**Responses to reviewer 1 (Koen-Alonso)**

We thank Mariano for his constructive comments and for his encouraging words.

* *Reviewer: Minor thing. This should be Figure 1, not 2. and this one should be Figure 2.*Figures 1 and 2 are now appearing in the same order as they are cited in the report.
* *Reviewer: It may be worth adding here the standard width of the gear used for analysis. Even though the individual set info is not provided here, this is the only piece of info missing to fully characterize the set as used for calculation of random-stratified indices.*More details were given about the standardization of catch data (which is by trawled distance only), and an additional table (new Table 1) detailing the vessels and fishing gears was added to the document. Section 2.4 now explains why no correction factors were applied to the catch data.
* *Reviewer: I understand the use of fathoms because that was the actual unit used in the design of the strata, but given that we are formally metric, it would be useful to add a column here with Depth Ranges in meters.*  
  Table 2 (was previously Table 1) was modified to also include the depth range of each stratum in meters. Additionally, the figure showing the strata map is now colour-coded by depth.
* *Reviewer: Another minor detail. It maybe useful to add here some comment on how the sets are standardized for calculations (i.e. by distance, by tow duration, etc). Real sets are never perfect and it can be valuable to clarify here how this is done. We often get questions on these details, so it can save you time and data requests if you write this down here.*More details were given about the standardization of catch data by trawled distance, and an additional table (new Table 1) detailing the vessels and fishing gears was added to the document. Section 2.4 now explains why no correction factors were applied to the catch data.
* *Reviewer: Some species are not id to the species level, but to the lowest reliable taxonomic level (e.g. lanternfishes). It may be worth noting this here.*   
  The taxonomic groupings used for the 103 species is now better described in section 2.3, and includes a sentence stating that some grouping of species exists for redfish and lanternfish.
* *Reviewer: A couple of observations. First, the consistent recording on inverts only happens since 1999. This kind of structural changes may be worthwhile including in Figure 1 together with the description of platforms and gears. Second, Table 3 suggests that only major macroinverts are caught in the survey with some degree of regularity. I imagine these are the key species, but many others I'm sure are also caught, even if they fall into the SR category (e.g. seasnails, sea urchins, etc). A brief paragraph describing this can be useful to avoid confusions with those readers that may not be actually familiar with RV surveys.*

As suggested by the reviewer, we modified the survey timeline figure to include the timespan of L and S species. Additionally, Section 2.3 was amended to include a better description of the species selection since there are a number of species that appear in the trawl survey database but are not presented in our report.

* *Reviewer: MKA. the power value has a lot of influence in the output. I know this is done to provide a general characterization of the spatial distribution, but a little bit more of the rational used for choosing 10, as opposed to some other exponent (e.g. 2) can be useful here.*

The justification for using an exponent value of 10 for the IDW spatial interpolation (which is chosen to achieve a visually appealing map) was added to section 2.4.1.

* *Reviewer: Not quite sure if this enough as an explanation. Again thinking on potentially non-too fishery readers, a sentence explaining what this is can be useful.*

Section 2.4.4 now includes an additional sentence that describes what “stratified numbers-at-length” represent.

* *Reviewer: This should be allometric; in the traditional use of this equation, isometric growth is related to beta=3; if you are estimating beta, then this is your traditional allometric equation.*

“Isometric” was updated to “allometric” in the text.

* *Reviewer: this needs a little bit more detail. Is this calculated for all individual fish lumped together -the regression-, and then averaged by year, or there is some stratified scaling (e.g. calculating averages by stratum, and them combining), or this is put together in some other way?*Text is now updated in section 2.4.5., and it now includes a reference (Le Cren 1951) with additional details for this calculation.
* *Reviewer: the section above only indicates the calculation of the biomass index, not abundance. Add abundance in the section above.*Both the abundance and biomass indices are calculated in our analyses, but only one is reported in the figures. The text was clarified to better explain this nuance.
* *Reviewer: you need to define Yi as the overall abundance in here   
  Reviewer: this should be capital Y to match the equation above..*

The density-dependence habitat selection model equation in section 2.4.7 was corrected.

* *Reviewer: this sentence is twice.*The duplicated sentence in section 2.5.1 was removed.
* *Reviewer:* *This should be "complement", but I have no doubts that the information in this Atlas deserves compliments too.*  
  “Compliment” changed to “complement” in section 4.1.
* *Reviewer: With this distribution seems hard to believe this is LI. Is this classification correct?*   
  As described in Section 2.3 and shown in Table 3, classification LI is based on the number of records (between 200 and 1500 records), not on distribution. So the classification of cusk (*Brosme brosme*) as category LI is correct and remains unchanged.
* *Reviewer: I have conflicting thoughts about this discussion, mainly because it doesn't really discuss much. The two main sections provide a useful summary of work done for mapping and ongoing efforts toward MSP and related initiatives, but none of these sections is actually a discussion of the results presented in this report. Furthermore, both sections are heavily focused on the spatial aspects with little attention to time series and changes over time. These sections are better suited for an extended introduction than a discussion. Some elements (like the warnings about the proper contextual used of these results) do have a place in a discussion section, but if the summary of prior work wants to be kept in the discussion, it really needs to be connected somehow with the actual results. This can be an onerous task, which is likely better invested in developing primary publications.* *With this in mind, my suggestion would be to move most of the discussion text to the introduction (i.e. generating a lengthy introduction with sections in it), and leaving only key elements in the actual discussion section. Still, these elements are likely insufficient as a proper discussion, so some general summary of the key signals would need to be written from scratch (I imagine the authors may already have text that can be used here, and if not, I'm pretty sure they known what signals are the ones worth talking about). It doesn't need to be long (after all, this report is not really intended as a full analysis, but as a useful companion for other analyses and to stimulate questions that deserve analysis), it only needs to highlight the key messages in terms of trends and/or major changes the authors want to bring up, and lead the reader to which figures (or species) are the ones to be examined with more attention. Finally, while a discussion section like I have suggested can be useful and help the reader to pick up the key messages, it is not a must have for a report like this; the results generated are plenty. If the authors want to only have key warning at the end, I would suggest writing those but under a different heading (e.g. "Caveats and proper use of this document" or something along those lines).*We agree with the reviewer that a thorough discussion of all the information and results presented in this report could be invested in developing one, or more, primary publications. We also agree that providing a series of key messages is not a “must have” for a report like this as it would be more appropriate for readers to investigate outputs for each species directly to avoid potential misleading generic statements. We replaced the last title in the discussion to "Caveats and proper use of this document for marine spatial planning purposes", and included some context about the additional remarks focused particularly on the spatial considerations: “The following sections describe considerations related to the spatial analysis presented in this report in hopes of kickstarting future discussions in the context of spatial planning. In the future, it is our hope that other working groups (e.g. Ecosystem based Fisheries Management), as well as science processes (e.g. State of the Ocean reporting, secondary groundfish stock assessments) can use the reproducible outputs provided in this document as a stepping stone to support and advance their commitments and priorities in a more efficient manner”.

**Responses to reviewer 2 (anonymous)**

We thank reviewer 2 for his/her constructive comments and for his/her encouraging words.

* *Reviewer: Page vii, line 182 and throughout – feel free to ignore this nit-picky question, but is preferences the right word? Personally, I tend to doubt that I know the preferences of a species and, as such, I tend to use the word associations.*

The term “environmental preferences” was changed to “environmental associations” in the title of the report. in the text, and in both the French and English abstracts.

* *Reviewer: Page 6, lines 312-313 – consider clarifying what is meant by number of observations / records. I assume it is the number of sets each species has been found across the whole survey time-series and not the total number of individuals recorded.*   
  What is meant by “number of records” was clarified in the text, it now explicitly states that it is the number of sets where a species was caught, to distinguish it from the number of individuals caught.
* *Reviewer: Page 7, line 325 – for those not familiar with the Maritimes survey, consider clarifying why 1999 is the year after which invertebrate species have been consistently sampled. Why would this be the case if the performance of the gear-vessel combos has been fairly consistent across the whole time-series (Page 15, lines 336-338)?*The reason explaining why invertebrate species were not consistently identified prior to 1999 has been added to section 2.3.
* *Reviewer: Page 16, lines 371-372 – this looks like LeCren’s relative condition. Consider adding a citation to LeCren 1951. Also, clarify how large and small fish are defined.*

The relative fish condition defined in section 2.4.5 now cites LeCren (1951). The figures showing fish conditions was simplified and the presentation of separate trends for large and small fish was removed.

* *Reviewer: Page 16, line 383 – wording suggestion: “… we implemented a generalized linear model using …”*Rewording in section 2.4.7 to “we implemented a generalized linear model” as suggested.
* *Reviewer:* *Page 16, lines 385-388 – should a capital Y be used ($Y\_{h,i}$)? Should the < and > symbols be used rather than the <= symbol? Should the sentence end with the symbol $Y\_{i}$? Finally, note here that approximately equal means “within a standard error from zero” (Page 18, line 431).*The density-dependence habitat selection model equation in section 2.4.7 was corrected.
* *Reviewer:* *Page 17, line 394 – consider adding a short preamble noting that the appendix figures are organized into types A-F and these letters are used as a suffix in the appendix figure labels.*A preamble was added at the beginning of the Section with the figures.
* *Reviewer: Page 17, line 396 – suggestion: “For Category LF, LI, and SF”.   
  Reviewer:* *Page 17, line 406 – suggestion: “For Category LF, LI, and SF”*

We updated the relevant sections with the appropriate categories.

* *Reviewer: Sections 3.1 and 3.2 – I suppose this information could be considered methods or results. Consider shifting it to the methods section.*   
  Sections 3.1 and 3.2 were kept in the Results section as they describe the actual number of sets that were conducted and the cumulative frequency distributions of depth, bottom temperature and bottom salinity of those sets. The reviewer suggested moving these sections to the Methods section but they really are results.
* “wasd” changed to “was” in section 4.2
* *Reviewer: Page 23, lines 463-464 – wording suggestion: “…interpolating observed weight by species using inverse-distance weighting (IDW), and …” & Page 25, line 530 – consider adding a sentence here to segue into the next section (e.g., “This presents challenges for developing consistent spatial products for marine spatial planning”).*The text was modified in the discussion to include all the wording and editorial changes suggested by reviewer 2.

**Responses to reviewer 3 (Cook)**

We thank Adam for his constructive comments and for his encouraging words.

* *Reviewer: Line 241– The database isn’t really the source of the information its the storage mechanism. The source is the survey.*The wording was updated to clarify this.
* *Reviewer: Line 271– at least 2 separate trips*

This is now updated in the text.

* *Reviewer: Figure 3– Sable Island is missing. Colour coding by depth grouping would be helpful. Table 1– Some explanation of 443-445 is needed.*Table 1 (now Table 2) and Figure 3 were modified, and some text was added to section 2.2 to address the reviewer’s comments about color-coding the strata map by depth, and the mixed depths encountered in strata 443, 444, 445 and 459. The revised map in Figure 3 is inspired by Figure 1 in Cook and Bundy (2012) and strata with mixed depths are now clearly identified.
* *Reviewer: Line 301– Abundance is also captured.*   
  Both the abundance and biomass indices are calculated in our analyses, but only one is reported in the figures. The text was clarified to better explain this nuance.
* *Reviewer: Line 313– An important consideration which is not often captured is the move from spring scales to electronic balances, off the top of my head it was early 90s, but would be worth noting.*Two sentences were added to section 2.3 to document the change from spring scales to electronic balances, and how it changed the error structure of weight measurements.
* *Reviewer: Line 320– Switch numbers to 200 and 1000 (order of numbers smallest to largest or people wonder if typo).*   
  Table 2 (now Table 3) was changed so that category LI is described as “between 200 and 1500 catch records”. Stratum areas in square kilometers in Tables 4 to 6 (now Tables 5 to 7) were rounded to the nearest integer.
* *Line 333– type=1.*  
  Added “type=1” in the sentence about using representative tows in section 2.4.
* *Reviewer: Line 335– So no correction factors? Not even for gear type with different wing spread and hence different trawlable units? I recommend using the wing spread differences. I assume you are converting all densities to biomass or abundance per km2 as you present all strata areas as such so the wing spread is important to mention.*More details were given about the standardization of catch data (which is by trawled distance only), and an additional table (new Table 1) detailing the vessels and fishing gears was added to the document. Section 2.4 now explains why no correction factors were applied to the catch data.
* *Reviewer: Line 351– Stratified mean biomass. Should be using bootstrapped CI’s, standard normal theory does not work for these types of surveys (Smith 1997).*We now cite the paper by Smith (1997) in Section 2.4.2 and note in the text that the Cis could be overestimates of the true stratified variance. We also note that negative estimates are presented as zeros in Section 2.5.2.
* *Reviewer: Line 374–More details on the Perry and Smith 1994 analyses / plots is needed. No many are familiar with this (although that disappoints me).*An additional sentence was added to section 2.4.6 to provide more details about the Perry and Smith plots. However, for brevity, we decided not to repeat the equations appearing in Perry and Smith (1994).
* *Reviewer: Line 381– Total abundance was not defined above, only biomass.*Both the abundance and biomass indices are calculated in our analyses, but only one is reported in the figures. The text was clarified to better explain this nuance.
* *Reviewer: Line 430– Need to define median abundance, not defined in analyses above.*

The methodology associated with the density-habitat habitat selection is now described in more details.

* *Reviewer: Line 385– Please fix the case on yhi .*The density-dependence habitat selection model equation in section 2.4.7 was corrected.
* *Reviewer: Line 399– Probability of occurrence should be reported as design weighted area occupied.*The probability of occurrence presented is the number of sets with capture of a given species divided by the total number of sets. The figure showing the distribution indices now includes the design-weighted area of occupancy.
* *Reviewer: Line 409– So were these CI’s the bootstrapped? Different from what was reported above in analysis section.*   
  The confidence intervals are based on the stratified variance. We realize that these variance estimates are likely larger than those obtained by bootstrapping (as described in Smith 1997), and made a note to that effect in Section
* *Reviewer: Line 411– Plus and minus 50% of the SD seems a little strange to me. Perhaps a range of values, like the IQR.*To include some suggestions made by reviewer #3, the figure showing the indices of distribution was modified from the one that appeared in the draft document. It now includes the surveyed area, the design-weighted area of occupancy, D75% and D95%.
* *Reviewer: Line 416 on–What category of species get these plots?*Category LF species – this is now updated in the text. The fact that figure Types C, D, E and F are for Category LF species was added in Section 2.5.
* *Reviewer: Line 484– Did she really use quintiles? I didn’t see the paper, but wanted to check if was spelling.*Line 484, the report by Serdynska et al. did use quintiles, and we have also updated the reference as the report was recently published.
* *Reviewer: IDWmaps– In caption describe what P(occ) represents.*

The caption for IDW figures now describes what P(occ) represents. Note that P(occ) is also described in section 2.5.1.

* *Reviewer: Range by year plots– I assume its a loess smoother, pls include in caption.*The caption for the temporal evolution of distribution indices now describes the loess smoother. Note that this is also described in section 2.5.2.
* *Reviewer: Condition plots– Lots of lines going on in these plots, need to describe in the caption what they represent. I would suggest changing the data series to type = ’p’ to remove at least one of the sets of lines.*

The condition plots were modified based on the reviewer’s comments, and are now clearer. The figure captions now describe all aspects of the plot.

* *Reviewer: Perry and Smith plots– I think you have these lines labelled incorrectly. The Depth, Temperature and Salinity lines should not vary by species, the catch weighted lines will vary. The solid black lines labelled ’Catch’ are constant across species whereas the dashed lines vary. Also it is useful to show where the maximum difference between catch and sampled. Hate to be that guy, but I really like Figure 55 https://publications.gc.ca/collections/collection\_2013/mpo-dfo/Fs70-5-2013-024-eng.pdf, shows the time series of habitat sampled, selected and where the*

*maximum deviance between cumulative distribution curves occurs.*The “Perry & Smith plots” (Type E figures in our report), were indeed labelled incorrectly, the survey-level and the species-specific empirical cumulative distributions are now correctly labelled. Additionally the 5th, 25th, 50th, 75th and 95th percentiles are now drawn as lines on the plots.

* *Reviewer: DDHS plots Need to define what the red and black lines represent in these plots.*The red and black lines were removed from the DDHS plots.