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Flexibility versus Generalizability in Model Selection

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Abstract

Quantitative methods used to compare the performance of mathematical models of cognition measure the ability of models to redescribe experimental data. Some also weight various properties of the models. Two theoretical approaches to model selection were compared in the first part of the paper. One emphasizes flexibility, the other generalizability. In the second part, simulations were carried out to gain a better understanding of the results of Massaro, Cohen, Campbell, and Rodriguez (2001). Findings provide further insight into why measures of generalizability, such as BMS, are preferable to measures of flexibility, such as RMSD, and also show that the results from model recovery tests can be misleading if not interpreted relative to the data on which they were evaluated.

Flexibility versus Generalizability in Model Selection

Myung and Pitt (1997) introduced a method for selecting among mathematical models of cognition, dubbed Bayesian Model Selection (BMS). It goes beyond current selection methods by not only evaluating a model's ability to fit data (i.e., measuring goodness-of-fit using mean squared error or percent variance accounted for), but also by considering another dimension of a model, its complexity, which conceptually refers to how many different data patterns the model can fit *a priori*. Massaro, Cohen, Campbell, and Rodriguez (2001) carried out a more extensive series of simulations, the primary purpose of which was to show that prior work demonstrating the superiority of FLMP (Fuzzy Logical Model of Perception; Massaro, 1998) relative to WTAV (WeighTed AVeraging model) was still valid when BMS was used in place of RMSD (Root Mean Squared Deviation), their preferred selection method.

These two selection methods are defined as follows:

$$BMS = \ln \int f(D|\theta)\pi(\theta)d\theta$$

$$RMSD = \sqrt{\frac{\sum_{i=1}^{N}(prd_i - obs_i)^2}{N}}$$

f(D|2) is the likelihood function of observed data D, 2 is the parameter vector, B(2) is the prior density of 2, *ln* denotes the logarithm of base 10, *prd* and *obs* denote predicted and observed data, respectively, and finally, N is the number of data points being fitted. BMS originated in Bayesian statistics, and represents as the logarithm of the mean likelihood, averaged across the full range of parameter values and weighted by the prior density. The model that maximizes BMS should be selected. On the other hand, RMSD represents the square root of the average deviation between predicted and observed data. This selection method prescribes that the model that minimizes RMSD should be preferred.[1]

Our interest in Massaro et al (2001) was in the performance of the two selection methods, not the models that were compared. In some of the testing situations examined, they found that RMSD performed just as well as BMS, even better in some cases, and concluded that for their prototypical experimental setup, both methods can be used. In addition, they clarified their position on some key theoretical issues that go to the core of the model selection enterprise. In the current paper, we continue this dialogue by first clarifying our own approach to model selection and distinguishing it from theirs. This discussion is followed by a re-examination of their simulations. Large-scale simulations combined with new analysis techniques provide a broader understanding of why the two selection methods performed as they did across various tests. In addition, they further clarify the relationship between the two selection methods. The paper closes with some thoughts on when and how both methods can be used productively.

The Goal of Model Selection

The goal of model selection is to identify the one model, from a set of competing models, that best captures the regularities underlying the cognitive process of interest. The computational methods that are applied in choosing among models should be an extension of this goal. The measures that have been developed for this purpose differ in the approach taken to achieve this goal and in the properties of the model that are evaluated.

Goodness-of-fit (GOF) measures, such as RMSD, have been the primary tools used to compare models in psychology, and they are also championed by Massaro et al (2001; Massaro, 1998). They measure how well a model fits the experimental data. The model that provides the best fit is selected because it is assumed to approximate most closely the underlying cognitive process.

This approach to model selection entails comparing competing models on their ability to fit data sets generated by all participants in an experiment. For each set of data, the parameters of the model vary freely to obtain the optimal fit. The model that yields the best mean fit when averaged across participants is chosen. In defense of their method, Massaro et al contend that fitting individual participants' data is necessary to avoid the potential distorting effects of fitting averaged data, and also maintain that allowing parameters to vary freely is a necessary part of modeling.

We are in agreement with Massaro et al that care must be taken when fitting averaged data (see Myung, Kim, & Pitt, 2000) and that without a minimal amount of initial exploratory modeling, it is nonsensical to fix the parameters of a model prior to testing it on a given data set. How else could good parameter estimates be obtained?

We have reservations, however, about their approach to model selection and the types of models it favors. To begin with, we takes issue with the practice of generating new parameter estimates for each new set of data. By allowing parameters to vary anew when fitting each participant's data using a GOF measure like RMSD, the model is, in effect, being optimally tuned to each set of data. When these RMSD values are then averaged, the result is a measure of the flexibility of the model in capturing the range of response patterns exhibited across participants. Models that provide a good mean fit (i.e., small RMSD) are sufficiently agile to cope most of the variability in the data. Less flexible models will provide poorer mean fits because they are unable to do so. In essence, the model that is most adept at absorbing variability among response patterns is selected, and it is this model that is assumed to approximate most closely the underlying cognitive process.

A shortcoming of this approach become apparent when one asks why the most flexible model should be preferred. As flexibility increases, specificity about the form of the underlying process decreases, because a more flexible model can mimic many more response patterns (i.e., potentially unique cognitive processes) than a less flexible one (Myung & Pitt, 1997). If the goal of model selection is to narrow the set of candidate models to one, then emphasizing flexibility works against this goal because models are kept in the running solely by virtue of their ability to fit data, not because they are good approximations to a particular cognitive process. In the limit, the preferred model would be a universal Turning machine, capable of fitting any data set perfectly, while at the same time providing little information about the form of the cognitive process being studied.

Emphasizing model flexibility leads to nontrivial errors in model selection. Variation in the data due to sampling error, to individual differences, and to the cognitive process itself are undifferentiated in Massaro et al's flexibility approach to model selection. All are considered meaningful forms of variation, and models are evaluated on their ability capture all three, yet a GOF measure like RMSD cannot distinguish among them. The following simulation illustrates this problem.

RMSD was used to choose between two models from data that were generated to vary in sampling error, individual differences, and the model (i.e., cognitive process) itself. One model was a 16 parameter version of the original Fuzzy Logical Model of Perception (FLMP_F; Massaro & Cohen, 1983) and the other was a restricted (i.e., nested) version of this same model with only

8 free parameters ($FLMP_R$), obtained by fixing half of the sixteen parameters to constants. The mathematical equation for FLMP and other models discussed in the present paper are listed in Table 1. The ability of RMSD to recover the correct model was tested in five conditions. In the first and fifth, all simulated subjects differed only in sampling error. In the second and fourth, they differed in both sampling error and individual differences, with half the data sets being generated using one set of parameters, and the remainder using the other set. In the third condition, half of the simulated subjects were generated by one model, and half by the other model.

In each condition and for each model ($FLMP_F$ or $FLMP_R$), a given set of parameters was run through the model to create a set of 64 response probabilities in an 8 x 8 factorial design. From these, a sample of 64 simulated proportions was obtained by introducing sampling error using the binomial probability distribution of sample size 20. This was repeated 50 or 100 times, depending upon the condition, thereby creating the same number of replication samples (i.e., response patterns). Both models were then fit to each response pattern separately using RMSD.

When looked at from the standpoint of variability, the goal of model selection is to ignore variation due to sampling error and individual differences, and capture only that due to the cognitive process of interest. In the current test, an accurate selection method should ignore sampling error (SE) and parameter variation (i.e., individual differences, ID), but not model variation (different models, DM). Shown in each cell in Table 2 is the mean fit and the percentage of time the particular model yielded the best fit. $FLMP_F$, the more flexible model because it has 8 additional parameters, was always selected regardless of the source of the data, demonstrating that RMSD cannot discriminate between the types of variation. While $FLMP_F$

should have been selected in conditions in which it generated the data (SE_F and SE_F+ID, in rows 3a and 3b in Table 2, respectively), it should not have been chosen when data were generated by the restricted model, $FLMP_R$. No matter whether data differed in sampling error only (SE_R, row 1a), or both sampling error and individual differences (SE_R+ID, row 1b), $FLMP_F$ always provided the best fit. Finally, RMSD also failed to distinguish the models when the data were equally likely to come from either model (DM condition, row 2).

These results show that flexibility measures such as RMSD can be highly error prone. Put simply, they get side-tracked by the quest to capture variability in the data, and end up selecting the most flexible model, which may not be the best approximation of the cognitive process.

This undesirable property of the flexibility approach to model selection is why we favor an alternative, one in which the selection methods themselves do a better job of achieving the goal of model selection -- to infer the form of the cognitive process. The approach that we advocate differs in the conceptualization of the problem, though in practice it can appear, and sometimes perform, quite similarly (reasons why will be explained later). In brief, model selection is viewed as a statistical inference problem, analogous to estimating population parameters from sample statistics. It begins with a recognition of the variability problem discussed above: Data are always contaminated by noise. To model a mental process accurately, one must devise a way to disentangle the sources of noise (sampling error, individual variability) from the variation due to the underlying cognitive process.

The problem of error is solved by shifting the focus of model selection from measuring a model's fit to all response patterns (i.e., flexibility) to estimating a model's ability to fit unseen, future data sets generated by that same process (i.e., generalizability). That is, the goal of model selection is to choose the model that generalizes best across all samples, because the one that does has surely captured the cognitive process of interest, and not the random fluctuations that any one sample will exhibit. This is the essence of generalizability, and a model should be judged on its ability to generalize correctly, not on its flexibility to fit different data patterns.

To measure a model's generalizability, the selection method must be sensitive to properties of the model in addition to GOF. Collectively they define the complexity of the model, which among other things includes the number of parameters in the model and the way the parameters are combined in the model equation (i.e., functional form). RMSD and other flexibility measures are insensitive to model complexity. For a more detailed discussion of complexity and various measures of generalizability and its theoretical foundations, see Linhart, and Zucchini (1986), Myung, Balasubramanian, and Pitt (2000), Pitt, Myung, and Zhang (in press). One of these measures, cross validation, is mentioned in passing by Massaro et al (2001; p.15), but they express uneasiness with it and with a central concept of generalizability: prediction. It is used in the following simulation to demonstrate why generalizability, not flexibility, should be the goal of model selection.

In cross validation, a model's generalizability is assessed by fitting the model to one sample of data, holding those best-fitting parameters constant, and then measuring the model's fit to another sample of data generated by the same cognitive process. Parameter retuning on the second data set is not allowed. If the model perfectly captures the underlying process and there is no noise (i.e., sampling error) in the data, the two fits will be equal. Most often, the second fit is worse precisely because the data are noisy. The amount by which it is worse provides an estimate of how much the model overfit the first data set, capturing the spurious fluctuations due to sampling error.

The two versions of FLMP used in the preceding simulation were again compared. One hundred data sets were generated by FLMP$_R$ and fitted by both models. Mean RMSD of the fits of each model to the data are shown in the first row of Table 3. This test is equivalent to the SE$_R$ condition in Table 2, and the results are identical. FLMP$_F$ fitted the data best 100% of the time. Because FLMP$_R$ generated the data (i.e., it is the true model), it should have yielded the best fit at least some of the time. The only way FLMP$_F$ could have always provided a better fit is by fitting the variation due to sampling error.

That FLMP$_F$'s superior fit is due to overfitting the data can be seen in the second row of Table 3, where the generalizability of the models was assessed by fitting them to a second set of 100 samples, also generated by FLMP$_R$. Mean fit was worse for both models, but the increase in fit for FLMP$_F$ (.30) was double that for FLMP$_R$ (.16), indicating that FLMP$_F$ absorbed twice as much sampling variation as FLMP$_R$ in fitting the first 100 samples. As a result, FLMP$_F$ not only yielded the poorest mean fit, but also provided the best fit least often. FLMP$_F$'s overfitting must be caused by its eight extra parameters because its functional form is identical to FLMP$_R$'s.

This simulation illustrates that the model that provides the best fit may not generalize the best. From the standpoint of generalizability, a suitable method of model selection must balance these opposing goals. On the one hand, the model must provide a sufficiently good fit to the data to capture the underlying process. On the other hand, the fit must not be so good as to sacrifice generalizability. Measures of GOF, such as RMSD, concern themselves only with the first goal, whereas measures of generalizability try to satisfy both.

It is from the perspective of generalizability that BMS was offered as a measure of model selection (Myung & Pitt, 1997). It is a good measure of generalizability precisely because it is sensitive to the many aspects of model complexity in addition to measuring GOF. RMSD is a poor measure precisely because it is insensitive to complexity. Massaro et al's wariness of one feature of BMS, what they term "parameter invariance," reveals a fundamental difference in the flexibility and generalizability approaches to model selection. In BMS, a model is selected that does the best job of fitting a set of data across the full range of parameter values. Averaging fits across parameter values is exactly what should be done if one is interested in the generalizability of the model beyond the data in hand. Sampling error will cause variability in future data sets, which the model (with parameters fixed) should still fit well if it does a good job of capturing only the underlying process, and not also the ever-present noise. Within the flexibility framework, such averaging over the parameter range is counterproductive, as it unfairly penalizes models that possess the flexibility necessary to capture all of the variation in the data, regardless of its source.

The superiority of generalizability over flexibility is easily shown by rerunning the simulation in Table 2, but substituting BMS for RMSD. Model recovery is virtually perfect (Table 4). The selection method is sensitive to model differences, but not sampling error or individual differences. When variation due to sampling error (SE_R, SE_F) or both sampling error and individual differences (SE_R+ID, SE_F+ID) were present in the data, they were ignored, and the model that generated the data was almost always chosen. When the data came from each model half of the time (DM), BMS was also able to determine which model generated the data, selecting the correct model almost all of the time.

The data across Tables 2-4 can give the impression that BMS is superior to RMSD, which is not always what Massaro et al found. RMSD performed as well as BMS in at least half of their simulations. In a few instances, BMS performed quite poorly. What is the cause of these seemingly discrepant outcomes? In the remainder of this paper, we show that the discrepancies are more apparent than real, and that BMS's "failures" are readily explainable by a consideration of the details of the simulations and the limitations of all selection methods. We focus first on the three simulations in which BMS underperformed RMSD. These results serve as a back-drop for understanding why both selection methods performed so similarly in the other simulations.

Re-Examination of Massaro et al’s (2001) Simulations

Evaluating the Selection Methods by Adding Noise to the parameters

In their Table 4, Massaro et al compared RMSD and BMS on their ability to recover the correct model (FLMP or WTAV) when Gausian-distributed error and sampling error were added to the population proportions (P_{ij} in Table 1). Our focus is on the effects of adding Gausian noise. The Gausian error distribution had one of seven standard deviations spanning a range from 0.00 to 0.80. Performance of the two selection methods was evaluated by measuring how well each could identify which of the two models (FLMP or WTAV) generated a sample of data. The sample was also generated by one of these two models, so the test assessed how well RMSD and BMS "recovered" the data-generating model. Higher accuracy generally indicate better recovery (i.e., ability to discriminate between the models).

Model recovery of both selection methods was measured at each noise level. Their data are reproduced in the first four columns of Table 5 of the present paper. As one might expect, performance of the two selection methods declined as the standard deviation of the noise increased. Massaro et al were particularly concerned about the asymmetry that emerged with BMS at the higher noise levels. When the data were generated by WTAV, recovery of the correct model (WTAV) leveled off at 88%, misattributing the data as belonging to FLMP only 12% of the time. When the data were generated by FLMP, correct recovery leveled off at a much lower value, 61%. Massaro et al conjectured that this asymmetry is due to an inherent bias in the selection method of favoring less complex models. In actuality, it is due to a simulation error.

The addition of Gaussian-distributed error to P_{ij} necessitated truncating the error distribution at 0 and 1so that simulated response probabilities would stay within the range of 0 and 1. An unwanted side effect of truncating the error distribution in this way is that the distribution itself becomes asymmetric. For example, when simulated probabilities are generated by introducing truncated Gaussian errors around a given response probability, say 0.80, values above 1 will be obtained more often than values below 0 so truncated probabilities will include more 1's than 0's. As a result, the mean of these probabilities will not in general be the same as the original probability, unless the original value is exactly equal to 0.5. Consequently, the Gaussian truncation procedure resulted in the creation of distorted response patterns that are no longer FLMP response patterns. As such, FLMP could not have provided a perfect fit to the data even in the absence of error. A prerequisite for evaluating model recovery is that the model be able to do so. Otherwise, the performance of the selection method is misrepresented.

Evidence that demonstrates the distorting effect of error truncation is provided in the last two columns of Table 5. At each noise level, FLMP and WTAV were fitted to the response patterns generated by each model when Gausian error and sampling error should have been zero. The expectation of each observed proportion (i.e., response value) generated by each model was estimated by sampling each P_{ij} and the two noise distributions 10000 times and then averaging the resulting values. If an error distribution is symmetrical, the averaged value will be zero. Asymmetrical distributions will add a non-zero value, and thus distort P_{ij}. If both error distributions were symmetrical, the effects of error would have been eliminated and each model should have fitted its own data perfectly, which would be reflected in values of 100% in the two cells in the off diagonal at each noise level. Scanning across the seven noise levels, one can see that FLMP was affected much more by noise truncation than WTAV. Even at the .80 noise level, WTAV absorbed 99.93% of the variance. In contrast, FLMP provided a poorer and poorer fit as noise level increased, until by the .80 noise level, the model absorbed only 93.16% of the variance, which is even less than that for WTAV (94.2%). As noise level increased, the data-generating FLMP model became less and less FLMP-like, whereas WTAV changed little.

It is this asymmetry in the effects of noise truncation on the two data-generating models that caused BMS to discriminate between the two models less well when the data were generated by FLMP rather than WTAV. The parallel between the performance of BMS and the FLMP-likeness of the data generating model is striking. When the data were generated by FLMP, correct model recovery under BMS dropped the most between the .20 and .40 noise levels, from 78% to 62%. Between these same noise levels is where the largest change in the data generating model is found. Percent variance accounted for dropped 2.80% (from 98.66 to 95.86). Prior to this point, the largest drop was 0.59%.

Further evidence that demonstrates that truncation of the noise distribution was the cause of the asymmetry in how BMS performed is shown in Table 6. The simulations in Table 5 were rerun using binomially distributed noise, which is an appropriate way of simulating sampling variation given the experimental design (two response choices) and because it eliminates the truncation problem. When binomially-distributed noise is used, noise level cannot be varied by changing the standard deviation of the distribution. Instead, the sample size must be varied, with smaller sample sizes being the equivalent of more noise (i.e., there is less information about the identity of the model underlying the data). Model recovery using BMS (right side of Table) shows no such bias in favoring the simpler model (i.e., WTAV), as Massaro et al argued, performing exceptionally well all the way down to a sample size of 10. Only with a sample size of five does BMS begin to have difficulty, and it is just the opposite of what Massaro et al hypothesized: BMS recovered the correct model more often when FLMP generated the data than when WTAV generated the data. Model recovery using RMSD (left side of Table) performed as it did in Table 5, exhibiting a consistent bias to favor FLMP as noise level increased.

Replication and Extension of Myung and Pitt (1997)

In Tables 1 and 2 of their paper, Massaro et al (2001) extended a model recovery simulation carried out by Myung and Pitt (1997) in which BMS and RMSD were compared across three parameter sets using two models, FLMP and LIM. Massaro et al included two additional parameter sets and the WTAV model. The results of Myung and Pitt were replicated. Across the nine conditions, BMS always recovered the correct model most often. RMSD, in contrast, recovered the correct model in only three of the nine conditions (always and only when FLMP generated the data). When LIM generated the data, RMSD's lack of sensitivity to model complexity caused it to choose FLMP most often. When WTAV generated the data, FLMP also won out, but twice by very little (~4%).

These same biases emerged with RMSD when model recovery was tested using the two new data sets: The correct model was chosen only when FLMP generated the data. BMS performed better, recovering the correct model in four of the six cases. In two cases, however, BMS chose the wrong model most often. One was with their parameter set 4. BMS's performance was absolutely appalling, choosing WTAV instead of FLMP 92% of the time when FLMP generated the data. With parameter set 5, BMS performed more like RMSD, choosing FLMP when WTAV actually generated the data, but only by a slight margin (4%). We were rather surprised by these results and set out to learn why BMS performed in this way.

The unexpected results with parameter set 5 are in fact not a failure of BMS, but rather the numerical integration method. Massaro et al used the Simple Monte Carlo (SMC) method to estimate the marginal likelihood for BMS. This method can require a very large number of iterations to settle on a solution. Massaro et al used a cut-off of one million. Even this can be insufficient when the models are difficult to distinguish, causing the solution to be an under- or overestimate of the true value. In these (and most) situations, Markov Chain Monte Carlo (MCMC) methods (Gilks, Richardson & Spiegelhalter, 1996) will yield more accurate results.[2]

When MCMC was used in place of SMC, BMS performed as expected. Shown in the first three columns of Table 7 are the BMS model recovery results of Massaro et al using parameter set 5. BMS's failure can be seen in the third column, where WTAV data were thought to have been generated by FLMP slightly more often that WTAV (52% vs 48%). The last column in the Table contains a replication of these simulations using MCMC. Mean marginal likelihood values reversed between FLMP and WTAV, and the correct model was chosen 89% of the time.

BMS's failure to choose the correct model when parameter set 4 was used is not due to a convergence problem with the numerical integration method. When this simulation was rerun using MCMC, BMS chose WTAV (the wrong model) 83% of the time when FLMP generated the data. The magnitude of this failure intrigued us because BMS did not just have difficulty distinguishing between the models (which would have led to a 50% recovery rate). Rather BMS consistently chose the wrong model. Massaro et al offered no explanation for the result. We hypothesized that it might be due to the set of parameters used to generate the data. That is, perhaps FLMP response patterns generated using parameter set 4 were so much like typical WTAV response patterns that BMS erroneously ascribed the data to WTAV. In a sense, the data tricked BMS into choosing the wrong model.

To explore this idea, a massive simulation was undertaken in which the entire parameter space was sampled. The purpose was to identify FLMP response patterns that are very WTAV-like, and determine whether they lead to the confusion that BMS exhibited. The same 2x8 design of the preceding simulations was used. Ten thousand sets of parameters were generated (10 parameters for FLMP plus a weight parameter for WTAV) using a uniform density on [0,1]. Each of these parameter sets was then run through both models to create 10,000 FLMP response patterns and 10,000 WTAV response patterns. Because we were interested in identifying FLMP response patterns that resemble WTAV response patterns, WTAV was fitted to each of the 10,000 FLMP response patterns using RMSD as the measure of fit. Next, all parameter sets were then sorted as a function of this fit, with those yielding the smallest RMSD (i.e., best fit) at the top of the list, and the worst fit at the bottom, essentially ranking the parameter sets by confusability of the response pattern. Those at the top of the list yielded FLMP response patterns that were most like WTAV response patterns; those at the bottom of the list yielded response patterns that were more uniquely FLMP.

The rank-ordered parameter sets were then divided into six bins, and 100 parameter sets were randomly selected from each to carry out a couple of analyses. In the first analysis, all 10,000 parameter sets were compared with parameter set 4 of Massaro et al to find the closest match. Not surprisingly, this parameter set is one of the most confusable, coming from bin 2. This and another similar parameter set are listed in Appendix A along with parameter set 4. Also listed are two other parameter sets that caused BMS to recover the wrong model. What is common among them is that they yield virtually parallel response functions, a characteristic of WTAV, not FLMP, which is known for its football-shaped response patterns (Massaro, 1998).

Shown in the upper graph in Figure 1 are the FLMP response functions using parameter set 4 and the FLMP response functions using one of the most similar parameter sets. The similarity of the functions in shape and actual values suggests that parameter set 4 produced confusable data and would most likely have belonged to bin 1 or 2 were it one of the 10,000 generated in the simulation. WTAV accounted for 99.81 of the variance when fitted to data generated by parameter set 4 and 99.92 when fit to the other parameter set. BMS's failure in this instance is therefore not surprising. In fact, the response pattern is so much like WTAV, one might well wonder whether BMS's performance should be considered a failure at all: There is every reason to believe the data were generated by WTAV. Given how WTAV-like the FLMP response pattern is, one might also wonder why RMSD performed so well, recovering the correct model (FLMP) 77.5% of the time. (In contrast, BMS performed correctly only 8% of the time.) An answer to this question is provided below.

In the second analysis of these data, the 100 parameter sets in each of the six bins were used in six model recovery simulations, as in Table 5. For each set of parameters, a single data sample (sample size of 20) was generated from each model and RMSD and BMS were evaluated on their abilities to determine whether FLMP or WTAV generated the data. Shown in the lower graph in Figure 1 are the percentages of errors made by each selection method in each bin. Also shown is the percentage of errors in which both methods failed on the same parameter set (i.e., the union of RMSD and BMS errors). Mean WTAV fit (percent variance accounted for) to the 100 FLMP response patterns in each bin is shown on the y axis for reference.

The data provide additional insight into the similarities and differences of the two selection methods, and the conditions in which they are likely to fail and succeed. Bin 1 contained the 500 parameter sets that yielded the most WTAV-like FLMP response patterns. As can be seen, both selection methods faired quite poorly. The error rate for BMS was 55%, and that for RMSD was 80%. As the parameter sets yielded more distinguishable response patterns, errors decreased for both selection methods, but much more so for BMS than RMSD. The divergence of the two selection methods is most apparent in bin 3, where RMSD dropped from 80% to 60% errors and BMS dropped a proportionately much larger amount (from 45% to 18%). Error rates in bins 4-6 did not fluctuate greatly from the values in bin 3.

These data clearly demonstrate how much more error-prone RMSD can be than BMS. The differences are quite large when averaged over bins. RMSD failed 65.0% of the time (weighted averaged) and BMS failed 18.9% of the time. Both methods failed on the same parameter sets in 15.7% of the tests (hashed bars). Comparison of the bars across bins shows that the hashed bar is only slightly below the BMS bar, indicating that there were very few parameter sets in which BMS failed and RMSD did not (3.2%). Looked at another way, if BMS failed, RMSD was almost certain to fail. The reverse, however, was not true.

The data in this graph lump together the different types of errors that a selection method made. These data were therefore broken down to reveal the types of errors that each selection method made, and are shown in Table 8 in the familiar format used in preceding Tables. RMSD exhibited its typical error pattern: When WTAV generated the data, there was a strong bias to choose FLMP, the more complex model. That this bias remained stable across bins reminds us that the bias is an inherent property of the selection method, having nothing to do with the parameter set or the discriminability of the resulting response patterns. When FLMP generated the data, RMSD performed surprisingly well in bin 1 (71% accuracy) given how WTAV-like the response patterns were. Performance continued to improve across bins.

When BMS was the selection method, model recovery was good and consistent across all bins when WTAV generated the data, selecting the correct model 90% or more of the time. When FLMP generated the data, BMS performed as expected given the rank ordering of the parameter sets. Model recovery was at chance in bin 1, which is where it should be when FLMP response patterns are highly similar to WTAV response patterns. As the parameter sets yielded response patterns that were more typical of FLMP (bins 2-6), recovery quickly improved, becoming virtually perfect by bin 4.

In bin 1, RMSD performed unusually well when FLMP generated the data, better than

BMS (71% vs 50%). Given that this outcome is found in what is surely the most difficult condition in the simulation (i.e., where the response patterns of the two models were most confusable) it can give the impression that RMSD will perform best when it really counts, and seems at odds with the results in Figure 1. However, when evaluating RMSD's performance in this condition, or in any model recovery test, it is important to remember that performance cannot be attributed solely to the model recovery abilities of the selection method. Rather, performance is partially due to the method's inherent bias in favoring the more complex model, which just happens to be FLMP in this case. That is, RMSD's bias contributes to its successes as well as its failures. The bias is omnipresent. It inflates performance when the bias works in favor of the more complex model (i.e., FLMP), but in this situation it masquerades as accurate recovery, making the bias invisible. This gives the appearance of RMSD performing better than it really does. The magnitude of the bias is visible when it works against the less complex model (i.e., WTAV). This condition is a more accurate reflection of the method's inability to recover the correct model.

The severity of RMSD's bias is visible in the adjacent column in Table 8, where WTAV generated the data. FLMP was incorrectly chosen as the data-generating model two–thirds of the time, and this bias is evident over all bins. If the 71% recovery rate in bin 1 were due solely to RMSD's good ability to discriminate between the models when the response patterns were highly confusable, then recovery in other cells in the Table should have been equally good, if not perfect. That they are not clearly indicates that the 71% recovery rate in bin 1 is an artifact of bias.

A more accurate interpretation of the results in bin 1 is that the 50% correct recovery rate by BMS when FLMP generated the data is to be expected under the circumstances. The most WTAV-like FLMP response patterns were present in this bin. Recovery should have been near chance because FLMP data mimicked WTAV data. The correct model was unrecoverable because the data provided little or no information about the true identity of the data-generating model. As one would expect, this situation quickly corrected itself across bins as the FLMP data became less WTAV-like.

The data in Figure 1 and Table 8 together should also dispel beliefs about the omnipotence of any model selection method. No method, no matter how good it is, should perform well when confronted with data from bins 1and 2. How could it when information is essentially misleading? In this regard, Massaro et al's (2001, p. 6) observation that selection method performance appears to be data-dependent is right on the mark. One implication of the data-dependent nature of model selection is that data patterns can always be found that will cause a selection method to fail. Recovery results from one or two atypical parameter sets can be misleading, especially when they produce response patterns that are representative of the typical performance of the competing model. Only by sampling the entire parameter space, as we did in this simulation, can a more accurate picture of the true recovery ability of a selection method be obtained. Of course, simulations like this are only suggestive of general tendencies. They are approximations, not proofs, of what is likely to be found.

Looked at another way, knowledge of the parameter sets and response patterns is necessary to interpret model recovery results correctly. In a 2x2 matrix of recovery data, values of 100% in the off diagonals is not always the correct prediction. By this criterion, what appears to be a failure in model recovery can actually be reasonable selection behavior given the characteristics of the response patterns (e.g., when they are highly similar). Conversely, good model recovery in situations like this can serve as a red flag that the selection method is performing incorrectly, as was the case with RMSD in this same simulation.

A Test of Newton's Law

The third situation in which Massaro et al (2001) found BMS misrecovered the correct model a high percentage of the time was when Newton's law (NMP) was compared with a weighted additive version of Newton's law (WNAV). The data from this simulation are reproduced in Table 9. RMSD again performed as expected, exhibiting a strong bias to select the more complex model (NMP in this case) when WNAV generated the data, but correctly chose NMP 100% of the time when it generated the data. BMS exhibited the opposite bias, choosing WNAV instead of NMP when NMP generated the data (78% of the time), but correctly choosing WNAV when WNAV also generated the data. Massaro et al again attributed BMS's failure to an inherent bias in the selection method to favor the less complex model.

BMS's misrecovery rate was so great in this instance that we wondered whether, as in the preceding section, the simulation is actually unrepresentative of how BMS typically performs. Perhaps, as with parameter set 4, the parameter set generated an NMP response pattern that is very much like a WNAV response pattern. To explore this possibility, we reran the simulations and analyses in the preceding section using these two models. Ten thousand parameter sets were generated to sample a large portion of the parameter space. They were then sorted as a function of how WNAV-like the NMP resulting response pattern was, divided into six bins, and then analyzed as before.

The data are shown in the two graphs in Figure 2, and tell a very similar story to those in

Figure 1. In the upper graph, two NMP response patterns are plotted, one using the parameter set of Massaro et al and the other the parameter set from the current simulation that most closely matched theirs (parameter values are listed in Appendix A). The best-matching parameter set came from bin 1. The similarity of the two response patterns confirms our suspicion that the parameter set Massaro et al chose produced a particularly confusable (i.e., very WNAV-like) response pattern, which is why BMS chose WNAV so frequently (78%) when NMP actually generated the data. That RMSD produced no errors (NMP was chosen 100% of the time) with this parameter set is unbelievable, and again indicates that RMSD's bias to favor complex models had a significant influence on model selection. RMSD should have made errors precisely because the underlying model is not easily identifiable from the data. All selection methods should.

Overall model recovery performance of the two selection methods across bins is shown in the lower graph. Both made the most errors when the parameter sets came from bins 1 and 2, where NMP response patterns were most WNAV-like. As confusability of these response patterns decreased, so did error rate. In each bin, including bin 6 in which the parameter sets yielded the most discriminable data, RMSD made more errors than BMS. Overall, RMSD made an average of 46.5% errors. BMS made almost one-third less (16.0%). As in Figure 1, the frequency with which both methods erred on the same parameter set (10.9%) was slightly less than BMS's error rate, replicating the findings that if BMS failed to recover the correct model, RMSD almost certainly failed as well. In sum, there are actually very few parameter sets (5.1%) that led RMSD to outperform BMS.[3]

Knowing that the parameter set used in the model recovery test in Table 9 created an

NMP response pattern that was very WNAV-like casts a very different light on how the results should be interpreted when NMP generated the data. Like BMS, RMSD should have been fooled into thinking the data were generated by WNAV much of the time. Instead, RMSD exhibited a strong bias to choose the more complex model (NMP). The extent of this bias is visible when WNAV generated the data; RMSD incorrectly chose NMP 90% of the time. When the two outcomes across data-generating models are considered together, RMSD's performance seems paradoxical: How can the selection method discriminate perfectly between two models given what are very confusable response patterns, and yet fail so miserably on another set of response patterns, which in all likelihood are not nearly as confusable?

These two large-scale simulations demonstrate that model recovery results must be interpreted with a thorough understanding of the models and the data on which recovery is evaluated. In Massaro et al's simulations, the errors that BMS made, if they can even be called that, stem from a limitation of the method in a very specific context: The parameter sets that were used yielded response patterns that mimicked the typical behavior of the competing model, causing mis-recovery. All selection methods should perform poorly in this situation simply because there is not enough information in the data. That RMSD did not, and instead frequently chose the more complex model, is strong evidence that bias played a significant role in guiding model selection.

How Trustworthy is the Selection Method?

When RMSD and BMS Perform Similarly

The results in the preceding sections might lead one to think that BMS should always outperform RMSD, yet in three of the model selection tests that Massaro et al carried out (5x5 bimodal integration data, 8-alternative bimodal integration data, and Pitt [1995] data), the two selection methods performed equivalently. Why? Part of the answer, as Massaro et al discuss, is that FLMP and WTAV are better matched in complexity than, for example, FLMP and LIM. Whether these two selection methods will perform similarly will also be determined by the data. To understand why, consider the data in the lower graph in Figure 1 again. Just as there were many parameter sets that yielded response patterns that one or both methods failed on, there were also many parameter sets whose response patterns led to correct recovery by both methods. A rough estimate of these can be gleaned from the graph. In each bin, the space to the right of the bars (i.e., 100 minus the error rate) is an estimate of the percentage of response patterns that each selection method would recover correctly. Collapsed over bins, RMSD and BMS performed correctly on 45% and 79.1% of the response patterns, respectively. Although these estimates will change as a function of other factors (e.g., experimental design, sample size), they demonstrate that there is plenty of opportunity for RMSD and BMS to yield the same (correct) answer.

Because BMS takes into account model complexity and RMSD does not, the data in Figure 1 also provide information on how much model complexity influenced model selection in this experimental setup. The difference between the BMS and RMSD bars is a rough estimate of this value. If the models were equal in complexity, then the two bars in each bin should be equal in length because complexity would have had no effect on model recovery using RMSD. The RMSD bars will lengthen relative to the BMS bars as the complexity of one of the models increases. The difference between these two bars can therefore also provide an indication of how much more complex one model is than the other.

The frequency with which the two selection methods will perform similarly makes it reasonable to ask whether they are frequently interchangeable. The similar performance of RMSD and BMS led Massaro et al to conclude that RMSD is likely to yield accurate and reliable results in their testing situations. To the extent that such experiments advance the science, these testing situations will undergo both small and large changes, particularly when better models are introduced in place of ones shown to be inferior. An advantage of using BMS is that it will perform far more faithfully across these situations, as the simulations in the next section demonstrate.

Generalizing Model Selection Performance

A virtue of selection methods that are sensitive to model complexity, like BMS, is that they will perform more accurately and consistently across testing situations (e.g, variation in sample size, data, experimental design, and the models themselves) than methods that do not. The data in Figures 1 and 2 are proof of this. Differences in the reliability of the two methods is further demonstrated in Table 10, where BMS and RMSD were compared in their ability to recover different pairs of models across samples sizes. Shown in each cell of the Table is the mean of the given selection criterion across simulated samples and the percentage of time the particular model was selected under the selection method.

A comparison of FLMP and WTAV is shown in the middle of the Table for reference. Just as Massaro et all found in some of their simulations, RMSD and BMS performed equivalently and they did so across sample sizes. This outcome can give the impression that the two selection methods are interchangeable, when in fact it is situation specific, as demonstrated by the data on the left and right sides of the Table. On the left, WTAV was compared with

FLMP_w, a geometrically weighted version of FLMP (see Table 1 for its model equation), making the models equal in the number of parameters (11). BMS outperformed RMSD, but only when the data were generated by WTAV. As sample size increased, RMSD's performance approached that of BMS's. The same result was found on the right side of the Table, where FLMP was compared with LIM_T, a truncated version of LIM, defined in Table 1. RMSD mis-recovered the correct model, in particular for sample size 12, when LIM_T generated the data. BMS performed much more robustly.

The law of large numbers is responsible for the improvements in recovery across sample size. As it grows, sampling error diminishes, making it easier to discriminate between the models. Essentially, there is less error to confuse the selection methods. The fact that both methods perform similarly as sample size increases reminds us that BMS and RMSD are asymptotically equivalent: Both will perform identically given an infinitely large sample of data. This fact is one reason why the two theoretical approaches to model selection that were discussed in the first part of the paper, flexibility and generalizability, will yield the same answer. They differ primarily in how sampling error is treated, as potentially meaningful variation (flexibility) or as meaningless variation (generalizability). When sample size is large enough, sampling error becomes negligible to the point where its contribution to model selection can be ignored. There is no more noise for a flexible model to erroneously absorb, and when there is no more noise, models will generalize perfectly. However, because small samples are the norm in experimentation, being constrained by a host of factors, including experimental design and choice of stimuli (not to mention modelers being forced to use whatever data are available), the accuracy and trustworthiness of BMS make it the safer choice a priori.

Perhaps not surprisingly, sample size and data (i.e., response patterns) interact to influence model selection. We close this section with an example that illustrates this point. The rectangle in Figure 3 represents the space of all possible response patterns. Models A and B occupy a region in this space. When the experimental data fall in a region that is close to only one model (point C), model selection should be successful regardless of whether there is a small or large amount of noise (concentric rings). When the data fall close to both models, it is much more difficult to discriminate the source of the data when error is large, but easier when error is smaller. Thus, model selection methods can be highly data dependent at small sample sizes because of the presence of noise. As sample size increases, this dependency diminishes (see Table 10). In the limit (i.e., asymptotically), model selection is data independent.

Conclusion

Selecting among models of cognition given a limited amount of data is a difficult problem. In psychology, it is particularly challenging because the mental process being studied is not directly observable and our only tie to it is noisy data. By making generalizability the goal, the problem of noise is mitigated and model selection becomes statistical inference based on fit and complexity. The superiority of this approach is demonstrated by the robustness of its selection methods, such as BMS (see Myung et al, 2000, Pitt et al, in press). The intuitiveness of generalizability makes it the approach of choice in other fields, such as computer science (Rissanen, 1983; Vitanyi & Li, 2000; see also Hansen & Yu, 2001) , where a related selection method, Minimum Description Length, is proving quite valuable. We believe it should be preferred in psychology as well.

The simulations presented here, along with reanalyses of those carried out in Massaro et al (2001), reveal severe limitations of RMSD and clearly demonstrate the superiority of BMS as a selection method. Nevertheless, we are not claiming that BMS is infallible or even that it is bias-free. It may indeed be biased to select the simpler of two models, but the small pool of evidence that Massaro et al presented to make this case is weak, and now even smaller given the present results. Even if BMS is biased, its biases are far smaller than those of RMSD, as BMS's consistently good recovery performance demonstrates. In this regard, it is important to understand that BMS does not penalize the more complex model just because it is more complex. Rather, it weighs a model's complexity relative to what is needed to provide a good fit to the data. The more excess complexity a model has, the more it is penalized. If a model's complexity is justified by the data, then the complex model will be preferred over the simpler model (see Pitt et al [in press] for further discussion).

Despite the many shortcomings of RMSD that the present and prior simulations reveal, we do not advocate abandoning it. On the contrary, the work has been quite informative in identifying when and how RMSD can safely to be used to guide model selection. In particular, RMSD will probably performs just as well as BMS when the models are similar in complexity. This is the only condition, besides very large sample sizes, in which BMS and RMSD should perform similarly, because complexity will minimally affect model selection. The fact that such similar performance was found with FLMP and WTAV in many simulations suggests that these models are closer in complexity than FLMP and LIM, as Massaro et al suggested.

The knowledge gained from this collective body of work suggests a productive way in which to use RMSD in model selection. It can be used if it is first shown to perform well in a model recovery simulation using similar response patterns and the same sample size. If recovery is good, the models are probably close enough in complexity that this factor is likely to have a negligible impact on selection. When RMSD fails in such a situation, then it should be an indication that the models are sufficiently different in complexity to require the use of a selection method that takes into account this property of a model. Such a simulation can serve as a useful diagnostic tool to assess the relative complexity of the models (albeit indirectly) and determine which selection method to use.

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Appendix A

Parameter set 4 from Tables 1 and 2 of Massaro et al (2001)

2_1	2_2	8_1	8_2	8_3	8_4	8_5	8_6	8_7	8_8
0.45	0.55	0.11	0.22	0.33	0.44	0.55	0.66	0.77	0.88

Four parameter sets that caused BMS to misrecover the correct model. The first two are the most similar to those used by Massaro et al

0.3971	0.4523	0.1157	0.2402	0.397	0.5114	0.5982	0.6399	0.8038	0.9166
0.4416	0.4831	0.1451	0.3053	0.3731	0.4296	0.5549	0.7351	0.8252	0.86
0.7986	0.8093	0.1904	0.2707	0.3175	0.5356	0.5755	0.5942	0.7419	0.7928
0.5133	0.6254	0.3599	0.4845	0.4943	0.6659	0.6704	0.7655	0.8063	0.8201

Parameter set from Table 6 of Massaro et al (2001)

2_1	2_2	8_1	8_2	8_3	8_4	8_5	8_6	8_7	8_8
0.97	0.98	0.01	0.15	0.3	0.4	0.6	0.7	0.85	0.99

Four parameter sets that caused BMS to misrecover the correct model. The first two are the most similar to those used by Massaro et al

0.9525	0.9642	0.1083	0.2472	0.3498	0.4403	0.5835	0.6462	0.8228	0.9651
0.9616	0.9858	0.03096	0.09027	0.2251	0.3899	0.5833	0.6806	0.8251	0.9022
0.7109	0.829	0.4783	0.484	0.5315	0.6371	0.662	0.7617	0.8098	0.8921
0.8165	0.8278	0.2478	0.2513	0.2929	0.6194	0.7119	0.7276	0.8841	0.9251

Appendix B

Mean model recovery performance of RMSD and BMS in each of the six bins. Parameter sets were rank ordered as a function of how WNAV-like the response function was that NMP produced using each parameter set.

		Selection Method			
		RMSD		BMS	
Rank ordered	Model	Data From		Data From	
parameter set	Fitted	NMP	WNAV	NMP	WNAV
bin 1	NMP	0.0538	0.0611	-14.70	-18.00
(1-500)	% win	66	50	72	8
	WNAV	0.0565	0.0609	-15.41	-16.34
	% win	34	50	28	92
bin 2	NMP	0.0509	0.0608	-14.36	-17.51
(501-2000)	% win	72	39	73	11
	WNAV	0.0559	0.0602	-15.26	-16.22
	% win	28	61	27	89
bin 3	NMP	0.0477	0.0657	-14.08	-17.80
(2001-4000)	% win	77	49	86	10
	WNAV	0.0583	0.0660	-15.43	-16.54
	% win	23	51	14	90
bin 4	NMP	0.0485	0.0635	-14.34	-17.90
(4001-6000)	% win	90	37	92	3
	WNAV	0.0686	0.0614	-16.18	-16.08
	% win	10	63	8	97
bin 5	NMP	0.0442	0.0648	-14.00	-17.99
(6001-8000)	% win	99	32	98	8
	WNAV	0.0852	0.0623	-17.02	-16.22
	% win	1	68	2	92
bin 6	NMP	0.0315	0.0649	-13.02	-17.80
(8001-10000)	% win	100	32	100	7
	WNAV	0.1210	0.0594	-19.82	-15.94
	% win	0	68	0	93

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Footnotes

[1] RMSD defined here and used in Massaro et al (2001) differ from the RMSD of Myung and Pitt (1997), where it was defined as $RMSD = \sqrt{\sum (prd_i - obs_i)^2 / (N - k)}$.

[2] All BMS results reported in the present paper were obtained using MCMC methods.

[3] For completeness, the data in Figure 2 are presented in Appendix B broken down by the types of errors made in each of the six bins. For the most part, the data add little to what was learned from Table 8, so they will not be discussed further.

Table 1

Five models of information integration employed in the present work. Each model assumes that the probability of categorizing an input stimulus to one of two categories, denoted by P_{ij}, is a function of two parameters, 2_{i} and 8_{j}. The two parameters represent the degree of support for the category of interest given the specific i and j feature dimensions of the stimulus where i = 1,...,q1, j = 1,...,q2, and take on values between 0 and 1.

Model	Model Equation
FLMP	$P_{ij} = \dfrac{\theta_i \lambda_j}{\theta_i \lambda_j + (1-\theta_i)(1-\lambda_j)}$
FLMP_{W}	$P_{ij} = \dfrac{\theta_i^{\,w} \lambda_j^{\,(1-w)}}{\theta_i^{\,w} \lambda_j^{\,(1-w)} + (1-\theta_i)^{w}(1-\lambda_j)^{(1-w)}}$
WTAV	$P_{ij} = w\theta_i + (1-w)\lambda_j$
LIM	$P_{ij} = \dfrac{\theta_i + \lambda_j}{2}$
LIM_{T}	$P_{ij} = \min(\max(\theta_i + \lambda_j - 0.5{,}0){,}1)$

Table 2

Model recovery rates across five conditions using RMSD.

	FLMP$_R$		FLMP$_F$		Model fitted	
Data Generated From	21	22	23	24	FLMP$_R$ (k=8)	FLMP$_F$ (k= 16)
1. Reduced Model with						
a) Sampling Error (SE$_R$)	100	-	-	-	0.062 (0%)	0.054 (100%)
b) Sampling Error + Individual Diffs (SE$_R$+ID)	50	50	-	-	0.063 (0%)	0.054 (100%)
2. Different Models (DM)	-	50	50	-	0.092 (0%)	0.053 (100%)
3. Full Model with						
a) Sampling Error (SE$_F$)	-	-	-	100	0.100 (0%)	0.055 (100%)
b) Sampling Error + Individual Diffs (SE$_F$+ID)	-	-	50	50	0.114 (0%)	0.053 (100%)

Note: Sample size = 20

Table 3

Comparison of Two Models differing in Complexity Using Two Selection Methods, Goodness of Fit and Generalizability.

	Model fitted	
Selection Method	FLMP_R (true model)	FLMP_F
Goodness of Fit	0.062 (0%)	0.054 (100%)
Generalizability	0.078 (81%)	0.084 (19%)

Note: Sample size = 20

Table 4

Mean Model Recovery Rates and Percentages of Wins Across Five Conditions Using BMS.

	FLMP$_R$		FLMP$_F$		Model fitted	
Data Generated From	21	22	23	24	FLMP$_R$ (k=8)	FLMP$_F$ (k= 16)
1. Reduced Model						
a) Sampling Error (SE$_R$)	100	-	-	-	-41.85 (99%)	-46.41 (1%)
b) Sampling Error + Individual Diffs (SE$_R$+ID)	50	50	-	-	-42.44 (100%)	-46.99 (0%)
2. Different Models (DM)	-	50	50	-	-51.02 (50%)	-46.96 (50%)
3. Full Model						
a) Sampling Error (SE$_F$)	-	-	-	100	-53.67 (2%)	-49.98 (98%)
b) Sampling Error + Individual Diffs (SE$_F$+ID)	-	-	50	50	-55.85 (1%)	-46.58 (99%)

Note: Sample size = 20

Table 5.

Mean Model Recovery Rates and Percentages of wins for FLMP and WTAV using RMSD and BMS at Five Noise Levels. The last two columns contain the mean percent variance accounted for when the specified model was fit to the data without sampling error. Values in the off diagonal should be 100% .

		Selection Method				% Variance	
		RMSD		BMS		accounted for	
Noise (SD)	Model Fitted	Data From		Data From		Expected Data From (Error-free)	
		FLMP	WTAV	FLMP	WTAV	FLMP	WTAV
0.00	FLMP	0.0367	0.0864	-35.9	-41.9	99.99	94.67
	% win	99	1	96	3		
	WTAV	0.1085	0.0539	-41.0	-37.5	93.39	99.99
	% win	1	99	4	97		
0.05	FLMP	0.0524	0.0920	-38.1	-43.4	99.93	95.12
	% win	98	2	95	4		
	WTAV	0.1127	0.0650	-43.1	-38.1	93.43	99.98
	% win	2	98	5	96		
0.10	FLMP	0.0767	0.1071	-41.9	-46.0	99.69	95.45
	% win	96	6	89	6		
	WTAV	0.1233	0.0881	-45.6	-40.4	93.49	99.94
	% win	4	94	11	94		
0.15	FLMP	0.1021	0.1267	-44.8	-49.3	99.25	95.53
	% win	94	13	78	11		
	WTAV	0.1384	0.1137	-48.9	-43.2	93.51	99.9
	% win	6	87	22	89		
0.20	FLMP	0.1273	0.1478	-48.9	-52.6	98.66	95.39
	% win	88	22	78	11		
	WTAV	0.1558	0.1390	-52.8	-46.5	93.54	99.86
	% win	12	78	22	89		
0.40	FLMP	0.2150	0.2278	-66.7	-70.1	95.86	93.87
	% win	78	47	62	12		
	WTAV	0.2292	0.2281	-70.9	-63.0	93.73	99.9
	% win	22	53	38	88		
0.80	FLMP	0.3089	0.3141	-91.9	-95.2	93.16	91.92
	% win	66	42	61	12		
	WTAV	0.3173	0.3192	-96.9	-86.4	94.02	99.93
	% win	34	59	39	88		

Table 6

Mean Model Recovery Rates and Percentages of Wins for FLMP and WTAV Across Five Sample Sizes

		Selection Method			
		RMSD		BMS	
Sample Size	Model Fitted	Data From		Data From	
		FLMP	WTAV	FLMP	WTAV
50	FLMP	0.0275	0.0829	-75.60	-139.56
	% win	100	0	99	0
	WTAV	0.1036	0.0453	-141.07	-92.93
	% win	0	100	1	100
24	FLMP	0.0372	0.0917	-61.23	-96.63
	% win	99	0	100	0
	WTAV	0.1075	0.0628	-92.45	-76.74
	% win	1	100	0	100
15	FLMP	0.0452	0.0995	-51.29	-76.85
	% win	99	13	100	1
	WTAV	0.1129	0.0820	-71.31	-66.73
	% win	1	87	0	99
10	FLMP	0.0576	0.1147	-45.44	-64.73
	% win	96	21	98	5
	WTAV	0.1231	0.1000	-59.03	-58.55
	% win	4	79	2	95
5	FLMP	0.0741	0.1385	-33.27	-46.09
	% win	96	49	98	26
	WTAV	0.1390	0.1407	-40.78	-44.57
	% win	4	51	2	74

Table 7

Shown each cell are the mean maximum log likelihood estimates and the percentage of wins by that model. The first three columns contain the data from Table 2 of Massaro et al (2001) in which parameter set 5 was used.. The last column is a replication of the simulation using MCMC when the data were generated by WTAV.

	Data From			MCMC replication
Model Fitted	FLMP	LIM	WTAV	WTAV
FLMP	-12.64	-15.53	-15.85	-16.52
%win	94	12	52	11
LIM	-21.93	-15.28	-17.74	
%win	0	88	0	
WTAV	-16.01	-16.25	-15.98	-15.88
%win	6	0	48	89

Table 8

Mean model recovery performance of RMSD and BMS in each of the six bins. Parameter sets were rank ordered as a function of how WTAV-like the response function was that FLMP produced using each parameter set.

		Selection Method			
		RMSD		BMS	
		Data From		Data From	
Rank ordered parameter set	Model Fitted	FLMP	WTAV	FLMP	WTAV
bin 1	FLMP	0.0541	0.0647	-15.6	-18.31
(1-500)	% win	71	66	50	5
	WTAV	0.0564	0.0653	-15.84	-16.64
	% win	29	34	50	95
bin 2	FLMP	0.053	0.063	-15.4	-18.27
(501-2000)	% win	66	69	60	5
	WTAV	0.0568	0.0642	-15.81	-16.57
	% win	34	31	40	95
bin 3	FLMP	0.0482	0.0627	-15.08	-18.36
(2001-4000)	% win	85	52	85	3
	WTAV	0.0652	0.0632	-16.41	-16.59
	% win	15	48	15	97
bin 4	FLMP	0.044	0.0615	-14.91	-17.96
(4001-6000)	% win	100	55	97	4
	WTAV	0.0795	0.0645	-17.44	-16.2
	% win	0	45	3	96
bin 5	FLMP	0.0335	0.0577	-13.88	-17.37
(6001-8000)	% win	100	69	98	10
	WTAV	0.0967	0.0611	-18.51	-16.14
	% win	0	31	2	90
bin 6	FLMP	0.0228	0.0562	-12.98	-17.11
(8001-10000)	% win	100	61	100	10
	WTAV	0.139	0.0598	-21.91	-16.04
	% win	0	39	0	90

Table 9

Reproduction of the Model Recovery Data in Table 6 of Massaro et al (2001).

	Selection Method			
	RMSD		BMS	
	Data From		Data From	
Model Fitted	NMP	WNAV	NMP	WNAV
NMP	0.0517	0.0154	-23.19	-30.69
% win	100	90	22	0
WNAV	0.0764	0.0167	-13.81	-13.81
% win	0	10	78	100

Table 10

Mean recovery rate and percentages wins comparing RMSD and BMS on three pairs of models at three sample sizes.

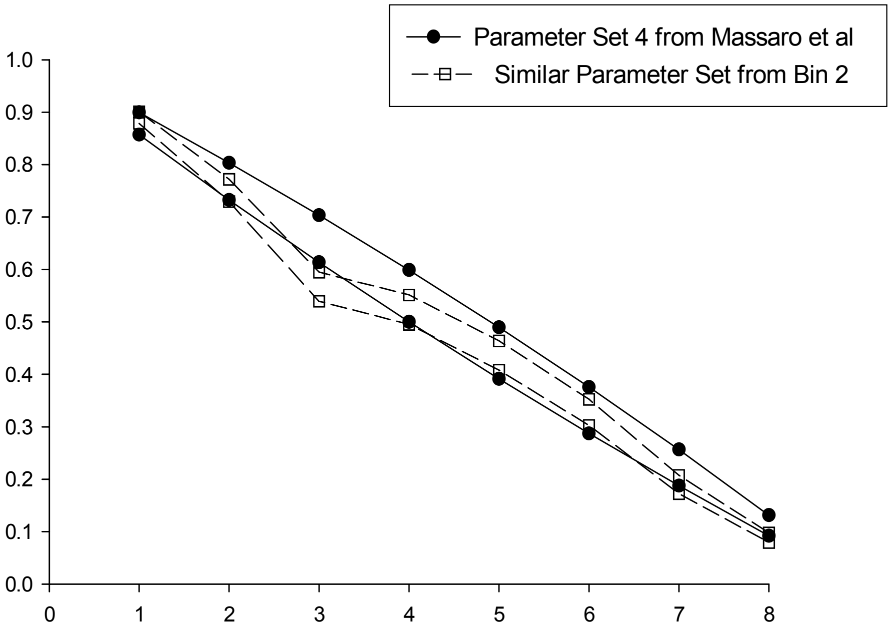
		Selection Method					Selection Method					Selection Method			
		RMSD		BMS			RMSD		BMS			RMSD		BMS	
Sample	Model	Data From		Data From		Model	Data From		Data From		Model	Data From		Data From	
Size	Fitted	FLMP_W	WTAV	FLMP_W	WTAV	Fitted	FLMP	WTAV	FLMP	WTAV	Fitted	FLMP	LIM_T	FLMP	LIM_T
50	FLMP_W	0.025	0.0424	-40.67	-41.68	FLMP	0.0252	0.0793	-32.63	-55.58	FLMP	0.0252	0.0462	-32.63	-40.47
	% win	100	18	98	6	% win	100	0	99	0	% win	100	6	100	0
	WTAV	0.0899	0.0369	-54.01	-37.51	WTAV	0.1027	0.0369	-61.14	-37.51	LIM_T	0.0888	0.0361	-57	-32.24
	% win	0	82	2	94	% win	0	100	1	100	% win	0	94	0	100
24	FLMP_W	0.036	0.0544	-30.44	-32.17	FLMP	0.0373	0.0865	-26.41	-38.32	FLMP	0.0373	0.0573	-26.41	-30.34
	% win	99	39	99	17	% win	100	2	100	1	% win	99	22	99	2
	WTAV	0.0952	0.0533	-36.8	-30.65	WTAV	0.1079	0.0533	-40.04	-30.65	LIM_T	0.0955	0.0526	-36.77	-26.8
	% win	1	61	1	83	% win	0	98	0	99	% win	1	78	1	98
12	FLMP_W	0.0518	0.0714	-23.51	-24.83	FLMP	0.0511	0.0967	-20.67	-27.42	FLMP	0.0511	0.073	-20.67	-23.3
	% win	100	61	100	32	% win	99	10	99	7	% win	100	52	97	10
	WTAV	0.1064	0.0747	-26.46	-24.38	WTAV	0.1183	0.0747	-27.94	-24.38	LIM_T	0.1061	0.0753	-25.34	-21.78
	% win	0	39	0	68	% win	10	90	1	93	% win	0	48	3	90

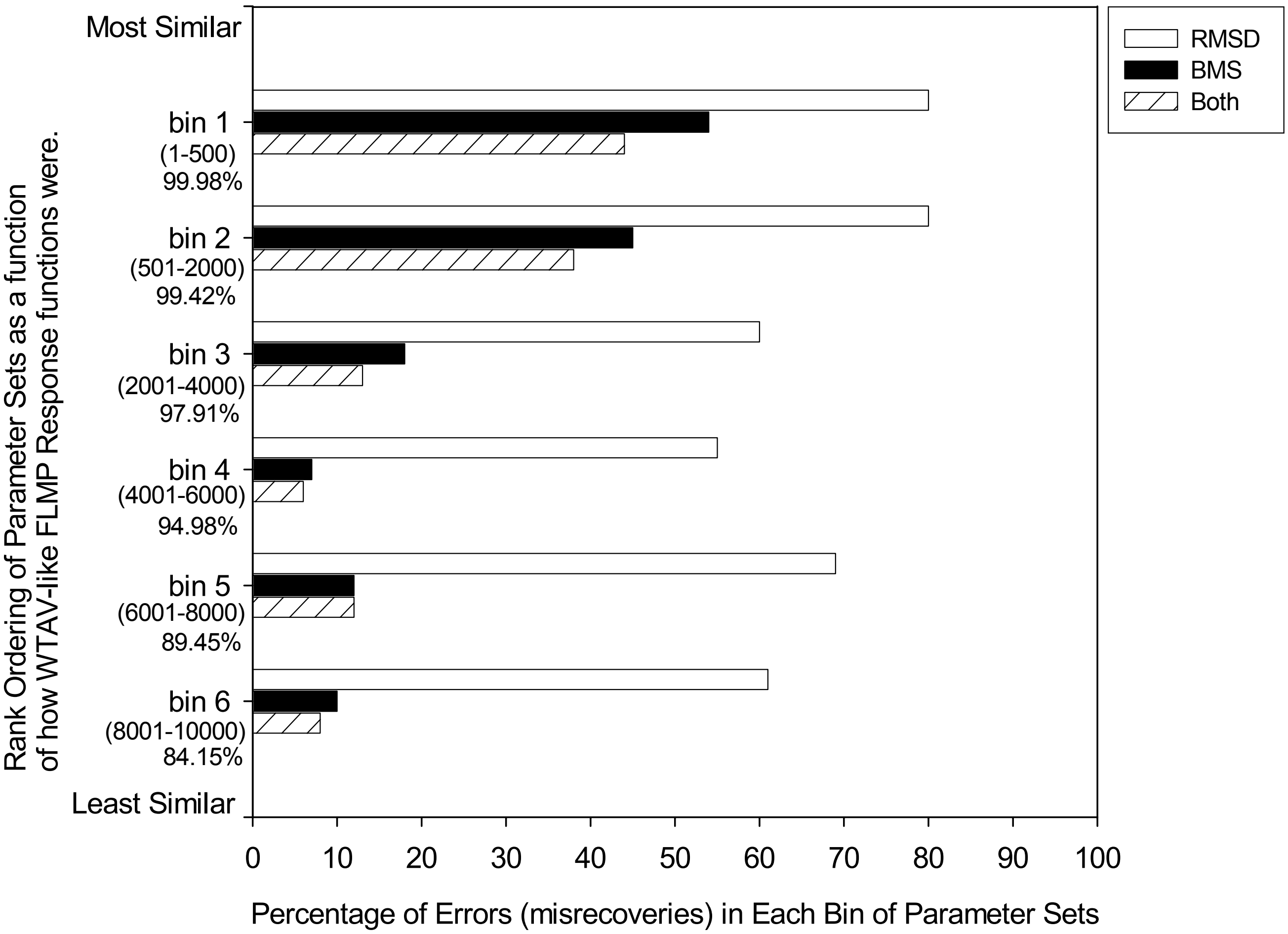
Figure Captions

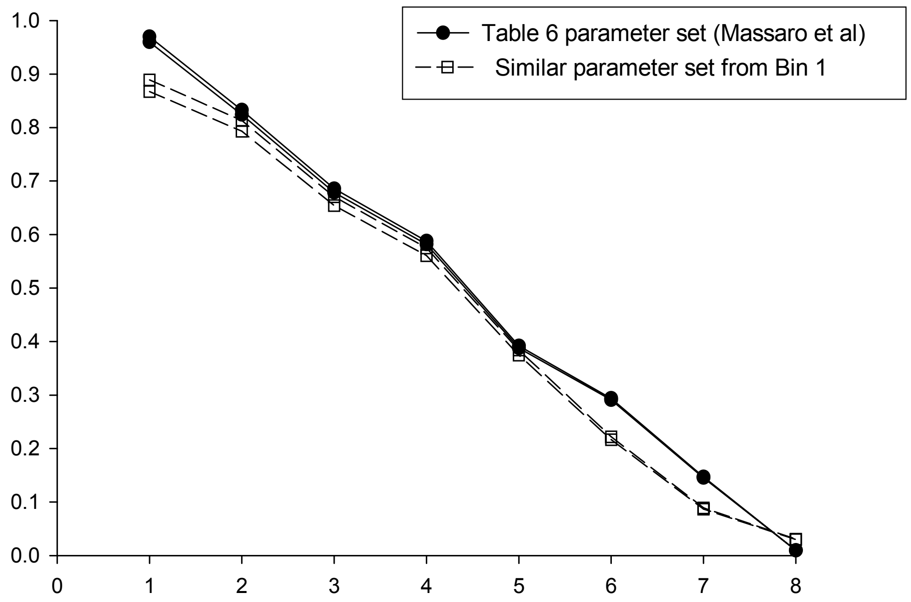
Figure 1. Top graph is a plot of two FLMP response functions in a 2x8 experimental design. One set of functions was produced using parameter set 4 from Table 2 of Massaro et al. The other was produced using the parameter set from the current simulation that most closely matched parameter set 4 (see Appendix B for parameter values) In the bottom graph, the percentage of misrecoveries by BMS, RMSD, and those common to both, are plotted for each of the six bins of parameter sets. The percentages along the y axis are the mean percent variance accounted for when WTAV was fitted to the 100 FLMP response patterns in each bin.

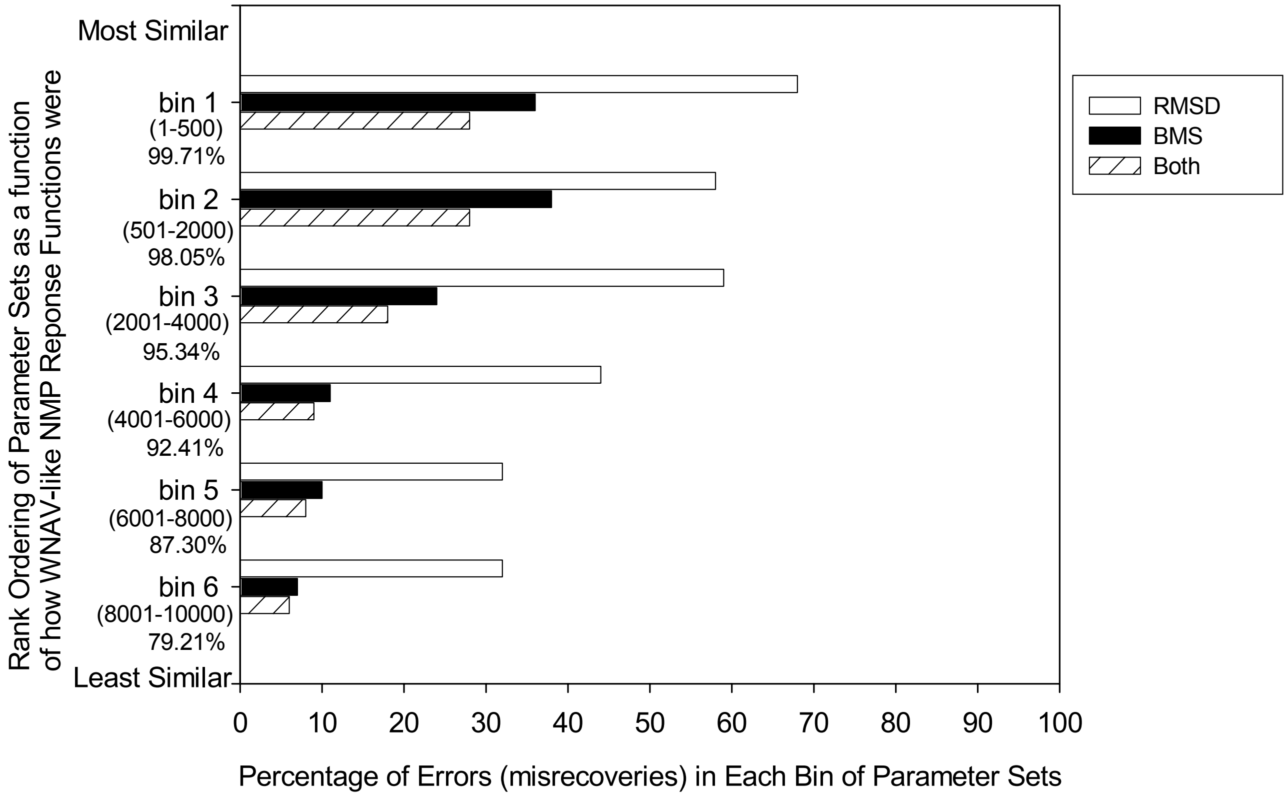
Figure 2.Top graph is a plot of two NMP response functions in a 2x8 experimental design. One set of functions was produced using the parameter set Massaro et al used in the simulation in their Table 6. The other was produced using the parameter set from the current simulation that most closely matched their's. In the bottom graph, the percentage of misrecoveries by BMS, RMSD, and those common to both are plotted for each of the six bins of parameter sets. The percentages along the y axis are the mean percent variance accounted for when WNAV was fitted to the 100 NMP response patterns in each bin.

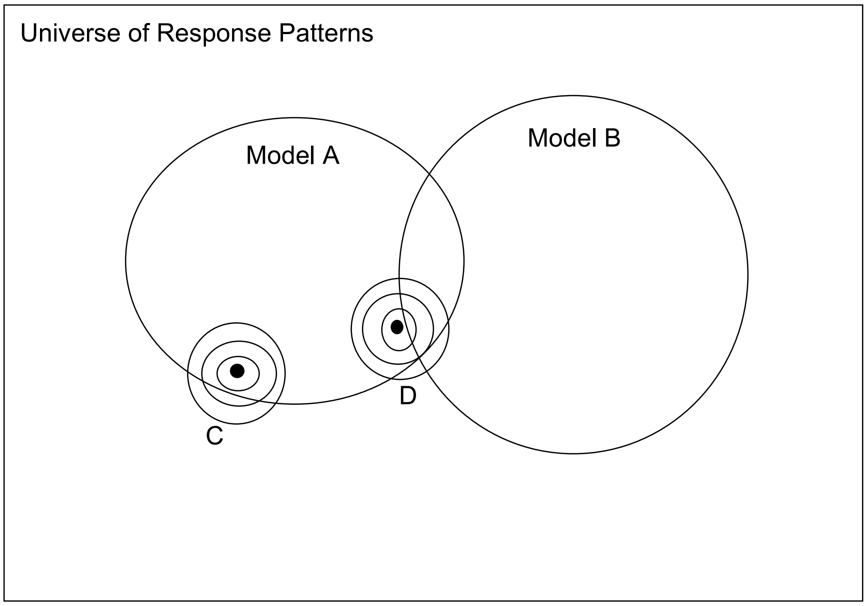
Figure 3. Illustration of how model recovery is influenced by data and sample size. Two models, A and B, occupy a region of the space of all possible response patterns (rectangled area). Two points represent different response patterns, with the concentric circles denoting the amount of sampling error in the data (larger rings indicate more error).


Parameter Set 4 from Massaro et al
Similar Parameter Set from Bin 2
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0.0
0
1
2
3
4
5
6
7
8


Most Similar
bin 1
(1-500)
99.98%
bin 2
(501-2000)
99.42%
bin 3
(2001-4000)
97.91%
bin 4
(4001-6000)
94.98%
bin 5
(6001-8000)
89.45%
bin 6
(8001-10000)
84.15%
Least Similar
Rank Ordering of Parameter Sets as a function of how WTAV-like FLMP Response functions were.
RMSD
BMS
Both
0
10
20
30
40
50
60
70
80
90
100
Percentage of Errors (misrecoveries) in Each Bin of Parameter Sets


1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0.0
0
1
2
3
4
5
6
7
8
Table 6 parameter set (Massaro et al)
Similar parameter set from Bin 1


Most Similar
bin 1
(1-500)
99.71%
bin 2
(501-2000)
98.05%
bin 3
(2001-4000)
95.34%
bin 4
(4001-6000)
92.41%
bin 5
(6001-8000)
87.30%
bin 6
(8001-10000)
79.21%
Least Similar
Rank Ordering of Parameter Sets as a function of how WNAV-like NMP Reponse Functions were
0
10
20
30
40
50
60
70
80
90
100
Percentage of Errors (misrecoveries) in Each Bin of Parameter Sets
RMSD
BMS
Both


Universe of Response Patterns
Model A
Model B
C
D