Reviewers' comments:

Reviewer #1: Thanks for considering the reviewer comments and re-submitting, the article is improved.

The only comments I have are that in the abstract suggest should be plural (unclear what should be plural – lake trout is already used in the plural).and for the discussion:

210 - should be past tense.  Done

Although I think this paragraph could be re-worded as a paragraph that starts with "Our data showed a lack of variation in lipid concentrations among three regions of the Lake Champlain, indicating that lake trout do not experience differences in prey availability across the main lake." (Maybe just taking the sentence at 266 - 268 and starting that PP at 269?   And then expanding on the other two hypotheses and any evidence to suggest these are plausible.   Just suggestion.

This is a valid alternative structure, but we felt the existing text follows a more logical flow of hypothesis, rebuttal, and then consideration of alternatives.

Reviewer #2: I have reviewed the author's revisions and found that they have done a good job of addressing most of my edits and suggestions.  I believe the paper is ready for publication and have added a few additional comments that the authors may find useful.  I also think that careful attention is needed when the authors are discussing the results of their comparisons concerning main effects because they indicate that the interactions between factor levels confounded the examination of main effects.  The authors should state and defend the assumptions they are using when discussing the main effects.  I also provided some comments concerning dissecting factor level effects and confining analyses to areas with sufficient data.

Overall, we appreciate the insights of reviewer 2, and agree that the data could be subjected to further and different analysis. However, we feel that the data we have in hand, although publishable, are not sufficient for the level of analysis and interpretation suggested by the reviewer. The data show very interesting and publishable results that we believe will be of interest to readers, and propel future research on the topic. Their suggestions would be more appropriate for a more rigorous study on the subject in the future. We have made changes to the text as appropriate, noted below

General Comments:

I understand the author's view that the species lake trout should be referred to using the singular form of verbs (i.e., was) but still advise that readability of the sentences where they do that (e.g., line 31) would be improved by using the plural.  While their indication that lake trout "was" extirpated from Lake Champlain is grammatically correct, it is clunky and left up to the reader to decide that they are talking about the species as a whole and not all of the individuals killed during the population collapse since the author's do not state that they are referring to the species (e.g., lake trout were among several native species that declined following …; or the lake trout population was extirpated).

Done

I still have an issue with the way the authors use the term "relative abundance" to represent their abundance measures.  While relative abundance does have a specific definition in community ecology referring to the proportion of a species in context with other species within a community, I've seen it used in fisheries science as a defined measure that relates CPUE of a particular unit of catch (e.g., species, life stage or site specific density) to other factors (e.g., abundance in relation to something).  CPUE in my opinion would mostly concisely be stated as CPUE or as abundance.

Done

In the results the authors indicated there were interactions between factors in their tests so main effects could not be interpreted, but then they go on to talk about main effects through out the rest of the paper without presenting assumptions why they think those trends in main effects are valid.

In figure 3 the authors present seasonal differences as means within sites but across sizes/ages after acknowledging size has a significant effect on the data.  Some clarification needs to be presented for why they present it this way.

See first comment, above

I continue to strongly encourage the authors to present a data table within the manuscript or in supplemental data tables to allow for future comparisons.

Specific Comments:

Line 60: delete "to".

Done

Lines 69-70:  Size may be more important than age in some cases, but life stage can trump that.  First year growth can often have different trajectories than older juvenile growth or mature life stage growth based on environmental and physiological factors (e.g., fast growth vs. predation risk, physiological changes related to gonad maturation, etc.).  That is why I would be careful about lumping comparisons across age-0, age-1 and older juvenile fish.

However, fast growth to escape predation risk is also dependent on absolute size, early feeding is dependent on gape size, and maturation in fish is generally more size-related that age-related, so we feel size is more important than age.

Line 90:  I'm not sure that a hypothesis connected to the seasonal lipid cycle and prey base abundance is necessary or helpful here since a lot of things confound prey abundance (e.g., thermal segregation, prey yoy availability, invertebrate cycles, etc). It would be just as informative to indicate seasonal lipid cycles were examined as an important factor with the potential to confound site related comparisons.

Age-0 lake trout feed on Mysis (age-0), YOY alewife and smelt (age-1), and small sculpin, smelt, and alewife at ages 2 and 3 – most of these prey are predictably more abundant in early and mid-summer than at other seasons, hypothesis of seasonality of lipid content is appropriate.

Line 95: further clarify this sentence by inserting "captured" after the parenthesis and moving "of Lake Champlain" after "Main Lake basin".

Done

Line 150-151:  The wording of this sentence indicates that the post-extraction weight of each sample was divided by the pre-extraction weight", that description yields %lipid-free fish dry mass and not percent lipid.  The difference of pre-extraction weight minus post-extraction weight should yield lipid weight which then would be divided by the pre-extraction weight to get %lipids.  Is that what the authors did?

Yes, we have corrected the explanation of the calculation

Line 158:  Was "source" defined previously?  If not define here.

Done

Lines 174-175: the fish obtained from the hatchery were analyzed separately as a third "source" so why do the authors indicate that those were "in the stocked group", it's confusing.

Good point, text has been changed

Lines 180-182 (and statistical comparisons in general):  The authors state that all significant main effects were confounded by interactions and do not allow for interpretation.  I suggest not indicating the main effects were "significant" if interactions are present making the main effects less interpretable; and when later talking about main effect trends (e.g. lines 184-187 and below in the results and discussion) to add some kind of wording to indicate that "despite the presence of interactions" the general trends in whatever main effect were….

For this and following comments, refer to our first note

Did the authors dive deeper to examine whether the interactions were caused by factor influences on %lipid changing between sites or seasons or were they the result of an unbalanced sample collection outcome.  I'm not sure the bootstrapping technique the authors employed would account for completely missing factor level data (e.g., no fall data for 2 sites, no spring data from one site, no age-2 stocked fish from summer and fall samples) or unaccounted for sources of variance like age or prey switching (see below).

By examining figure 3, age appears to be a substantial factor in the analyses.  The authors indicate in the introduction and discussion that learning to feed impacts stocked fish condition during the time following release.  If you assume that this may be going on throughout the first year at large then that alone could explain the age-1 seasonal lipid decrease for stocked fish like the authors suggest in the discussion.  The few age-2 stocked fish, which appear only in the length-based regression for stocked fish collected in springtime, may be having an undue effect on the slope of the spring TL vs. lipid relation as those age-2s are the fish that survived the second winter and presumably learned to feed.   Does removing those fish from the ANCOVA (of stocked fish only) remove the seasonal difference in those stocked fish slopes?  Because the learning effect is an unaccounted-for source of variance for the stocked fish, that does not likely exist with the wild fish, the authors might want to constrain their stocked fish groups to just the age-1s and present the age-2s separately in a simple comparison to the age-2 wild fish.

Size may be acting as both a discrete and continuous variable influencing lipid content across life stages because of diet switching.  It's likely, as small wild lake trout begin to turn from zooplankton consumption to Mysid consumption and then from Mysids to inclusion of yoy preyfish, that lipid accumulation potential greatly increases with the lipid density and size of the prey.  So, if those prey switches occur for groups that are used in regressions (e.g., for wild age-Is which are included with wild age-2s in the spring regression; those spring wild age-1s were quite small and sampled prior to larval preyfish availability) the slopes of relations will be biased by discrete prey selection shifts and not solely due to growth related lipid storage.  In those cases, ANCOVA with TL as the covariate should produce interactions with TL.  What happens when wild spring fish are removed from the ANCOVAS and what happens when the wild fish ANCOVAS are constrained by age?

Some accounting for the unbalanced design across ages for stocked fish and for the likelihood of prey switching at early life stages especially in the wild fish samples may improve comparisons.  This could start with using length corrected means (predicted %lipid from a 200mm fish from the specific TL vs. lipid regressions) instead of grand means in comparisons.  It could also look at comparisons constrained by age (e.g., between age-1 stocked, age-1 wild, and age-2 wild).  The authors could confine the site analysis to just spring and summer while still looking to see if length still interacts with anything.  The authors could also compare data across sites within seasons, for example compare sites in summer when they assume yoy prey fish are available and have samples across all sites.

The results of the statistics the authors have already done likely have the information to do some of these comparisons and others could be looked at by some simple re-analysis or by discussing the sample distribution a little more in the discussion.  I can draw some of the conclusions already by looking at figure 2, but figure 3 just raises questions for me because the site comparisons are confounded by the sample distribution and using grand means produces results biased by the sampling.  Both figures 2 and 3 are not completely valid to me because they are looking at levels of the main effects (grouped across sites for figure 2; grouped across sizes for figure 3) that the authors indicated were uninterpretable at the beginning of the results section.  If the authors want to look at trends in main effects, they need to present their assumptions concerning why they think it may be appropriate to group across factors that were part of interactions.

Lines 229-230:  Were the wild and stocked lake trout growing at the same rate?

Data from a related study on growth rates of wild and stocked lake trout in Lake Champlain have been added to address this question.

Lines 245-246:  Add clarity here by indicating how the lipid content of stocked fish influenced the trend.

Re-reading the second part of this sentence (in red on the manuscript), I’m inclined to simply delete it, I’m not sure what we’re trying to say.

Line 263: change "first post-stocking winter" to the "first winter post-stocking" to keep the adjective (first) next to its subject (winter).

Done

Figure 3: Because of the strong size effect and the appearance of an age effect a size corrected mean (e.g., the predicted mean lipid % at 200mm from length/lipid regressions) would be better to use for seasonal comparisons rather than the grand mean.  The grand mean will be influenced both by the total number of samples and the age/size distribution of samples.

We agree with this idea; however, our sample sizes are too small to calculate an appropriate confidence interval around the regressions (e.g., the CI around wild north summer fish was -150.8 and 201.8).

Since these are seasonal within site comparisons there is no need to join the data points with lines.

We agree that lines are not necessary, but in this figure they assist in visually connecting the data series and improve readability of the data

I would also consider making the x-axis season and the colors of the points sites.  That way the error bars would help show whether sites were different within season.

We agree with this suggestion and have changed the figure