**Response to reviewers**

**Thank you to the reviewers and editor your helpful comments. They have definitely helped to improve the manuscript making it more rigorous and easier to read.**

**Editors comments**

1. *In particular, the role of climate in the forest dynamics should be shown by statistical analyses in your manuscript.*

**To deal with this issue we have carried out a statistical analysis of temporal changes in growing season temperature from 1964-2013. This analysis shows that there has been a consistent increase in temperature in the area our study site is located in. We also show that though there has been no obvious change in growing season precipitation during this time there have been a series of droughts. Of particular importance is the finding that there was a drought that caused similar climatic water deficits to those observed in 1976 in 1995. Previously, much of the mortality in the forest was attributed to the 1976 drought, our results suggest that the 1995 drought and other less severe droughts may have played a role. We now make reference to this in our results (lines 316-322) and discussion sections (lines 386-413).**

Unfortunately, because surveys of subplots in our study site were carried out either 3 or 5 times over the period 1964-2014 it is difficult to directly analyse the role of climate in the dynamics of forest structure. Our data lacks the resolution to pinpoint the effects of individual droughts. We highlight this issue in our discussion (lines 417-421).

1. *Moreover, the discussion about the specific mechanisms involved in tree mortality and differences on regeneration patterns among species should be much improved.*

**To address this concern we have added a section (see lines 401-413) that details the mechanisms by which drought is likely to cause death of beech trees. In our original manuscript we do not discuss differences in regeneration between species, because it is not clear that there are any. Over the 50 year time period there have only been 14 new individuals recruited. Rather than there being any difference in recruitment between species, the differences in community composition of mature trees result from relatively high mortality of large beech and oak and low mortality of smaller holly trees. There are details of this in a manuscript we currently have in preparation which models mortality and recruitment at the site. However, to address this concern we have added more detail to a section on recovery and how this relates to resilience (see lines 457-467).**

**Reviewer #1 comments**

1. *Using long-term data on tree composition, density, basal area and understory plant composition and cover, the authors present evidence of significant forest decline over 50 years. With minor revisions I believe it will make a strong contribution to Forest Ecology and Management…. The conclusions with respect to forest management and conservation are broad in scope and apply to forests well beyond the study area.*

**We thank the reviewer for these comments.**

1. *Throughout the manuscript you refer to "pests and diseases". These terms have strong negative connotations and overlook that many of the insects and pathogenic disturbances are native to the forests and important for ecosystem function. I suggest minor rewording to differentiate important biological disturbances from introduced pathogens and shifting disturbance regimes driven by climate change.*

**We agree with the reviewer that many insects and pathogenic disturbances, especially those native to a given forest, can play an important role in ecosystem functioning. To meet the request of the reviewer we have removed the term ‘pests and diseases’ and replaced it with more neutral language. We have also added a sentence in the introduction pointing out the importance of insects and pathogenic disturbances in structuring forests (lines 46-48).**

1. *L 316-319. The majority of losses in BA were due to mortality of beech and oak trees with DBH >45cm. What are the average lifespans of these two species? Do you know the ages of the large, canopy dominant trees? Or is there information on when the forest established and if it was initiated by a stand-level disturbance? If the trees are similar in age and established following a stand-replacing disturbance in the past, could the observed mortality be the decline and death of a post-disturbance cohort? It seems unlikely that such a large proportion of trees would die simultaneously over a 50 year period simply due to stand development, especially given the compelling discussion of possible causes of tree death. Nevertheless, disturbance and stand dynamics could explain some mortality but are not addressed in the current discussion. One or two additional sentences in the study area description or discussion could easily address this possibility.*

**We thank the reviewer for this useful observation. Beech and oak trees can live for up to 500 years (Newton et al., 2013) and it is likely that some of the trees that died were approaching this age. We do not have estimates of the trees ages from dendrochronology at our site, but using measurements from other sites in the New Forest many of the large canopy dominants are likely to be 150-350 years old (Newton et al., 2013).**

**Regarding the history of the site, it is thought that it was used for harvesting of timber in the 1500’s. In 1870 part of the woodland was fenced, resulting in increased regeneration (Mountford et al., 1999). Since the 1950’s grazing pressure is thought to have increased, resulting in reduced recruitment (Mountford et al., 1999). However, there is no record of any large stand-level disturbances that might have led to the trees of similar age being dominant. It is thought that this site has maintained forest cover for at least the past 500 years. Even if the forest was established following a large disturbance more than 500 years ago it is unlikely that any pioneers would have survived long enough to still be present in the forest today.**

**We also acknowledge that we cannot precisely determine the causes of death. However, given that some subplots showed increases in basal area, it is likely that some mortality was due to self-thinning. One disturbance that may have caused additional mortality is a large storm in 1987. We have added extra detail to lines 382-384 to address this concern of the reviewer.**

1. *The supplemental information is too long. I recommend replacing Figures S1 and S2 with one table. Omit Figures S3 and S4 as they do not present statistically significant results, as stated in the text. The numerous tables on tree density models could be replaced with just two tables - Tables S1, S3, S5, S7, S9 and S11 can be combined into one table with subheadings for each of the tree size classes; the complimentary statistics in Tables S2, S4, S6, S8, S10 and S12 can be combined as well. Similarly Tables S13, S15, S17, S19, S21, S23 and S25 can be combined; S14, S16, S18, S20, S22, 24 and S26 can be combined. Note, there are two versions of S23 and S24 - I suspect there are actually 28 supplemental tables rather than 26.*

**We thank the reviewer for this observation. We have revised the supplementary information accordingly. Specifically we have removed Figures S1 and S2 and summarised this information in Tables S1 and S2. These tables also include the information previously contained in Tables S2-S12. Tables S3 and S4 cover the information previously given in Tables S13-24. Tables S25 and S26 have been cut from this section. We have removed Figure S4 but retained figure S3 (now Figure S2) because we feel that though the result is not statistically significant, it is still important.**

1. *L285 Write out "1000" to start the sentence or revise so it does not start with a number*

**We have revised this text as requested (see lines 245-250), to now read “We performed 1000 bootstrapped iterations to generate median values and 95% confidence intervals of Moran’s I values at lag distances of 20-1000 m, the minimum and maximum distance between subplots in the same transect.”**

1. *Figure 2 - Revise the title to indicate that the figure includes data from the enclosed transect subplots (eg move this information from the end of the caption to the title)*

**We have now removed this figure and combined it with Figure 1. We have modified the caption to reflect the concerns of the author, to make it clear that this figure includes data from both the enclosed and unenclosed transect subplots.**

1. ***Reviewer #2 comments***
2. *First, the paper is largely focused to demonstrate non-linear responses and thresholds, but the evidences in favour of them is weak because (1) non-linear response is a trivial result if mortality is caused by extreme conditions in a single year determining mortality close to this date; (2) the regeneration threshold is not calculated, instead it is established "a priori" by authors. So, I feel that this topic of state transitions is oversized (for instance in Introduction). In fact, the paper mostly describes temporal patterns, at a quite coarse scale, and the involved feed-back mechanisms are not very much documented.*

**We thank the reviewer for this insightful comment. We agree with the point that a non-linear temporal response is trivial if extreme conditions cause widespread mortality in a single year. However, with droughts in forests it is not clear that this is the case. Beech trees affected by drought can apparently die many years after the initial perturbation (Mountford et al., 1999; Packham et al., 2012; Peterken and Mountford, 1996). However, in the case of basal area the precise trajectory of the decline is not as important as the effects of the decline. We have shown that as basal area declines there is a strong non-linear shift to a grass dominated system, which provides evidence of transition to a non-forest state when dieback occurs.**

**Regarding the ‘regeneration threshold’ we agree that we established this *a priori*. To address this we have rerun statistical analysis to compare the trajectories of subplot BA following decline. This analysis shows that, on the whole, subplots do not recover BA following decline. This presents stronger quantitative evidence for our assertion that the forest is undergoing a transition. We detail these results on lines 325-329 and discuss the implications on lines 457-467.**

1. *One of the aims of the study is to identify the ecological processes that might be driving the changes, but while drought is identified as a main driver of the observed mortality, this fact is supported by previous work not by this study, which does not provide further data about the contribution of climatic conditions on the observed patterns. I would recommend a more accurate demonstration of the role of climate in the reported forest dynamics by checking climatic series. In fact in lines 474-478, when discussion treats about the contribution of several droughts the rationale will be reinforced by climate documentation, that should be preferably corroborated by statistical analysis.*

**We agree with the reviewer on this point. We have subsequently used data from the closest weather station to show that mean growing season (April-September) temperatures have increased since 1964. However, there is no significant trend in precipitation. Previous work identified a drought in 1976 as a major source of mortality (Mountford et al., 1999), and using the climatic water deficit (Lutz et al., 2010) we show that in 1995 there was a drought of similar magnitude. In addition we show that there have been three droughts since the year 2000. Given that temperature is increasing, even without significant trends in precipitation this increases the likelihood of stress from water deficits in our study area. Thus, while we cannot say that the mortality we have seen is definitively caused by climate change, drought is likely to have caused increased mortality in the forest. See lines 192-208 for the methodology that we used for these new analyses and lines 316-322 for the results of this analysis. See Figure 2 and lines 386-413 ofr a more detailed discussion of these results.**

1. *Overall some parts are really too extended, particularly in Introduction and Results. Introduction is sometimes lengthy and repetitive, particularly when dealing with the shift or state transition topic. For instance the third and fourth paragraphs in Introduction may be merged and simplified. Then, the third paragraph in the middle of them, which deals with long data sets, does not help to keep the continuity of reading and it has more sense closer to the final paragraph where the study system is described.*

**We agree with the reviewer on this point and have significantly shortened the manuscript as a result of these comments. The introduction has been cut by approximately 300 words. In this section we have merged the two paragraphs relating to resilience/transition as suggested by the reviewer (see lines 66-79). We have also moved the section on long-term data sets so it is now the penultimate paragraph, just before the study aims (lines 81-90). We have also trimmed the results section by approximately 500 words.**

1. *In Methods, Tanner Index is calculated in a species basis, but it is not clear how it is used to obtain integrated measures of composition changes in the community (i.e. Sorensen index calculations include all species, but in the paper it is not clear how it is done with the Tinner index).*

**The Tanner Index used in the paper is a modified Sorensen index that takes account of both stem density and species basal area. We have altered the text to communicate this more clearly (see lines 163-170).**

1. *About data analysis, collapse is in fact not a gradient, instead it is a categorical variable. Then, why not call it "collapse status"? Even better, have the authors tried to analyse BA loss as a continuous variable (then it would be a real "gradient")? It seems that with the current analysis, there is information about the variability of BA loss that could be missed. Note also that to analyse "changes in BA" in relation to 2 categories defined by BA loss is redundant, unless when the analysis is performed separately for each species or for non-dominant ones. Also the establishment of the 25% BA loss criterion to define collapse could be suspicious of arbitrarity.*

**We thank the reviewer for this observation. The division of subplots into ‘collapsed’ and ‘stable’ was purely to show how plots with different basal area dynamics differed in their temporal changes in overall stem density, as well as stem density of particular size classes of trees. We agree that the use of the 2 categories for analysis of basal area is somewhat circular, and thus have removed this section replacing the previous Figure 1 with a histogram showing different dynamics of plots. However, without using a categorical variable of collapse, it would not be possible to see how subplots that were apparently very similar in 1964 differed so much by 2014. In particular our figure on grass cover (now Figure 4) shows that plots that collapsed and those that didn’t had similar grass cover in 1964, but differed once they had collapsed in 1996 and 2014. We do in fact analyse the BA loss as a continuous gradient in our analyses of thresholds, showing that there is non-linearity of some variables (grass cover, ground flora richness, tree community composition) and not in others.**

**We agree that the establishment of 25% basal area loss as a criterion to define collapse could be seen as arbitrary. However, there is also a clear peak in our histogram of basal area loss (Figure 1a) around this point. This suggests that when plots did lose basal area, they tended to lose 25% or more of their original basal area. However, defining any categorical variable is inherently somewhat arbitrary.**

1. *The Results section is too long and a bit confusing (writing in lines 333-334 is unclear; lines 364-365: at which date?), with too many details. In some case, the information is repeated, for instance the two first paragraphs can be merged, the paragraph of lines 381-388 can be simplified, and so on. Overall, I encourage to highlight the results message, then illustrate it with quantitative data; but try to limit information about trivial results, such as that collapsing subplots reduced BA or density (f.e. lines 336-339); in contrast emphasize the results about the effect of initial BA and density on collapsing.*

**We have now significantly shortened and simplified the results based on the concerns of the reviewer, cutting approximately 500 words from the original. Our results section now largely discusses the results of quantitative statistical tests.**

1. *If spatial correlation was not significant (an important result nevertheles!), the paragraph can be reduced and the figure can be moved to supplementary material (lines 372-378)*

**Following the advice of the reviewer we have moved this figure to the supplementary materials and reduced the amount of detail in the paragraph (lines 304-307).**

1. *In discussion, when talking about transition states and feed backs it would be useful to go into the involved mechanisms that in this case implies mortality/survival and poor replacement. Here, the discussion about the specific mechanisms involved in tree mortality is very poor (lines 478-481). Similarly, there is not discussion about mechanisms about differences on regeneration patterns among species.*

**Following the advice of the reviewer we have more detail throughout the discussion on the mechanisms involved in the transition we present in our study as well as cutting information considered irrelevant. We have now included a section detailing the potential effects of drought on beech and the mechanisms that make the species prone to drought (lines 386-413). We have also included a section detailing the lack of regeneration in the forest, and the causes and consequences of this (lines 457-467).**

**References**

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