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Another Warning about Median Reaction Time

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Contrary to the warning of Miller (1988), Rousselet and Wilcox (2020) argued that it is better to summarize each participant's single-trial reaction times (RTs) in a given condition with the median than with the mean when comparing the central tendencies of RT distributions across experimental conditions. They acknowledged that median RTs can produce inflated Type I error rates when conditions differ in the number of trials tested, consistent with Miller's warning, but they showed that the bias responsible for this error rate inflation could be eliminated with a bootstrap bias correction technique. The present simulations extend their analysis by examining the power of bias-corrected medians to detect true experimental effects and by comparing this power with the power of analyses using means and regular medians. Unfortunately, although bias-corrected medians solve the problem of inflated Type I error rates, their power is lower than that of means or regular medians in many realistic situations. In addition, even when conditions do not differ in the number of trials tested, the power of tests (e.g., *t*-tests) is generally lower using medians rather than means as the summary measures. Thus, the present simulations demonstrate that summary means will often provide the most powerful test for differences between conditions, and they show what aspects of the RT distributions determine the size of the power advantage for means.

Keywords: reaction time, power, means, medians, within-subjects comparisons

Introduction

In typical reaction time (RT) experiments, researchers collect many RTs per participant in each condition that are then compared via repeated-measures *t*-tests or ANOVAs. When they want to determine whether the central tendencies of the RTs differ between conditions, they are faced with the problem of how to summarize the many within-condition RTs per participant into a single number for use in the repeated-measures test. Various summary measures have been used for this purpose—most commonly the means and medians of the within-condition RTs for each participant.

Miller (1988) warned that when RT distributions are skewed, as they usually are, median RTs are biased. Furthermore, this bias is larger when the number of trials per condition is small. He therefore recommended that medians should not be used when comparing conditions with different numbers of trials, because the larger bias could cause conditions with fewer trials to appear slower, *even with identical RT distributions in both conditions*. Rousselet and Wilcox (2020; henceforth, R&W) recently disputed this recommendation based on an extensive series of simulations examining means, medians, and several other summary measures. In particular, they used a standard percentile bias correction procedure (e.g., Efron, 1979, Efron and Tibshirani, 1993) and found that it successfully eliminated the bias problem identified by Miller (1988). In brief, their procedure estimates the median bias as the difference between the observed median and the average median across many bootstrap samples. The observed median is then corrected by subtracting this estimated bias, and the final result of this subtraction is taken as the bias-corrected median estimate (for further details, see Rousselet and Wilcox, 2020). In view of the fact that the correction procedure eliminated median bias and other aspects of their analysis, R&W concluded that "the recommendation by Miller (1988) to not use the median when comparing distributions that differ in sample size was ill-advised" (p. 31). Their conclusions have been influential in encouraging researchers to analyze median RTs (e.g., Gordon et al., 2020, Maksimenko et al., 2019, Thornton and Zdravković, 2020).

The present article reexamines the use of mean RT, median RT, and bias-corrected median RT as summary measures for the central tendency of an individual participant's RTs observed in a particular experimental condition, focusing on the statistical power of each summary measure. It is obviously desirable to use a summary measure that provides as much power as possible while staying within the chosen level of Type I error rate[1]. In particular, the present simulations sought to identify the summary method that would provide the greatest power when comparing condition *means of the summary scores* across participants via parametric tests (e.g., *t*-tests or ANOVAs), as is most commonly done. Although this question has been looked at previously, it appears that power is sometimes higher for means and sometimes higher for medians (e.g., Ratcliff, 1993, Rousselet and Wilcox, 2020), and there has been no clear characterization of the conditions under which each one is superior.

The primary simulations reported in this article used the ex-Gaussian distribution as an ad hoc descriptive model of RT, because this simple distribution generally provides good fits to observed RT distributions (e.g., Luce, 1986, Hohle, 1965). The ex-Gaussian can be conceived of as the sum of two independent random variables. One is a normal with mean μ and standard deviation σ, the other is an exponential with mean τ, and the overall mean RT is the sum of μ and τ. Examples of these distributions are shown in Figure 1, which illustrates that the exponential τ parameter reflects the skewness of the RT distribution—that is, the length of the long tail of slow responses characteristically seen in real RT data (Burbeck and Luce, 1982 ,Luce, 1986, Hohle, 1965). The flexibility of the ex-Gaussian in describing distributions with different amounts of skew makes it a useful model for simulations investigating Type I error rates, because these depend on skew (e.g., Miller, 1988).

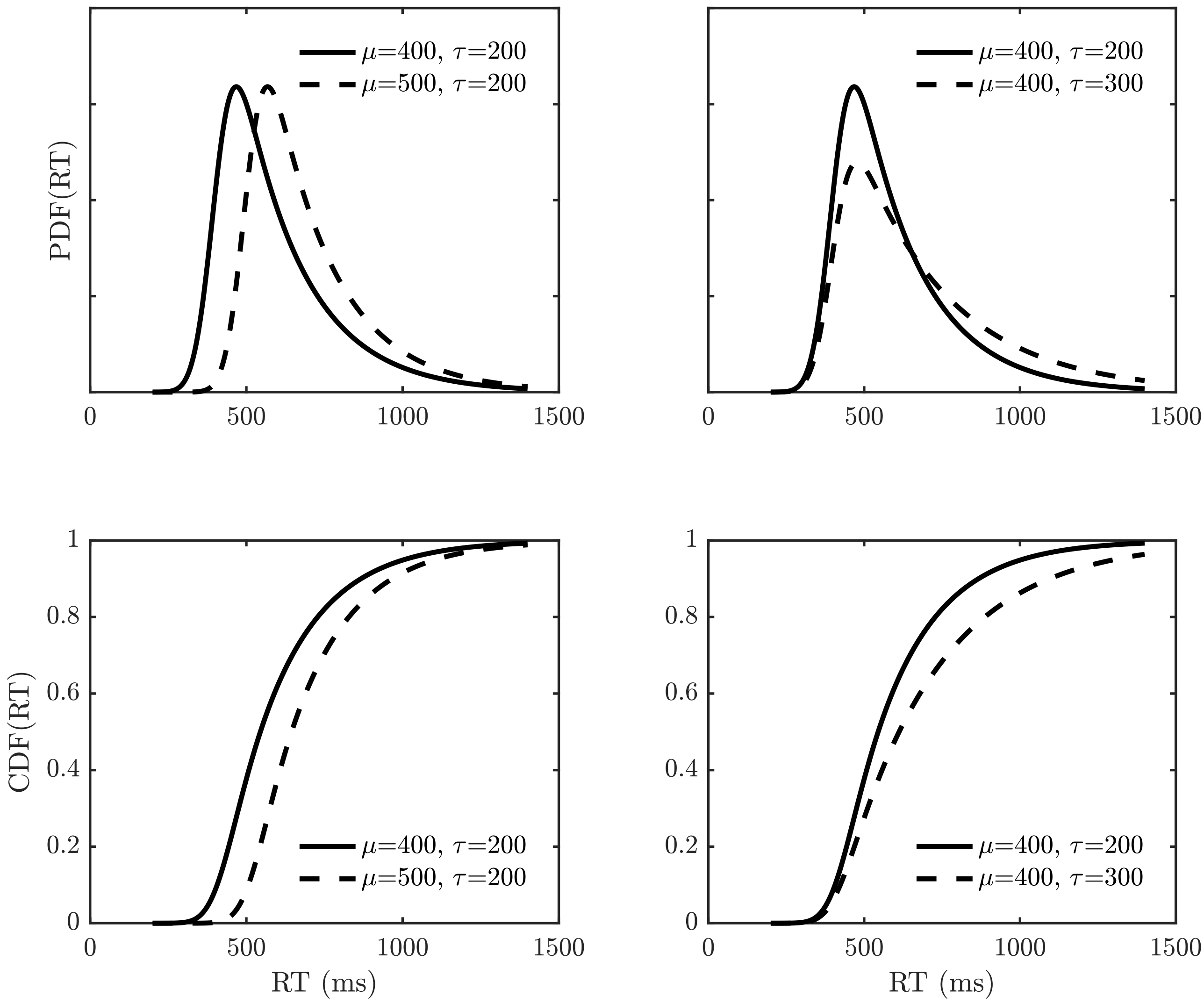
In addition to the ex-Gaussian, simulations were also carried out using four other statistical models for RT distributions in order to make sure that the obtained results were not idiosyncratic to the ex-Gaussian. Specifically, these were the ex-Wald distribution (e.g., Schwarz, 2001), the shifted lognormal distribution, the shifted gamma distribution, and the three-parameter (i.e., shifted) Weibull distribution. As is illustrated with the examples in Figure 2, these are all similar to observed RT distributions in that they are skewed with a long tail at the high end. For each of the different ex-Gaussian distributions that we examined, parallel simulations of 1,000 experiments were also carried out with each of these alternative distributional models. For these parallel simulations, the parameters of each alternative distribution were adjusted so that the alternative distribution matched the corresponding ex-Gaussian at the 5th, 50th, and 95th percentile points, so we will refer to these as the "percentile-matched" distributions. To foreshadow the results, the patterns obtained with all of these percentile-matched distributions closely matched the presented patterns obtained with the ex-Gaussian. More specifically, although the relative performance of the mean, median, and bias-corrected median summary measures depends strongly on RT skewness, it depends hardly at all on the precise underlying distribution family producing that skewness.

The ex-Gaussian and other skewed distributions are helpful not only in describing single RT distributions but even more so in describing the effects of experimental manipulations on these distributions. Observed RT distributions can easily differ in ways that are too complex to summarize in a single measure of central tendency such as a mean, so other descriptors of distributional changes can provide useful clues about the causes of experimental effects (e.g., Balota et al., 2008, Balota and Yap, 2011, Heathcote et al., 1991). Besides being of interest in their own right, these distributional differences may also have implications for the choice of the most appropriate measure of central tendency to be used when that is the research focus. One possibility, illustrated by the pair of ex-Gaussians on the left of Figure 1, is that the experimental manipulation *shifts* the distribution to the right in the slower condition, which is described within the ex-Gaussian model by an increase in the μ parameter with no change in skewness. For example, using a spatial Simon paradigm (e.g., Hommel, 2011), Luo and Proctor (2018) asked participants in their Experiment 1 to respond with the left versus right hand to red versus green squares that appeared irrelevantly to the left or right of fixation. Even though location was irrelevant, responses were faster when the square appeared on the same side as the required response than when it appeared on the opposite side. At the distributional level, this RT difference was well described as a shift effect reflected entirely in the μ parameter, with no change in skewness (τ). Another possibility, illustrated by the pair of ex-Gaussians on the right side of Figure 1, is that the experimental manipulation *stretches the tail* of the RT distribution in the slower condition, essentially increasing its skew, which can be described as an effect that is entirely on τ. For example, in their Experiment 3, Luo and Proctor (2018) asked participants to respond with the left versus right hand to red versus green arrows that pointed irrelevantly to the left or right, and responses were faster when the arrow pointed to the same side as the required response than when it pointed to the opposite side. This time, however, the RT difference was mainly due to a stretched

[1]R&W also evaluated different summary measures with respect to various criteria for identifying "the typical value of a distribution, which provides a good indication of the location of the majority of observations" (p. 2). I will not address those criteria in the present article, but only consider the value of the measures for standard hypothesis testing, which is a very common statistical procedure with such data.

Figure 1

Example probability density functions (PDFs) and cumulative distribution functions (CDFs) for three ex-Gaussian distributions differing in μ and τ, all with $\sigma = 50$. A reference distribution with $\mu = 400$ and $\tau = 200$ (solid lines, mean 600 ms, median 544.82 ms) is shown on all panels to facilitate visualization of the effects of changing μ versus τ. The comparison distributions (dotted lines, with mean 700 ms) differ with respect to either μ (left panels, median 644.82 ms)

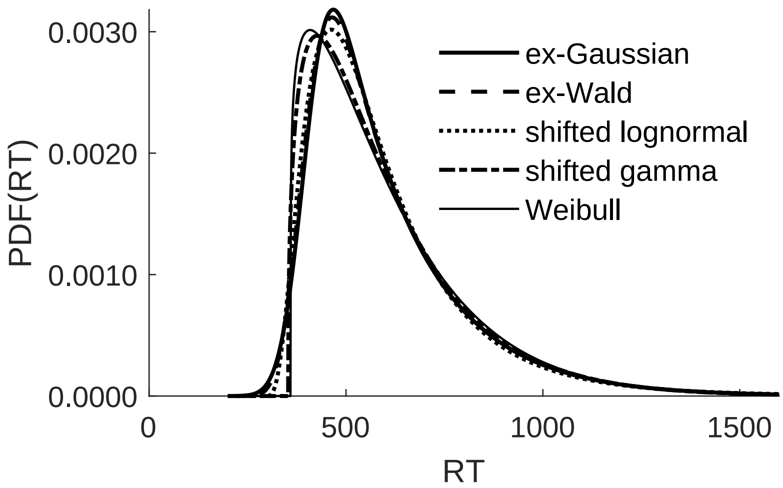


tail, with increased skew reflected in a larger τ, and there was little change in μ.

Since the introduction of the ex-Gaussian by Hohle (1965), many studies have examined the shifting versus tail-stretching effects of various experimental manipulations on the shapes of RT distributions as described in terms of μ and τ. Both μ and τ are typically larger in the slower condition than in the faster one, indicating that most experimental manipulations have both shifting and stretching effects, in varying mixtures. There is unfortunately no consensus about the psychological meanings of changes in these different parameters, because there are at best weak distinctions between experimental manipulations with shifting versus stretching effects (e.g., Matzke and Wagenmakers, 2009, Rieger and Miller, 2020), but the ex-Gaussian distribution nevertheless remains useful as a way of describing changes in the shapes of RT distributions as well as their means.

Figure 2

Example probability density functions (PDFs) for the different RT distribution families examined. The ex-Gaussian distribution has parameters of $\mu = 400$*,* $\sigma = 50$*, and* $\tau = 200$*. The parameters of the other distributions were adjusted to match the ex-Gaussian at the 5th, 50th, and 95th percentile points, leading to the following parameter values: ex-Wald: Wald* $\mu = 399.9$ *and* $\sigma = 50.9$*, and exponential* $\tau = 199.9$*; shifted lognormal:* $\mu = 312.9$ *and* $\sigma = 215.6$*, with a shift of* $C = 287.2$*; shifted gamma:* $\mu = 246.3$ *and* $\sigma = 206.1$*, with a shift of* $C = 353.0$*; Weibull:* $\mu = 239.8$ *and* $\sigma = 205.2$*, with an offset of* $C = 359.5$*.*



For the present purposes, the distinction between shifting and stretching effects is relevant because—as will be seen—statistical tests based on means, medians, and bias-corrected medians are especially different in their power to detect stretching effects.

Type I Error Rates

For completeness, and to make the simulation process more concrete, this section reviews briefly the well-established fact that Type I error rates are inflated by sample-size-dependent bias when medians are used to compare RTs across conditions with unequal numbers of trials (which I will call unequal trial "frequencies" rather than "sample size", to avoid confusion with the number of participants). This bias is an artifact that would contaminate comparisons of conditions with different trial frequencies if medians were used to summarize the RTs in each condition. Originally, comparisons of such conditions were used particularly in studies of the main effects of stimulus and response probability (e.g., Hyman, 1953), attentional cuing (e.g., Posner et al., 1978), and expectancy (e.g., Mowrer et al., 1940, Zahn and Rosenthal, 1966). In addition, trial frequencies have often been varied across conditions to explore a variety of cognitive processes by investigating their interactions with probability (e.g., Broadbent and Gregory, 1965, Den Heyer et al., 1983, Miller and Pachella, 1973, Sanders, 1970, Theios et al., 1973). Currently, trial frequencies are commonly varied in studies of spatial and temporal statistical learning (e.g., Flowers et al., 2021, Gibson et al., 2021, Liesefeld and Müller, 2021, Vadillo et al., 2021), the modulation of attentional control processes by environmental contingencies (e.g., Cochrane et al., 2021, Huang et al., 2021, Kang and Chiu, 2021), action-outcome contingency learning (e.g., Gao and Gozli, 2021), adaptation to the frequency of congruent versus incongruent information (e.g., Bausenhart et al., 2021, Ivanov and Theeuwes, 2021, Thomson et al., 2021), and between-task resource sharing (e.g., Miller and Tang, 2021), to name just a few areas. Unfortunately, median bias is still sometimes overlooked and may contaminate published comparisons of conditions with different trial frequencies (e.g., Bulger et al., 2021).

As noted by Miller (1988) and confirmed by R&W's Table 2, sample medians are biased with skewed distributions, and the bias is greater when the number of trials is smaller. If medians are used to compare conditions with different trial frequencies, this bias causes the Type I error rate to be inflated—perhaps seriously. Specifically, the low-frequency condition will often appear to be statistically slower than the high-frequency condition, even if the true RT distributions are identical in the two conditions.

A simple simulation of 5,000 experiments illustrates the problem. In each simulated experiment, RTs were generated for 60 participants. Each participant was tested for 51 trials in the "frequent" condition and 5 trials in the "infrequent" condition, with odd numbers of trials used so that the median of each sample would be the unique middle score. The null hypothesis was always true—that is, RTs for both conditions were sampled from the same underlying ex-Gaussian distribution with $\mu = 400$, $\sigma = 50$, $\tau = 200$ shown in Figure 1. Within each simulated experiment, the RTs sampled for each participant were summarized by computing the median in each condition. Using these medians as the dependent variable, a paired t-test comparing the means of these medians was then computed across the 60 participants, with $\alpha = 0.05$, two-tailed. Since the null hypothesis was true in the simulated experiments, one would theoretically expect approximately 5% significant results (i.e., Type I errors) by chance, with half of these yielding significantly larger scores in the frequent condition and half significantly larger scores in the infrequent condition. However, the simulation actually produced 17.8% Type I errors where the infrequent condition appeared slower versus only 0.1% where the frequent condition appeared slower. Thus, in accordance with the warning of Miller (1988), comparing the means of participant/condition median RTs produced far too many Type I errors in the direction that would lead researchers to conclude that responses are slower in the infrequent condition.

The inflated Type I error rate for medians arises for purely statistical reasons. As is described in the Appendix, the full sampling distribution of the sample medians can be computed numerically using the known properties of order statistics (i.e., the median of the smaller sample is the third order statistic in a sample of five, and the median of the larger sample is the 26th order statistic in a sample of 51), and these sampling distributions are shown in Figure 3. Crucially, the means of these sampling distributions are 561.4 and 546.7, respectively, so the long-run mean of the smaller sample medians really is larger than that of the larger samples. The t-test results simply reflect this true difference in average medians for samples of these two sizes from this distribution. In comparison, across exactly the same simulated datasets using each participant's condition mean or bias-corrected median[2] as the summary measure, approximately 2.5% Type I errors in each direction were obtained, as expected.

Parallel simulations were carried out to determine the extent of error rate inflation under a variety of different simulation conditions, and representative results are shown in Figure 4. The different simulation conditions used: (a) varying numbers of trials N in the infrequent condition (the frequent condition always had 51 trials), as shown along the horizontal axis; (b) ex-Gaussian (or corresponding percentile-matched distributions) with different values of μ and τ to vary the degree of skewness, shown as different lines; and (c) 30 or 60 participants in the experiment, shown in the panels on the left or right. The vertical axis shows the proportion of simulated experiments in which researchers would reject the null hypothesis and conclude that responses were slower in the infrequent condition. Since scores in both conditions were actually always drawn from the same distribution, these would again be Type I errors in that direction. Obviously, the Type I error rates for the median analyses can far exceed the appropriate 2.5% with small Ns in the infrequent condition, whereas the error rates for the means do not. Bias-corrected medians also produced appropriate error rates, replicating R&W's results.

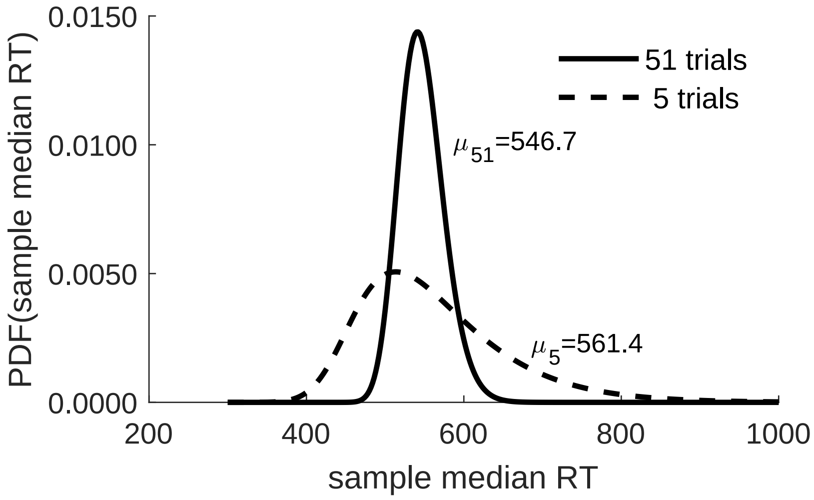
Very similar patterns of Type I error rates were obtained in the simulations with the other four percentile-matched distributions used as RT models (i.e., ex-Wald, shifted lognormal, etc). For example, across the 32 simulation conditions shown in Figure 4, the average Type I error rate for the median was 6.7% for the ex-Gaussian, whereas it ranged from 6.1% to 6.7% with the other four distributions. Similarly, the Type I error rate exceeded 15% for all distributions in the worst case (i.e., the simulation with 60 participants, five trials in the infrequent condition, and the most-skewed distribution percentile-matched to the ex-Gaussian with $\mu = 350$ and $\tau = 250$). Meanwhile, the Type I error rates for the mean and bias-corrected medians were always around 2% for these other distributions, just as they were with the ex-Gaussian (Fig. 4). Thus, the finding of inflated Type I errors for medians seems relatively independent of the precise shape of the skewed RT distribution.

The simulations presented so far have all used pure, uncontaminated RT distributions, but there are reasons

[2]For all simulations in this article, bias-corrected medians were based on 200 bootstrap samples.

Figure 3

Probability density function (PDF) for the theoretical sampling distribution of the median for samples of five and 51 trials from an ex-Gaussian distribution with $\mu = 400$, $\sigma = 50$, and $\tau = 200$, together with the mean μ of each sampling distribution.



to suspect that observed RT distributions contain occasional outliers (e.g., Ratcliff, 1993, Ulrich and Miller, 1994), perhaps because the participant's attention momentarily wanders away from the task. It is an empirical question whether the results shown in Figure 4 would change markedly if the simulations included outliers. For example, since means are more affected by extreme scores than medians, the Type I error rates associated with mean-based analyses might be inflated when outliers are included. To look at the effects of outliers, additional simulations were conducted using each of the different RT models already introduced. These simulations included either 2% or 4% outliers, and the outliers were formed by summing an RT from the uncontaminated distribution with a random number distributed uniformly between 0–1,000 ms to reflect a distraction delay[3]. Such outliers had hardly any influence on the Type I error rates obtained using means, medians, or bias-corrected medians, so it seems unlikely that outliers in real RT data would reduce the Type I error rate advantage for means and bias-corrected medians relative to regular medians.

R&W acknowledged the problem of inflated Type I errors when using sample medians for comparing population means (e.g., with t-tests), and their Figure 10B even shows simulation results displaying the problem. Nonetheless, they essentially dismissed this problem because "the bias can be strongly attenuated by using a percentile bootstrap bias correction" (p. 31), which is a procedure that was not considered by Miller (1988). Indeed, their Figure 10C shows that the bootstrap bias correction completely cures the Type I error rate problem, as is also shown in the present Figure 4. Thus, it is reasonable to consider the bias-corrected median as a possible summary measure of RTs, and the next step is to check its power.

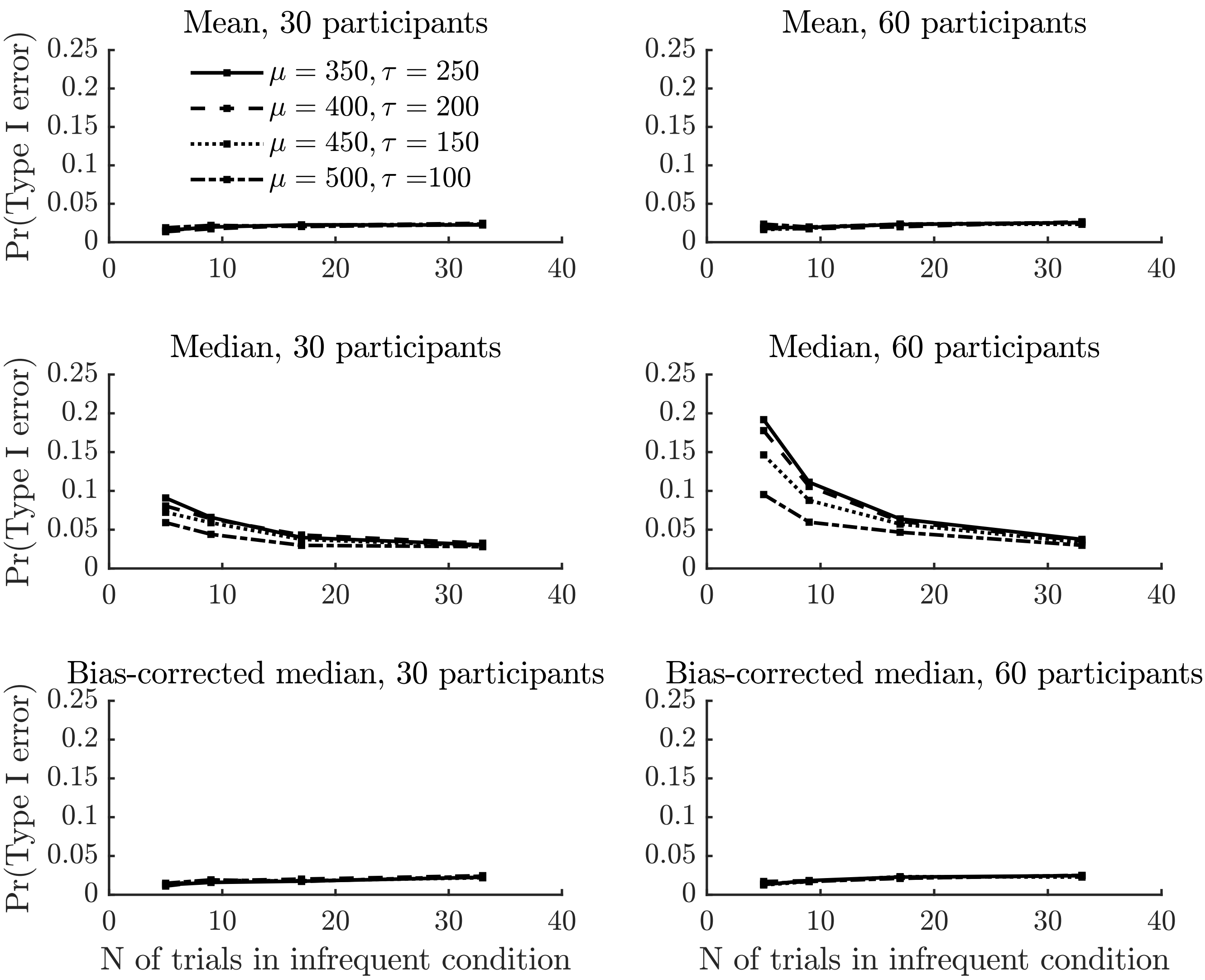
Power

Given that bias correction solves the median's problem of Type I error rate inflation, it is tempting to suspect that bias-corrected medians would be preferable to means, because the median is often the preferred measure of central tendency with skewed dis-

[3] Ratcliff (1993) introduced outliers varying uniformly between 0–2,000 ms, but it seems that responses delayed by 1,000–2,000 ms would be easily identified and excluded by commonly-used outlier rejection techniques.

Figure 4

Proportions of "infrequent mean larger" Type I errors obtained when using means, medians, or bias-corrected medians to compare conditions with different numbers of trials N in the infrequent condition, with an expected Type I error rate in this direction of 0.025 based on $\alpha = 0.05$. Each point indicates the proportion of significantly larger means in the infrequent condition across 10,000 simulated experiments with the indicated number of participants. The true distribution was always an ex-Gaussian with $\sigma = 50$. Its value of μ was 350, 400, 450, or 500, with $\tau = 600 - \mu$. There were always 51 trials in the frequent condition, and the true underlying RT distributions were always identical ex-Gaussians in the frequent and infrequent conditions. For the bias-corrected medians, 200 bootstrap samples were used to correct the median separately for each simulated participant/condition pair.



tributions. Contrary to this intuition, however, Ratcliff (1993) reported that regular medians provide less statistical power than means. R&W acknowledged Ratcliff's report, but they downplayed it because of the small trial frequencies used in Ratcliff's analysis. In addition, it remains an open question how the power of bias-corrected medians compares with that of means. The present simulations investigated these issues.

Fortunately, it is easy to compare the power of means versus bias-corrected medians using simulations similar to those described above for assessing Type I error rate. Instead of using the same RT distributions for the two conditions being compared, one simply uses different distributions and checks the proportion of simulated experiments yielding a statistically significant difference—this proportion is an estimate of statistical power. To model the different types of experimental effects for which researchers might test, one can allocate different amounts of the RT increase in the slower condition to different amounts of shifting versus skewing (i.e., tail-stretching) effects on the RT distribution. Within the ex-Gaussian RT model this amounts to increases in the μ versus τ parameters, and changes in other parameters produce comparable shifting versus stretching effects within the other RT distribution models.

The first set of power simulations examined the ability of the different summary measures to reveal a true between-condition RT difference in experiments where the two conditions had *unequal* trial frequencies, and the results of these simulations are displayed in Figure 5. Regular medians would not be appropriate in this situation because of the Type I error rate problem described in the previous section, so these simulations only compared the power of tests using means and bias-corrected medians. Naturally, these two types of testing were compared under identical simulation conditions, and in fact identical samples of simulated RTs were always analyzed with the two summary measures.

In total, there were 32 simulation conditions using ex-Gaussian RT distributions, corresponding to the 32 points shown in Figure 5, for each of the mean-based and bias-corrected median-based tests. In all 32 simulation conditions, 51 RTs per participant were sampled from the faster condition, and the true mean RT in the faster condition was 600 ms. The 32 conditions were formed as the factorial combination of eight different dataset sizes and four conditions differing with respect to RT skewness. The eight dataset sizes consisted of 30 or 60 participants factorially combined with 5, 9, 17, or 33 trials in the slower condition. The four skewness conditions were formed using two amounts of skewness of the RT distribution in the faster condition (i.e., $\mu_f = 350$ and $\tau_f = 250$ or $\mu_f = 500$ and $\tau_f = 100$, with $\sigma = 50$ in both cases) and by allocating the RT increase in the slower condition either 25% to μ_s and 75% to τ_s, or the reverse[4]. Thus, in different simulation conditions the faster RT distribution was either more or less skewed to begin with and the mean RT difference between conditions arose either mostly from shifting the distribution in the slower condition or mostly from stretching it. Finally, the true mean RT difference between the fast and slow conditions was adjusted individually for each of the 32 simulation conditions to produce an intermediate power level (i.e., approximately 25%–75%) for tests using means as the summary measure. Intermediate power levels are desirable because they provide the best opportunity to observe power differences between means and bias-corrected medians; with very low or high power levels, the differences between analysis methods are compressed by floor or ceiling effects. Across the 32 simulation conditions, the true mean difference varied from 9–41 ms.

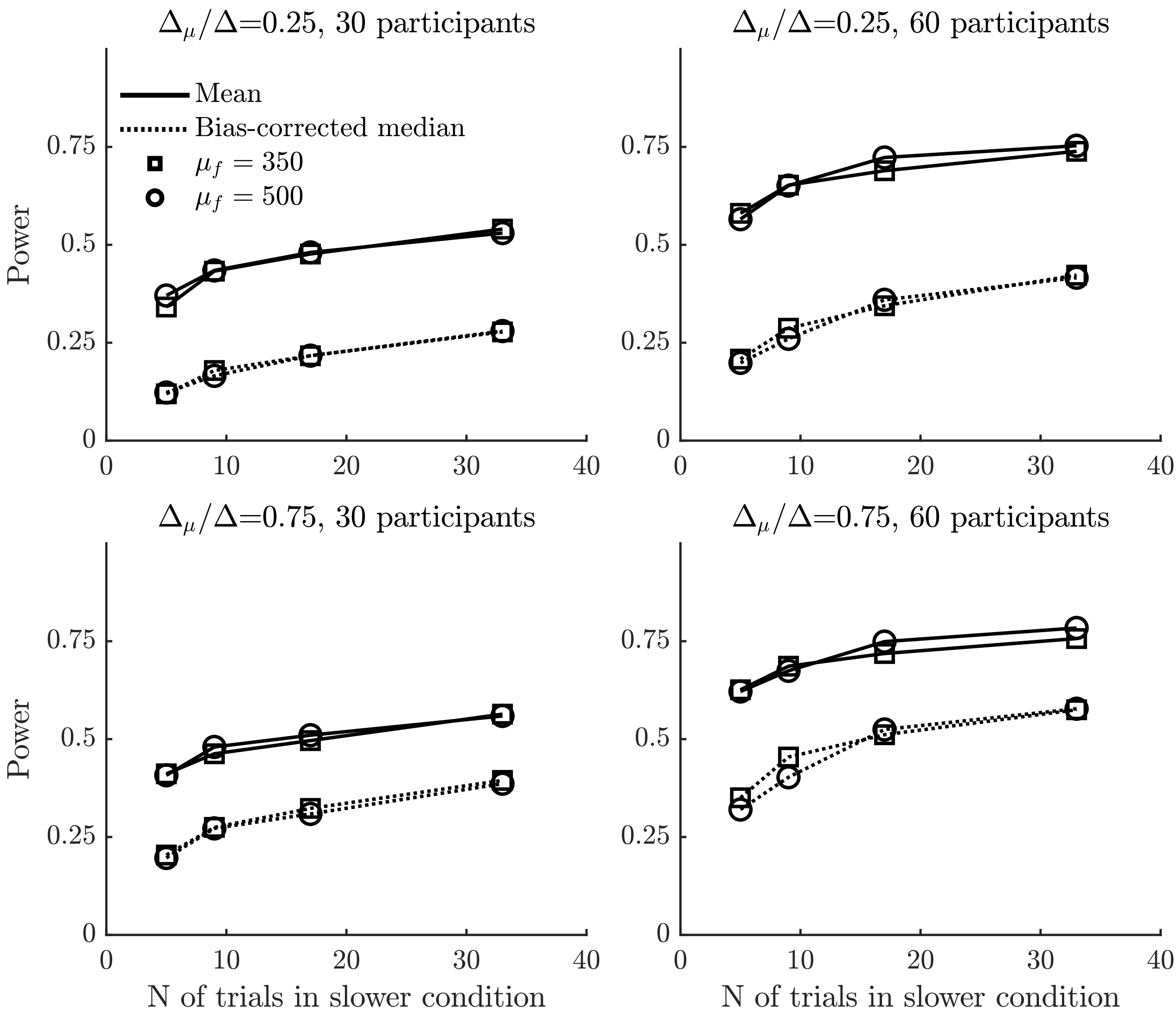
Not surprisingly, the results shown in Figure 5 indicate that the power of t-test comparisons increases with the number of participants and the number of trials per participant, and in fact these power increases are even more dramatic than are shown because the true differences were adjusted to smaller values with larger datasets in order to avoid ceiling effects on power. More critically, they also show a clear power advantage for using means rather than bias-corrected medians. Thus, although the bias-correction procedure lets the median do as well as the mean with respect to Type I errors (Fig. 4), this summary measures seems to have much less power than the simpler option of using means. The advantage for mean-based testing depends little either on the number of trials in the slower condition or on the skewness of RTs in the faster condition (i.e., $\mu_f = 350$ versus $\mu_f = 500$). It is clearly larger, however, when the experimental effect arises mainly from a tail-stretching effect (i.e., $\Delta_\mu/\Delta = 0.25$; upper panels) rather than from a shifting effect (i.e., $\Delta_\mu/\Delta = 0.75$; lower panels). Indeed, further simulations (not shown) indicate that the power of mean-based analyses is only slightly higher than that of analyses using bias-corrected medians when slowing is almost entirely due to a shift (i.e., $\Delta_\mu/\Delta = 0.95$). The reasons for this pattern will become clearer after the next set of simulations, which reinforce and extend the pattern.

Once again, the results of the simulations with the other, percentile-matched RT distributions closely

[4] In the corresponding 32 simulation conditions using each of the percentile-matched distributions, adjustments of the parameters of those distributions were made as needed to match the percentiles of the other distributions to those of the ex-Gaussians used in the fast and slow conditions.

Figure 5

Power of mean- and bias-corrected median-based tests for true differences in ex-Gaussian RT distributions in experiments comparing a faster condition with 51 trials per participant against a slower condition with fewer trials per participant. Each point indicates the proportion of significant results across 5,000 simulated experiments (α = 0.025, one-tailed). Simulation conditions also differed with respect to the skewness of the faster RT distribution (μ_f = 350 and τ_f = 250 or μ_f = 500 and τ_f = 100) and the proportion of the total RT slowing (Δ) associated with the μ parameter (Δ_μ/Δ = 0.25 or 0.75). For the bias-corrected medians, 200 bootstrap samples were used to correct the median separately for each simulated participant/condition pair.



match those of the ex-Gaussian RT distributions, with these simulations also showing greater power for mean-based testing. For example, across the 32 simulation conditions in Figure 5, the average power levels of the mean- and bias-corrected median-based tests were 0.58 and 0.32, respectively. With the other distributions, the average power for means ranged from 0.55–0.58, and the average power for bias-corrected medians ranged from 0.26–0.31. Similarly, across all distribution types and all simulation conditions, the minimum and maximum power levels ranged from 0.30–0.37 and 0.77–0.80 respectively for means, whereas these ranges extended from 0.11–0.12 and 0.51–0.61 for bias-corrected medians. In further simulations including 2% or 4% outliers of the same type used in the earlier Type I error rate simulations, power decreased for both mean- and bias-corrected median-based tests, but average power across simulation conditions was still more than 10% higher for the mean than for the bias-corrected median with all distributions.

In view of the fact that mean-based RT summaries have demonstrably greater power than bias-corrected median-based summaries for experiments with unequal trial frequencies (Fig. 5), it is also sensible to compare power levels in experiments with *equal* trial frequencies. As noted by Miller (1988) and R&W, regular medians are not associated with Type I error rate inflation in this situation because they would be equally biased in both conditions, so regular medians can also be considered as an appropriate summary of single-trial RTs in this case. It is, however, useful to compare the power of these three candidate measures of central tendency (i.e., means, medians, bias-corrected medians).

Figure 6 shows the results of simulations analogous to those shown in Figure 5, except with equal numbers of trials per participant in the faster and slower conditions, and naturally power again increased in the simulation conditions with more participants and trials even though these conditions had smaller true mean differences to avoid ceiling effects. Power is consistently lower for bias-corrected medians than for regular medians, suggesting that the bias correction should not be used with equal trial frequencies. Mean-based tests again have the most power, although the power difference between means and medians depends heavily on whether the experimental manipulation has mostly a shifting or tail-stretching effect. As can be seen in the upper panels of Figure 6, means have substantially more power than medians when a minority of the RT difference results from a change in μ (i.e., $\Delta_\mu/\Delta = 0.25$). The power advantage for means is much reduced when a majority of the RT difference results from a change in μ (i.e., $\Delta_\mu/\Delta = 0.75$), and medians can actually have slightly more power when the RT difference is a pure shift (i.e., $\Delta_\mu/\Delta = 1.00$; not shown). The same qualitative patterns are evident in Figs. 14–16 of R&W.

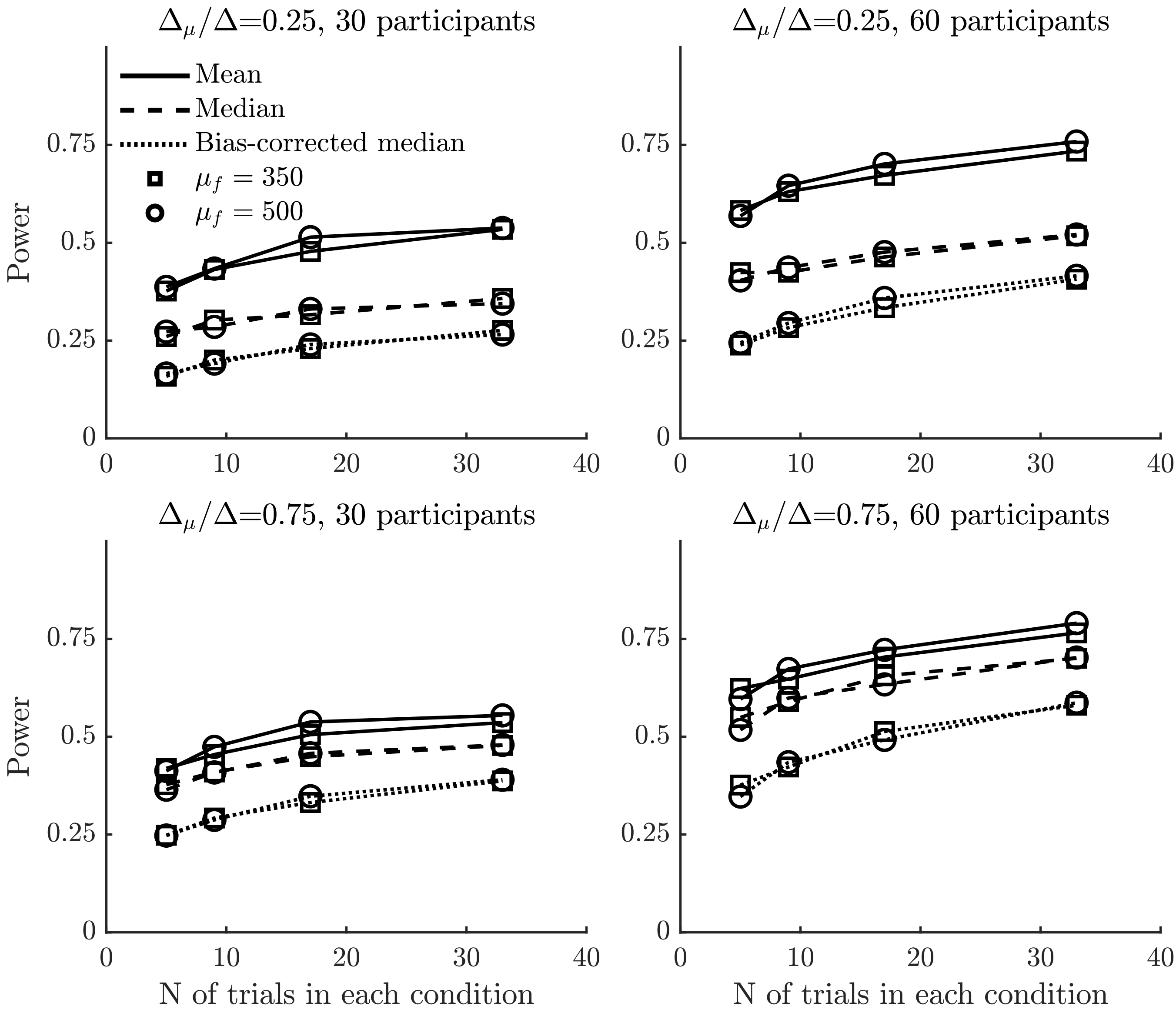
Overall, the pattern of greater power for mean-based testing shown in Figure 6 was again consistent across distributions and outlier conditions. Averaging across the different dataset sizes and skewness combinations shown in the figure, the average power levels of mean-based testing ranged across distributions from 0.55–0.58, whereas the ranges for median- and bias-corrected median-based testing were 0.38–0.45 and 0.27–0.33, respectively. The presence of 2% or 4% outliers reduced these average power levels overall, but average power was still largest for means (ranging across distributions from 0.46–0.49 and 0.41–0.43 with 2% and 4% outliers, respectively), second-largest for medians (ranging from 0.37–0.43 and 0.35–0.41), and smallest for bias-corrected medians (ranging from 0.26–0.31 and 0.26–0.30).

Why is it that using participant mean RTs as the summary measure has so much more power when the experimental effect is mostly a stretch in the slow tail? The main reason is simply that power increases with effect size, as is true for all statistical tests. Consider two conditions whose true mean RTs differ by 40 ms. In that case, the expected difference in mean RTs between those two conditions is 40 ms, regardless of how the effect is distributed between shifting and stretching and regardless of how many trials there are per participant in each condition. The situation is far more complicated for differences in medians, however, as is illustrated with the ex-Gaussian distribution in Figure 7. Figure 7A shows the expected value of the difference between the medians of the fast and slow conditions (Δ_{mdn}) as a function of (a) how much of the 40 ms mean RT difference is produced by changes in μ versus τ (i.e., Δ_μ versus Δ_τ), and (b) how many trials per participant are tested in each condition[5]. Critically, the expected difference between medians is always less than the 40 ms expected difference in means, and it is far less when the conditions differ mostly in τ (i.e., $\Delta_\mu = 10$ and $\Delta_\tau = 30$) rather than mostly in μ (i.e., $\Delta_\mu = 30$ and $\Delta_\tau = 10$), particularly when the number of trials is large. The fact that the numerical differences are larger for means than medians strongly suggests that tests using means would have more power. In theory, medians could provide more statistical power despite their smaller effect size in milliseconds if they had much smaller standard errors. They do not, however, as is clear in Figure 7B, which shows the corresponding ratios of the standard error of the difference in means to the standard error of the difference in medians. These ratios are quite close to 1.0, which means that the standard errors of the means and medians are nearly equal in all of these cases. Figure 7C shows the comparison of means versus medians plotted in terms of Cohen's d, a standard effect size measure. Effect sizes increase with the number of trials, as expected because the standard error of the sample statistics (i.e., mean and median) decrease as the number of trials increases. More importantly, it is clear that effect sizes are larger for means than for medians across all conditions, and this is the source of the power advantage for means.

[5] The results shown in this figure were obtained by computation rather than by simulation, using methods explained in the Appendix.

Figure 6

Power of mean-, median-, and bias-corrected median-based tests for true differences in ex-Gaussian RT distributions in experiments comparing faster and slower conditions with equal numbers of trials per participant. The parameters other than the numbers of trials per condition are the same as those in Figure 5.

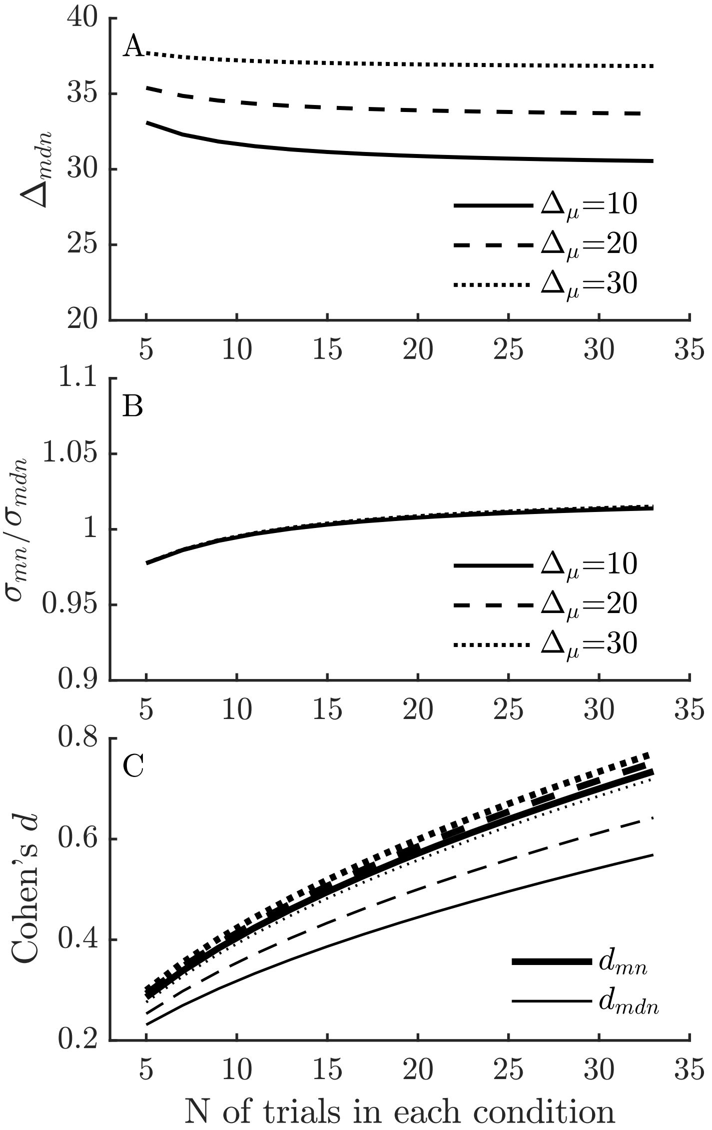


In essence, when much of an experimental manipulation's effect is to stretch the long upper tail of the RT distribution, the median's relative insensitivity to this part of the distribution eliminates part of the very between-condition difference that the researcher is looking for[6]. This is particularly ironic because insensitivity to skew is often cited as one of the median's benefits, and it is supposed to make the median especially tempting with skewed distributions (e.g., Hays, 1973, Marascuilo, 1971). As noted by Yule (1911), for example, "The median *may* [italics in original] sometimes be preferable to the mean, owing to its being less affected by abnormally large or small values of the variable" (p. 120), although he also commented that the median's "limitations render the applications of the median in any work in which theoretical considerations are necessary comparatively

[6]The same problem would arise with trimmed means, though to a lesser extent, because trimming also reduces the contribution of the high end of the RT distribution, where the condition difference is greatest.

Figure 7

A: Expected difference between fast- and slow-condition median RTs (Δ_{mdn}) for two conditions whose true means differ by 40 ms as a function of the number of trials per participant in each condition and of the division of the 40 ms effect between the μ and τ parameters of the ex-Gaussian RT distribution (i.e., $\Delta_\mu = 10$ and $\Delta_\tau = 30$, $\Delta_\mu = 20$ and $\Delta_\tau = 20$, or $\Delta_\mu = 30$ and $\Delta_\tau = 10$). In all cases, the expected difference between the mean RTs of these conditions is 40 ms. B: The ratio of the standard error of the difference in means (σ_{mn}) to the standard error of the difference in medians (σ_{mdn}), illustrating that the standard errors of the differences are approximately equal. C: Cohen's d for testing the condition effect using means (d_{mn}, thick lines) versus medians (d_{mdn}, thin lines) as the summaries of the individual-participant performance in each condition.



circumscribed" (p. 119). As the present simulations show, however, means can have much higher power to detect between-condition RT differences when experimental manipulations increase skewness, as they often do (e.g., Heathcote et al., 1991, Hockley, 1984, Hockley and Corballis, 1982, Luo and Proctor, 2018, Mewhort et al., 1992, Moutsopoulou and Waszak, 2012, Possamaï, 1991, Singh et al., 2018). In a re-analysis of datasets from seven published articles, for example, Rieger and Miller (2020) found significant ($p < 0.05$) increases in τ in 15 of 25 different statistical comparisons involving various distinct experimental manipulations. Evidence from research on bilingualism also suggests that the RT advantage for bilinguals is mostly due to the reduced number of quite long RTs and that the power of bilingual/monolingual comparisons diminishes greatly when long RTs are not considered (Zhou and Krott, 2016).

Distribution of Differences

In addition to comparing the effectiveness of means, medians, and bias-corrected medians as summaries of individual-participant RTs, R&W also compared three different methods of testing for a significant difference between conditions after a summary measure had been obtained for each participant in each condition. They did this using simulations based on a "g&h" distribution (see below). One method was to conduct a one-sample *t*-test using the individual-participant between-condition differences in the summary scores. This method is equivalent to testing with a repeated-measures ANOVA or a paired *t*-test as in the present simulations (e.g., Fig. 4), which appear to be the most common methods of testing for overall RT differences between conditions. The second method was to conduct a test on 20% trimmed means—that is, a test excluding participants with the most extreme between-condition differences. Finally, the third method was to test whether the median of the participants' between-condition differences differed from zero.

It is important to realize that R&W's g&h simulations comparing the three different methods of testing for differences in summary measures address a different question than that of how the individual RTs of a given participant should be summarized in the first place. Specifically, comparing hypothesis testing procedures addresses the question of how best to test for a significant effect of conditions *after summarizing the original individual-participant RTs in each condition*. This is a different question because researchers could initially summarize individual-trial RTs with any of the summary methods (i.e., means, medians, bias-corrected medians) and then subsequently test for condition differences with any of the hypothesis testing methods (i.e., *t*-test, 20% trimmed means test, median test). In principle, any one of these nine options could provide the most statistical power. Thus, the conclusions of the present simulations comparing different summary methods are specific to the *t*-tests and these simulations might have a different outcome if the summary measures were compared across conditions with some other method.

In their comparison of different hypothesis testing procedures using the g&h distribution, R&W did not distinguish between the three different methods of summarizing individual-trial RTs (i.e., means, medians, bias-corrected medians). In fact, they only generated a single random number for each simulated participant, and this number represented the difference (i.e., condition effect) for that participant summarized from the individual-participant RTs with "any type of differences between means, medians or any other quantities" (p. 17). These individual-participant difference scores were generated from "g&h" distributions, which allow convenient parametric variation of distribution skewness and kurtosis (i.e., tail heaviness) through g and h parameters, respectively. Although it might seem more appropriate to simulate single-trial RTs and examine all nine possible analysis combinations (i.e., 3 summary methods $\times$ 3 hypothesis testing methods), it is not clear how to do that realistically. Even assuming that all of the individual-participant RT distributions were ex-Gaussians, the participants would surely differ in their distribution parameters and in their between-condition differences in these parameters (e.g., effects on μ and τ). The distribution of individual-participant difference scores would be heavily influenced by this participant-to-participant variation as well as by the choice of summary method, but there does not yet exist an appropriate model for this individual variation. Thus, it was not unreasonable for R&W to model the final distribution of individual-participant difference scores directly with the g&h distribution rather than attempting to specify a model in which these difference scores would emerge from varying individual RT distributions under each summary method.

R&W's simulations comparing the effectiveness of the different hypothesis testing methods produced two particularly important results (e.g., their Figs. 12 and 13). First, each of the hypothesis testing methods tends to lose power when the distribution of participant-to-participant difference scores is more skewed or has heavier tails (i.e., larger kurtosis). Second, this tendency to lose power with increasing skew or kurtosis is much stronger for the *t*-test than for the tests using trimmed means or medians. Naturally, then, R&W

suggested that researchers should consider carefully the amount of skew and kurtosis in their distributions of participant-to-participant difference scores when deciding which procedure to use in testing for a condition effect.

Although R&W's simulations comparing hypothesis testing methods do not speak directly to the question of how the individual-participant RTs should be summarized in the first place, as was mentioned earlier, one might suspect that they do so indirectly. In particular, their results suggest that researchers should prefer the summary measure for which the participant-to-participant difference scores are the least skewed and have the lightest tails. Intuitively, it might seem reasonable to assume that medians—by virtue of their smaller sensitivity to extreme scores—would produce difference score distributions that are less skewed and have lighter tails than those produced by means, but this assumption must be checked empirically.

To do that, I examined the two large, publicly available RT datasets of Ferrand et al. (2010) and Hutchison et al. (2013), both involving lexical decision tasks. In both datasets, responses to words were faster than responses to nonwords, which provided a convenient condition effect to examine. Since these are real datasets, they have realistic trial-to-trial RT variability and participant-to-participant variability in condition effects, by definition, obviating the need to specify a formal model for these. Thus, I computed three separate nonword-minus-word difference scores for each participant—once each using the participant's condition mean RTs, condition median RTs, and bias-corrected condition median RTs. The normalized frequency distributions of these difference scores for the two datasets, tabulated across 944 and 503 participants, respectively, are shown in Figure 8.

Perhaps somewhat counterintuitively, the empirical distributions of individual-participant difference scores shown in Figure 8 are both less skewed (smaller values of skew and g) and lighter tailed (smaller values of kurtosis and h) when the difference scores are computed from mean RTs than when they are computed from either of the median-based summary measures. In combination with R&W's finding of greater power with less skewed and lighter-tailed difference score distributions, this pattern provides clear evidence that researchers would have more power when using means rather than medians to summarize RTs. Based on R&W's results, it seems that this would be true regardless of which hypothesis testing procedure was used, but it appears that the mean advantage would be especially large with standard t-tests or ANOVAs.

A distinctive feature of the SPP and FLP datasets, relative to many published studies, is that there were unusually many trials in each condition. One might therefore wonder whether the results shown in Figure 8 would generalize to datasets with fewer trials per condition (perhaps because there were more conditions). To examine this issue, I conducted simulations with smaller random subsets of the RTs for each participant in each condition. To increase the stability of the simulation results, 20 subsets of a given number of RTs were randomly selected for each participant, selecting without replacement for each subset but with replacement across subsets (because there were not enough RTs to sample without replacement for the larger subsets). For each randomly selected subset of RTs from one participant, the condition effect was computed using each of the three summary measures (i.e., mean, median, bias-corrected median). Finally, across all simulated subsets for a given number of RTs, the distribution of condition effects was analyzed using the same computations as those shown in Figure 8 for the full datasets.

Figure 9 shows the results of these simulations, which nicely extend the results obtained with hundreds of trials per participant in each condition (Fig. 8) to datasets with smaller numbers of trials. With virtually any number of trials per condition per participant selected from these real datasets, the between-participant difference score distributions would be less skewed (i.e., smaller skewness and g) and less heavy-tailed (i.e., smaller kurtosis and h) when differences were computed from mean RTs than when they were computed from medians or bias-corrected medians. Thus, as with the full datasets, these results in combination with R&W's demonstration of greater power with less skew and lighter tails, provide a further argument for using the mean to summarize the central tendency of observed RTs.

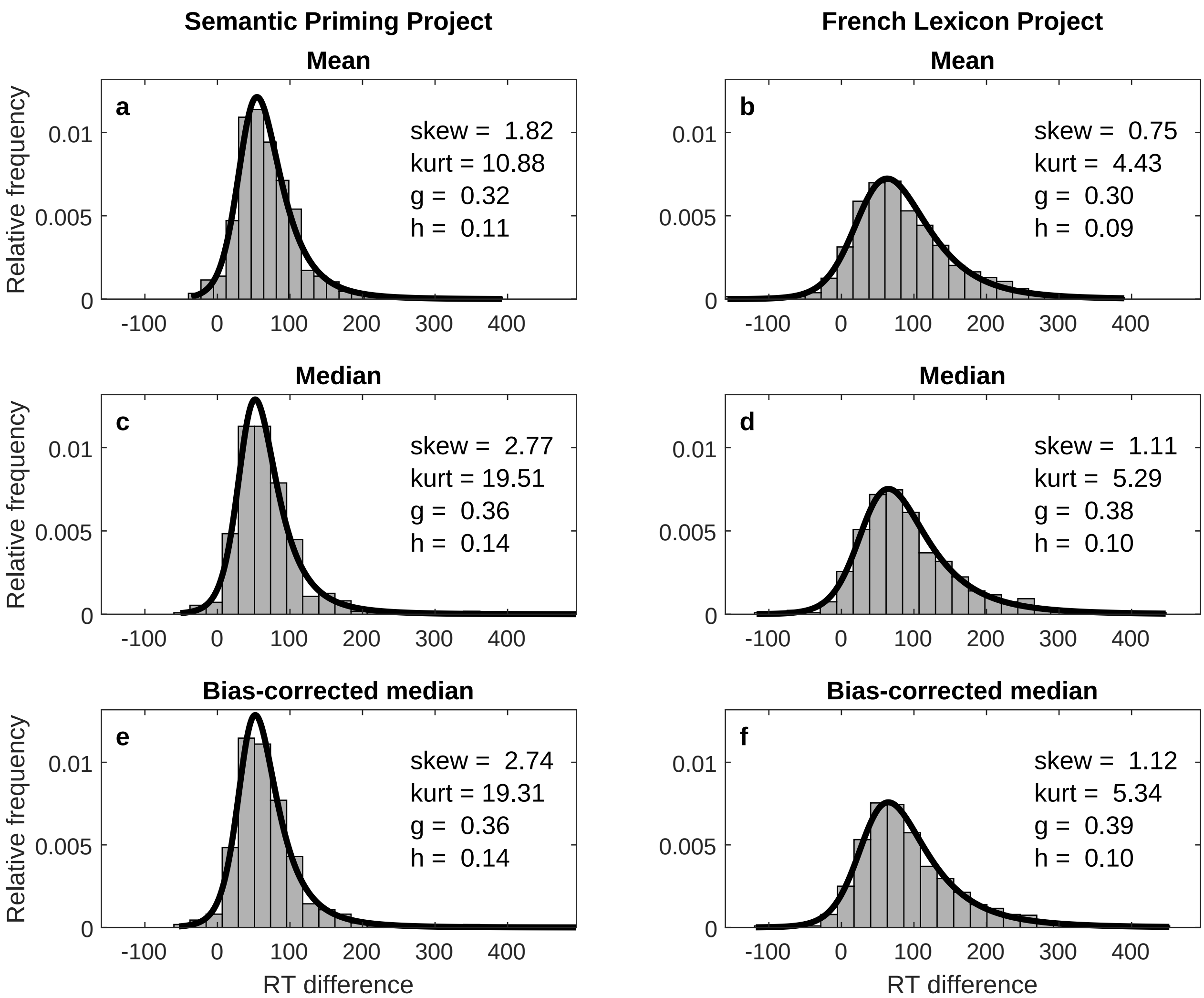
Conclusions

R&W concluded that "there seems to be no rationale for preferring the mean over the median as a measure of central tendency for skewed distributions" (p. 31). On the contrary, when performing hypothesis tests to compare the central tendencies of RTs between experimental conditions, the present simulations show that there may be an extremely clear rationale involving both Type I error rate and experimental power.

When comparing conditions with unequal numbers of trials, the sample-size-dependent bias of regular medians can lead to clear inflation of the Type I error rate (Fig. 4), so these medians definitely should not be used. Means and bias-corrected medians are both free of this bias and thus have acceptable Type I error rates, so either could be considered as a possible summary mea-

Figure 8

Normalized histograms of individual-participant RT difference scores computed from three different summary RT measures in the lexical decision task datasets from the Semantic Priming Project (a, c, e; Hutchison et al., 2013) and the French Lexicon Project (b, d, f; Ferrand et al., 2010). Each participant's observed 800–1,000 word and nonword RTs were first summarized by computing the mean, median, or bias-corrected median, and the nonword minus word difference was then computed for each measure. The histograms (bars) depict the frequency distributions of these differences across participants, and the skewness (skew) and kurtosis (kurt) values computed from these observed difference scores are shown on the panel. The solid line is the best-fitting (maximum likelihood) g&h distribution for each set of differences, and the g and h parameters of these distributions are also shown.

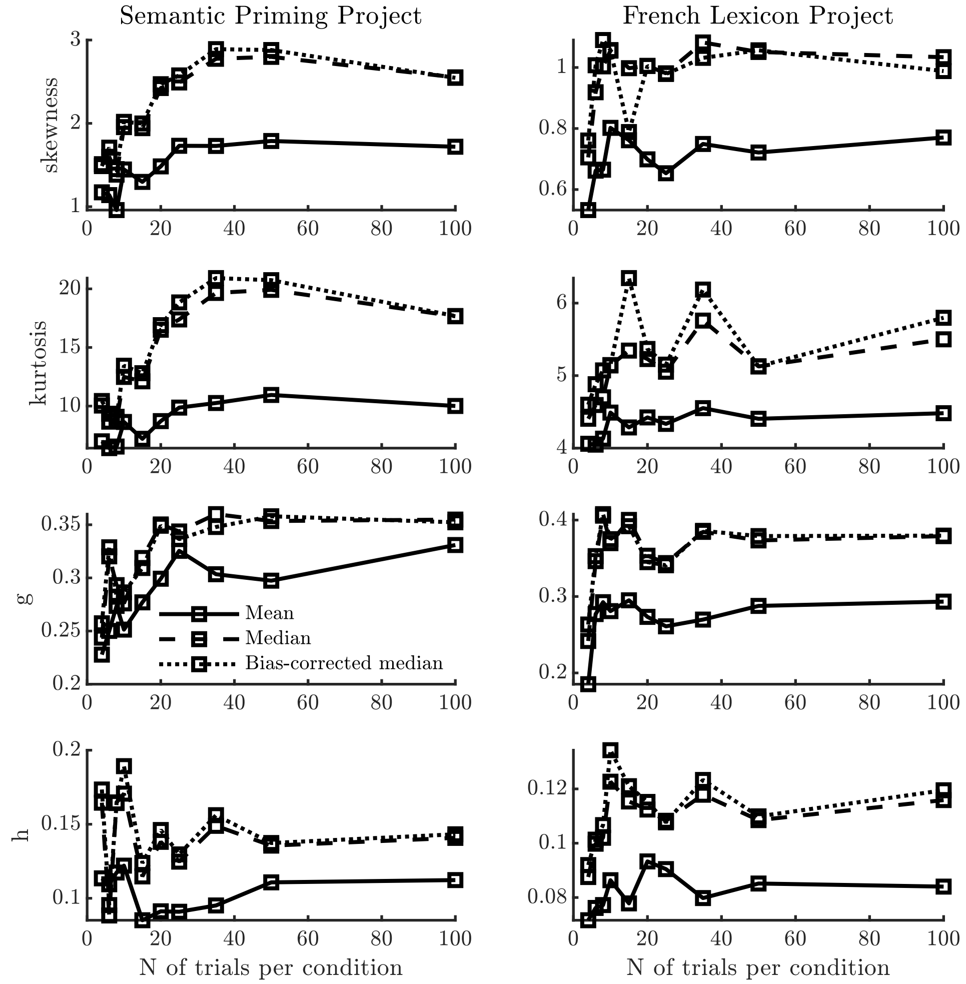


sure in this situation. Means clearly have greater power (Fig. 5) than bias-corrected medians in most situations, however, which would nearly always make them the preferred choice.

When comparing conditions with equal numbers of trials, means, medians, and bias-corrected medians all have appropriate Type I error rates, so any of these might be the preferred summary measure in this situation. Bias-corrected medians always seem to have less power than regular medians, however, so here the choice is really between means and regular medians, depending on which of those has the higher power. As can be seen in Figure 6), the answer depends on how the experimental manipulation affects skewness. Thus, to choose between means and medians as the summary measure maximizing power, researchers must consider the effect of the experimental manipulation at the level of the RT distribution.

Figure 9

Measures of skewness and kurtosis, plus maximum-likelihood estimates of parameters g and h, as a function of the number of trials per condition in the lexical decision task datasets from the Semantic Priming Project (Hutchison et al., 2013) and the French Lexicon Project (Ferrand et al., 2010). Random subsets of the indicated N of trials per condition were taken for each participant and parameters were estimated as in Figure 8.



The results in Figure 6 suggest that the two measures will have approximately equal power when RT skewness is unaffected by the manipulation, whereas medians will have greater power if skewness decreases in the slower condition and means will have greater power if skewness increases in the slower condition. Although the ex-Gaussian τ is one way of assessing skewness, it is not always necessary to estimate ex-Gaussian parameters from RT distributions. Instead, one can use a simpler skewness measure—namely, the difference between the mean and median of RT—as a proxy for τ. If this difference is smaller in the slower condition than the faster one, that is a sign that power will be better using medians. On the other hand, if this difference is larger in the slower condition, power will be better using means.

An important caveat concerning the choice of summary measure is that this choice should not be made based on the data being analyzed. To avoid the inflation of Type I error rate that arises when researchers try multiple alternative analyses in the attempt to obtain significant results (i.e., "p-hacking"; Simmons et al., 2011), researchers must choose the best summary measure in advance, based on theoretical considerations regarding the expected effect, on prior experience with similar experimental manipulations, or on pilot data. It would be inappropriate to decide whether to analyze mean or median RTs based on whichever gave the larger effect in a given dataset, because this would inflate the researcher's Type I error rate.

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Conflict of Interest and Funding

The author declares that he had no conflicts of interest with respect to the authorship or publication of this article.

Author Contributions

Jeff Miller: Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Resources, Data Curation, Writing - Original Draft, Writing - Review & Editing, Visualization, Supervision, Project administration, Funding acquisition.

Code Availability

The ex-Gaussian, ex-Wald, shifted lognormal, shifted gamma, and Weibull distributions used in this code are part of the Cupid package available at https://github.com/milleratotago/Cupid.

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Appendix

Expected Values and Standard Errors of Differences in Means and Medians

This appendix describes the numerical procedures for computing the expected values and standard errors of between-condition differences in mean RTs and between-condition differences in median RTs that are depicted in Figure 7.

Let $X_{1,i}$ and $X_{2,i}$, $i = 1 \ldots n$, be random samples of n RTs from the two conditions being compared. These come from assumed probability distributions (e.g., ex-Gaussian, etc) with means μ_1 and μ_2, variances σ_1^2 and σ_2^2, and cumulative distribution functions (CDFs) $F_1(t)$ and $F_2(t)$, respectively. For simplicity in dealing with medians, assume that n is odd.

Means

To analyze the between-condition difference in mean RTs, the researcher computes for each participant

$$D_{mn} = \bar{X}_2 - \bar{X}_1 = \sum_{i=1}^{n} X_{2,i}/n - \sum_{i=1}^{n} X_{1,i}/n, \qquad (1)$$

which has expected value $E[D_{mn}] = \mu_2 - \mu_1$. The variance of this difference is $\text{Var}[D_{mn}] = \sigma_1^2/n + \sigma_2^2/n$, because $X_{1,i}$ and $X_{2,i}$ are independent samples of trials.

Medians

To analyze the between-condition difference in median RTs, the researcher computes for each participant

$$D_{mdn} = X_{2(k)} - X_{1(k)} \qquad (2)$$

where $X_{\cdot(k)}$ indicates the k'th order statistic in the sample of n RTs. The median is the k'th order statistic for $k = (n+1)/2$ when n is odd.

Given the CDF $F(t)$ for the RTs in either condition, the CDF of the median in that condition $X_{(k)}$ is

$$F_{X_{(k)}}(t) = \sum_{j=k}^{n} \binom{n}{j} F(t)^j \cdot [1 - F(t)]^{n-j} \qquad (3)$$

(e.g., Arnold et al., 1992). As is illustrated in Figure 3, the probability distribution of the median RT in this condition is uniquely determined by this CDF, so the median's expected value $E[X_{(k)}]$ and variance $\text{Var}[X_{(k)}]$ in the condition can be computed by numerical integration. Once this computation is carried out for each of the two conditions individually, the expected value and variance of the difference between conditions are

$$E[D_{mdn}] = E[X_{2(k)}] - E[X_{1(k)}] \qquad (4)$$

and

$$\text{Var}[D_{mdn}] = \text{Var}[X_{2(k)}] + \text{Var}[X_{1(k)}]. \qquad (5)$$