AUTHORS: We would like to thank the referee for their further comments on this work. We took their suggestions into account as much as possible and modified the Manuscript accordingly. Below you can find our response to individual comments. Relevant changes are highlighted in boldface.

REFEREE: As a general statement, there is a number of statements in the work that take a rather speculative tone. There is more cases of "<some result> may be due to <some explanation>" than I quite feel comfortable with. Most of these proposed explanations are not tested by any further analysis. While this is not wrong, or a reason to reject the paper, it makes for a lack of rigour which, I think, only serves to weaken the value of the work. I will leave it to the authors to take on or leave aside the task of streamlining their material accordingly.

AUTHORS: We have tried to streamline the paper where possible. As we are presenting non-correlations we feel the need to speculate slightly more as to why we are not seeing any correlations. However, we moved these points to the end of the respective discussion section and have tried to put less emphasis on more speculative ideas.

REFEREE: With respect to my first major comment from the first report, while the authors have added some text and a comparison of the velocity dispersions in Figure 2, I feel that the discussion around Figure 8 and Sec 5.2 is still unbalanced. As I had outlined in the first report, the evidence mostly supports an explanation that is
related to secular processes that are applicable to the particular galaxies in the WISDOM sample. Indeed, while there is scatter in the velocity dispersion, the case for higher dispersions in the WISDOM sample is clear from Figure 2. I believe the authors should be much more clear about the particular selection effects that apply to the WISDOM sample when interpreting Figure 8. In light of these effects, it is not warranted to read too much into the lower central molecular gas concentrations in the sample of this work compared to GB+21. I urge the authors to be clear about this: I submit that the current discussion in Sec 5.2 is somewhat circumspect.

AUTHORS: We have added in the text that the WISDOM sample are mostly early type galaxies but the other properties are similar to the GATOS sample (as clearly illustrated in Figure 2). It is unclear why the BH would care about the large scale properties of the galaxy so this indeed supports the secular processes argument. However, we have also added discussion about how the parameter space studied by Garcia-Burillo+21 represents a turnover region where below a certain luminosity the molecular gas concentration is set by secular processes whilst above this limit we could be seeing the effects of AGN feedback. We hope this balances out the discussion on this section.

Moving on to more specific remaining issues:

REFEREE: 1) On the topic of the X-ray luminosities of the sample galaxies, the paper now incorporates a Chandra archival study for the majority of the Lx measurements. My understanding from the current text is that only the 10 or so galaxies that do not have entries in Bi+ (2020) have been assessed for the effects of XRB and CGM contamination. However, please be aware that the ~ 1" PSF of Chandra (assuming all galaxies were observed on axis) still subtends hundreds of pc for most of the galaxies in the WISDOM sample. Even among the Bi+ (2020) Chandra fluxes, there could be low-luminosity systems where XRBs (which also tend to be centrally concentrated in galaxies) could contaminate or even dominate the Chandra fluxes. I would advise that the authors state this clearly in the their discussion of X-ray luminosities.

AUTHORS: We thank the referee for bringing up this point. However, we note that the catalogue of Bi et al. (2020) is a dedicated collection of 2-10 keV luminosities obtained from Chandra observations, and derived with the precise aim of including only nuclear emission from the unresolved AGN core. This means that the extraction and analysis of the X-ray spectra have been carefully carried out with this aim in mind on a source-by-source basis (taking into account also possible XRB contamination). While some contamination may still be present, this is clearly negligible in these cases. We modified the text in Section 2.3.1 to briefly explain all the above (mentioning also the possible residual XRB contamination)

REFEREE: 2) In their response, the authors state the following: "We cannot indicate what regimes one if preferred other the other. Rather you would use X-ray as the accretion measure in radiative mode AGN and [OIII] in kinetic mode AGN."

I don't understand the basis for this statement. Why is [O III] preferred as an accretion measure in kinetic-mode (which I take to mean radio jet-dominated or LERG) systems? Both X-rays and the AGN-ionised part of the [O III] line can be attributed to photoionisation in radiatively-powerful AGN. In jet-dominated or radiatively-weak AGN, there is no guarantee that [O III] is powered by accretion. It may come from large-scale shocks, or even other excitation mechanisms such as AGB stars (in low-luminosity LINERs).

Figure A1 is a bit concerning, because the Lbol based on [O III] is almost always much higher than Lbol based on Lx, by orders of magnitude at the low-luminosity end. This strongly suggests that the accretion rate estimates based on [O III] are exaggerated and probably incorrect. I suggest that the authors carefully reconsider the validity of any analysis that is built upon [O III]. As [O III] is not really used in any of the key results, the authors may consider removing their [O III]-based content without any impact on the paper.

AUTHORS: We have clarified that X-ray is the best proxy in radiative mode AGN due to X-rays in these sources expected to be produced in the accretion disc coronae. However, in kinetic mode AGN the accretion disc is expected to not be present so using X-rays is less reliable as an accretion rate tracer. However, [OIII] should be a good tracer of accretion rate in both radiative mode and kinetic mode AGN. We want both X-rays and [OIII] present to check if there is correlation in using one tracer but not the other for completeness.

We noted the scatter in Figure A1 and have explicitly stated this in the text. We believe the scatter in Lbol is due to the mix of radiative mode and kinetic mode AGN we have in our sample and in the kinetic mode AGN the X-ray will not be estimating Lbol accurately.

REFEREE: 3) Sec 1, Line 1: "almost all massive galaxies (> 10^9 Msun)."

Previous well-established works have defined galaxies with mass < a few 10^9 Msun as "dwarf galaxies" (e.g., Reines & Volonteri 2015) and find a much lower AGN fraction in such systems. While it is possible that most of these galaxies have IMBHs or SMBHs, it is certainly not well-establised that "almost all" have them at this low mass end.

This is quibbling, but it is important to make well-evidenced and well-accepted statements in an introduction, so it is troubling that the very first line of the paper has such a loose and poorly-motivated statement.

AUTHORS: We have updated this statement.

REFEREE: 4) Sec 1: "the studies have been conducted on small samples of about 10 objects spanning relatively small ranges of Eddington ratios".

The stated Eddington ratio range is about 4 orders of magnitude. Most readers would consider this to be a substantial range, not a small range.

AUTHORS: We have changed this to say they only look at the same AGN types

REFEREE: 5) Sec 1: "The total molecular gas mass of a sample of nearby ETGs (most of which are LERG hosts) have also been observed to correlate with the AGN jet power, providing further evidence of a close connection between the two (Babyk et al. 2019)."

A close reading of the relevant part of the Babyk+ (2019) paper shows that they report at best a marginal correlation. The visual appearance of the correlation in their paper shouldn't be used to judge its strength, because it can be driven by distance-based biases, as noted in that paper. Please restate accordingly. The lack of a strong physical correlation in Babyk+ should also reflect on the statements made in Sec 5.1.2.

AUTHORS: We have changed the phrasing from strongly to weakly correlating.

REFEREE: 6) Sec 2.3.3: "In all the other cases, the ratios are > 1, as typical in AGN-dominated objects."

Whether the emission is "AGN-dominated" or not also depends on the location in the BPT diagrams, which minimally require [N II]/Halpha. A ratio of 1 is well into the star-forming part of the diagram for objects with low [N II]/Ha. Assuming that WISDOM galaxies have a high [N II]/Ha ratio, typical of more massive galaxies, an [O III]/Hb ratio of ~ 1 is in the composite part of the diagram and could be produced by star-formation or a mixture of stellar and AGN emission. It certainly isn't in the "AGN-dominated" part of the diagram, where [O III]/Hbeta ratios are ~ 10.

AUTHORS: We have gathered the [NII]/Halpha ratios as well and produced the BPT diagrams for the 18 galaxies with all the ratios available. We find all but one of these 18 galaxies fall outside the star-forming region. We have included text that details this in Section 2.3.3.

REFEREE: 7) Sec 4.1: How is jet power estimated?

AUTHORS: We have moved how the jet power was estimated into this section.

REFEREE: 8) Sec 5.1.1: I still disagree with the balance of this discussion. The authors have brought up a host of potential, but untested, reasons to explain why their result differs from Izumi+, when studies that are better analogs (equivalent molecular gas tracers, for e.g.) are only given a line of acknowledgement. Rather, I feel the authors should start off this section noting that their result is consistent with and supported
by these earlier studies, building in a more complete comparison to highlight the differences in luminosity and CO mass that WISDOM may probe. For e.g., directly in Figure 4, an inclusion of the range in Lx and MH2 that GB+21 probe would be very handy to firm up this comparison.

AUTHORS: We have rearranged this section to put more emphasis on the works that agree with us and then move into how these works disagree with Izumi+16. We have included the ranges of Lx and MH2 that GB+21 probed in the text to aid in comparison.

REFEREE: 9) Sec 5.1.2: Babyk+ (2019) primarily report a link between the X-ray atmosphere in their galaxies and the molecular content, which they assert is the primarily relationship in their paper. In BCGs and dense groups, the hot gas mass is correlated with the radio power due to the feedback cycle that maintains the intra-cluster medium in such systems. This may be the primary driver between the weak radio and molecular gas trend that they report.

AUTHORS: We have included this detail in the discussion as to why we may not see a correlation between the circumnuclear gas mass and the radio emission from the AGN.

REFEREE: 10) There remain a few presentation issues in the paper. Figures 1 and 2 have illegible axes and key labels. In my reading, I still found a few remaining typographical errors, especially in the newly edited text in bold.

AUTHORS: We have tidied up Figure 1 whilst also increasing the size of the axis labels to improve legibility. For the same reason, we have changed the histograms in Figure 2 to violin plots and increased their size. We also carefully reviewed the manuscript to catch any typographical errors.