

METHODOLOGICAL STUDIES

Why We (Usually) Don't Have to Worry About Multiple Comparisons

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Abstract: Applied researchers often find themselves making statistical inferences in settings that would seem to require multiple comparisons adjustments. We challenge the Type I error paradigm that underlies these corrections. Moreover we posit that the problem of multiple comparisons can disappear entirely when viewed from a hierarchical Bayesian perspective. We propose building multilevel models in the settings where multiple comparisons arise. Multilevel models perform partial pooling (shifting estimates toward each other), whereas classical procedures typically keep the centers of intervals stationary, adjusting for multiple comparisons by making the intervals wider (or, equivalently, adjusting the p values corresponding to intervals of fixed width). Thus, multilevel models address the multiple comparisons problem and also yield more efficient estimates, especially in settings with low group-level variation, which is where multiple comparisons are a particular concern.

Keywords: Bayesian inference, hierarchical modeling, multiple comparisons, Type S error, statistical significance

INTRODUCTION

Researchers from nearly every social and physical science discipline have found themselves in the position of simultaneously evaluating many questions, testing many hypothesis, or comparing many point estimates. In program evaluation this arises, for instance, when comparing the impact of several different policy interventions; comparing the status of social indicators (test scores, poverty rates, teen pregnancy rates) across multiple schools, states, or countries; examining whether treatment effects vary meaningfully across different subgroups of the population; or examining the impact of a program on many different outcomes.

The main multiple comparisons problem is that the probability a researcher wrongly concludes that there is at least one statistically significant effect across a set of tests, even

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when in fact there is nothing going on, increases with each additional test. This can be a serious concern in classical inference, and many strategies have been proposed to address the issue (see Hsu, 1996, and Westfall & Young, 1993, for reviews). A related multiple comparisons concern is that, in a setting where nonzero true effects do exist for some of the phenomena tested, a researcher applying multiple tests may identify additional statistically significant effects that are not in fact real.

Our approach, as described in this article, has two key differences from the classical perspective. First, we are typically not terribly concerned with Type 1 error because we rarely believe that it is possible for the null hypothesis to be strictly true. Second, we believe that the problem is not multiple testing but rather insufficient modeling of the relationship between the corresponding parameters of the model. Once we work within a Bayesian multilevel modeling framework and model these phenomena appropriately, we are actually able to get more reliable point estimates. A multilevel model shifts point estimates and their corresponding intervals toward each other (by a process often referred to as “shrinkage” or “partial pooling”), whereas classical procedures typically keep the point estimates stationary, adjusting for multiple comparisons by making the intervals wider (or, equivalently, adjusting the p values corresponding to intervals of fixed width). In this way, multilevel estimates make comparisons appropriately more conservative, in the sense that intervals for comparisons are more likely to include zero. As a result we can say with confidence that those comparisons made with multilevel estimates are more likely to be valid. At the same time this “adjustment” does not sap our power to detect true differences as many traditional methods do.

Rather than correcting for the problems that can arise when examining many comparisons (performing many significance tests), when we work within the Bayesian paradigm all of the relevant research questions can be represented as parameters in one coherent multilevel model. Simply put, rather than correcting for a perceived problem, we just build the multiplicity into the model from the start. This puts more of a burden on the model, and a key goal of this article is to demonstrate the effectiveness of our procedure in realistic examples.

Sections 2 and 3 present the multiple comparisons problem from the classical and Bayesian perspectives, respectively. Both are described within the context of a common illustrative example and then potential solutions are outlined. In Section 4, we bolster our argument against traditional multiple comparisons corrections through a series of small examples that illustrate several of the scenarios just described. Section 5 concludes.

MULTIPLE COMPARISONS PROBLEM FROM A CLASSICAL PERSPECTIVE

Illustrative Example

In this section we walk through a relatively simple example using data from a real study to illustrate the issues involved in performing multiple comparisons from classical and multilevel perspectives. We use data from the Infant Health and Development Program, an intervention that targeted premature and low-birthweight infants and provided them with services such as home visits and intensive high-quality child care. The program was evaluated using an experiment in which randomization took place within site and birth weight group. The experimental design was actually slightly more complicated (as described in Infant Health and Development Program, 1990); we simplify here for expository purposes. In fact, for this first illustration we assume that it was a simple randomized block experiment with eight sites as blocks.

In this context, we're not just interested in the overall treatment effect. Given that the composition of participating children was quite different across sites and that program implementation varied across sites as well, we would like to know for *each site individually* whether or not a statistically significant effect was present. However, we may be concerned that, in the process of conducting eight different significance tests, we are misperceiving our overall risk of making a false claim. This overall risk of error (formally, the probability that we have any rejections when the null hypothesis in fact holds) is sometimes referred to as the familywise error rate (Tukey, 1953). A similar problem arises if we are interested in comparing whether there are significant differences in treatment effects across sites.

Classical Perspective

A classical model fit to these data might look like:

$$y_i = \sum_{j=1}^8 (\gamma_j S_i^j + \delta_j S_i^j P_i) + \varepsilon_i,$$

$$\varepsilon_i \sim N(0, \sigma^2),$$

where y_i denotes student i 's test score, S_i^j is an indicator for living in site j , and P_i is an indicator for program status. Although this may not be the most common way to specify this model, it is useful here because δ_j represents the treatment effect in the j th site and γ_j represents the average test score for the untreated in each site.¹ This allows us to directly test the significance of each site effect.

For any given test of a null hypothesis, say $H_0^j : \delta_j = 0$, versus an alternative, say, $H_A^j : \delta_j \neq 0$, there is a 5% chance of incorrectly rejecting H_0^j when in fact it is true. Of course if we test two independent hypotheses at the same significance level ($\alpha = .05$) then the probability that at least one of these tests yields an erroneous rejection raises to $1 - \Pr(\text{neither test yields an erroneous rejection of the null}) = 1 - 0.95 \times 0.95 = 0.098 \approx 0.10$. Following the same logic, if we performed (independent) tests for all eight sites at a .05 significance level there would be a 34% chance that at least one of these would reject in error.

Bonferroni Correction

One of the most basic and historically most popular fixes to this problem is the Bonferroni correction. The Bonferroni correction adjusts the p value at which a test is evaluated for significance based on the total number of tests being performed. Specifically, the working p value is calculated as the original p value divided by the number of tests being performed. Implicitly, it assumes that these test statistics are independent. So in our current example an overall desired significance level of .05 would translate into individual tests each using a p value threshold of $.05/8 = .0062$. These thresholds could also be used to create wider confidence intervals for each point estimate as displayed in Figure 1, which

¹The actual analysis also included birthweight group as a predictor in this model, but we ignore this in this description for simplicity of exposition.

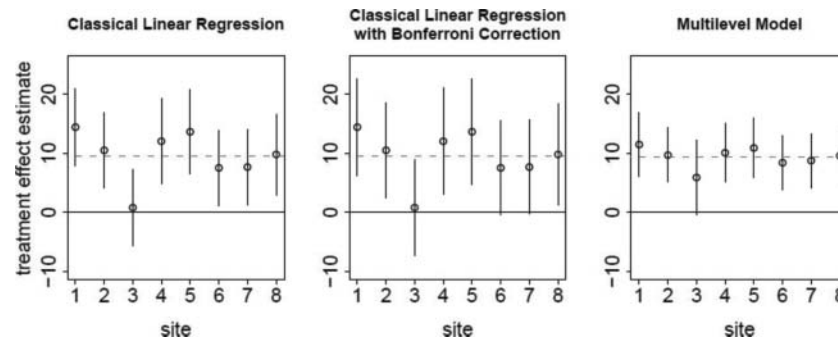


Figure 1. Treatment effect point estimates and 95% intervals across the eight Infant Health and Development Program sites. *Note.* The left panel display classical estimates from a linear regression. The middle panel displays the same point estimates as in the left panel but with confidence intervals adjusted to account for a Bonferroni correction. The right panel displays posterior means and 95% intervals for each of the eight site-specific treatment effects from a fitted multilevel model.

plots the point estimates from the model above along with both uncorrected and Bonferroni-corrected uncertainty intervals corresponding to a nominal 05 significance level. Although the standard intervals reject the null hypothesis of no effect of the intervention for seven of the eight sites, the multiple-comparisons-adjusted intervals reject the null hypothesis for only five sites.

The Bonferroni correction directly targets the Type 1 error problem, but it does so at the expense of Type 2 error. By changing the p value needed to reject the null (or equivalently widening the uncertainty intervals) the number of claims of rejected null hypotheses will indeed decrease on average. Although this reduces the number of false rejections, it also increases the number of instances that the null is not rejected when in fact it should have been. Thus, the Bonferroni correction can severely reduce our power to detect an important effect.

Other Classical Corrections

Motivated by the shortcomings of the Bonferroni correction, researchers have proposed more sophisticated procedures. The goal of these methods typically is to reduce the family-wise error rate (again, the probability of having at least one false positive) without unduly sacrificing power. A natural way to achieve this is by taking account of the dependence across tests. A variety of such corrections exist that rely upon bootstrapping methods or permutation tests (see, e.g., Westfall & Young, 1993).

A more recent class of approaches to this problem focuses not on reducing the familywise error rate but instead on controlling the expected proportion of false positives, the “false discovery rate” (FDR; Benjamini & Hochberg, 1995; Genovese & Wasserman, 2002). The rationale is that the researcher should be more worried about a situation in which many tests show up as statistically significant and an unknown proportion of these are erroneous than a situation in which all but a few tests show up as insignificant. Controlling for the false discovery rate rather than the familywise error rate leads to a less conservative testing procedure with respect to Type 1 error but is more powerful in terms of detecting effects that are real. These tests sit squarely within the Type 1 paradigm. As with

procedures to control for the familywise error rate, the initial versions assumed independence across tests; however, procedures to control the FDR have also been extended to account for certain types of dependency across tests (Benjamini & Yekutieli, 2001).

Methods that control for the FDR may make particular sense in fields like genetics where one would expect to see a number of real effects amidst a vast quantity of zero effects such as when examining the effect of a treatment on differential gene expression (Grant, Liu, & Stoeckert, 2005). They may be less useful in social science applications when we are less likely to be testing thousands of hypotheses at a time and when there are less likely to be effects that are truly zero (or at least the distinction between zero and not-zero may be more blurry). For our Infant Health and Development Program example, using a standard procedure (the Simes procedure; see Benjamini & Hochberg, 1995) to control the FDR at a .05 level, the tests with the six smallest p values would reject.

Informal Calibration

Still others might argue that in many situations there is no need to formally control for these error rates. Many of us are already used to informally performing appropriate calibrations. For instance, consider a researcher who presents a table of mean differences in pretreatment variables across an experimental and control group in which there is one statistically significant difference. The classical perspective would have us worry if we performed 20 tests at a .05 significance level that there is a 64% chance that at least one of these will yield a statistically significant result inappropriately. Thus, for instance, a Bonferroni correction could be performed to change the critical value to reflect a p value of $.05/20 = .0025$. However, it is probably more common (and at least equally helpful) for the researcher to simply note that given 20 such tests we would *expect* to see at least one such deviation given a .05 significance level. Alternatively, research organizations sometimes recommending prespecifying the tests that will be performed in an officially sanctioned analysis. In either case, the goal is to manage expectations in a way similar to a multiple comparisons correction but perhaps less strictly tied to familywise error rates.

A DIFFERENT PERSPECTIVE ON MULTIPLE COMPARISONS

Classical methods typically start with the assumption that the null hypothesis is true—an unhelpful starting point, as we discuss next. Moreover, we argue that classical procedures fail by insufficiently modeling the ensemble of parameters corresponding to the tests of interest (see Louis, 1984). We cannot hope to have the goal of proposing an optimal statistical method for all circumstances. Rather we present an entirely different perspective on the issue and its implications and argue that, when viewed from a Bayesian perspective, many of these problems simply disappear.

Abandoning the Type 1 Error Paradigm

The classical perspective worries primarily about Type 1 errors, and we argue that these should not be the focus of our concern. Suppose we've established the following two hypotheses regarding our site-specific treatment effects τ_j for $j = 1, \dots, J$: $H_0^j : \tau_j = 0$,

and $H_A^j : \tau_j \neq 0$. A primary concern from the classical multiple comparisons perspective is that we might erroneously accept H_A^j when, in fact, H_0^j is true (Type 1 error). But do we ever believe that τ_j exactly equals zero? What is the practical importance of such a test? Similarly, a Type 1 error occurs when we mistakenly accept the H_0^j that $\tau_j = \tau_k$ when in fact the H_A^j that $\tau_j \neq \tau_k$ is true. Again, under what circumstances do we truly believe there are absolutely no differences between groups? There may be no *practical* differences, but this is a distinct point which we discuss shortly. Moreover, if true effects are zero, we do not want anything close to a 5% chance of finding statistically significant results.

A more serious concern might be that we claim $\tau_j > 0$ when in fact $\tau_j < 0$, finding a positive effect when in fact the effect is detrimental. A similar phenomenon occurs if we claim that $\tau_j > \tau_k$ when in fact $\tau_j < \tau_k$, for instance, claiming that a treatment effect is larger in Miami than in New York when in fact the reverse is true. These are both examples of what is referred to as “Type S” (sign) errors (Gelman & Tuerlinckx, 2000).

However in policy analysis, there is also a fair bit of concern about examples where the differences might actually be very close to zero: for example, comparing different educational interventions, none of which might be very effective. Here we would want to be thinking about “Type M” (magnitude) errors: saying that a treatment effect is near zero when it is actually large, or saying that it’s large when it’s near zero (Gelman & Tuerlinckx, 2000). In that setting, underpowered studies present a real problem because type M errors are more likely when uncertainty is high. For instance it is not uncommon in an underpowered study for a researcher to state that although his estimate is not statistically significantly different from 0, that could simply be a function of the overly large standard error. Ironically, however, large estimates are actually a *byproduct* of large standard errors.

This property is illustrated in Figure 2. This plot displays two sampling distribution in a situation in which the true effect is zero (or very close to 0). It’s clear from this plot that the estimator with the sampling distribution with greater uncertainty ($SD = 3$) is much more

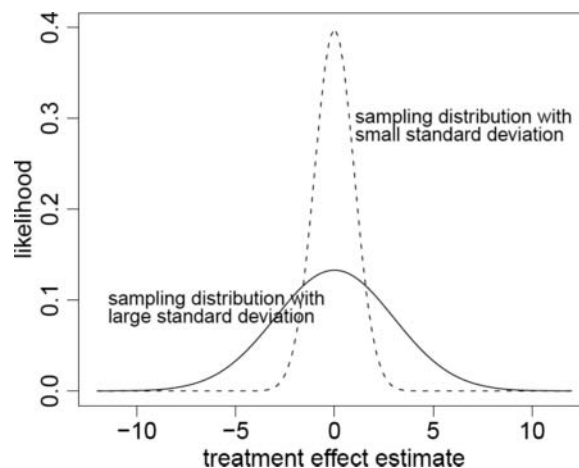


Figure 2. Two error distributions with differing levels of uncertainty in a situation when there is no effect. The estimator with the sampling distribution with greater uncertainty ($SD = 3$) has a greater probability of yielding a larger estimate than the estimator with the sampling distribution with smaller uncertainty ($SD = 1$). The researcher is more likely to commit a Type M (magnitude) error when the standard deviation is large.

likely to produce effect estimates that are larger in magnitude than effect estimates resulting from an estimator with relatively less uncertainty ($SD = 1$). Thus, for instance, when we switch from examining main effects to subgroup effects, for example, we automatically increase our probability of seeing large estimates and tricking ourselves into thinking that something is going on. Bayesian modeling helps here too, as we see next.

Multilevel Modeling in a Bayesian Framework

More strongly, we claim that when viewed within a Bayesian framework, many of these problems disappear, or in the case of Type S and Type M errors, are at least substantially ameliorated. We illustrate with a relatively simple multilevel model for a setting in which individuals in a common site experience the same effect on test scores, as in

$$y_i \sim N(\gamma_{j[i]} + \delta_{j[i]} P_i, \sigma_y^2),$$

Here $\delta_{j[i]}$ is the parameter for the treatment effect corresponding to person i 's site (indexed by j), and it is assigned its own distribution, for example

$$\delta_j \sim N(\mu, \sigma_\delta^2).$$

We have also allowed the intercept, γ , to vary across sites in a similar manner. It does not seem a strong assumption to think of these as realizations from a common distribution and this addition should strengthen our model. In addition, our Bayesian analysis requires us to specify prior distributions for the parameters μ , σ_y , and σ_δ^2 . However (particularly for this kind of simple model) it is not difficult to choose weakly informative priors (Gelman, 2006). Finally, we could (and should, in a real analysis) easily include other predictors to the model to increase our predictive power—most notably, the group-level intercept γ_j can be a predictor for the group-level treatment effect δ_j —but have refrained from adding predictors in this example, so we can focus on primary issues.

Partial Pooling. Multilevel modeling can be thought of as a compromise between two extremes. One extreme, complete pooling, would assume the treatment effects are the same across all sites, that is, $\delta_j = \delta$, for all j . The other extreme, no pooling, would estimate treatment effects separately for each site. The compromise found in the multilevel model is often referred to as *partial pooling*. Figure 1 graphically illustrates this compromise with a plot of the multilevel intervals next to the classical estimates and intervals (with and without Bonferroni corrections). The horizontal dashed line in each plot displays the complete pooling estimate. We also display a horizontal solid line at zero to quickly show which estimates would be considered to be statistically significant. This process leads to point estimates that are closer to each other (and to the “main effect” across all sites) than the classical analysis. Rather than inflating our uncertainty estimates, which does not really reflect the information we have regarding the effect of the program, we have shifted the point estimates toward each other in ways that reflect the information we have. (More generally, if the model has group-level predictors, the inferences will be partially pooled toward the fitted group-level regression surface rather than to a common mean.)

The Intuition. Why does partial pooling make sense at an intuitive level? Let's start from the basics. The only reason we have to worry about multiple comparisons issues is because we

have uncertainty about our estimates. If we knew the true (population-average) treatment effect within each site, we would not be making any probabilistic statements to begin with—we would just know the true sign and true magnitude of each (and certainly then whether each was really different from 0 or from each other). Classical inference in essence uses only the information in each site to get the treatment effect estimate in that site and the corresponding standard error.

A multilevel model, however, recognizes that this site-specific estimate is actually ignoring some important information—the information provided by the other sites. While still allowing for heterogeneity across sites, the multilevel model also recognizes that because all the sites are measuring the same phenomenon it does not make sense to completely ignore what has been found in the other sites. Therefore each site-specific estimate gets “shrunk” or pulled toward the overall estimate (or, in a more general setting, toward a group-level regression fit). The greater the uncertainty in a site, the more it will get pulled toward the overall estimate. The less the uncertainty in a site, the more we trust that individual estimate and the less it gets shrunk.

To illustrate this point we ran our multilevel model on slightly altered versions of the data set. In the first altered version we decreased the sample size in Site 3 from 138 to a random sample of 30; results are displayed in the center panel of Figure 3. In the second altered version we increased the sample size in Site 3 to 300 by bootstrapping the original observations in that site; results are displayed in the right panel of Figure 3. The leftmost panel displays the original multilevel model results. The key observation is that the shrinkage of the Site 3 treatment effect estimate changes drastically across these scenarios because the uncertainty of the estimate relative to that of the grand mean also changes drastically across these scenarios. Note, however, that the overall uncertainty increases in the rightmost plot even though the sample size in Site 3 increases. That is because we increased the sample size while keeping the point estimate the same. This leads to greater certainty about the level of treatment effect heterogeneity across sites, and thus greater uncertainty about the overall mean.

The Algebra. Partial pooling tends to reduce the number of statistically significant comparisons. To see this algebraically, consider the estimate for the treatment effect in a single group in a simple normal-normal hierarchical model:

$$\text{posterior } E(\theta_j) = \left(\frac{1}{\sigma_\theta^2} \mu + \frac{1}{\sigma_{\bar{y}}^2} \bar{y} \right) / \left(\frac{1}{\sigma_\theta^2} + \frac{1}{\sigma_{\bar{y}}^2} \right).$$

The corresponding uncertainty for this estimate is

$$\text{posterior } sd(\theta_j) = 1 / \sqrt{\frac{1}{\sigma_\theta^2} + \frac{1}{\sigma_{\bar{y}}^2}}$$

The smaller the prior variance σ_θ^2 , the more the posterior estimates for different groups are pooled toward a common mean value. At the same time, their posterior variances are shrunk toward zero, but much more slowly.

As a result, the z score for any comparison—the difference in posterior means, divided by the posterior standard deviation for that difference—decreases, and statistically

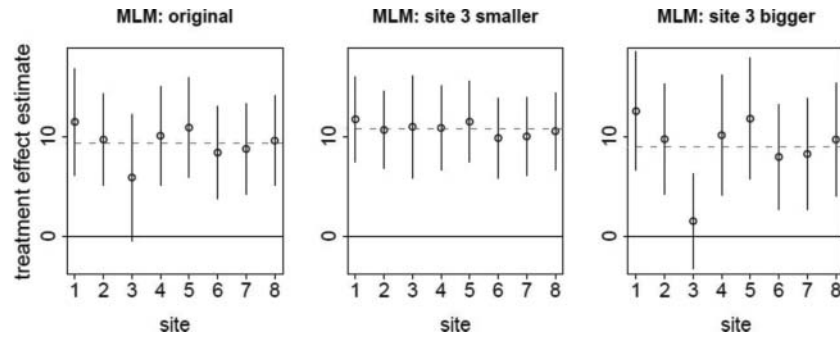


Figure 3. Comparison of results from multilevel model (MLM) using different versions of the data. *Note.* The left panel displays results from the original data. The center panel displays results from a model fit to data where Site 3 has been reduced to a random sample of 30 from the original 138 observations in that site. The right panel displays results from a model fit to data in which Site 3 observations were bootstrapped to create a sample size of 300.

significant Bayesian comparisons become less likely. Algebraically:

$$\begin{aligned} \text{posterior } E(\theta_j - \theta_k) &= \frac{\sigma_\theta^2}{\sigma_{\bar{y}}^2 + \sigma_\theta^2} (\bar{y}_j - \bar{y}_k) \\ \text{posterior sd}(\theta_j - \theta_k) &= \sqrt{2} \sigma_{\bar{y}} \sigma_\theta / \sqrt{\sigma_{\bar{y}}^2 + \sigma_\theta^2} \\ \text{posterior } z\text{-score of } \theta_j - \theta_k &: \frac{(\bar{y}_j - \bar{y}_k)}{\sqrt{2} \sigma_{\bar{y}}} \cdot \frac{1}{\sqrt{1 + \sigma_{\bar{y}}^2 / \sigma_\theta^2}}. \end{aligned}$$

The first factor in this last expression is the z score for the classical unpooled estimates; the second factor is the correction from partial pooling, a correction that is always less than 1 (i.e., it reduces the z score) and approaches zero as the group-level variance σ_θ^2 approaches zero; see Figure 4. The actual adjustment to z scores and significance levels is slightly more complicated because the variance parameters and the group-level mean are estimated from data, but the basic pattern holds, which is that posterior means are pulled together faster than posterior standard deviations decrease.

Greenland and Robins (1991) make a similar argument about the advantages of partial pooling, going so far as to frame the multiple comparisons problem as an

opportunity to improve our estimates through judicious use of any prior information (in the form of model assumptions) about the ensemble of parameters being estimated. Unlike conventional multiple comparisons, EB [empirical Bayes] and Bayes approaches will alter and can improve point estimates and can provide more powerful tests and more precise (narrower) interval estimators. (p. 249)

Poole (1991) supported these claims from a practical perspective.

Model Fitting. One barrier to more widespread use of multilevel models is that researchers are not always sure how to fit such models. We often recommend fitting multilevel models in a fully Bayesian way using a software package such as Bugs (as described in detail in

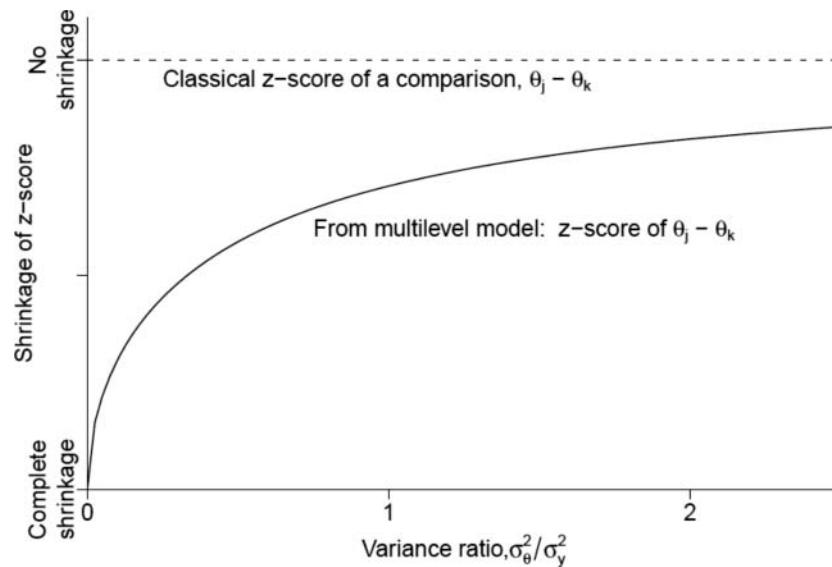


Figure 4. Shrinkage of the z score for a comparison, $\theta_j - \theta_k$, as estimated using multilevel modeling as compared to classical inference. When the variance ratio is small—that is, when the groups are similar to each other—plots of two sampling distributions with differing levels of uncertainty. When the group-level variance is small, there is a lot of shrinkage.

Gelman & Hill, 2007). However many simple models can be fit quite well using packages that have been built in (or can be easily installed into) existing software packages. For instance the model above can be fit easily in R, as

```
ihdp.fit <- lmer(y ~ treatment + (1 + treatment | group)).
```

Further functions exist in the *arm* package in R to help the user sample from the posterior distribution for each site-specific treatment effect (or any other parameter from the model or functions thereof; see Gelman & Hill, 2007). Similar options for fitting the model are available in Stata and SAS as well (see Appendix C of Gelman & Hill, 2007).

If we want to make comparisons across two sites, say Site 1 and Site 3, we do not need to refit the model using different contrasts or perform any algebraic manipulations. All can be done using posterior simulations.

EXAMPLES

We explore our ideas on multilevel models and multiple comparisons through a series of examples that illustrate different scenarios in which multiple comparisons might arise as an issue.

Comparing Average Test Scores Across All U.S. States

This next example illustrates how these issues play out in a situation in which all pairwise comparisons across groups are potentially of interest. Figure 5 shows a graph that

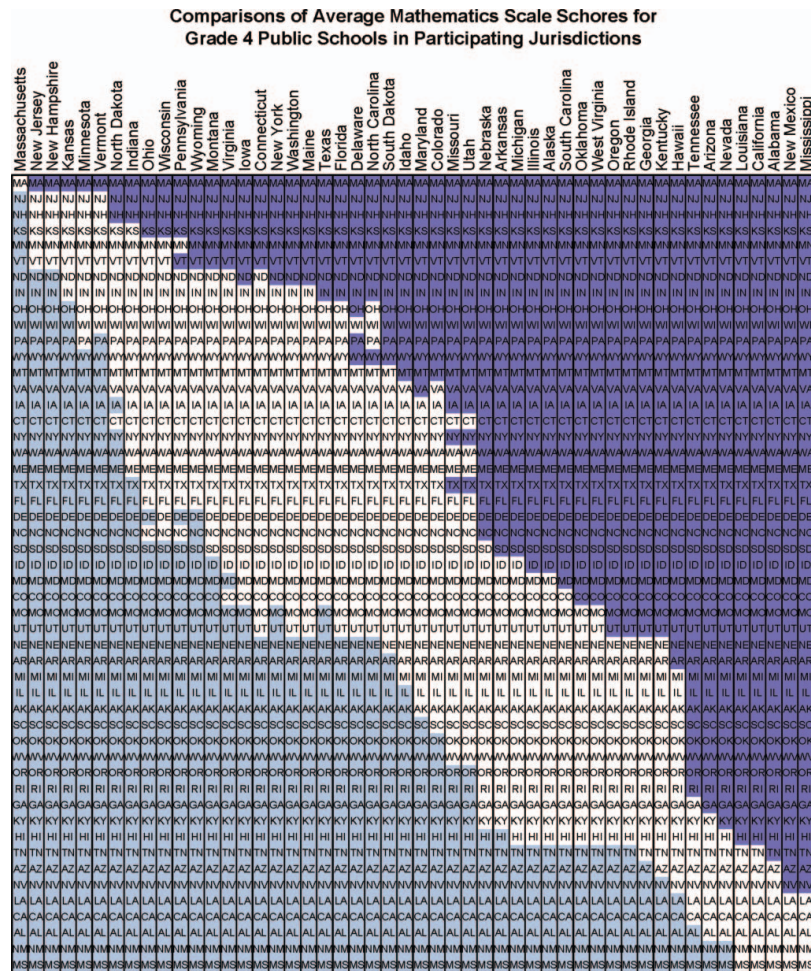


Figure 5. Graph that mimics one produced for the National Center for Education Statistics (1997) report comparing average math test scores of students in different states. *Note.* Shaded comparisons represent differences that are statistically significant after a false discovery rate multiple comparisons correction. We argue that these multiple-comparisons adjustments make no sense, as they are based on the irrelevant model in which true population differences are exactly zero (Color figure available online).

mimics one produced for a National Center for Education Statistics (1997) report that ordered all states based on average scores on the National Assessment of Educational Progress fourth-grade mathematics test. Our version makes use of 2007 fourth-grade mathematics scores and performs the FDR correction currently used by National Center for Education Statistics for this sort of problem (many comparisons). In the graph, statistically significant comparisons have been shaded. In theory, this plot allows us to answer questions such as, Does North Carolina have higher average test scores than neighboring South Carolina? This information could be displayed better (Almond, Lewis, Tukey, & Yan, 2000; Wainer, 1996, Wainer, Hambleton, & Meara, 1999), and maybe should not be displayed at all (Wainer, 1986), but here our concern is with the formulation as a multiple comparisons problem.

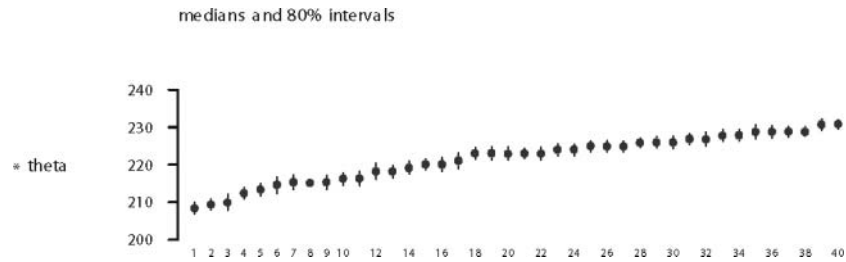


Figure 6. Summary of inferences for average math scores by state (see Figure 5) based on a fitted multilevel model. *Note.* States have been ordered by increasing average test score in the raw data. This graph could be improved in many ways; actually, though, it portrays the comparisons fairly clearly while being nothing more than a piece of the default graphical output obtained by fitting the model in R and Bugs (Spiegelhalter, Thomas, Best, Gilks, & Lunn, 2002).

Concerns With the Classical Multiple Comparisons Display. Here is a situation in which most classical multiple comparisons adjustments, such as the FDR adjustment that was used, will not be appropriate, because we know ahead of time that the null hypothesis (zero average differences between states) is false, so there is no particular reason to worry about the Type 1 error rate. Therefore, any motivation for multiple comparisons then rests either on (a) wanting more than 95% of the 95% intervals to contain the true values, or (b) wanting a lower Type S error rate, in other words, minimizing the chance of, for instance, stating that New Jersey has higher average test scores than Pennsylvania when, in fact, the opposite is the case.

With regard to 95% intervals, we can do better using multilevel modeling, either on the raw state averages from any given year or, even better, expanding the model to include state-level predictors and test scores from other years. If Type S error rates are a concern, then, again, a multilevel model will more directly summarize the information available.

The objection may be raised that although we know that the true differences cannot be *exactly* zero, what about the null hypothesis that they are *nearly zero*? Our reply is that this weaker version of classical hypothesis testing does not work here either. One way to see this is that the data used to create Figure 5 clearly reject either of these null hypotheses. But classical multiple comparisons procedures just plug along ignoring this information, widening the confidence intervals as new comparisons are added.

Multilevel Model and Corresponding Display of Comparisons. As an alternative, we fit a multilevel model: $y_j \sim N(\alpha_j, \sigma_j^2)$, where $j = 1, \dots, J$ are the different states, and y_j is the average fourth-grade mathematics score for the students who took the test in state j .² The parameters α_j represent the true mean in each state—that is, the population average that would be obtained if all the students in the state were to take the test. We model these population averages with a normal distribution: $\alpha_j \sim N(\mu_\alpha, \sigma_\alpha^2)$. Finally, we assign non-informative uniform prior distributions to the hyperparameters $\mu_\alpha, \sigma_\alpha, \sigma_j$. We display the resulting estimated state-level parameters in Figure 6.

²We recognize that this model could be improved, most naturally by embedding data from multiple years in a time series structure. The ability to include additional information in a reliable way is indeed a key advantage of multilevel models; however, here we chose a simple model because it uses no more information than was used in the published tables.

One advantage of the Bayesian paradigm in which models are fit using simulation is that the output is easy to manipulate in order to examine whatever functions of the parameters are of interest. In this case, based on the fitted multilevel model, we simulate 1,000 draws of state effect parameters to construct a posterior interval for the difference in true means for each pair of states. For the purpose of comparing to the classical approach, we set a .05 cutoff: for each pair of states j, k , we check whether 95% or more of the simulations show $\alpha_j > \alpha_k$. If so—that is, if $\alpha_j > \alpha_k$ for at least 950 of the 1,000 simulations—we can claim with 95% confidence that state j outperforms state k . We plot the results in Figure 7. States that have effects that are significantly lower are shaded lightly, the ones which are higher are shaded more darkly, and ones that are not significantly different are left as white.

Compared to the classical multiple comparisons summaries in Figure 5, the multilevel estimates in Figure 7 are more informative, with more claims with confidence and fewer

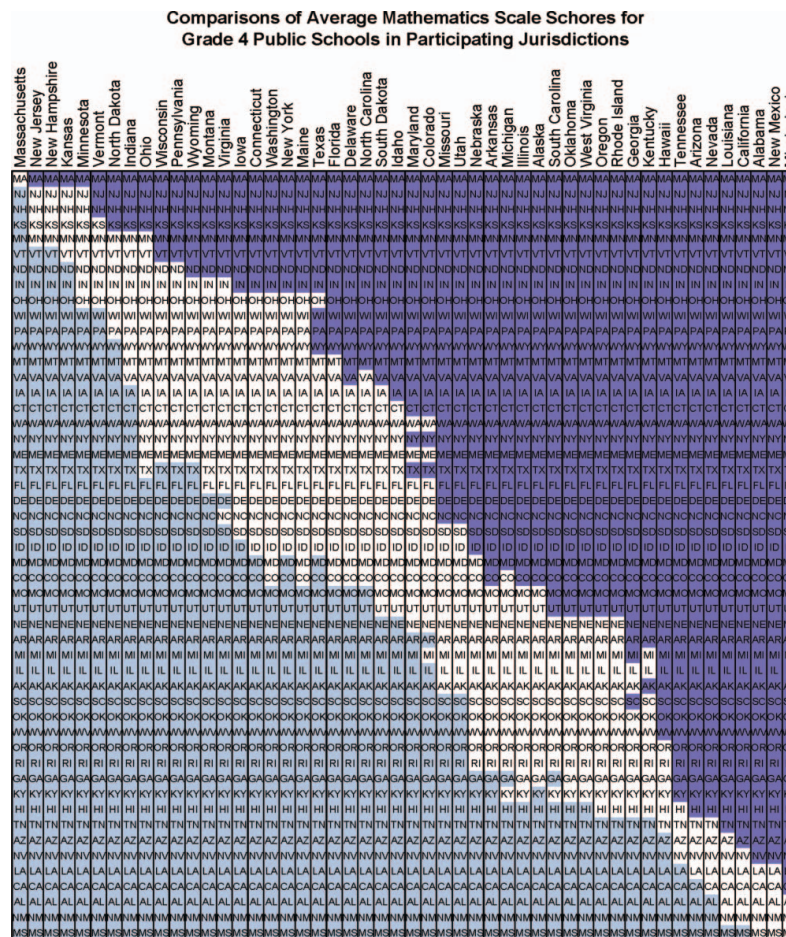


Figure 7. Posterior inference of National Assessment of Educational Progress fourth-grade mathematics test scores from 2007 by state, fit using a multilevel model. *Note.* Compared to the classical multiple comparisons summaries in Figure 5, we have fewer cases of ambiguity, that is, more claims with confidence. This makes sense because the hierarchical model adapts to the setting, which in this case is a high variance ratio and thus little need to worry about noise in the comparisons. (Color figure available online).

cells in the central region where comparisons are not statistically significant. The classical procedure overcorrects for multiple comparisons in this setting where the true differences between states are large. (At the other extreme, classical procedures undercorrect for multiple comparisons when true differences are small, as we discuss in the next section, SAT Coaching in Eight Schools.)

When there is evidence for a multiple comparisons problem, our procedure makes corrections. When there is no evidence for a multiple comparisons problem, our procedure is similar to the direct inference without a multiple comparisons correction.

SAT Coaching in Eight Schools

Rubin (1981) discusses an example (reprinted in Chapter 5 of Gelman, Carlin, Stern, & Rubin, 2003) of a meta-analysis of randomized experiments of coaching for the Scholastic Aptitude Test in eight high schools in New Jersey. This example is notable as one of the first fully Bayesian analysis of a hierarchical model and because there was no evidence in the data of differences between the treatment effects in the different schools. (And, in fact, the total estimated effects are small.) The first two columns of numbers in Table 1 give the data. Just to get a sense of the variation, the standard deviation of the eight school estimates is 10, which is of the same order as the standard errors.

Classical and Bayesian Analysis. This is the sort of situation where one might worry about multiple comparisons. (In the actual data in Table 1, none of the raw comparisons happen to be statistically significant, but as we discuss next, they could be in a replication of the study.)

Table 1. First two columns of numbers: Data from the eight-schools experiment of Rubin (1981)

School	Raw Estimate of Treatment Effect, y_j	Standard Error of Raw Effect Estimate, σ_{y_j}	Bayes Posterior M	Bayes Posterior SD
A	28	15	11	8
B	8	10	7	6
C	-3	16	6	8
D	7	11	7	7
E	-1	9	5	6
F	1	11	6	7
G	18	10	10	7
H	12	18	8	8

Note. A separate randomized experiment was conducted in each school, and regression analysis gave separate treatment effect estimates (labeled as y_j) and standard errors (labeled as σ_{y_j}). Effects are on the scale of points in the SAT-Verbal test (which was scored from 200 to 800). An effect of eight points corresponds to approximately one additional test item correct. Last two columns: Posterior mean and standard deviation of treatment effects, as estimated from a Bayesian multilevel model. The evidence is that the effects vary little between schools, hence the estimates are pooled strongly toward the common mean. None of the comparisons from the Bayesian inference are even close to statistically significant.

The hierarchical Bayesian analysis of Rubin (1981) has no multiple comparisons problems, however. The group-level variance is estimated to be low—the marginal maximum likelihood or posterior mode estimate is zero, and the Bayesian analysis averages over the posterior distribution, which is largely below 10—and as a result the Bayes estimates are pooled strongly toward the common mean.

Simulation Study With Small Effects. To get further insight into this example, we perform repeated simulations of a world in which the true treatment effects in different schools come from a normal distribution with standard deviation of 5 (a plausible estimate given the data in Table 1). For each replication, we simulate eight true values $\theta_1, \dots, \theta_8$ from this distribution, then simulate data y_1, \dots, y_8 from the eight separate normal distributions corresponding to each θ_j . The standard deviations σ_j for each of these distributions is given by Figure 1. Relative to the within-group standard deviations, the between-group standard deviation of 5 is small. We then performed both classical and hierarchical Bayesian analyses. For each analysis, we computed all $(8 \cdot 7)/2 = 28$ comparisons and count the number that are statistically significant (i.e., where the difference between the estimates for two schools is more than 1.96 times the standard error for the difference), and of these, we count the number that have the correct sign.

We performed 1,000 simulations. Out of these simulations, 7% of the classical intervals were statistically significant, and of these, only 63% got the sign of the comparison correct. Multiple comparisons corrections are clearly necessary here if we want to avoid making unreliable statements. By comparison, only 0.5% of the Bayesian intervals are statistically significant (with 89% getting the sign of the comparison correct). The shrinkage of the Bayesian analysis has already essentially done a multiple comparisons correction.

To look at it another way: The classical estimates found at least one statistically significant comparison in 47% of our 1,000 simulations. In the Bayesian estimates, this occurred only 5% of the time. The Bayesian analysis here uses a uniform prior distribution on the hyperparameters—the mean and standard deviation of the school effects—and so it uses no more information than the classical analysis. As with a classical multiple comparisons procedure, the Bayesian inference recognizes the uncertainty in inferences and correspondingly reduces the number of statistically significant comparisons.

Simulation Study With Large Effects. To get a sense of what happens when effects are more clearly distinguishable, we repeat the above simulation but assume the true treatment effects come from a distribution with standard deviation of 10. This time, 12% of the classical comparisons are statistically significant, with 86% of these having the correct sign. From the Bayesian analysis, 3% of the comparisons are statistically significant, with 96% of these having the correct sign. Whether using classical multiple comparisons or Bayesian hierarchical modeling, the price to pay for more reliable comparisons is to claim confidence in fewer of them.

Teacher and School Effects in New York City Schools

Rockoff (2004) and Kane, Rockoff, and Staiger (2007) analyzed a huge panel data set of teachers and children from the New York City school system to assess the importance of factors such as educational background, training, and experience in determining the effectiveness of teachers. One of the findings was that variation in teacher “effects” (we are not interpreting these finding causally) on student grades was moderately large, about

0.15 standard deviations on a scale in which standard deviation was calculated using test scores for all students of a given grade level in the system. The researchers, using an approach that approximates the fit from a multilevel model, learned from the scale of unexplained variation in teacher effects over time—the residual group-level variance—that teacher effects are important and often persistent and are largely not explained by background variables, except for a small improvement in performance during the first decade of a teacher's career.

More broadly, there has been an increasing push by certain school districts and policy advocates to use data like these to compare the “effectiveness” of individual teachers (or schools) to award merit pay or provide other incentives or sanctions. (Grudgingly) leaving the problematic causal issues aside (see Rubin, Stuart, & Zanutto, 2004, for a relevant discussion), we note that a outstanding methodological problem thus far has been that analyses used to make such comparisons rarely if ever address the extreme multiple comparisons problems involved. This study could have been set up as a multiple comparisons problem, trying to get appropriate p values for comparing thousands of teachers or for distinguishing individual teacher effects from zero. However, we know that there are true differences across teachers and that teachers should not have no effect (an individual teacher's effect could be negative or positive but will not be precisely zero), and we therefore should primarily be concerned with Type S and Type M errors. Therefore a multilevel model would be a far more appropriate choice for such analyses—and, in fact, this is essentially what the Kane, Rockoff, and Staiger did.

Fishing for Significance: Do Beautiful Parents Have More Daughters?

In an analysis of data from 2000 participants in the U.S. adolescent health study, Kanazawa (2007) found that more attractive people were more likely to have girls, compared to the general population: 52% of the babies born to people rated “very attractive” were girls, compared to 44% girls born to other participants in the survey. The difference was statistically significant, with a t value of 2.43. However, as discussed by Gelman (2007), this particular difference—most attractive versus all others—is only one of the many plausible comparisons that could be made with these data. Physical attractiveness in the survey used by this article was measured on a 5-point scale. The statistically significant comparison was between category 5 (*very attractive*) versus Categories 1–4. Other possibilities include comparing Categories 4–5 to Categories 1–3 (thus comparing “attractive” people to others), or comparing 3–5 to 1–2, or comparing 2–5 to 1. Moreover these data are from one of three potential survey waves. Therefore there are 20 possible comparisons (five natural comparisons by four possible time summaries (Wave 1, Wave 2, Wave 3, or average). It is not a surprise that one of this set of comparisons comes up statistically significant.

In this study, classical multiple comparisons adjustments may not be such a bad idea, because actual sex ratio differences tend to be very small—typically less than 1 percentage point—and so the null hypothesis is approximately true here. A simple Bonferroni correction based on our count of 20 possible comparisons would change the critical value from 0.05 to 0.0025 in which case the aforementioned finding (with p value of .015) would not be statistically significant.

With a properly designed study, however, multiple comparisons adjustments would not be needed here either. To start with, a simple analysis (e.g., linear regression of proportion of girl births on the numerical attractiveness measure) should work fine. The predictor here is itself measured noisily (and is not even clearly defined) so it would probably be asking

too much to look for any more finely grained patterns beyond a (possible) overall trend. More important, the sample size is simply too low here, given what we know from the literature on sex-ratio differences. From a classical perspective, an analysis based on 2,000 people is woefully underpowered and has a high risk of both Type S and Type M errors.

Alternatively, a Bayesian analysis with a reasonably uninformative prior distribution (with heavy tails to give higher probability to the possibility of a larger effect) reveals the lack of information in the data (Gelman & Weakliem, 2009). In this analysis the probability that the effect is positive is only 58%, and the estimated effect size is well under 1 percentage point.

Examining Impacts Across Subgroups

We build on our Infant Health and Development Program example from the Illustrative Example section to illustrate how a multisite analysis could be expanded to accommodate subgroup effects as well. The most important moderator in the IHDP study was the birthweight group designation. In fact there was reason to believe that children in the lighter low-birthweight (less than 2 kg) group might respond differently to the intervention than children in the heavier (more than 2 kg) low-birthweight group.

We expand our model to additionally allow for differences in treatment effects across birthweight group,

$$y_i \sim N(\gamma_{j[i]} + \delta_{j[i]}^L P_i(1 - B_i) + \delta_{j[i]}^H P_i B_i, \sigma_y^2),$$

Here the treatment effect corresponding to person i 's site (indexed by j) depends on whether the child belongs to the lighter low-birthweight group, δ_j^L , or the heavier low-birthweight group, δ_j^H . This time each of these sets of parameters gets its own distribution

$$\delta_j^L \sim N(\mu_L, \sigma_{\delta L}^2), \text{ and}$$

$$\delta_j^H \sim N(\mu_H, \sigma_{\delta H}^2).$$

In this case we allow the treatment effects for the lighter and heavier low-birthweight children to have a correlation ρ . Again we have allowed the intercept, γ_j , to vary across sites j and have specified prior distributions for the hyperparameters that should have little to no impact on our inferences.

Figure 8 plots some results from this model. The estimates for the lighter low-birthweight group are quite volatile in the classical setting, where the information we have about the relationship between the sites is ignored. The Bonferroni correction serves only to reinforce our uncertainty about these estimates. On the other hand, the results from the Bayesian multilevel model for this group have been shrunk toward the main effect across groups and thus are less subject to the idiosyncrasies that can arise in small samples. (The sample sizes across these sites for this group range from 67 to 93.) The point estimates for the heavier low-birthweight children are more stable for all analyses relative to those for the lighter low-birthweight group, reflecting that there is generally less treatment heterogeneity for this group of children. The classical and corrected standard errors are larger than for the lighter low-birthweight children most likely because the sample sizes across sites for this group are slightly smaller (ranging from 35 to 51).

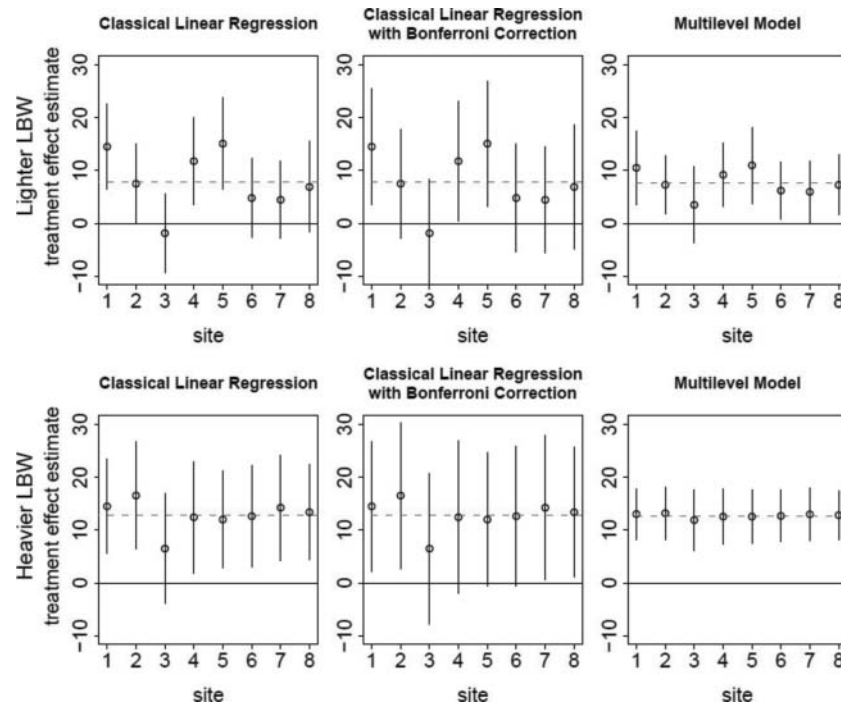


Figure 8. Treatment effect point estimates and 95% intervals across the eight Infant Health and Development Program sites now broken down by birthweight group as well. *Note.* The left panel display classical estimates from a linear regression. The middle panel displays the same point estimates as in the left panel but the confidence intervals have been adjusted to account for a Bonferroni correction. The right panel displays 95% intervals and means from the posterior distributions for each of the eight site-specific treatment effects generated by fitting a multilevel model. LBW = low birthweight.

Overall, the results from the Bayesian multilevel analysis are the most stable, and they lead to substantively different conclusions than the classical analyses. None of the Bayesian 95% intervals even comes close to covering zero. This contrasts sharply with the results from the Bonferroni-adjusted classical intervals, all of which are quite wide (typically at least twice the width of the Bayesian intervals) and four of which actually include zero (the other four end quite close to zero).

MULTIPLE OUTCOMES AND OTHER CHALLENGES

Similar issues arise when researchers attempt to evaluate the impact of a program on many different outcomes. If you look at enough outcomes, eventually one of them will appear to demonstrate a positive and significant impact, just by chance. In theory, multilevel models can be extended to accommodate multiple outcomes as well. However, this often requires a bigger methodological and conceptual jump. The reason that multilevel models were such a natural fit in the examples previously described is because all the estimated groups effects were estimates of the same phenomenon. Thus it was reasonable to assume that they were *exchangeable*. Basically this means that we could think of these estimates as random draws

from the same distribution without any a priori knowledge that one should be bigger than another. If we had such information then this should be included in the model, and in more complicated settings it is not trivial to set up such a model.

On the other hand, there are some situations when modeling multiple outcomes simultaneously within a simple multilevel model might be fairly natural. For instance, sometimes researchers acquire several different measures of the same phenomenon (such as educational achievement, behavioral problems, or attitudes toward related issues). It is also common to measure the same attribute at several different time points over the course of the study. This might require a slightly more complicated modeling strategy to account for trends over time but is otherwise a reasonable choice. If many outcomes have been measured across several disparate domains, however, more effort may be needed to set up a suitable multilevel model.

We illustrate a simple example of multiple outcomes by returning to the IHDP data. This time we allow treatment effects to vary by site and type of test. We include eight different types of cognitive tests that were administered either at Year 3 (Peabody Picture Vocabulary Test-Revised [PPVT-R]; Stanford Binet), Year 5 (PPVT-R; Weschler Preschool and Primary Scale of Intelligence Revised Verbal and Performance subscales), or Year 7 (PPVT-R; Weschler Intelligence Scale for Children Verbal and Performance subscales). In this new formulation test-specific individual level outcomes (indexed by i) are allowed both site-specific γ_j^{site} and test-specific γ_k^{test} contributions and the treatment effects, δ_l , are also allowed to vary by site and outcome. Here l indexes site \times test (i.e., $j \times k$) combinations.

$$y_i \sim N(\mu + \gamma_{j[i]}^{\text{site}} + \gamma_{k[i]}^{\text{test}} + \delta_{l[i]} P_i, \sigma_y^2),$$

As with the previous models, the site and test-specific intercepts, γ_j^{site} and γ_k^{test} , respectively, are assumed to follow normal distributions each with mean zero (because the model already includes a parameter for overall mean, μ) and each with its own variance.

What is more interesting here is the model for the δ_l ,

$$\delta_l \sim N(\delta_0 + \delta_{j[l]}^{\text{site}} + \delta_{k[l]}^{\text{test}}, \sigma_\delta^2).$$

Here the site-specific contributions to the treatment effects δ_j^{site} are simply modeled with a normal distribution with mean zero and separate variance component. However the test-specific contributions, δ_k^{test} , are allowed to systematically vary based on the age at which the test was taken and whether the test measures verbal skills

$$\delta_k^{\text{test}} \sim N(\phi_1 \text{year5} + \phi_2 \text{year7} + \phi_3 \text{verbal}, \sigma_{\delta_{\text{test}}}^2).$$

This last piece of the model increases the plausibility of the exchangeability assumption for the test scores. We also simplify the model by first standardizing each of the test scores (to have $M = 0$ and $SD = 1$) within the sample.

The results from this model are displayed in Figure 9. Each row of the figure corresponds to the test score outcome from a different year (row 1 for age 3, row 2 for age 5, row 3 for age 8). Within each plot, 95% intervals are displayed for each site. The treatment effects are larger on average for tests taken directly as the intervention ended at age 3. Similar patterns appear across sites for each outcome—this phenomenon reflects an assumption that could be relaxed by including site by outcome interactions.

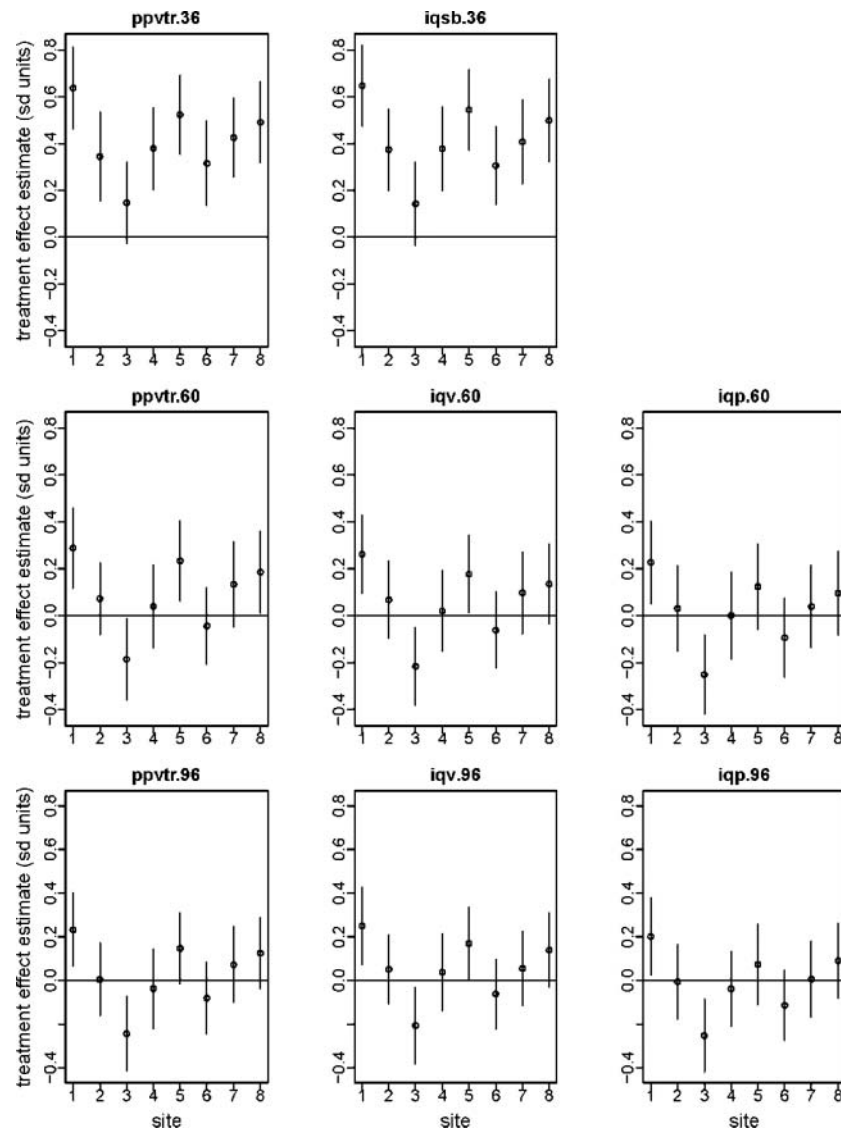


Figure 9. Example of inference for multiple outcomes. *Note.* Estimated 95% intervals and medians from the posterior distributions for each of 64 site-and-test-specific treatment effects in the Infant Health and Development Program analysis, from by fitting a multilevel model.

A comparison to a classical correction such as a Bonferroni adjustment is even more extreme in this setting, as illustrated in Figure 10. When taking all eight outcomes into consideration, the Bonferroni correction applied to the classic linear regression fit results in even larger uncertainty bounds than in our original example because now there are 64 comparisons rather than simply eight. The multilevel model estimates are now additionally shrunk toward the grand mean across outcomes, after adjusting for differences in mean test scores attributable to the year they were administered and the type of test. This shrinkage causes the estimates to be more conservative than they previously were. However the

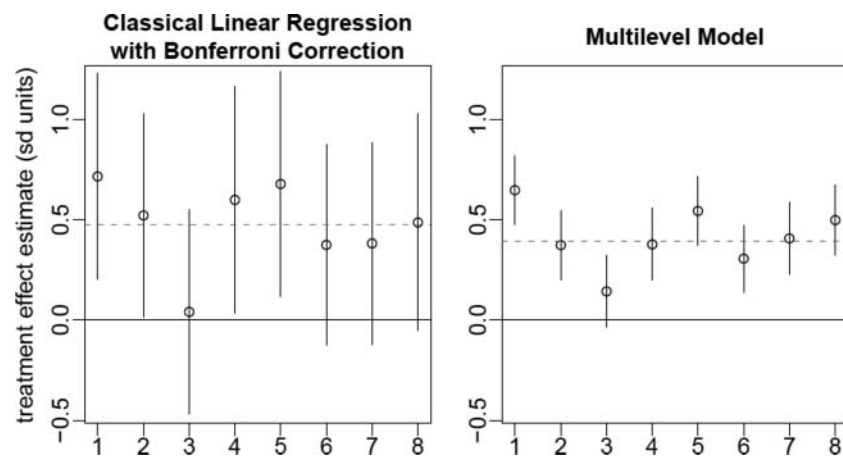


Figure 10. Comparison of treatment effects by site between classical linear regression with Bonferroni correction and multilevel model fit for the multiple-outcomes analysis of the Infant Health and Development Program (see Figure 9). Both account for eight sites and eight outcomes though only one outcome, Stanford Binet IQ score at age 36 months, is displayed.

overall precision of the multilevel model results is vastly superior to the Bonferroni-adjusted intervals.

Further Complications

Harder problems arise when modeling multiple comparisons that have more structure. For example, suppose we have five outcome measures, three varieties of treatments, and subgroups classified by two sexes and four racial groups. We would not want to model this $2 \times 3 \times 4 \times 5$ structure as 120 exchangeable groups. Even in these more complex situations, we think multilevel modeling should and will eventually take the place of classical multiple comparisons procedures. After all, classical multiple comparisons strategies themselves assume exchangeability in the sense of treating all the different comparisons symmetrically. And so, in either case, further work is needed for the method to match the problem structure. For large problems, there can be more data for estimating variance parameters in multilevel models (this is sometimes called the blessing of dimensionality). Similarly, classical procedures may have the potential to adaptively vary tuning parameters in large, complex structures.

CONCLUSION

Multiple comparisons can indeed create problems, and it is useful to address the issue. However, statistical methods must be mapped to the applied setting. Classical Type 1 and Type 2 errors and false discovery rates are based on the idea that the true effect could really be zero (see Efron, 2006, and Johnstone & Silverman, 2004, for connections between these ideas and hierarchical Bayes methods, and Krantz, 1999, for other criticisms of the null-hypothesis testing framework). Effects that are truly zero (not just “small”) can make sense in genetics (Efron & Tibshirani, 2002) but are less likely in social science or education research. We prefer to frame the issue in terms of Type S or Type M errors.

Therefore, when doing social science or program evaluation, we do not recommend classical methods that alter p values or (equivalently) make confidence intervals wider. Instead, we prefer multilevel modeling, which shifts point estimates and their corresponding intervals closer to each other (that is, performs partial pooling) where necessary—especially when much of the variation in the data can be explained by noise. Therefore fitting the multilevel model should result in the positive externality of yielding more reliable estimates for individual groups.

We recognize that multilevel modeling can prove to be more of a challenge for complicated structures. More research needs to be done in this area. However, we believe it is more worthwhile to invest research time and effort toward expanding the multilevel model framework than to invest in classical multiple comparisons adjustments that start from a perspective on the problem to which we do not adhere.

Applied researchers may balk at having to learn to fit a different kind of model. However, functions for fitting multilevel models are now available in many statistical software packages; therefore, implementing our suggestions should not be overly burdensome. Moreover, multiple comparisons problems arise frequently in research studies in which participants have been clustered because of interest in examining differences across these program sites, schools, cities, and so on; arguably, data from these types of studies should be fit using a multilevel model anyway to correctly reflect the within-group correlation structure of the errors. Thus the multilevel model will not only yield better results than the simplest multiple comparisons corrections, it should not pose a greater burden than performing one of the fancier types of classical types of multiple comparisons corrections.

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