Reviewer: 1  
  
Comments to the Author  
This paper opens an interesting perspective on how changes in environmental oxygen conditions can shape the success of invasive species.  The authors use a simplified approach successfully to show how hypoxia sensitivity comes into play. ed. The smaller scale findings in this study relating to variabilities in ambient oxygen are very interesting. This also has wider implications, e.g. in large scale biogeography (cf. Deutsch et al. Science 2015) or oxygen dependent species distributions in large scale hypoxia areas (Storch et al, 2014 in Global Change Biology). Some widening of the literature base seems justified. In this context the terms “invasive” and “native” need to be clearly defined, e.g. invasive from where, is this only a redistribution of endemic communities or are wide ranging expansions of species ranges involved? Also the metabolic background (oxyconformers vs. regulators) needs to be detailed more (see below).  
  
1- The paper has jargon in it that should at least be explained, e.g. “lowest R\* value, sensu Tilman 2004“ is not an upfront explanation that makes the paper readable to the non-expert. An explanation such as in l. 376 should be moved up.

*R: Agreed, we now provide more exposition on R\* for nonexperts.*

2- 123: The original literature to show that critical PO2 indicates the onset of anaerobic metabolism is in fact referred to and used to elaborate wider principles by Portner and Grieshaber 1993. There are different metabolic types, not all organisms are oxyregulators and differentiating between oxyconformity and oxyregulation may have implications for the approach used in this study. This should clearly be addressed in the discussion based on literature largely produced in the Grieshaber lab in the 80ies and 90ies.

*R: Agreed, we now reference the work of the Grieshaber lab, and have revised both the introduction and discussion. In the revised introduction we discuss the difficulty in identifying the onset of anaerobic metabolism in oxyconformers, and in both the revised introduction and the revised discussion we are more clear about exactly what our metric of critical po2 represents (i.e. we state clearly that this metric is a measure of reduction in aerobic metabolism but may not be associated with an increase in anaerobic metabolism).*

3- 159: The shortcomings of closed-system respirometry need to be kept in mind.  
*R: Agreed, we now include a discussion of the issues associated with closed system respirometry.*

4- 164: I do not think Presens is based in Aachen.

*R: Apologies, we have made this change.*  
5- 187 to 191: not clear how this manipulation relates to physiological reality?

*R: We now discuss issues regarding the independence of body size and our PCrit proxy* *and note that regardless, our parameter of interest is not affected by this assumption.*  
  
6- 216: clearly a discussion is needed as how this assumption is potentially wrong, i.e. the uncertainty involved as the transition to anaerobic metabolism has not been tested.

*R: We agree. A detailed the paragraph in the discussion was has now been added.. Lines xxx*7- 262: Here an assessment of the true rate of oxygen consumption in relation to tolerance would also appear extremely useful.  
*R: Agreed, we now include the rates as well as the relative differences.*

8- 289: the terms in invasive and native need to be clearly defined, e.g. invasive from where, is this only a redistribution of endemic communities?  
*R: We now provide a definition and description how we defined native and invasive species in both the Introduction and the Methods.*

9- 300: size distributions could be mentioned within each sub-graph as well as morphotypes  
*R: We were concerned that the figure would be a little busy with this information but agree that we should include them somewhere, so we now include status, morphology and mass in table 1. If the reviewer or editor think that this information would be better in the figures, we’re happy to change.*