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USING STRONG INFERENCE TO FALSIFY DIFFERENTIAL EQUATION MODELS OF SUGAR MAPLE HEIGHT GROWTH—REJOINDER

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Abstract.We provide here our response to Zeide (2010) comment (Zeide, B. 2010. Using strong inference to falsify differential equation models of sugar maple height growth—Discussion. MCFNS 2(1):12-14) on our earlier article of the same title.

Keywords: cohort dynamics, Lyapunov exponent, tree questioning, chaotic models

Zeide (2010) concludes his discussion by stating:

> "With regard to the differential equation models of height growth sugar maple, I think that we need one comprehensive model describing height growth as well as that of other tree dimensions for sugar maple and any other species. The specifics of height or diameter, of sugar or red maple should be reflected by parameters and not the model form."

While we agree with the concluding sentence:

> "The specifics of height or diameter, of sugar or red maple should be reflected by parameters and not the model form."

We disagree with the 'method' prescribed by Zeide (2010) – one comprehensive model – in the previous sentence.

Our experience with comprehensive growth models, that include all/most others as special cases, is that they become over-parameterized. Over-parameterized models seldom yield to efforts to associate parameters with physical or biological factors controlling growth, in our experience, because of ultra-high inter-parameter correlations. For example, while a 'tour-de-force' of algebraic insight, Grosenbaugh's (1965) comprehensive growth model is seldom seen in the literature these days. Why is that? Books have been written about 'beauty' in science and, based on decades of experience, scientist in many advanced disciplines have found that the 'simplest' solutions turn out to be more 'lawful' than more complex solutions (Feynman 1965, Farmelo 2002). Bunge (1963) cautions that like all razors, Occam's should be used with caution, so simplicity is not an 'end' either, of course. It would seem to us Zeide (2010) is suggesting a direction for modeling research that may be algebraically fun for some, but would if followed in the decades ahead turn out to be decidedly unproductive of new understandings in the form of models with physical or biological interpretations for parameters.

We would suggest an alternative – those scientists interested in the question addressed in this manuscript should engaged in an international effort to test these equations, plus others that have escaped the author's attention until recently (Garcia 2005), on available height-age measurement sets on a coordinated and much wider scale, a sort of height growth model 'run-off' for the simple reason that one can not have a 'comprehensive model' without checking it against 'comprehensive' data sets. Of course calling for such a 'run off' is easier stated than accomplished. As forest research has been organized, at least in USA and Denmark, there were, or still are, too many small enclaves of scientists in too many places with too little funding. There has been some improvement in the last few decades in 'places' and 'enclaves'. Not sure about 'money'.

A preferred trajectory for a scientist's career is to move from singular descriptions to universal explanations (Leary 1985). Discovering which environmental factors affect/control which model parameters can be a huge step in the direction of explanation. Of course there are a couple of routes from a singular description – first to universal description and then to universal explanations OR first to singular explanations and then to universal explanations. But one scientist's career spans only a few decades, and we've been using mathematical models to represent tree/forest dynamics since at least 1825 – nearly 185 years. It seems like the dominant strategy for much of the 185 years has been to get better and better, i.e., more general, descriptions of growth patterns. So we have unconsciously followed the route through universal (i.e., comprehensive) descriptions. The worrisome part of Zeide (2010)'s suggestion to get a 'comprehensive' model of growth is that attaining such a universal description could well prevent deeper insight that might be possible by beginning from a less general/comprehensive model.

In an earlier sentence Zeide (2010) states:

> "Designing simultaneously several alternative hypotheses, a method proposed by Platt (1964) under the name 'strong inference,' calls for excluding all but one 'right' alternative. But the yes or no answer cannot resolve conflicting or complex problems."

and then goes on to ask far reaching questions.

> "Do we learn by taking things apart or by putting them together? Is nature causal or random? Is forest succession orderly and predictable? Is my knowledge objective or subjective?"

These questions are whopping philosophical ones, not scientific ones. We would also take issue with Zeidie (2010) description of how strong inference works. Form exhaustive alternative hypotheses and design experiments to exclude a least one alternative branch completely as a result of one experiment. Then the process is repeated on other branches that survived the first exclusion. And so on. Our example was mostly successful at excluding two branches, but not successful at excluding two additional branches.

Zeide (2010) writes

> "Unlike Platt's and Popper's approach, growth modeling does account for opposites. This is clearly described in Zeide (1993)."

The juxtaposition of a growth model (formed as LTD, TD, or YD) vs. Platt's and Popper's preference for *Modus tollens* over *Modus ponens* seems well wide of a valid comparison. The items are not even in the same category of thought. *Modus tollens* is an argument form:

If A then B
<u>Not B</u>
Therefore Not A,

A model [say, LTD, TD or YD] is but a single component of A , which is the set-theoretical union of three components{model, initial conditions, auxiliary assumptions}. If one concludes 'Not A' it simply means that one or more of {model + initial conditions + auxiliary assumptions} is false.

Reference is to Zeide (2010) comment:

> "Popper's assault on certainty and verification is equally self-defeating. If testing has no natural end point, the whole enterprise of science would be pointless. To Popper (1983), science has no certainty, no rational reliability, no validity, no authority."

Well, we would argue that most philosophers of science believe that if an idea can not be shown, in theory at least, to be false, then it isn't a scientific idea. Further, we feel most practicing scientists are not attracted to the notion of 'truth'; scientific research is a process used by scientists to seek closer and closer approximations to 'truth'. But there is never an 'end point', like Zeide (2010) is suggesting. Rather it is more like a point of diminishing returns – no need to go there (looking for tenure-ensuring research project, for example) because there is little opportunity to improve on the current state of knowledge. It appears Zeidie (2010) is very uncomfortable in this world of grays, rather than blacks and whites. We believe that if you have the closest known approximation to the truth about something (as evidenced by the repeated failure of others to falsify your propositions), you will likely have 'authority' on that question.

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