



Limitations of “Limitations of Bayesian Leave-one-out Cross-Validation for Model Selection”

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Abstract

In an earlier article in this journal, Gronau and Wagenmakers (2018) discuss some problems with leave-one-out cross-validation (LOO) for Bayesian model selection. However, the variant of LOO that Gronau and Wagenmakers discuss is at odds with a long literature on how to use LOO well. In this discussion, we discuss the use of LOO in practical data analysis, from the perspective that we need to abandon the idea that there is a device that will produce a single-number decision rule.

Keywords M-open · M-closed · Reality · Statistical convenience · Principle of complexity

What was not but Could be if

The most important aspect of communicating statistical method to a new audience is to carefully and accurately sketch out the types of problems where it is applicable. As people who think leave-one-out cross-validation (LOO-CV or LOO for short) is a good method for model comparison and model criticism, we were pleased to discover that Gronau and Wagenmakers (2018, henceforth GW) chose to write a paper aimed at explaining the nuances of LOO methods to a psychology audience. Unfortunately, we do not

think the criticisms and discussions provided in their paper are so relevant to LOO as we understand it. The variant of LOO that GW discuss is at odds with a long literature on how to use LOO well; they focus on pathologizing a known and essentially unimportant property of the method, and they fail to discuss the most common issues that arise when using LOO for a real statistical analysis. In this discussion, we try to discuss a number of concerns that everyone needs to think about before using LOO, reinterpret GW’s examples, and try to explain the benefits of allowing for epistemological uncertainty when performing model selection.

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We Need to Abandon the Idea that there is a Device that Will Produce a Single-Number Decision Rule

The most pernicious idea in statistics is the idea that we can produce a single-number summary of any data set and this will be enough to make a decision. This view is perpetuated by GW’s paper, which says that the only way that LOO can provide evidence for choosing a single model is for the pseudo-Bayes factor to grow without bound (or, equivalently, that the model weight approaches 1) as sample size increases. This is not a good way to use LOO and fundamentally misjudges both its potential and its limitations as a tool for model selection and model criticism.

For a Bayesian model with n data points $y_i \sim p(y|\theta)$ and parameters $\theta \sim p(\theta)$, LOO provides an estimate of the

expected log posterior predictive distribution (Gelman et al. 2014),

$$\begin{aligned} & \mathbb{E}_{\bar{y}} \left(\log \left(\int p(\bar{y}|\theta) p(\theta|y_{\text{all}}) d\theta \right) \right) \\ & \approx \frac{1}{n} \sum_{i=1}^n \log \left(\int p(y_i|\theta) p(\theta|y_{-i}) d\theta \right), \end{aligned}$$

where the expectation is taken with respect to new data, y_{all} is all n observed data points, and y_{-i} is all data points *except* the i th one.

There are two things to note here. Firstly, the computed LOO score is an empirical approximation to the expectation that we actually want to compute. This means that we must *never* consider it as a single number without also considering its inherent randomness. Considering both the LOO estimate and its empirical variance has been recommended since at least (Breiman et al. 1984, chapter 11). The model with higher estimated performance should not be automatically selected; rather, uncertainty in the estimates should be considered (see, also Vehtari et al. 2017).

Since GW cite Vehtari et al. (2017) and Yao et al. (2018), we wish to emphasize that those papers of ours explicitly recommend computing the uncertainties due to not knowing the future data, and do not recommend LOO weights as used in the experiments by GW. Hence, the claimed “limitations of Bayesian leave-one-out cross-validation” from GW do *not* apply to the version of Bayesian leave-one-out cross-validation that we actually recommend.

At the very least, we recommend that GW replace their pseudo-Bayes factors with the pseudo-BMA+ weights of Yao et al. (2018), which assume a normal distribution of the empirical mean of the log predictive densities and makes the appropriate modification to the model weights. Although this does not make a difference in their very specialized examples, we worry that people would see GW’s work and assume that the method they describe is a “best practice” use of LOO rather than a version that happens to work in these extremely specialized situations. In “If You Use LOO Better, You Get More Enlightening Cases of Failure” we suggest that stacking (Yao et al. 2018) is a much better way to define model weights as it uses the LOO principle directly rather than simply defining ad hoc model weights.

The second thing to note is that the difference of LOO scores of two models can be decomposed into a sum of differences of log predictive densities for each left-out data point. This means that a two-way model comparison with LOO has more in common with a paired difference test than something like a Bayes factor. Vehtari et al. (2017) argue that plots of these individual differences can be useful, especially as an exploratory tool, and can bring attention to cases where the preferred model is affected

by some unmodeled structure in the data. Considering this, decomposition does not lead to a formal statistical test, but it can be useful when the aim of the analysis is to understand the data at hand rather than reach a “discovery” threshold.

This second property is extremely useful for applied work as it can be used to identify problems with model fit. As such, we recommend plotting the differences between the LOO log predictive densities even if LOO is not being used to perform model selection. Similarly, the \hat{k} diagnostic produced by the `loo` package in R can be used as a generalized measure of leverage: if an observation has a \hat{k} value larger than 0.7, there is a very large difference between the predictive distribution that is conditioned on that observation and one that is not. These observations should be investigated as potentially problematic. Gabry et al. (2018) has a detailed discussion of how plots generated in from LOO cross-validation are a useful part of the applied Bayesian work flow.

Hence, we strongly feel that the question raised by GW is not “Should we use LOO at all?,” but rather, “Should we make model selection decisions based on LOO?”

Closed Thinking in an M -Open World: Some Tensions Between Reality and Statistical Convenience

Cross-validation and LOO have many limitations. Gronau and Wagenmakers (2018) focus on a variation of one known limitation: “in the idealized case where there exists a data set of infinite size that is perfectly consistent with the simple model M_S , LOO will nevertheless fail to strongly endorse M_S .”

The strongest assumption in GW’s paper is that the true model is one of the models under consideration, which Bernardo and Smith (1994) call the M -closed case. Cross-validation is usually recommended for the M -open case, where the true model is not included in the set of models compared. In real life, we are just about always in the M -open scenario; like nearly all models, M -closed is a simplification of reality. While it can sometimes be a useful simplification, we should never lose sight of the fact that methods that rely strongly on the M -closed assumption may be irretrievably biased.

Assuming that a model selection problem is M -closed allows the scientist to ignore epistemic uncertainty about their understanding of the process, and focus entirely on different models of aleatory uncertainty (the inherent randomness in the system). The extent to which this is a serviceable assumption will depend strongly on context of. With their exclusively focus on M -closed model selection, GW have severely limited the scope of their paper compared to the real applicability of LOO methods.

When we estimate the predictive performance of a model for future data that are not yet available, we need to integrate over the true future data distribution $p_T(y)$, which is typically unknown. Vehtari and Ojanen (2012) provide an extensive survey of alternative scenarios including also different cases of conditional modeling $p_T(y|x)$, with stochastic, fixed or deterministic x . In the M -closed case, we know that one of the models is $p_T(y)$, but we do not know which one. In that case, it makes sense to model $p_T(y)$ as a posterior weighted average of all models under consideration, that is, replace $p_T(y)$ with a Bayesian model average (BMA) as proposed by San Martini and Spezzaferri (1984). This approach has the same model-selection-consistent behavior as the Bayes factor. Thus, if the assumption is that one of the models is true, the BMA reference model approach could be used, although asymptotic model selection consistency does not guarantee good finite sample performance, and BMA may have worse performance than LOO for small-to-moderate sample sizes even if one of the models is true.

On the other hand, in an M -open scenario, the BMA weights will still asymptotically converge to a single model (the one closest in Kullback-Leibler divergence to the data generating model); however, this will no longer be the truth. In this case, it is harder to make a case for using marginal likelihood-based weights.

Instead of using any explicit model or model averaging, LOO makes the assumption that future data can be drawn exchangeably from the same distribution as the observed data. This conditional independence structure is *vital* to the success of LOO methods as it allows us to re-use observed data as pseudo-Monte Carlo draws from the future data distribution (Bernardo and Smith 1994).

Cross-validation methods can have high variance when used on data sets that do not fully represent the data distribution. In such cases, some modeling assumptions about the future data distribution would be useful, in the same way that we use poststratification to adjust for extrapolations in sampling.

If you use LOO Better, you get more Enlightening Cases of Failure

The main claims in GW is that when LOO is used for model selection (in a way that is not in line with best practice), it is unable to select the true model and the LOO model weights are prior-dependent. Their claims are true to some extent, but we do not feel they do a good job at representing the behavior of LOO or are representative of how we would recommend these tools are used.

The inconsistency of LOO model weights reported by GW for the first two examples is entirely an artifact of

their poor application of the LOO principle. To wit, if you want to construct LOO model weights, they should be based on the weighted predictive distribution. Yao et al. (2018) recommend using stacking weights, which choose the model weights that give the best expected log predictive density

$$\max_{\substack{w: w_k \geq 0, \\ \sum_{k=1}^K w_k = 1}} \frac{1}{n} \sum_{i=1}^n \log \left(\sum_{k=1}^K w_k p(y_i | y_{-i}, M_k) \right),$$

where M_k are the models under consideration. Yao et al. (2018) demonstrate with several M -open examples better performance of stacking compared to pseudo-BMA and BMA.

Looking at the examples in GW, it turns out that stacking can also work well in the M -closed case.

In the beta-Bernoulli examples, the stacking weights can be derived analytically. The weights are 1 for H_0 and 0 for H_1 . To see this, we note that model H_0 makes a perfect prediction, so $p(y_i | y_{-i}, H_0) = 1$ and model H_1 also has a constant predictive performance $p(y_i | y_{-i}, H_1) = (a + n - 1)/(a + n - 1 + b) = c < 1$. The stacking objective function is $n \log(w_1 + w_2 c)$ which is maximized at $w_1 = 1$ and $w_2 = 0$. This is independent of both the prior distribution and sample size n . Moreover, when model H_0 does better than model H_1 in every data point (example 1) or every pair of data point consistently, the stacking weight corresponding to H_0 has to be 1.

The lack of dependence on n may look suspicious. But intuitively when each data point (example 1) or each pair of data point (example 2) uniformly supports H_0 more than H_1 , it does not require $n \rightarrow \infty$ to conclude that H_0 dominates H_1 . This conclusion instead follows from the cross-validation assumption that the current data is representative of the future data.

In examples 2 and 3 and idealized data, stacking with symmetric-leave-two-out would also converge faster due to the contrived nature of the data generation process. This is not to claim that stacking weights will generally give asymptotically consistent model selection in M -closed problems, just that it does for these two cases due to the simple data chosen by GW.

The examples that GW chose are so specialized that the empirical mean of the log predictive LOO densities has zero variance. This is a consequence of the data being both balanced and perfectly consistent with one of the hypotheses as well as the priors in examples 2 and 3 being centered on the mean of the data. These three properties imply that $p(y_i | y_{-i})$ is constant for all i , and hence Pseudo-BMA+ reduces to Pseudo-BMA used by GW.

The zero variance of the log predictive densities and the lack of dependence on n also demonstrates a clear problem with the cross-validation assumption that we can use subsets

of existing data to approximate the distribution of future data. The problem is that, for any finite n , the observed data sets in these examples are very strange (either all 1s or all $\{0, 1\}$ pairs) with zero variance. The LOO assumption will then assume that *all* future data is also like this with zero variance, which is problematic. Because even though we get consistency of the stacking weights in this case, if H_0 was $\theta = 1 - \epsilon$ for some small $\epsilon > 0$, there would be a very high chance of observing a data set that was all 1s even though the future data would include some values that are 0. This means that LOO will be biased when the true model lies very close to an edge case.

The experiments in GW could be modified to better illustrate the limitation of LOO by considering cases where the complex model is correct. Consider the following alternative experiments where the more complex model is true are as follows:

- In example 1, let the true $\theta_T = 1 - \epsilon$, but for the simpler model keep $\theta_0 = 1$.
- In example 2, let the true $\theta_T = \frac{1}{2} + \epsilon$, but for the simpler model keep $\theta_0 = \frac{1}{2}$.
- In example 3, let the true $\theta_T = \epsilon$, but for the simpler model keep $\theta_0 = 0$.

If we choose ϵ very small (but within the limits of the floating point accuracy for the experiments), we should see the same weights as in the original experiments as long as we observe the same data, and only when we occasionally observe one extra 0 in example 1, one extra 1 in example 2, or extra positive value in 3 we would see differences.

One way to illustrate the limitations of LOO (and cross-validation and information criteria in general), would be to compare the behavior of the LOO stacking weights as ϵ moves from very small to much larger, plotting how large ϵ needs to be before we see that the more complex model is strongly favored.

We know that we can get the standard deviation of the parameter posterior smaller than ϵ with $\mathcal{O}(\epsilon^{-2})$ data points. However, because we need enough data to be confident about the distribution of future data, we expect to require far more data points than that for the stacking weights to confidently select the correct model in the above ϵ -experiment. See the demonstration in Vehtari (2018a) and related results by Wang and Gelman (2014).

This is the price we pay for not trusting any model, and thus not getting benefits of reduced variance through the proper modeling of the future data distribution! This variability makes cross-validation bad for model selection when the differences between the models are small, and it just gets worse in case of a large number of models with similar true performance (see, e.g., Piironen and Vehtari 2017). Even with just two models to compare, cross-validation has also a limitation that the simple variance estimate tend to be optimistic (Bengio and Grandvalet 2004).

It Really Helps to Look at the Estimates

GW demonstrate that LOO is not consistent for model selection by showing three examples where the pseudo-BMA weight for the true model does not approach one as more data arrives. We feel there is a more nuanced way to look at this, namely the models that GW compare are nested, which means that the more complex model contains the simpler model. This means that the posterior predictive distribution from the more complex model will eventually be the same as the posterior predictive distribution constructed from the simpler model if the data is consistent with the simpler model. One way to interpret this phenomenon is that the two hypotheses are no longer distinct after we see enough data. Viewed this way, the prior dependence that GW demonstrated was just the prior choice slightly affecting the speed at which the two hypotheses are converging to each other.

A quick look at the parameter estimates in any of the three GW models shows that in each case the posterior for θ is rapidly converging on the specific value of θ (respectively 1, 1/2, and 0) that defines the null model. For concreteness, let's consider example 2, where the difference between the models is that the null model sets $\theta = 1/2$ while the alternative model uses $\theta \sim \mathcal{B}(a, b)$. We do not see a large difference between reporting " H_0 best models the data" or " H_1 best models the data and $\theta \approx 1/2$."

Our recommendation is that if the LOO comparison taking into account the uncertainties says that there is no clear winner, then neither of the models should be selected and instead model averaging or expansion should be used. If two models are being compared and they give similar predictions, then it should not matter which one is used. Unlike GW, we typically prefer to use the more complex one.

There is some tension between our preference for predictive models and GW's implied interest in discriminative ones. GW's preference for parsimony appears to come in part from their desire to make statements like "x is not associated with y." Whereas, when we use our models to make statements about future data, we would prefer the more complex model in the nested model in order to be certain that uncertainties are not underestimated.

We do not recommend using the more complex model thoughtlessly. We require strict model checking and calibration of the complex model, and then proceed using projective variable selection (Goutis and Robert 1998; Dupuis and Robert 2003; Piironen and Vehtari 2017; Piironen et al. 2018; Vehtari 2018b) to decide if some parts of the model can be dropped safely. Overfitting can be viewed as the use of too flat a prior distribution—but we and others use flat, or relatively flat, priors all the time, so overfitting is a real and persistent concern. Beyond concerns of stability in estimation and prediction, simpler models

can also be motivated by a desire to reduce measurement costs in the future or to make it easier to explain the model for application experts, but this decision task then includes implicit or explicit costs for the measurements or complexity of the explanation. This final step is not considered by GW, but we consider it to be a *vital* part of any work flow that attempts model selection using LOO.

To clarify, we recommend using the encompassing model if (a) the model is being used to make predictions, (b) the encompassing model has passed model checking, and (c) the inference has passed diagnostic checks. From a Bayesian perspective, a larger and smaller model can be considered as two different priors on a single larger set of distributions, and there is no reason why the larger, less restrictive model, should be associated with flat priors for the additional parameters, even though this has often been the tradition in model comparison.

Bayesian theory says that we should integrate over the uncertainties. The encompassing model includes the sub-models, and if the encompassing model has passed model checking, then the correct thing is to include all the models and integrate over the uncertainties. For the encompassing model to pass model checks, good priors will typically be required that appropriately penalize the complexity of the model (Simpson et al. 2017). If the models have similar cross-validation performance, the encompassing model is likely to have thicker tails of the predictive distribution, meaning it more cautious about rare events. We think this is good.

Here are some reasons why it is common in practice to favor more parsimonious models:

- The maximum likelihood inference is common and it does not work well with more complex models. Favoring the simpler models is a kind of regularization.
- Bad model misspecification. Bayesian inference can perform poorly if the model has major problems, and with complex models there are more possibilities for misspecifying the model and the misspecification even in one part can have strange effects in other parts.
- Bad priors. Actually, priors and models are inseparable, so this is kind of same as the previous one. It is more difficult to choose good priors for more complex models, because it is difficult for humans to think about high-dimensional parameters and how they affect the predictions. Favoring the simpler models can avoid the need to think harder about priors.

We agree with Neal (1996), who wrote:

Sometimes a simple model will outperform a more complex model [...] Nevertheless, I believe that deliberately limiting the complexity of the model is not fruitful when the problem is evidently complex. Instead, if a simple model is found that outperforms some particular complex model, the appropriate response is to define a different complex model that

captures whatever aspect of the problem led to the simple model performing well.

What Else can go Wrong?

We cannot let this opportunity pass to discuss the shortcoming of LOO methods without talking about the assumptions of LOO and how they can be violated. We have already mentioned that the assumption that the existing data is a good proxy for future observations can lead to problems. This is particularly true when data sets contain rare events. But, there is one other major assumption that LOO methods (and cross-validation methods generally) make: the assumption of conditional exchangeability between observations (or groups of observations).

If the distribution of the observations, once conditioned on all of the model parameters, is not exchangeable, LOO methods will fail. This is because the fundamental idea of LOO is that $\frac{1}{n} \sum_{i=1}^n \log(\int p(y_i|\theta)p(\theta|y_{-i}) d\theta)$ is an unbiased approximation to the true expectation over unseen data $E_{\tilde{y}}(\log(\int p(\tilde{y}|\theta)p(\theta|_{\text{all}}) d\theta))$. This only holds for models where observations are conditionally exchangeable.

This means that we need to be particularly careful when using cross-validation methods to assess models with temporal, spatial, or multilevel structure in the observations. Typically, we can alleviate this problem using a cross-validation scheme that has been carefully designed to ensure that the blocks are (approximately) exchangeable (see, e.g., Roberts et al. 2017, Bürkner et al. 2018). Some evidence of correlation may be visible by plotting the difference in log predictive densities as a function of time or space (much in the same way we can check linear regression residuals for serial autocorrelation), as well as checking the individual models for uncorrelated residuals. Vivar and Ferreira (2009) present a visual check for cross-validation predictions.

To summarize our claim, we say that cross-validation methods will fail in all their tasks if the current data is not a good representation of all possible future data or if the observations are not exchangeable conditional on the model parameters. While stacking weights will consistently identify the correct predictive distribution, it will not necessarily choose the simplest model when multiple models asymptotically give the same predictive distribution. In the case of nested models, parsimony can be restored using projective prediction methods; however, for non-nested models, we do not know of a general solution.

Can you do Open Science with *M*-Closed Tools?

One of the great joys of writing a discussion is that we can pose a very difficult question that we have no real intention of answering. The question that is well worth pondering is

the extent to which our chosen statistical tools influence how scientific decisions are made. And, it is relevant in this context because of a key difference between model selection tools based on LOO and tools based on marginal likelihoods is what happens when none of the models could reasonably generate the data.

In this context, marginal likelihood-based model selection tools will, as the amount of data increases, choose the model that best represents the data, even if it does not represent the data particularly well. LOO-based methods, on the other hand, are quite comfortable expressing that they can not determine a single model that should be selected. To put it more bluntly, marginal likelihood will always confidently select the wrong model, while LOO is able to express that no one model is correct.

We leave it for each individual statistician to work out how the shortcomings of marginal likelihood-based model selection balance with the shortcomings of cross-validation methods. There is no simple answer.

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