarily include contributions from

trial noise and variability across individuals. The second column, s_θ reflects the model's partition of variance, that is, what is left over after trial noise, given by σ, is accounted for. Given the assumptions of the model, it reflects only the variability across individuals. Hence, it is the far better value for simulation.

We provide a second argument that may be more intuitive for understanding the 25 ms value. Consider the possibility that all people truly respond faster in the congruent than in the incongruent condition. Or, restated, nobody has a negative true effect. This condition is called *dominance* in Rouder and Haaf (2018), and is explored extensively in Haaf and Rouder (2017) and Haaf and Rouder (2019). The results from these studies is that dominance broadly holds. In the Stroop case, everyone Stroops, that is, in the large trial limit, everyone has truly faster scores for congruent than incongruent stimuli. If dominance holds, and the true mean effect is small across the population, say 50 ms, then the variance between individuals cannot be too high. For if it were large, then some proportion of people must have negative true effects. Dominance—which is natural and seems to hold in almost all sets we have examined—provides a limit on the size of variability. Figure 3 provides a graph of true values with a spread of 25 ms. As can be seen, there is only minimal mass for negative true values, and the spread of true values to us seems appropriate for a true 50 ms effect.

Expected Attenuation

The above results are useful for understanding how much attenuation of the correlations we should expect with the usual analysis in Figure 1. We consider the case where in each task there are L trials per task per condition, common trial variance σ^2 and common true variance σ_θ^2. The expected classical estimate, ρ^*, is given by

$$\rho^* = \rho \left(\frac{L\sigma_\theta^2}{L\sigma_\theta^2 + 2\sigma^2} \right).$$

This equation is most useful if written with the ratio $\eta = \sqrt{\sigma_\theta/\sigma}$, with this ratio interpreted as a ratio of signal (true variability) to noise (trial noise). Then, the attenuation factor, ρ^*/ρ is

$$\frac{\rho^*}{\rho} = \left(\frac{L}{L + 2/\eta^2} \right).$$

The last column of Table 1 shows the value of η for the various studies, and the values range from $1/11$ to $1/3$, with $\eta = 1/7$ corresponding to our typical case. Figure 4 shows the dependence of the attenuation factor on the number of trials (L) for various values of signal to noise. As can be seen, with the usual approach of tabulating participant-by-task scores, we expect attenuation to be a factor of about $1/2$.

Model-Based Recovery of Correlations Among Tasks

The critical question is then whether accurate estimation of correlation is possible. The small simulation in the introduction, which was based on the above typical settings for two tasks and a true population correlation of .80, showed that naive correlations among sample effects were greatly attenuated and Spearman's correction was unstable. We now assess the recoverability of true latent correlations with the hierarchical models used to simulate data and for several values of true correlations.

A Hierarchical Model for Correlation

Here we develop a hierarchical trial-level model for many tasks that explicitly models the covariation in performance among them. A precursor to this model is provided in Matzke et al. (2017) and Rouder and Haaf (2019). The difference is that these previous models are applicable for only two tasks and one correlation coefficient. They are not applicable to several tasks and coefficients.

At the top level, the model is:

$$Y_{ijk\ell} \sim \text{Normal}(\alpha_{ij} + x_k \theta_{ij}, \sigma^2).$$

The target of inquiry is θ_{ij} the effect for the ith participant in the jth task. The specification is made easier with a bit of vector and matrix notation. Let $\boldsymbol{\theta}_i = (\theta_{i1}, \ldots, \theta_{iJ})'$ be a column vector of the ith individual's true effects. This vector comes from a group-level multivariate distribution. The following is the case for three tasks:

$$\boldsymbol{\theta}_i = \begin{bmatrix} \theta_{i1} \\ \theta_{i2} \\ \theta_{i3} \end{bmatrix} \sim \text{N}_3 \left(\begin{bmatrix} \mu_1 \\ \mu_2 \\ \mu_3 \end{bmatrix}, \begin{bmatrix} \sigma_{\theta_1}^2 & \rho_{12}\sigma_{\theta_1}\sigma_{\theta_2} & \rho_{13}\sigma_{\theta_1}\sigma_{\theta_3} \\ \rho_{12}\sigma_{\theta_1}\sigma_{\theta_2} & \sigma_{\theta_2}^2 & \rho_{23}\sigma_{\theta_2}\sigma_{\theta_3} \\ \rho_{13}\sigma_{\theta_1}\sigma_{\theta_3} & \rho_{23}\sigma_{\theta_2}\sigma_{\theta_3} & \sigma_{\theta_3}^2 \end{bmatrix} \right).$$

More generally, for J tasks,

$$\boldsymbol{\theta}_i \sim \text{N}_J(\boldsymbol{\mu}, \boldsymbol{\Sigma}_\theta). \tag{1}$$

Priors are needed for $\boldsymbol{\mu}$, the vector of task means, and $\boldsymbol{\Sigma}_\theta$, the covariance across the tasks. We take the same strategy of using scientifically-informed priors. For $\boldsymbol{\mu}$, we place the normal in Figure 3A on each element. For $\boldsymbol{\Sigma}_\theta$, the classic choice is the *inverse Wishart* prior. This choice is popular because it is flexible and computationally convenient (O'Hagan & Forster, 2004). The inverse Wishart requires a scale parameter, and we set it so that the marginal prior on standard deviations of true variation matches the distribution in 3B.[2] It is the use of the inverse Wishart here that allows the model to be applicable to many tasks and

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[2]There is an alternative choice of prior for covariance that we extensively explored, the *LKJ prior* (Lewandowski, Kurowicka, & Joe, 2009). This prior is less informative than the Wishart because, unlike the Wishart, the estimation of correlation is independent of the specification of scale. Consequently, this prior is recommended (McElreath, 2016), and implementation is convenient in the R-package rstan (Stan Development Team, 2018). Yet, we found better performance for the inverse Wishart in simulations in that the posterior credible intervals were smaller and better covered the true value. The increased performance of the Wishart reflects the fact that researchers have a rough idea about the scale of individual differences—it is on the order of tens of milliseconds—and this is enough information for the improved performance of the inverse Wishart.

correlation coefficients.

Two Tasks

The first simulation is for two tasks. Using the typical sample sizes discussed above, each hypothetical data set consisted of 80,000 observations (200 people × 2 tasks × 2 conditions × 100 replicates per condition). One might hope that with such a large sample size and with a goal of estimating a single correlation, the true population correlation, ρ, might be recoverable. Supporting this hope is the success of the single run in Figure 4C. On the other hand, given the large degree of measurement noise and the instability of Spearman's correction (Figure 4B), it seems plausible that ρ may not be unrecoverable. For the simulations, true correlation values across the two tasks were varied on three levels with values of .2, .5, and .8. For each of these levels, 100 data sets were simulated and analyzed.

Figure 5A shows the results. Naive correlations from participant-by-task sample means are shown in red. As expected, these correlations suffer a large degree of attenuation from trial noise. Correlation estimates from Spearman's correction are shown in green. These values are better centered though some of the corrected values are greater than 1.0. The correlation estimates from the hierarchical model are shown in blue.

Overall, the correlation estimates from Spearman's correction and the hierarchical model have less bias than the naive sample-effect correlations. Yet, the estimates are quite variable. For example, consider correlations when the population value is .2. The model estimates range from -0.21 to 0.54 and miss the target with a RMSE of 0.15. Spearman corrected estimates are a slightly better and have an RMSE for this case of 0.14. Overall though, this variability is quite high especially given the large number of observations. We would not have confidence in substantive conclusions with it.

Are there risks in using model-based recovery? We see in simulation that the model

and Spearman-corrected recovery is variable. One potential problem is that in any one study, researchers using the model inflate the values of correlations. The attenuation in the naive correlations is conservative in that recovered correlations are never inflated, rather, they are dramatically deflated. In this regard, we can think of naive-correlations as having a fail-safe quality where high-value correlation estimates are avoided at the draconian expense of not detecting true high correlations. Spearman-corrected correlations do not share this fail-safe orientation. The variability in estimation results in values that are both inflated and deflated.

The critical question is about model-based recovery. Figure 5A shows only posterior mean estimates. Yet, in the Bayesian approach, the target is not just the posterior mean, but the entirety of the posterior distribution. Figure 5B-D shows the posterior 95% credible intervals for all runs with true correlations of .2, .5, and .8, respectively. There are two noteworthy trends. First, the 95% credible intervals tend to contain the true value on 90% of the simulation runs. This means that the posterior variability is relatively well calibrated and provides reasonably accurate information on the uncertainty in the correlation. Second, there is a fair amount of uncertainty meaning that the analyst knows that correlations have not been well localized. This lack of localization provides the needed hedge for over interpreting inflated values. With the Bayesian model-based estimates, at least we know how uncertain we are in localizing true correlations. With the Spearman correction, we have no such knowledge.

Six Tasks

We explored correlations across six tasks. Each hypothetical data set consisted of 240,000 observations. To generate a wide range of correlations, we used a one-factor model to simulate individuals' true scores. This factor represents the individual's inhibition ability. This ability, denoted z_i, is distributed as a standard normal. Tasks may require more or less of the individuals' inhibition ability. Therefore, task loadings onto this factor z_i are variable

and, as a result, a wide range of correlations occur. The following task loading values work well in producing a diversity of correlations: 0.5 ms, 5.4 ms, 10.3 ms, 15.2 ms, 20.1 ms, and 25 ms. Following the one-factor structure we may generate true scores, θ_{ij}, for each task and participant:

$$\theta_{ij} \sim \text{Normal}(\mu_j + z_i w_j, \eta^2),$$

where z_i is the true ability, w_j is the task loading, μ_j is the task overall mean, and η^2 is residual variability in addition to that from the factors. In simulation we set $\eta = 10$ ms, and this setting yields standard deviations across θ_{ij} between 10 ms and 30 ms, which is similar to the 25 ms value used previously. The true population variance for the one-factor model is $\boldsymbol{\Sigma} = \boldsymbol{w}\boldsymbol{w}' + \boldsymbol{I}\eta^2$, where $\boldsymbol{w}\boldsymbol{w}'$ is the matrix formed by the outer product of the task loadings. The true correlation matrix from the variance-covariance matrix $\boldsymbol{\Sigma}$ is shown in Figure 6A, and the values subtend a large range from near zero to 0.83.

The recovery of correlations is shown for a single simulation run in Figure 6B-D. The attenuation for the naive correlations is evident, as is variability in model-based and Spearman corrected estimates. Figure 7 shows the performance of the methods across the 10 simulation runs.

As can be seen, there remains the dramatic attenuation for the naive correlation of sample effects and excessive variability for the Spearman-corrected and model-based correlation estimates. Spearman corrected estimates are free to be outside the valid range from -1 to 1. We imagine that any researcher encountering these values could justifiably set them to the appropriate endpoint, and we do so in Figure 7. Nonetheless, the RMS errors remain high—across the whole range of true values they are 0.37 and 0.21 for the Spearman correlation and model, respectively. It is somewhat heartening that model recovery is somewhat informative.

Analysis of Rey-Mermet, Gade, and Oberauer (2018)

To assess real-world correlation recovery, we re-examined the flanker and Stroop tasks in Rey-Mermet et al.'s battery of inhibition tasks. The authors included two different types of Stroop tasks (a number Stroop and a color Stroop task, see the Appendix for details) and two different types of flanker tasks (a letter flanker and an arrow flanker task, see the Appendix for details). The question then is about the correlation across the tasks.[3]

The top three rows of Figure 8 show the estimated correlations from sample effects, Spearman's correction, and the hierarchical model. Given the previous simulations results, it is hard to know how much credence to give these estimated correlations. In particular, it is hard to know how to interpret the negative correlation between the arrow flanker and color Stroop task.

To better understand what may be concluded about the range of correlations, we plot the posterior distribution of the correlation (Figure 9A). These distributions are unsettling. The variation in most of these posteriors is so wide that firm conclusions are not possible. The exception is the null correlation between number and color Stroop which seems to be somewhat well localized. The surprisingly negative correlation between color Stroop and arrow flanker comes from a posterior so broad that the 95% credible interval is [-0.27,0.39]. Here, all we can say is that very extreme correlations are not feasible. We suspect this limited result is not news.

Analysis of Rey-Mermet et al. (2018) provides an opportunity to examine how

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[3]One of the elements that makes analysis complicated is how to exclude low-performing participants. In the previous analysis, where each task was analyzed in isolation, we retained all participants in a task who performed over 90% accuracy on that task. In the current analysis, however, we must have the same participants for all four tasks. We decided to retain those participants who have over 90% accuracy on all four tasks. With this strict criterion, we retain only 180 of the original 289 participants. The most noticeable effect of this exclusion is that the reliability for the arrow flanker task was reduced from .87 to .56. The fact that the reliability changes so much indicates that the high reliability was driven by a few participants with very large difference scores. This cutoff differs from Rey-Mermet et al. (2018), who used a 75% accuracy. With this lower cutoff, they included many more participants.

hierarchical models account for variation across trials as well as variation across people. Figure 9B shows sample effects across individuals for the color Stroop and arrow flanker tasks, the two tasks that were most negatively correlated. There is a far greater degree of variation in individual's effects for the color Stroop task than for the arrow flanker task. The model estimates (Figure 9C) reflect this difference in variation. The variation in arrow flanker is so small that it can be accounted for with trial variation alone. As a result, the hierarchical model shows almost no individual variability. In contrast, the variability in the color Stroop is large and the main contributor is true variation across individuals rather than trial variation. Hence, there is relatively little shrinkage in model estimates. The lack of variation in the arrow flanker task gives rise to the uncertainty in the recovered correlation between the two tasks.

General Discussion

A basic question facing researchers in cognitive control is whether inhibition is a unified phenomenon or a disparate set of phenomena. A natural way of addressing this question is to study the pattern of individual differences across several inhibition tasks. In this paper, we have explored whether correlations across inhibition tasks may be recovered. We consider typically large studies that enroll hundreds of participants. The answer is negative—correlations are difficult to recover with the accuracy that would allow for a definitive answer to this basic question. This statement of poor recovery holds for hierarchical models that are extended to the trial level.

Why this depressing state-of-affairs occurs is fairly straightforward. Relative to trial noise, there is little true individual variation in inhibition tasks. To see why this is so, consider an average effect, say one that is 50 ms. In inhibition tasks like Stroop and flanker, we can safely make a *dominance assumption*—nobody truly has a negative effect (Haaf & Rouder, 2017). That is to say nobody truly identifies incongruent stimuli faster than

congruent ones. Under this assumption, where all true scores are positive, a small mean necessarily implies a small variance. For example, if true Stroop effects are reasonably normally shaped and the mean is 50 ms and there can be no mass below zero, then an upper bound on variability across true scores is a standard deviation of 25 ms or so. This is a small amount of variation compared to trial variability, which is typically 7 times larger. This small degree of variation necessarily implies a small degree of covariation across tasks. This small degree of covariation is beyond the resolution of our experimental designs, and that is why our studies fail.

We believe this problem of localizing individual differences and correlations extends beyond inhibition tasks. It likely holds broadly in most task domains as most tasks have relatively small effects, whether on the order of 50 ms for RT, on the order of .08 for accuracy, or maybe on the order of 1/10th of the scale for Likert values. If we make a dominance assumption—each individual has a true effect in the same direction—then there cannot be much individual variability else these mean effects would be larger. And measuring correlations with small degrees of individual variability may be beyond the resolution of typical designs.

Recommendations

Based on the above correlation-recovery results, we can make the following recommendations:

Be mindful of attenuation. Researchers have certainly been aware of measurement error and understand the link between measurement error and attenuation. Yet, in our view, they have not asked the tough questions. Can correlations be estimated in low and medium reliability environments? Can the factor structures nonetheless be accurately recovered in low-reliability environments? How difficult is it to conclude that there is a lack or a small

correlation in such environments? Previous to our work, there were no estimates of the degree of attenuation, and hence no basis to understand its effects. Here, we argue that the critical factor—the ratio of true variability to trial noise—is on the order of 1-to-7, and may be as great as 1-to-10. Now, for various numbers of trials, researchers can compute how much attenuation is expected and use these values as context for interpretation.

Stress the number of trials in a task. In the usual course, researchers are quick to report the number of participants they have run. These numbers appear not only in method sections, but in abstracts and tables. And researchers may believe that with larger numbers of participants, results become more accurate. This is not a true statement for individual difference research with experimental tasks. The critical design element is the number of trials within a task per person. With few trials, there is much trial noise and much attenuation. Low numbers of trials add systematic bias whereas low numbers of people add unsystematic noise. Moreover, using high numbers of participants with low numbers of trials breeds high confidence in a wrong answer. We recommend researchers consider running fewer tasks and conditions to gain larger numbers of trials per task. Moreover, we recommend researchers stress the role of the number of trials in their discussion and report these numbers in their method sections, tables, and abstracts.

Stop Aggregating Trial Data / Use models that account for trial-level noise. These two recommendations go hand-in-hand. We think most psychologists would like to honor the wisdom that aggregated data is less useful than disaggregated data. Yet, to do so, one needs a plan to analyze disaggregated data. For us, that plan is to use trial-level hierarchical models. Throughout this paper, we have stressed that trial-level models may not recover correlations adequately. Yet, at least the researcher has a built-in fail safe. These models provide an estimate of how well or poorly correlations have been localized on a case-by-case basis. It is this information on the precision that allows the researcher to place correlation recovery in context. Moreover, without a trial-level model, we assuredly would have had high

confidence in a bad answer. We cannot stress this point enough—there is no loss in using models that account for trial variation. Conversely, the loss from aggregating the data in forming sample effects is dramatic as the resulting degree of measurement error is large.

The good news here is that individual difference researchers are well acquainted with hierarchical and latent variable models. The models we use here are run-of-the-mill linear mixed models with normally-distributed errors. Although we fit them in the Bayesian framework using `R` (R Core Team, 2018), there is nothing to prevent classical analysis. Classical trial-level model analysis should be convenient in a wide variety of packages including `lme4`, `Mplus`, and `AMOS`. Given the field's familiarity with mixed linear models and the accumulated expertise in application, the wide-spread use of trial-level hierarchical models is feasible. Not using such models in this context strikes us as leaving money on the table.

Don't Rely on Reliability. One maxim of individual-difference research is that correlations are interpretable with high reliability. This maxim is quite helpful in the classical test case where each individual performs a standard instrument such as a standard depression inventory. It is less helpful for experimental tasks. While reliability can tell us about the expected attenuation in correlation, it cannot tell us about the uncertainty in correlation in a sample, and it is this uncertainty that has been problematic in the face of trial noise. For our typical values, the reliability in a task was .5. That is neither high nor low given the reported data, and with it, correlations were quite variable. Moreover, in the real-world application, even though we had some tasks with high reliability, we were unable to localize correlations as well as we would have hoped. Hence, relying on high reliability to interpret correlations remains a risky proposition.

Draw a Sharp Distinction Between Tasks and Measures. We have focused here on experimental tasks where there is a theoretically-motivated contrast between conditions. The key construct is the difference score or effect, that is, the difference in performance between

congruent and incongruent conditions. The claim here is that correlations among difference scores across tasks are difficult to recover. But what about *measures*? With a measure, say a working-memory capacity measure, there is neither a critical manipulation nor a critical contrast. In practice, because there is no subtraction, measures are often highly reliable and lead to higher correlations [Draheim et al. (2019);Hedge:etal:2018]. We urge individual-difference researchers to draw a sharp distinction between scores from experimental tasks on one hand and psychological measures on the other. It is difficult to localize correlations with tasks but maybe not so with measures.

Strategies for Better Correlation Recovery

The above recommendations center on understanding how much variability and bias there is in recovering latent correlations. But they do not address the difficult situation head on. How can we improve the recovery? We consider the following possibilities:

More Trials: Perhaps the simplest solution is to run more trials per person per condition. The usual 50 or 100 trials per task per condition is clearly not enough. To calculate a good number, researchers should decide in advance how well they need to estimate an individuals' true effect. Suppose we wish to detect 25 ms true individual variability, then it is reasonable to set a maximum standard error of 10 ms on individual effects. With this value, we can calculate the number of needed trials. If people have 175 ms of trial-level noise, and we are computing a difference score, then the standard error is $175\sqrt{2/L}$, where L is the number of trials per condition per task. Setting this standard error to 10 ms yields $L = 613$, or about 1,225 trials per task per participant. Such a large number of trials per individual per task may be outside the practical constraints of some research agendas. Still, more trials is always better than less, and we can use this calculation to assess optimal conditions for individual differences research. As an aside, we recommend researchers never run neutral conditions. The contrast between incongruent and congruent is far more

important, and performance on neutral trials do not enter into correlational structures. Removing neutral conditions allows for larger numbers of congruent and incongreunt trials.

Better Tasks: Perhaps the most obvious solution is to search for inhibition tasks with greater individual variation. In practice, this means engineering tasks to have large overall effects. Yet, as far as we know, there is no magic bullet to increase effect sizes. Take, for example, manual Stroop tasks. Outside of increasing the number of responses, we do not know how to increase the size of the effect. And when the number of responses is increased, the trial variability may be increase as well. In summary, it may not be known at this time how to increase the size of effects over what is typically seen.

One way of increasing individual variability, from Engle and colleagues, is to dispense with contrasts and difference scores altogether (Draheim et al., 2019; Kane & Engle, 2003; Unsworth, Schrock, & Engle, 2004). In this approach, task scores reflect the average rather than the difference among conditions. For example, the Stroop effect could be defined as the average performance in congruent and incongruent conditions (Draheim et al., 2019). Indeed, there is far more individual variation in condition averages than in condition differences. And, as shown by Draheim and colleagues, reliability is much higher for these averages than for difference scores (see also Hedge et al., 2018). The downside of this approach, however, is interpretability. It is not clear that such condition averages can be interpreted as inhibition measures as they reflect the contribution of a host of processes and are likely dominated by a general speed component (Salthouse, 1996). Given these difficulties in interpretation, we are hesitant to recommend this approach.

Confirmatory Models: One future direction is the development of trial-level confirmatory latent-variable models. The approach we have taken here may be compared to exploratory factor modeling in as much as we add no constraints to the covariance matrix. Covariances are free to take on any values as long as the matrix meets the usual symmetry and positive-definite constraints. In this sense, any factor structure is plausible, and as a

result, we can think of the approach in Equation (1) as highly flexible. Yet, addressing substantive questions often does not require such flexibility. We think the next step is to use models with greater constraint, that is, models that are comparable to confirmatory factor models. For example, if we are interested in the basic question whether there is a unified concept of inhibition, we might develop a trial-level one-factor model or a trial-level bifactor model. There are two related sanguine possibilities. First, with the reduction of flexibility, it may be possible to better localize correlations among tasks. Second, and perhaps more importantly, localization of correlations may become secondary to model assessment and model comparison. How well does one trial-level confirmatory structure compare to another?

Of course, comparison of confirmatory models is not at all new. And while there has been a large corpora of studies, to our knowledge, none of the studies using task-based difference scores uses trial-level data. Hence, the inputs to these models, the sample correlations, are already attenuated by trial noise. The results to date with these confirmatory factor models have not been as productive as one might hope. In a large-scale review, Karr et al. (2018) compared results from seven leading factor models of executive function across 46 studies. They found no clear winner— no model fit well in more than half the studies and that no model was selected in greater than one-third of the studies. They conclude that there may be a tendency to publish well-fitting but underpowered models. We offer as conjecture that by using more of the data and by modeling trial noise, trial-based confirmatory models may be more informative and productive than their classical counterparts.

Concluding Thought

We have shown a deep and pervasive problem with the interpretation of individual differences with experimental tasks. Solving this problem is going to entail larger experiments, perhaps better tasks, and most importantly, trial-level latent-variable models

for analysis. We hope this paper lays a foundation for understanding what is at stake and motivates the needed developments. Although the message here can be viewed as disheartening, we think in the long run, given the talent in the field, individual-difference researchers are going to rise to the challenge because these solutions are within our grasp.

Appendix

Data Set 1, Von Bastian et al. (2015): The task was a number Stroop task. Participants were presented a string of digits. In each string, the digits were always replicates, say *22* or *444*, and the lengths varied from one digit to four digits. The participants identified the length of the string, for example, the correct report for *444* is 3. In the congruent condition, the length and the digits matched; e.g., *22* and *4444*. In the incongruent condition, the length and digits mismatched, e.g., *44* and *2222*. We used somewhat different data cleaning steps than the original authors. Ours are described in Haaf and Rouder (2017).

Data Set 2, Pratte, Rouder, and Morey (2010), Experiment 1: The task was a color Stroop task. Participants identified the color of the color words, e.g. the word *RED* presented in blue. In the congruent condition, presentation color and word meaning matched, e.g. *BLUE* presented in blue. In the incongruent condition, they did not match, e.g. *RED* presented in blue. We used the original authors' cleaning steps.

Data Set 3, Pratte et al. (2010), Experiment 2: The task was a sidedness judgment Stroop task. Participants were presented the words *LEFT* and *RIGHT*, and these were presented to the left or right of fixation. Participants identified the position of the word while ignoring the meaning of the word. A congruent trial occurred when position of the word and word meaning corresponded; an incongruent trial emerged when position and word meaning did not correspond. We used the original authors' cleaning steps.

Data Set 4, Rey-Mermet et al. (2018): The task was a number Stroop task. Participants identified the length of digit strings much like in Data Set 1. Cleaning proceeded as follows. First, note that in the original, trials ended at 2.0 seconds even if the participant did not respond. We call these trials *too slow*. 1. We discarded the five participants discarded by the original authors; 2. We discarded too-slow trials, error trials, and trials

with RTs below .275 seconds (*too-fast* trials). 3. We discarded all participants who had more than 10% errors, who had more than 2% too-slow trials, or more than 1% too fast trials.

Data Set 5, Rey-Mermet et al. (2018):   The task was a color Stroop task. Participants identified the color of the presented words (red, blue, green, or yellow). The presentation color and word meaning matched in the congruent condition and did not match in the incongruent condition. Cleaning steps were the same for Data Set 4.

Data Set 6, Hedge et al. (2018):   The task was a color Stroop task. Participants identified the color of a centrally presented word (red, blue, green, or yellow). In the congruent condition, presentation color and word meaning matched. In the incongruent condition, they did not match. Following Hedge et al. (2018), we combined data from their Experiments 1 and 2. Our cleaning steps differed from Hedge et al. (2018) and are described in Rouder and Haaf (2019).

Data Set 7, Von Bastian et al. (2015):   The task was a Simon task. Participants were presented either a green or red circle to the left or right of fixation. They identified the color, green or red color by pressing buttons with their left or right hand, respectively. The spatial location of the circle and of the response could be either congruent (e.g., a green circle appearing on the left) or incongruent (e.g., a green circle appearing on the right). Cleaning steps are described in Haaf and Rouder (2017).

Data Set 8, Pratte et al. (2010), Experiment 1:   The task was a Simon task almost identical to that in Data Set 7. Participants identified the color of a square presented to the left or right of fixation by making a lateralized key response. A congruent trial occurred when position of the square was ipsilateral correct key response.; an incongruent trial occurred when the position of the square was contralateral to the correct key response. We used the original authors cleaning steps.

Data Set 9: Pratte et al. (2010), Experiment 2:   The task was a *lateral-words* Simon task. Participants were presented the words *LEFT* and *RIGHT* to the left or right of fixation. Participants identified the meaning of the word while ignoring the location of the word. A congruent trial occurred when position of the word and word meaning corresponded; an incongruent trial occurred when position of the word and word meaning did not match. We used the original authors cleaning steps.

Data Set 10, Von Bastian et al. (2015):   The task was a letter-flanker task. Participants were presented strings of seven letters and judged whether the center letter was a vowel (*A*, *E*) or consonant (*S*, *T*). The congruent condition was when the surrounding letters came from the same category as the target (e.g. *AAAEAAA*); the incongruent condition was when the surrounding letters came from the opposite category of the target (e.g., *TTTETTT*). Cleaning steps are described in Haaf and Rouder (2017).

Data Set 11, Rey-Mermet et al. (2018):   The task was an arrow flanker task. Participants identified the direction of the central arrow (left/right) while ignoring four flanking arrows. Congruency and incongruency occurred when the center arrow matched and mismatched the direction of the flanker arrows, respectively. Cleaning steps were the same for Data Set 4.

Data Set 12, Rey-Mermet et al. (2018):   The task was a letter-flanker task almost identical to Data Set 10. Cleaning steps were the same for Data Set 4.

Data Set 13: Hedge et al. (2018):   The task was an arrow flanker task almost identical to Data Set 11. Following Hedge et al. (2018), we combined data from their Experiments 1 and 2. Our cleaning steps differed from Hedge et al. (2018) and are described in Rouder and Haaf (2019).

Data Set 14, Rouder et al. (2005):   The task was a digit-distance task. Participants were presented digits 2, 3, 4, 6, 7, 8, and had judged whether the presented digit

was less-than or greater-than five. Digits further from five are identified faster than those close to 5. Responses to digits 2 and 8 comprised the *far* condition; responses to digits 4 and 6 comprised the *close* condition. The difference in conditions comprised a *distance-from-five* effect. We used the original authors' cleaning steps.

Data Set 15, Rouder, Yue, Speckman, Pratte, and Province (2010): The task was a grating-orientation discrimination. Participants were presented nearly-vertical Gabor patches that were very slightly displaced to the left or right; they indicated whether the displacement was left or right. Displacements were $\pm 1.5°$, $\pm 2.0°$, and $\pm 4.0°$ from vertical. Responses from the $\pm 1.5°$ comprised the *hard* condition; responses from the $\pm 4.0°$ comprised the *easy* condition; the difference comprised a *orientation-strength* effect. We used the original authors' cleaning steps.

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Table 1

	Sample Sizes			Reliability		Sample		Parameters		Ratio
	Obs	Indv	Rep	Full	Split	Effect	s_d	$\hat{\sigma}$	$\hat{\sigma}_\theta$	$\hat{\eta}$
Stroop										
1. von Bastian	11,245	121	46	0.24	0.34	64	47	198	23	0.12
2. Pratte i	11,114	38	146	0.61	0.68	91	50	264	36	0.14
3. Pratte ii	12,565	38	165	0.19	-0.07	12	20	160	15	0.10
4. Rey-Mermet i	48,937	264	93	0.40	0.57	54	30	155	19	0.12
5. Rey-Mermet ii	48,966	261	94	0.86	0.84	59	69	175	64	0.36
6. Hedge	43,408	53	410	0.83	0.75	70	32	188	29	0.16
Simon										
7. von Bastian	23,453	121	97	0.60	0.61	79	36	128	28	0.22
8. Pratte i	17,343	38	228	0.46	0.62	17	24	186	18	0.10
9. Pratte ii	12,266	38	161	0.57	0.51	30	30	175	22	0.13
Flanker										
10. von Bastian	11,215	121	46	-0.02	-0.55	2	32	152	15	0.10
11. Rey-Mermet i	49,300	265	93	0.18	0.17	30	24	147	13	0.09
12. Rey-Mermet ii	39,275	207	95	0.87	0.87	36	43	107	40	0.37
13. Hedge	43,384	53	409	0.80	0.79	44	16	100	15	0.15
Other										
14. Rouder i	11,346	52	109	0.37	0.35	50	28	165	19	0.11
15. Rouder ii	16,859	58	145	0.62	0.62	142	72	351	52	0.15
Mean	26,712	115	156	0.51	0.47	52	37	177	27	0.16
Median	17,343	58	109	0.57	0.61	50	32	165	22	0.13

Note. All sample sizes and estimates reflect cleaned data. See the Appendix for our cleaning steps which differ from those of the original authors.
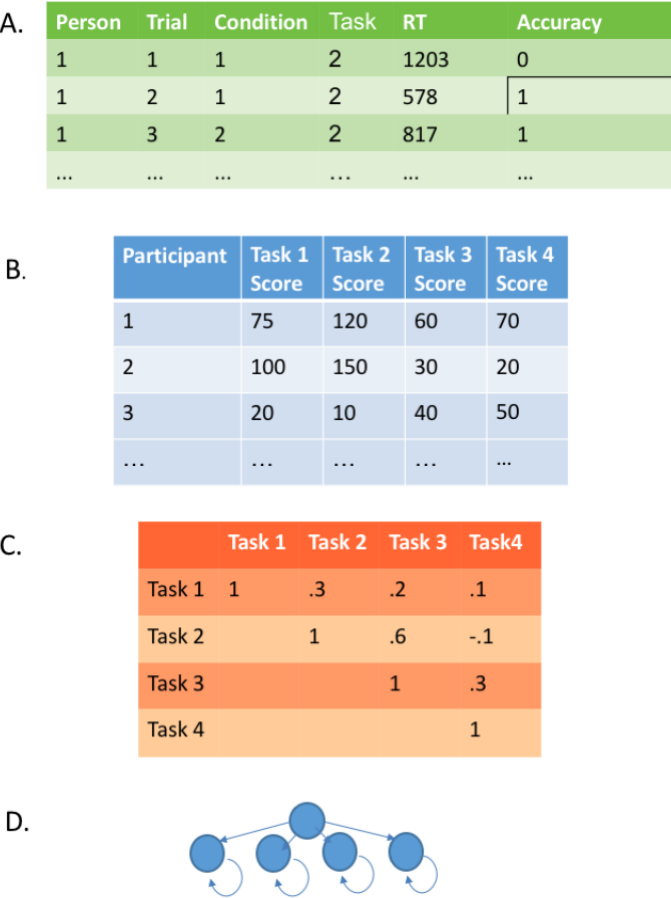
A.

Person	Trial	Condition	Task	RT	Accuracy
1	1	1	2	1203	0
1	2	1	2	578	1
1	3	2	2	817	1
...	...	...	...	...	...

B.

Participant	Task 1 Score	Task 2 Score	Task 3 Score	Task 4 Score
1	75	120	60	70
2	100	150	30	20
3	20	10	40	50
...	...	...	...	...

C.

	Task 1	Task 2	Task 3	Task4
Task 1	1	.3	.2	.1
Task 2		1	.6	-.1
Task 3			1	.3
Task 4				1

D.

Figure 1. In the usual course of analysis, the raw data (A) are used to tabulate sample effects (B). The covariation among these task-by-person sample effects (C) then serve as input to latent variable modeling (D).
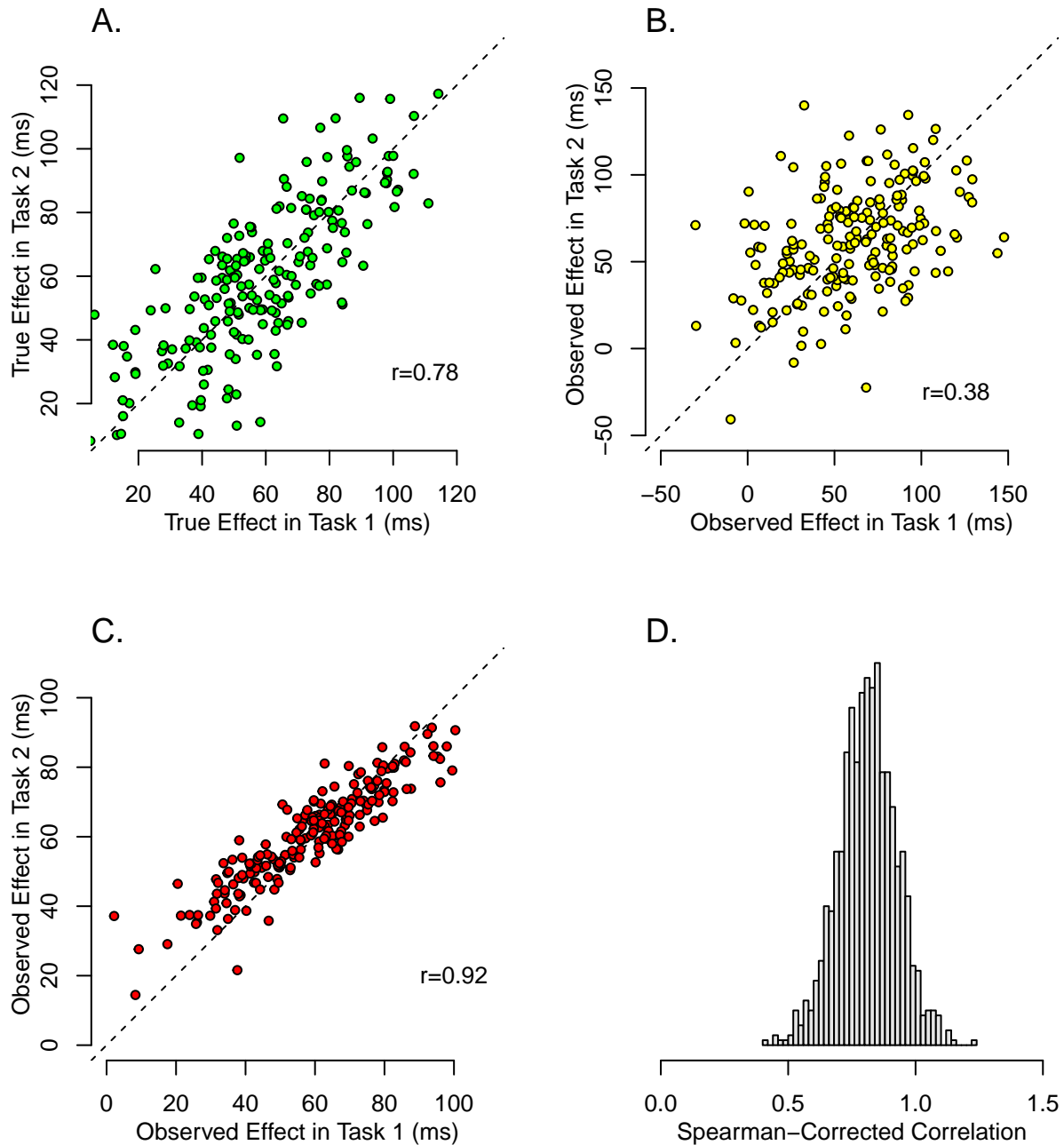
Figure 2. The effects of trial variability on the assessment of correlations among tasks. A: Hypothetical true individual effects show a large degree of correlation across two tasks. B: Observed effects are so perturbed by trial variability that the correlation is greatly attenuated. C: Hierarchical model recovery for the data in A. D: Spearman correction-for-attenuation in a small simulation with realistic settings.
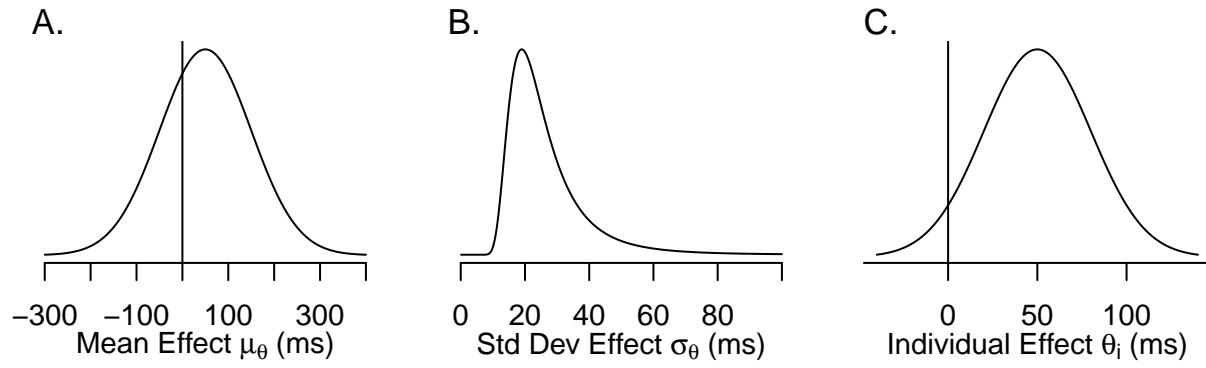
Figure 3. A, B: Prior distributions of μ_θ and σ_θ, respectively. C: Prior distribution of θ_i for $\mu_\theta = 50$ ms and $\sigma_\theta = 30$ ms.
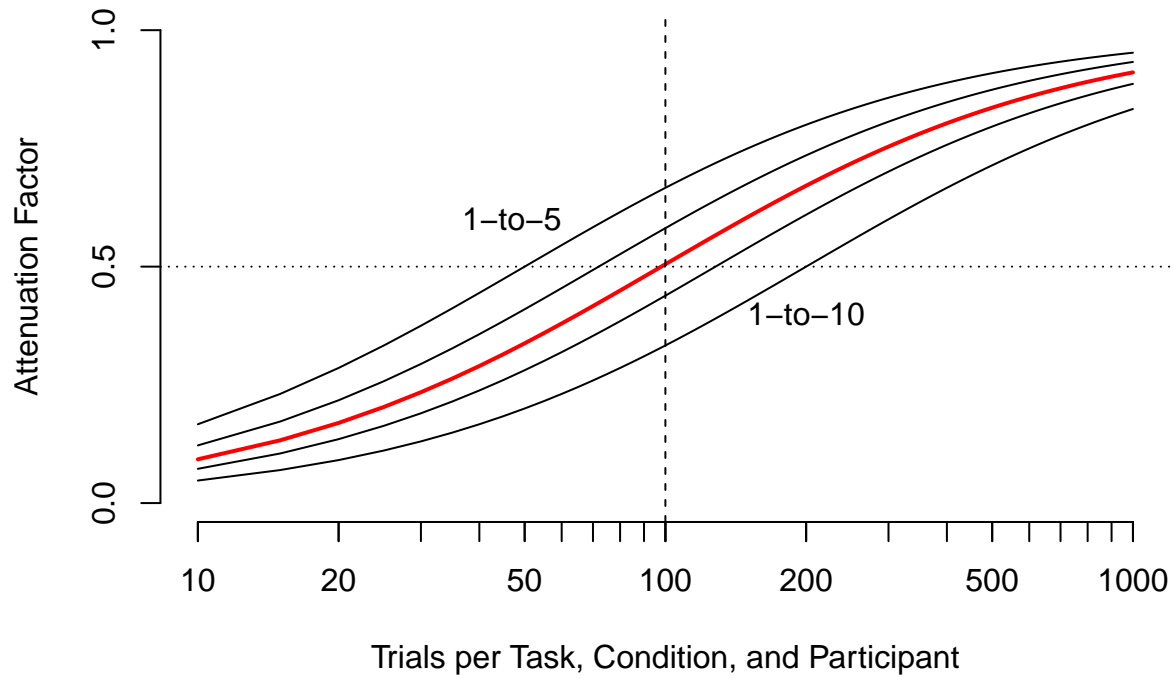
Figure 4. Attenuation of correlation as a function of number of trials (L) and signal-to-noise ratio η. For typical values ($L = 100$, η=1-to-7), the attenuation is a factor of $1/2$. The plotted values of η are 1-to-10, 1-to-8, 1-to-7, 1-to-6, and 1-to-5.
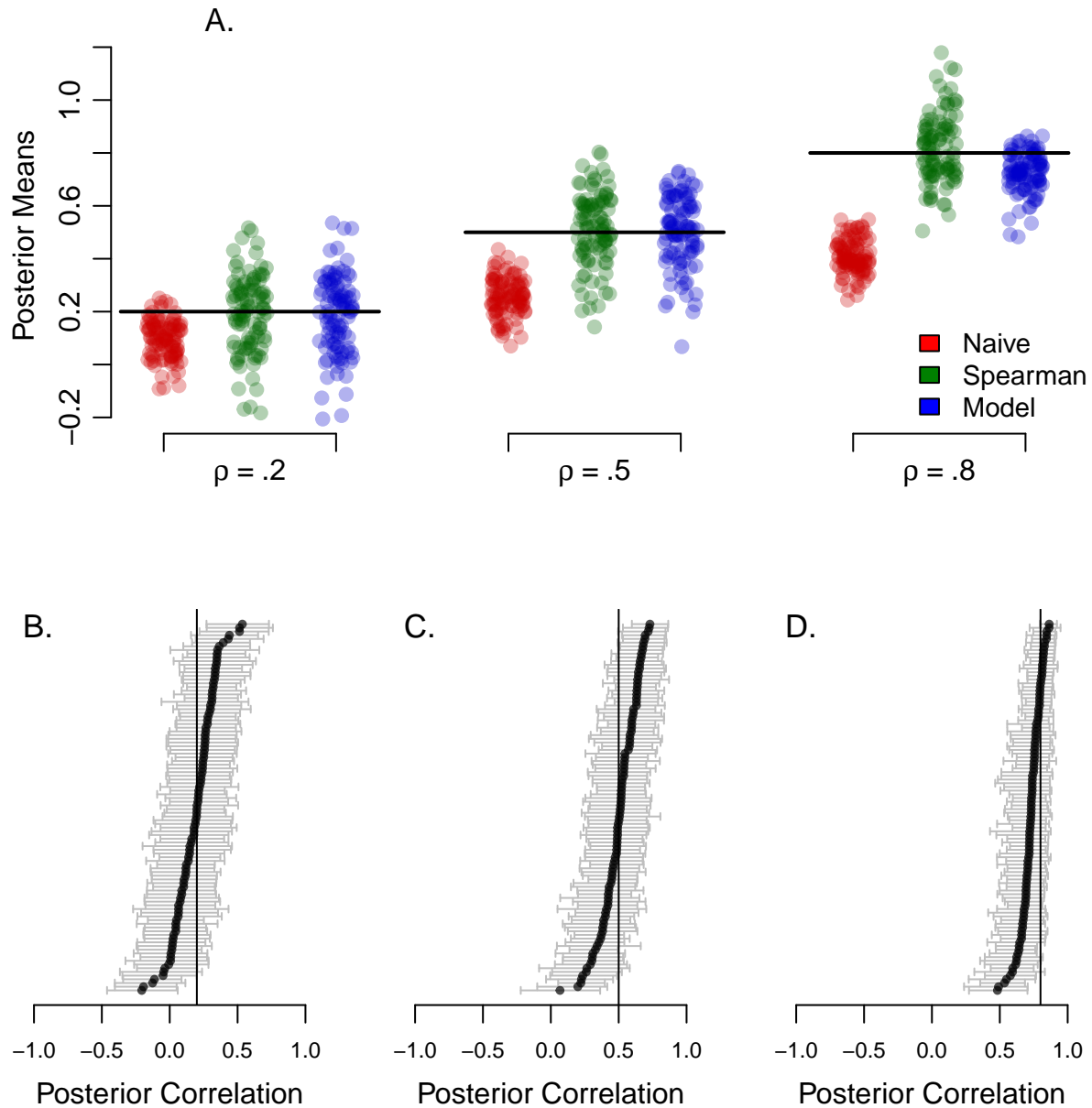
Figure 5. Revocery of correlations from two tasks. A: Boxplots of recovered correlations from naive sample correlations, Spearman's correction, and the hierarchical model. B-D: Posterior 95% credible intervals for the model-recovered correlations for true correlations of .2, .5, and .8, respectively.
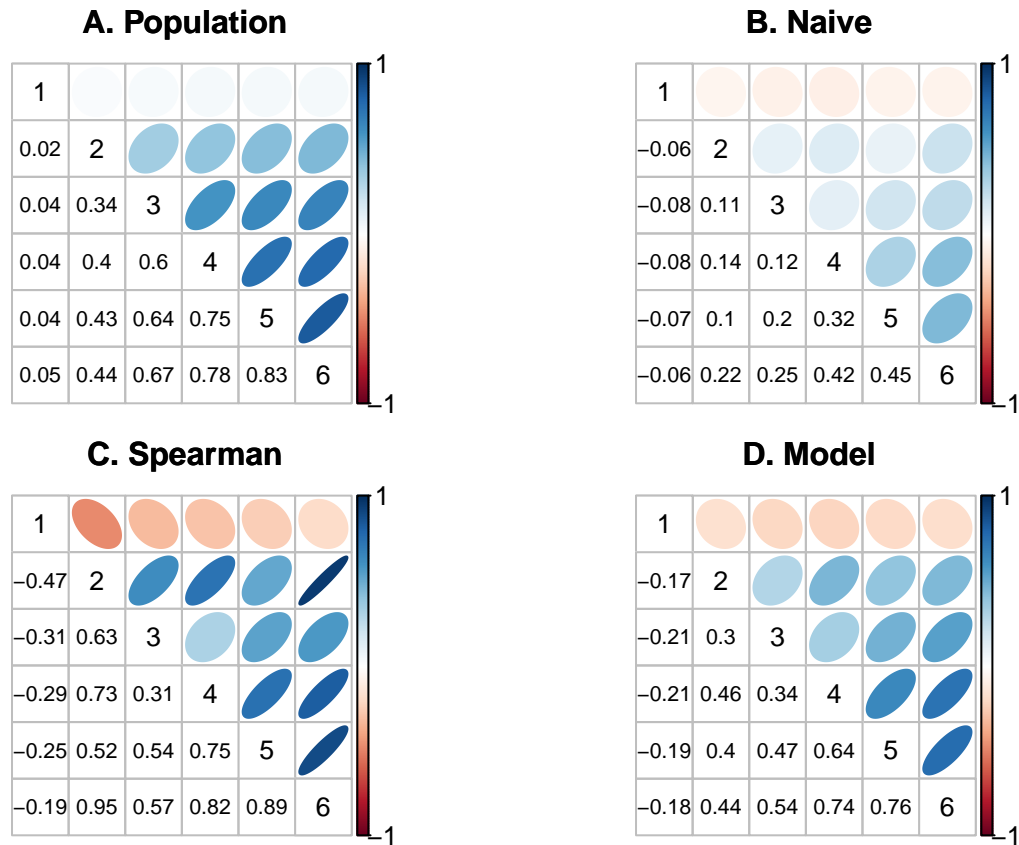
Figure 6. True and recovered correlation matrices for six tasks. A: True population correlations. B-D: Correlation estimates from a single run.
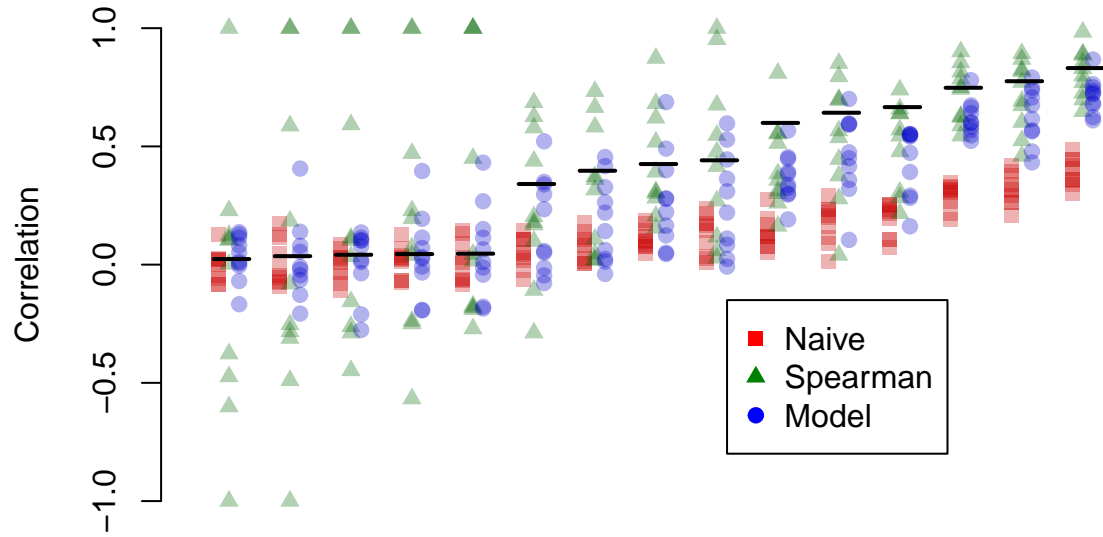
Figure 7. Recovery of correlations from six tasks. True correlations are derived from a one-factor model and are displayed in Figure 6.
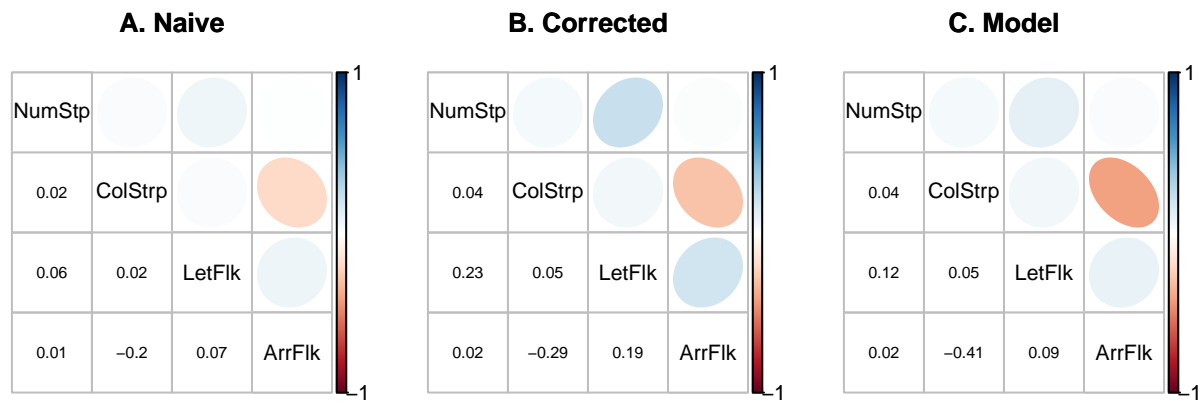
Figure 8. Correlations among select tasks in the Rey-Mermet data set. Tasks are a number Stroop task, a color Stroop task, a letter flanker task, and an arrow flanker task. Details of the tasks are provided in the Appendix.
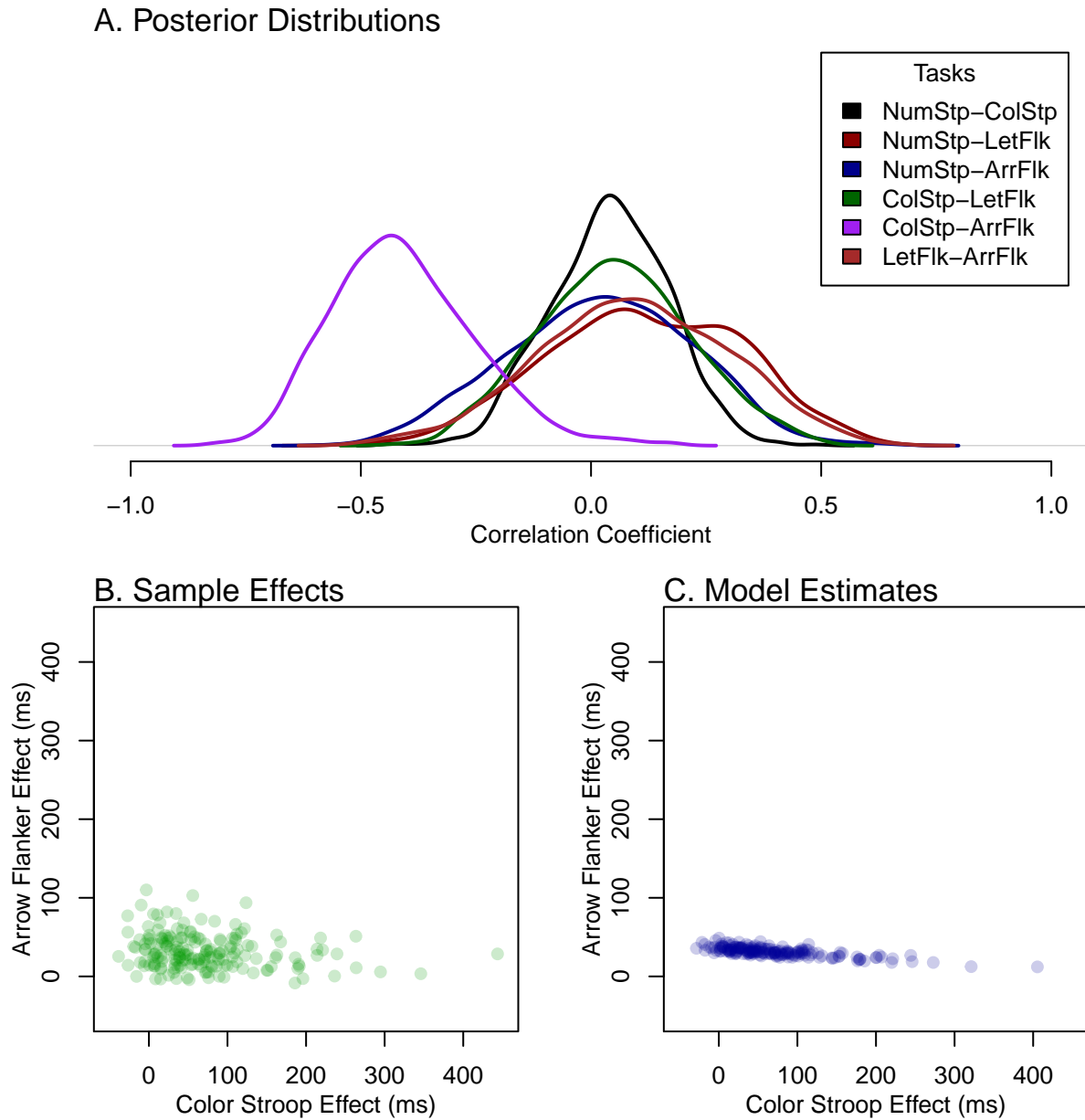
Figure 9. A. Model-based posterior distributions of population correlations among tasks. The large variance shows the difficulty of recovery. B. Individuals' sample effects for color Stroop and arrow flanker tasks show. C. Hierarchical model estimates show a large degree of shrinakge for arrow flankers but not for color Stroop reflecting the increased range of color Stroop effects.