Discussion of Brock and Durlauf's
"Economic Growth and Reality"

by

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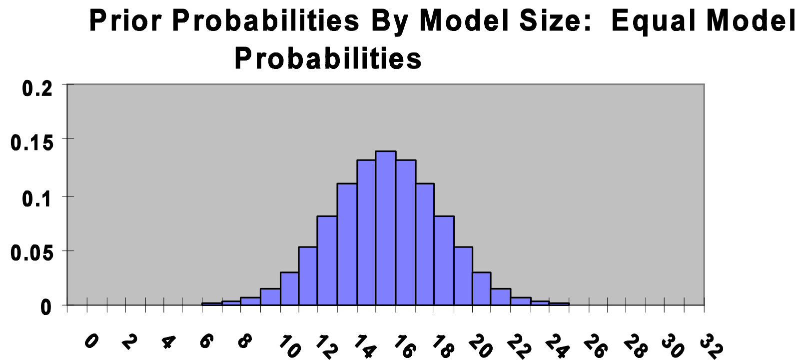
This is a nice paper that summarizes some of the recent research on Bayesian Model Averaging (BMA). Given the very constraining space restrictions imposed upon me, I will simply suggest a few ways to improve the paper in this comments. The paper makes a number of important points. One of them is that the empirics of growth faces three key problems: *model uncertainty*, *parameter uncertainty* and *endogeneity.* The paper argues that theory uncertainty can be dealt with using BMA methods. The key equations of the paper are Eq. (17), (18) and (19). The interpretation is the following. Suppose you are interested in the distribution of the partial derivative of the growth rate with respect to variable *z,* β_z . Let each set of every possible combination of explanatory variables be called a "model". Conditional on each model, there is a distribution of β_z , for a given data set. Equation (17) says that the posterior distribution of β_z is a weighted average of all these individual distributions, where the weights are proportional to the likelihoods of the models. Eq. (18) says that the mean of this distribution is the weighted average of the OLS estimates of all these models, where the weights are proportional the likelihoods. Eq. (19) makes a similar claim about the variance.

An initial important point of this paper is that weights are proportional to the likelihoods. In fact, this may be driving the first key empirical result of the paper, namely that the Easterly and Levine (EL) regression of growth on ethnolinguistic fractionalization (ELF) is "robuts" to BMA analysis. It is important to remember that models with more explanatory variables have larger likelihoods. It is also important to remember that Brock and Durlauf perform BMA analysis by combining the explanatory variables of the Easterly and Levine paper in all possible ways: sets of one RHS variables, sets of two, sets of three, and, eventually, one set with all the RHS variables at the same time. This last model is, at the same time, the one ran by EL and the largest model ran by Brock and Durlauf (and, therefore, the one that is likely to have the largest likelihood...and gets the largest weight). Hence, it is not surprising that the weighted average of all the models is very similar to the EL since most of the weight of the average goes to the EL specification...by construction. In other words, the result that the EL regression (Column 1 in Table 2) is "robust" to the BMA analysis because the weighted average of models (Column 2) is virtually identical, is likely to be an artifact of the weights used. At this point, I should confess that these are also the weights I used in my (1997) paper (where eqs (17) and (18) appear exactly). However, in that paper, I only averaged regressions with a fixed set of explanatory variables so I did not have the problem that I am pointing out here. Doppelhofer, Miller and Sala-i-Martin (2000) derive an alternative weighting scheme. The posterior density of model M_m is proportional to the likelihood (sum of squared of residuals or $SSE_m^{-T/2}$), multiplied by $T^{-k_m/2}$, where *T* is the number of observations and k_m is the number of explanatory variables in model *m:*

$$\mu(M_m|D) = \frac{\mu(M_m)\, T^{-k_m/2} SSE_m^{-T/2}}{\sum_{i=1}^{2^K} \mu(M_i)\, T^{-k_i/2} SSE_i^{-T/2}} \qquad \textbf{(1)}$$

Note that this weighting scheme penalizes larger models. It would be interesting to see whether Column (1) still looks very much like Column (2) when these alternative weights are used.

A second important assumption is the prior that allows them to eliminate the $\mu(M_m)$ from Eq. (16) in order to derive Eq. (17). They use the prior that "*all models are equally likely*". Imagine



that we had 32 possible right hand side variables. If we think all models are equally likely, then the prior distribution of model sizes is given by Figure 1. The average model size is 16. Instead, if we had 10 explanatory variables, then the implicitly assumption would be that the average model size of the prior distribution of cross-country regressions is 5. The problem with this is that Brock and Durlauf propose that, when we analyze (or discuss) a paper like EL, we take the key regression in that paper and we do BMA analysis with it. If we take this proposal literally, we would implicitly be assuming that the average model size of "the growth regression" is 5 when the original authors had 10 variables in their paper and we would be believing that the average size of the model is 16 when the original paper had 32 variables. On top of being arbitrary, this does not make sense: the prior model size should be invariant to the paper one is discussing! One way to solve this problem would be to follow Doppelhofer, Miller and Sala-i-Martin (2000) and specify our model prior probabilities by choosing a prior mean model size, κ, with each variable having a prior probability κ/K of being included, independent of the inclusion of any other variables, where K is total number of potential regressors. Equal probability for each possible model is the special case in which $\kappa = K/2$. Notice that the prior distribution of model sizes would be invariant to the paper analyzed. Moreover, one could do robustness checks of this prior by redoing the BMA exercise (or better yet, the BACE or Bayesian Averaging of Classical Estimates) for a different values of

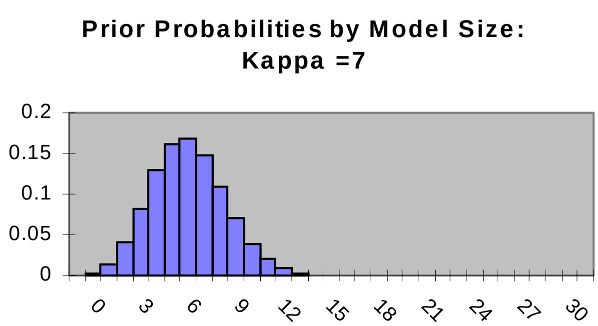


Figure 2

κ .

My third comment concerns the treatment of *parameter uncertainty.* I agree with the authors that this problem is analogous to that of *theory uncertainty*. But, if so, why do they propose a different solution? If we think that Africa needs a different slope for variable *z*, then, all we need to do is to construct a new variable (*z* times one for countries in Africa and z times zero otherwise) and put this new variable in the pool of potential variables to be included in the BMA analysis. Instead of columns 3 through 6, Table 2, should include one more row were the distribution of the $\beta_{j,\mathcal{L}_2}$ for this new variable is presented, as a regular additional variable subject to theory uncertainty.

When we think of parameter uncertainty as another form of theory uncertainty, an additional problem comes to mind. Why does one think that Africa needs its own slope? Why don't we have a special slope for Christian countries? Or hot countries? Or small countries? Of course we do not know (we do not have a theory; or we can have many open-ended theories that would call for a special slope for each of these groups of countries). However, in the spirit of Durlauf-Johnson (1995), shouldn't we then perform BMA or BACE analysis for each partition countries? How would we go about doing that?

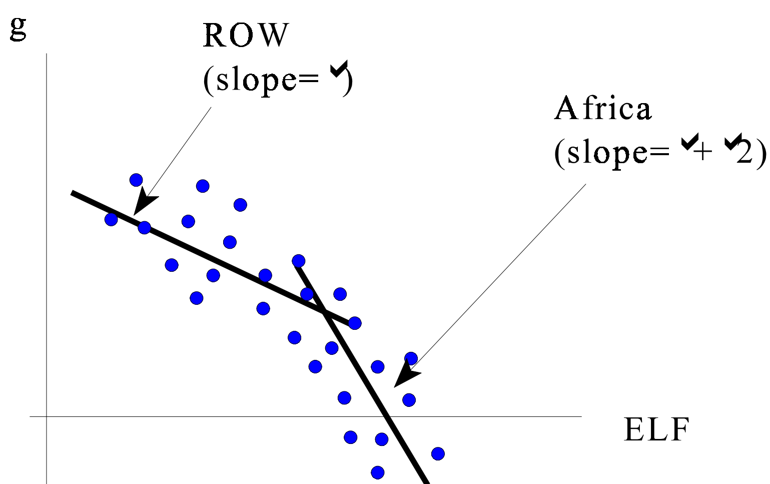


Figure 3

A perhaps related question is that of non-linearities. The paper does not allow for non-linearities. It is clear that African countries have both a lower average growth and a larger ELF. The conditional data, therefore, might look like Figure 3. If we think of the implications of Figure 3, we will arrive at the conclusion that if, somehow, we reduce ELF for African countries (and I do not know how, but if the authors think that ELF is a policy variable, then it must be possible to change it through some policy), then Africa will conditionally grow faster than the rest of the world forever (that is, we would move the African data points to the left along the steeper regression line). Since we do not have a theory of ELF, we do not know whether this is sensible or not. Alternatively, we could think that the partial relation between growth and ELF looks like figure 4. In fact, the data points in figures 3 and 4 are exactly the same. The only thing that changes is the functional form of the regression curve. Under this interpretation, if Africa manages to get the same ELF as the rest of the world, its growth rate will also be similar. Hence, the economic implications of a "*separate slope for Africa*" are very different from those of a "*non-linear relationship*". It would have been interesting to incorporate non-linearities in the analysis.

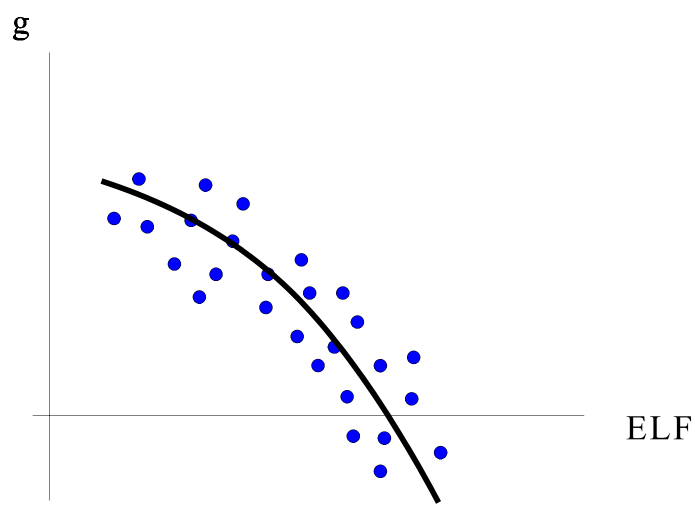


Figure 4

Finally, I think that the claim that growth economists have not dealt with *parameter uncertainty* is not quite true. In fact, *parameter uncertainty* is a particular form of what economists usually label as "*interaction terms*". For example, suppose that a claim is made that the partial derivative of growth with respect to z depends on variable y: $\frac{\partial g_i}{\partial z_j} = \beta_z + \beta_{z,y_j} \cdot y_j$.

The way to test this claim would be to run a regression of growth with z as an explanatory variable with an additional variable which would be a country-by-country product of z times y. That is, we should introduce "*interaction terms*". It should be clear that "*parameter uncertainty*" is nothing but an interaction term...when variable y is simply a *regional dummy!* (in the case of this paper, an African dummy). To the extent that economic growth researchers have introduced interaction terms, therefore, they have allowed for parameter heterogeneity.

Let me finish with two sources of disappointment about this, otherwise, excellent paper. First, I think that the paper is not really about the empirics of economic growth. ALL empirical analysis is subject to the problems discussed in this paper, especially those that are forced to use small data sets. In this sense, the title, although cute, is highly misleading and, to the extent that leads future young researchers away from economic growth analysis, potentially damaging. I think that a more appropriate title would be "*small sample econometrics*" because the problems discussed are common to all empirical analysis with small samples (which include ALL cross-country analysis in any field). After all, if we had a huge data set with zillions of observations, we could simply throw in all potential variables, with particular slopes for each potential set of countries, with all potential non-linearities, and so on, and the data would tell us which coefficients are zero and which are not. The fact that we have more potential variables than we have countries prevents us from following this strategy and this is where the problem starts. Notice, however, that this is a problem of small samples, not a problem of growth econometrics.

Second, even though endogeneity was discussed and introduced as a very important problem in the early parts of the paper, I was disappointed when I saw that nothing was done about it. Given the great reputation of the authors, when I started reading the paper I was excited at the prospect of a potential solution, perhaps along the lines of BMA. But this did not happen, and this was disillusioning.