General comments: The present study examines the variability of surface water temperatures at Long Island Sound (LIS) and how cold and warm events in this region are related to a dipole of atmospheric ridge and trough in the North Pacific, and to sea surface temperatures (SST) in the tropical Pacific Ocean.

The article addresses a relevant topic with important implications for the climate forecast and impacts community and overall appears to be technically sound. It makes interesting use of quite novel methods (e.g. statistical significance testing of event decomposition or wavelet analyses) to examine the variability and potential predictability of the LIS thermal system. However, the authors do not discuss any possible physical mechanism that could be responsible for these statistical associations (as stated in the Abstract). The choice of season for each analysis is not always clear and well justified which makes it quite difficult for the reader to understand the (lagged) relationship between LIS water temperatures, the atmospheric dipole and tropical SST variability. I think the overall structure of the manuscript could be improved, by being more succinct, by being careful not to over interpret results but rather highlighting how these results are new compared to the previous literature.

The authors the appreciate the comments provided and many changes have been to the original manuscript. Our responses to the comments are in plain text and the original comments are in bold text (bold text). Changes to the manuscript include the addition of a March 2012 case study, the inclusion of motivation for constructing the dipole index, and the addition of text describing the physical mechanism behind the dipole relationship with Long Island Sound temperature variability. Specific changes are described below.

Specific comments:

Abstract:

The physical mechanisms are not (or poorly) discussed in the text. The 2012 ocean heat wave across the mid-Atlantic Bight to my knowledge has not been discussed in detail either.

The authors agree that a physical mechanism is needed to explain the relationships identified in this study. To address the issue, we have included a discussion about how our dipole pattern is linked to LIS temperature variability using well-known ideas in meteorology.

1. Introduction:

It would be useful to explain why the LIS is an important region to study (in terms of impacts), and perhaps to describe in more detail previous results on the importance of the EP/NP pattern. If possible, please also add a reference discussing the lack of relationship with the Gulf Stream and NAO.

The authors agree that the reason for studying the LIS needs to be better motivated. As such, a discussion describing the relevance of the LIS to fisheries was included in the revised manuscript. The authors also agree more background material is needed. We now highlight previous work examining EP/NP pattern impacts and use those studies to motivate the construction of our dipole index. A reference to the Gulf Stream and NAO linkages with LIS temperature has also been included.

3. Methods:

Page 5 line 7: How many adjacent points are required to be considered an event?

According to the definition of an event, there is no number of adjacent points required for an event. This lack of length restriction is now made more explicit in the methods section for clarity.

4. Results:

Fig. 2: Where are the vertical dotted lines representing the occurrence of canonical and East Pacific El Nino? Perhaps it would be useful to highlight the most intense warm and cold LIS events in red and blue (just a suggestion).

The authors appreciate the suggestion regarding the clarity of the figure. The most intense LIS events are now highlighted with color; the CP and EP EL Ninos are now indicated with acronyms in the figure.

Section 4.1: It would be nice to clearly explain the added value of the event spectrum compared to the simple time series. The paragraph could also be shortened here. In what seasons are these extreme temperature anomalies more

likely to occur? How long do they last? More discussion of Table 1 would be helpful here.

The authors agree that the added value of the event spectrum has not been clearly explained. One reason for choosing the event spectrum approach is that we can treat time periods in which LIS temperature anomalies are of similar sign as individual events, which helps account for autocorrelation in the time series. Accounting for autocorrelation makes our results more statistically robust. Also, by separately looking at negative and positive events we are better able to uncover differences in the intensity of negative and positive events. The event spectrum also provides an easy way to objectively calculate the persistence of events, contrasting with lag-1 autocorrelation coefficients that need to be calculated over some predetermined time interval. Thus, persistence as measured using autocorrelation would be a function of the time window used.

We found that intense LIS temperature events tend to occur most frequently during the cool season (November-March), presumably because atmospheric forcing is stronger during the cool season. On average, LIS temperature events last about 2 months, but the most intense LIS temperature events can span several seasons. A discussion to this effect was incorporated into Section 4.1 of the revised manuscript to help better explain Table 1.

Section 4.2: Fig 3: How different do patterns look in each season? How coherent is this atmospheric dipole on different vertical levels? It would be worth detailing the possible physical mechanisms operating behind this forcing from the atmospheric dipole onto LIS water temperature variability. Fig. 4: I wonder how useful this figure is. It is very noisy and difficult to discern the 2012 event.

The atmospheric pattern related to LIS temperature variability is generally the same for each season, but the relationships are strongest in the winter. We now note this seasonality in the revised manuscript. The authors note that the LIS is well-mixed (especially during the winter) so that the atmospheric pattern related to LIS water temperature at one vertical level is the same as those at other vertical levels. This is now mention in the text in Section 4.2. The authors agree that a physical mechanism behind the dipole pattern relationship with LIS temperature is needed. We now describe the physical mechanism in terms of the jet stream and surface pressure systems. For example, we now describe how the ridge over Alaska induces a downstream anticyclone that can be inferred to advect Arctic air from northern portions of North America into the LIS region. The authors have deleted Figure 4 of the original manuscript but included a schematic of the physical mechanism underlying the LIS temperature-dipole relationship.

Section 4.3: The composite analyses are interesting (particularly the discussion of specific LIS events and El Nino years), with potentially important implications for the forecasting of LIS temperatures. However it is not clear how many events compose each of the LIS composites (counted in months, seasons?), which makes it difficult to really interpret in the context of ENSO and predictability. Perhaps it would be useful to show the correlation between the dipole index and SST anomalies (page 12, line 21). It might also be important to discuss the overall added value of the dipole index compared to the EP/NP index used in previous work.

The authors agree that more information regarding the number of events is needed. The authors note that the events are not counted in seasons or months. Rather, the events are an arbitrary length. However, many of the events used in the composites have associated peaks in the winter. Thus, the composites shown in the composite figures generally reflect winter conditions. This information is now provided in Section 4.3 to help guide the reader. The authors agree that it would be useful to show the correlation between the dipole index and SST because the correlation approach is more standard and will help readers interpret our results. We therefore have included a new figure in the manuscript for comparison.

We also now include a discussion highlighting the value of our dipole index. We now point out, for example, that the dipole index is optimized to explain the variance of LIS temperature, whereas the EP/NP index is constructed such that it explains a relatively large fraction of geopotential height variability. As such, the dipole index is a better predictor of LIS temperature than the EP/NP index. Also, the EP/NP index is curiously not defined in December, further complicating its use in a forecasting setting. We now mention that the dipole index is needed because patterns extracted from an EOF analysis are unable to capture LIS temperaure variability in December.

Finally, while the references to extreme LIS events are insightful, I think it might be helpful to consider the 2012 warm event more clearly as a case study

throughout the text. It would help with the readability of the text and our understanding of the climate teleconnections mentioned.

The authors agree that the inclusion of a case study will help readability of the manuscript. Thus, we included composite plots of the March 2012 event. We discuss in more detail the atmospheric and ocean features present during that event as well.