Response to Review 1 Comments

Reviewer 1 recommends only a few minor revisions, which are addressed below.

Response to Technical Comments:
Reviewer comments are in italics.

In the Introduction, the author expounds on the rationale for a thermodynamic approach, but omits the basic fact that ecosystems are open, energetic systems. What better reason is there to apply thermodynamics?

The open, energetic nature of ecosystems has been added to the introduction.

On Page 3, Page 18 when the MEP principle is introduced, correctly, it could also be mentioned that the goal function in fact depends on the stage of development, which was investigate in Aoki 2008, Ecological Modelling 215, 89–92.

I believe the reviewer is referencing Page 4, Line 18. Aoki examines entropy production using respiration as a segregate measure, which is a good first approximation. However, Aoki considers entropy production normalized by biomass (what I refer to as biological structure in the manuscript), and shows via observations that biomass specific entropy production first increases then decreases with time, presumable due to maturation of the ecosystem.

I have given considerable thought regarding entropy production and the concentration of biological structure. From a steady state perspective, an MEP state can be attained over a considerable range of biomass concentration. This is evident in Fig. 4b of the manuscript, which shows total N content as a function of internal entropy production, which ranges from ~100 to 10^5 mmol m^{-2} near the EP maximum. From a transient perspective, however, an ecosystem with high biomass content can be exploited by invaders that can consume the standing stock and produce considerable amounts of entropy via oxidation (respiration) of the standing stock. A forest fire accomplishes the same function. It seems plausible that mature ecosystems minimize their standing stock at the MEP state so as to minimize the probability of invasive by other species (or by fire). However, I have yet to develop this part of the application of MEP to ecosystems. Nevertheless, the Aoki reference does related to the N_{total} discussion, and has now been included in the paragraph following introduction of (16) on pg. 14.

On page 5, line 7, please correct the mistake that Equation (2) applies to heterotrophic organisms, since all organisms metabolize (plants and animals).

I agree that both plants and animals respire, but Eq. (2) does not represent general respiration, but rather the utilization of reduced organic carbon, in this case in the form of biological structure. There are two general classes of metabolic reactions, autotrophic and heterotrophic. In autotrophic reactions, CO_2 is the source of carbon, while heterotrophic reactions rely on reduced organic carbon. The statements following Eq. (2) do not imply that plants do not metabolize. Autotrophs, sensu stricto, are not considered in this version of the model.

On Page 6, line 5, eq (4). …Please move this to right when the equation is introduced (particularly kappa).

I agree, the units are now described in the sentence following their introduction.

On page 15, lines 1-3, please clarify what might happen if the N constraint is released. Was this scenario investigated?

I think Reviewer 1 misunderstood the statement, "…N_{Total} less than 10^4 mmol m^{-2}…". That the majority of the Monte Carlo simulations exhibited N_{Total} less than 10^4 mmol m^{-2} is simply an observation from the Monte Carlo results and does not represent a constraint. No constraints were placed on N_{Total}.