Dear Tim Coulson,

We thank you for your enthusiasm and interest in our work. We are grateful for your and the reviewers' comments that allowed us to improve the manuscript. In what follows, we address each comment (printed in black) and show how we answer them in the revised version.

On behalf of my coauthors,

Alice

Your comment:

'When evolution occurs in one or more species, has the system not changed? At least one species has changed due to evolution, so it fair to consider the ecosystem pre- and post-evolution to be an identical composition of interacting organisms that is part of some definitions of alternative stable states?'

This is an interesting point. Indeed evolution will change species so the system (in our case the lake) will contain a different set of traits pre and post evolution. Yet the mechanisms that could give rise to the alternative states are preserved; only their strength is changing due to selectives changes. In that sense, the details (trait composition) are different, but the context (mechanisms) is preserved. As we are here interested in the emergence of mechanisms that produce ASS in the lake, it does not matter if composition remains strictly identical or not. At the same time, keep in mind that we decouple ecological and evolutionary dynamics, by stopping evolution before applying an ecological perturbation. From that perspective, the system at the onset and at the end of the hysteresis are in fact here strictly identical.

Anonymous reviewer's comment:

'My one suggestion for improvement in the manuscript is for the authors to be clearer about what they mean by priority effects and asymmetric competition. I am mostly thinking about being clear that asymmetric competition is the process that leads to an outcome—a priority effect—in both light and nutrient competition. Often times the authors discuss nutrient competition as a priority effect and light competition as asymmetric (e.g. page 4 line 54 onwards).'

We thank you for pointing this confusion and we tried to clarify the text. By 'priority' we referred to the mechanism modeled by function q(z), which captures the fact that deeper traits are more efficient at removing nutrients from the water column. We agree that it is not the conventional take on priority effects and that it may confuse readers. Throughout the manuscript, we now replaced 'priority access' by 'unequal exploitation efficiency'.

Comments of JF Arnoldi:

1/ Is it a coincidence that regime shifts and diversification emerge from similar mechanisms?

It's not completely a coincidence. From previous works, we know that asymmetric competition favors diversification in eco-evolutionary dynamics (Kisdi 1999). We also know that such asymmetries generate positive feedbacks responsible for Alternative Stable States in ecological dynamics (Scheffer 1998). Nonetheless, asymmetric competition alone (without the third mechanism now called 'unequal exploitation efficiency') is not enough to generate ASS. Diversification driven by asymmetric competition for light is a pre-requisite for having ASS so the conditions of ASS are necessarily nested within the conditions for diversification. But this only became clear once we tested all the

mechanisms together and in isolation, as we could not know beforehand how much asymmetry was needed to get both diversification and ASS. We have worked to improve the discussion of the role of asymmetry in the paragraphs 'Diversification: the necessary condition for the emergence of alternative states' and 'Evolution allows for alternative states only for a limited range of conditions' in the discussion.

2/Are the strategies that coexist and allow for regime shifts resemble those found in shallow lakes? From the discussion I understood that this was not the case.
Indeed there might be only a few species of macrophytes that live free-floating as we modeled, but phytoplankton species can form different layers and might resemble more closely the organisms we model (we tackle this in the 'limitation' section of the discussion). While there is limited quantitative agreement with empirical patterns, this is not entirely surprising as our model is highly simplified. It is built to understand the mechanisms that underlie alternative stable states, not to investigate precise depths of algae.

3/Is it really necessary to have sympatric speciation in order to find regime shifts of this kind in nature? Isn't it enough for populations to exhibit some plasticity or enough intraspecific phenotypic variation?
The reviewer is right that it is not necessary to have sympatric speciation for ASS. The technique we use here (adaptive dynamics) is purely based on phenotypic variations and simplifies the genetic part of evolution. As such, we do not want to speculate on the fact that branching are (or not) speciation events (see Dieckmann & Doebeli 1999 vs Waxman & Gavrilets 2005 for a debate on this). We expect similar results if there is enough intraspecific variation (polymorphic population), plasticity, or community assembly through species sorting. Whether communities in nature resemble evolutionary stable communities is a general and interesting question (Edwards et al. 2018): communities resulting from branching may not be very different from species sorting in community assembly. We clarified the discussion on this point by adding a paragraph line 437 in the discussion.

4/Since we are considering eco-evolutionnary interplays, why not go one step further and allow strategies to evolve in reponse to nutrient enrichment?
This is indeed a very interesting question (partially addressed by Chapparro et al. 2021 Am Nat). Many scenarios would be possible depending on the relative speed of evolution and disturbances. We would like to keep the text focused on the issue of evolutionary emergence of the mechanism underlying alternative stable states and we feel that a complete analysis of possible evolutionary dynamics goes beyond the scope of our current manuscript.

A/The paper is very descriptive and quite long, I would suggest trying to make it more concise and focus on a clear message, simple and general enough to have a chance to be relevant in natural systems
We tried to shorten and focus the introduction so that the main question comes across more clearly.

B/It would be good to first recall the scenarios and parameters that lead to regime shifts in the ecological model, so that we can see more clearly how constraining it is to ask for those conditions to be evolutionnary stable.
We describe these scenarios and mechanisms in the third paragraph of the introduction. We believe that adding parameters values would make the text heavier, especially considering that Scheffer 2003 made a thorough investigation of those.

Fig.5 second line, please recall (e.g as a vertical line) the value N0 at which the evolution took place (5 mg/m2 according to the table), so that we see the reference point from which the ecosystem is perturbed.

Thank you for this recommendation, we changed the figure accordingly.