We agree with the referees that the original model lacked certain important features and was presented in a nondimenional form that made the mechanisms opaque and confusing. We have thus introduced a new model that is fully mechanistic and dimensional along with it’s non-dimensional version in the main text. The key mechanistic additions are and explicit treatment of biosynthesis and maintenance metabolism, resource growth rates and density, an explicit carrying capacity for the resource dynamics, and the saturating dynamics for resource consumption by the consumer. This more explicit treatment integrates concepts from ontogenetic growth, resource dynamics, and growth physiology. We then provide a explicit derivation of the nondimesionalization of this mechanistic model and show that we get a form that is very similar to our model but differs slightly in the resource consumption equation. This new mechanistic model and its nondimensionalization produce results that are qualitatively similar to our previous analysis, but additionally allow us to make set of quantitative predictions for data that we have added to the paper.

The main changes to the manuscript include:

* The model now includes an explicit treatment of maintenance that mirrors many previous efforts in ontogenetic growth spanning microbes to mammals. This treatment follows from the mechanistic and fully dimensional treatment of the dynamics that lead to the derivation of the slightly altered non-dimensional model, and involves the concepts of both biosynthetic and maintenance metabolism.

Because the model is greatly improved, and the model only subtly differs from our original version, we request that the manuscript be reconsidered.

**Reviewer Comments:**

Reviewer #1:

Suitable Quality?: Yes

Sufficient General Interest?: Yes

Conclusions Justified?: Yes

Clearly Written?: No

Procedures Described?: Yes

Comments :

This is a remarkable contribution to the understanding of ecological allometries. It does seems to be an step in the right direction, as the proposed Nutritional models accomplish a lot with rather little in therms of assumptions. However, there are a couple that may be important to discuss in a more explicit way in the manuscript. First, the model assumes that only hungry individuals die, while allometric data shows that mortality rate (e.g. Azbel 1994 PNAS 91, Brown et al 2004). More importantly, however, the authors assume that reproductive rate is independent of resources, and that reproducing individuals require less energy than that required to recover from the starving state. In part this generates the odd result that when there are no resources then (lambda > sigma) the population of well nourished individuals grows exponentially (lines 149-153).

In lines 410-415. In relation to the ecological constraints upon body size and how resources in particular can limit extreme sizes see (Marquet and Taper 1998 Evolutionary Ecology, 12(2), 127-139.). This result and a later paper by Burness in PNAS highlight that area exerts important constraints upon persistence, since energy and resources area spatially distributed.

Lines 440-452. Although R\* theiry predicts that those species able to maintain populations under low resource concentrations will out-compete those with higher R\* it is important to realize that as populations decrease in size several other factor affect viability (e.g demographic and genetic stochasticity) such that thresholds (e.g.Allee effects) are common.

Minor comments

Line 80. delete "many"

Line 460 change linneage to lineage

Several references need amendment (initial for authors in 36, 38 incomplete in 42 etc.

It would be interesting to see what the model can say regarding the scaling of extinction risk.

Reviewer #2:

Suitable Quality?: No

Sufficient General Interest?: No

Conclusions Justified?: No

Clearly Written?: Yes

Procedures Described?: Yes

Comments:

This paper proposes a nutritional-state structured model of consumer-resource interaction and uses the scaling rules resulting from metabolic theory to make inferences about expected types of population dynamics, the risk of extinction as well as the outcome of an evolutionary process in maximum body size. The paper is overall written with a lot of enthusiasm and a clear use of language and therefore has quite a bit of appeal. However, a more careful and critical read of the manuscript shows me that the starting point of the model development and analysis is seriously flawed when considered form a biological point of view. I therefore do not think this is a publishable paper.

• To start with, I fail to see how the proposed model is related to the authors motivation of the study as explained in the introduction. The motivation centers around the trade-off between investment in maintenance and growth on the one hand and reproduction on the other hand, as also emphasised in the significance statement. However, in my opinion the model that the authors propose has little or nothing to do with maintenance and certainly not with somatic growth.

We have now included a more complete treatment of maintenance as detailed below, and also agree that the original model lacked an explicit treatment of maintenance processes. The goal of the original model was to describe the simplest dynamics for starvation, recovery, and reproduction, however we realize that this model lack an explicit treatment of important biological characteristics and was confusing in it’s original form. We have now included an explicit dimensional and mechanistic model which we non-dimensionlize into a form which is only slight different than the original equations. It should be noted that this is an important improvement to the model because it allows us to make new quantitative predictions and to interact with additional data which we have added to the paper. However, the qualitative results are largely the same as the original model and the two models reduce to the same equations within certain regimes.

• However, my most fundamental problem with the model is not its motivation but its formulation. The authors model a consumer class that is hungry or starving and a consumer class that is full, satiated and reproducing. Both classes of consumers feed on the resource. The resource grows via a logistic growth equation and the authors off-handedly mention that its carrying capacity equals 1. This innocuous remark has huge implications as it means the resource density has to be interpreted as a scaled resource density. In turn, the authors assume that the transition from a full state to a hungry state is proportional to the absence of resource, which they identify with 1−R, where R is resource density. To define the difference between current resource density and its carrying capacity as “absence of resource” is in my opinion not correct, but biologically it is also an absurd assumption as in an unscaled version of the model (where the carrying capacity is not set equal to 1 and hence the term 1 − R, would read K − ˜R with K the carrying capacity of the resource and ˜R the unscaled density) it would imply that an individual consumer would more quickly end up in a hungry state if the maximum density of its resource increases. This makes no sense to me. I have tried to see whether there is some magic scaling of the resource density that would justify this choice of functional form. I failed, so I have come to the conclusion that the term σ(1−R)F describing the rate at which consumer transition from the full to the hungry state is not based on any sensible biological mechanism. It is a mathematical construct that has little or no connection with biological realism.

In our treatment of the dimensional model we have added an explicit treatment of carrying capacity and the saturation of the resource to an upper bound. We also show that within our dimensional system there is a choice of nondimensionalizations that leads to a form for resource dynamics that depends on (1-R) where R is the nondimenionalized resource that has been rescaled by the carrying capacity. We agree that the nondimensional model is confusing without a presentation of the fully dimensional form which we have added to the supplement along with our explicit non-dimensionalization procedure. Again, it should also be noted that the adjusted mechanisms of the dimensional model lead to a nondimensional form that differs from the original model for the resource dynamics (dR/dt) equation.

• A second issue arising from this assumption has to do with dimensions: the authors completely overlook the problem of dimensionality that results from the assumption about the starvation rate σ(1−R)F as well as the recovery rate ρRH. In a later section they derive estimates for the parameters σ and ρ, which they give the dimension time−1. This derivation, however, totally ignores that the parameters σ and ρ represent proportionality constants in terms that also involve the resource density. Again in an unscaled version of the model the product ρR should have the dimension time−1 when we only consider the ODE for H, which would suggest that ρ should have a dimension time−1 . resource−1. However, if in an unscaled version of the model we consider the ODE for R, it can be inferred that the product ρH should have the dimension time−1 and hence that ρ should have a dimension 1 time−1 . consumer−1. In short, considering the dimensions of the different terms the model does not make much sense either.

• Further biological issues with the model formulation are more subtle. For example, implicit in the model formulation is that the changes between the hungry and satiated state are taking place at the same time scale as consumer and resource population growth. In my opinion, this assumption is not very convincing as reproduction takes place at a lifetime scale, but individuals are hardly ever hungry for their entire life. It is for this difference in time scale that classic models usually involve a consumer functional response to account for the difference in time scale of the process of hungry/satiated switches and population dynamics.

• Apart from the biological issues with the model, there are also mathematical issues with its analysis. The authors report that for σ = λ a transcritical bifurcation occurs, but this statement is evidently wrong. A transcritical bifurcation indeed involves a real eigenvalue turning positive, as the authors explain, but apart from that a transcritical bifurcation involves 2 different fixed points, one that turns from stable to unstable, while the other turns from unstable to stable. Two different equilibria are hence necessarily involved in a transcritical bifurcation. Inspection of the expressions for the consumer and resource density in the internal equilibrium (equations [2]) shows however that neither the (0, 0, 0, ) nor the (0, 0, 1) equilibrium is involved in the TC. Although for λ ↑ σ R∗ approaches 0, F∗ and H∗do not. What happens around σ = λ is therefore mathematically unclear.

• Along the same lines, in figure 1 it is shown that a parameter region with cycles is bordering the line where σ = λ, where a real eigenvalue turns from negative to positive. From a mathematical point of view I do not understand the dynamics in this region, as the real eigenvalue turning positive relates to the already unstable non-trivial equilibrium. What happens at that border to the limit cycle? It should be explained for which parameter values the limit cycle is stable or unstable, the fact that the already unstable equilibrium becomes infeasible is unrelated to the fate of the limit cycles. Because of the more fundamental problems with the underlying model I will only shortly comment on the later sections, which deal with the parameterisation of the model based on the scaling rules from metabolic theory and with evolution of body size.

• The authors derive estimates for the recovery rate ρ from the ontogenetic growth model. Implicitly, the authors equate here the process of fattening up following starvation to the process of body size growth after birth. I find this also a questionable assumption, the latter has to do with developing body tissues and structures, whereas the former is a process of replenishing storage compartments. I hence disagree with the starting point that recovery from- starvation parameters can be deduced from the ontogenetic growth model.

• In the last section on evolution of body size, the authors identify the ESS with the consumer body size with the highest steady-state density of consumers. This assumption is in contrast to any other theory in consumer-resource interaction, in which it invariably holds that the consumer type with the lowest equilibrium resource density is the winner over evolutionary time. Unfortunately, in the model that the authors develop this is the non-sensical consumer with σ = λ as is made clear in figure 6 of the paper. In summary, I can only conclude that I am not at all convinced by this manuscript, which involves a lot of (mathematical) reasoning, but is founded on a flawed biological basis. I apologise for this harsh judgement, but I fail to see an alternative.