Reply on RC1
Michael Whitney

I appreciate the reviewer’s comments. The concerns and questions raised can be addressed with some additional analysis and a greatly expanded discussion. There is a clear need for updated 21st century forecasts for conditions in coastal hypoxic areas around the world. The paper presents and describes such forecasts and places them in context with global observations. The revised paper will more effectively motivate the objectives and much more fully discuss results, limitations, implications, and ways to improve forecasts. This paper should inform many scientists and managers about the intensity and spatial distribution of warming pressures confronting coastal hypoxic areas.

I have endeavored to provide an appropriate level of detail in conveying the main points raised by the reviewer and in describing the ways the paper will be revised.

The reviewer is looking for more in-depth discussion of results in terms of the observation/model simulation comparison and the global/coastal/hypoxic region. The discussion in these parts of the paper will be expanded and the helpful questions posed by the reviewer will help guide the expanded discussion.

The reviewer also is looking for discussion on the discrepancy/disagreement with previous studies. There are many reasons for this including the different time periods, areas, and depths analyzed and other differences in study designs. These differences and the underlying reasons will be discussed in more detail in light of the questions the reviewer poses.

The reviewer raises concerns (that I share) about the applicability of forecasts from global climate models to estuarine systems. Nevertheless, this approach has been used in previous studies which have been cited by many. The limitations are described in the introduction, but the revised paper needs to discuss these limitations in more detail. The discussion also will help highlight the need for long-term forecasts with high resolution regional or estuary-specific models. There are some examples where such modeling has been completed (these will be cited), but they represent an exceedingly small portion of the world’s estuaries.

The reviewer suggests that the important role of nutrients on hypoxia should be added to the discussion. The paper focuses on the warming-related pressures and alludes to the
nutrient role early on. It will be straightforward to add a paragraph in the discussion on nutrients. The paragraph also will help convey the requirements for improved long-term forecasts.

The reviewer points out that the oxygen loss from the ocean with rising temperature and getting more server towards higher latitude is predictable according to the nature of oxygen gas. This is a fair point, but it is important to quantify the forecasted changes and patterns as is presented in this paper. These results are very useful to many people studying and contending with hypoxia issues in a warming climate. The reviewer suggested potentially answering questions about how warming pressure compares to gains made by nutrient management strategies and/or treating case studies of particular coastal hypoxic systems. Refocusing the paper in these ways is beyond the scope of the revisions that will be submitted. However, these issues can be raised briefly in the discussion. My recent case study results for the Long Island Sound can be cited too, since that paper addresses management gains vs. warming pressures.

The reviewer also lists detailed comments that can be addressed straightforwardly with appropriate clarifying text.