<u>Reviewer 1 Comments</u>

In this manuscript, the authors present a compelling hypothesis of how compost in combination with winter cover crops can lead to accumulation of aromatic-rich subsurface soil carbon. The hypothesis is complex but plausible whereby cover crop roots improve soil structure/porosity facilitating greater transport of soluble C and nutrients derived from the compost directly to the subsurface where this C can be stabilized. While the hypothesis is compelling, unfortunately, I do not think the authors have collected the right data to test this hypothesis.

Response: We would like to again thank the reviewer for their feedback. We have addressed their concerns by making clearer what evidence we have for the different components of the hypothesis, better clarifying where there are limitations of the data, and drawing more on existing literature to support our synthesis as described below.

Utilizing a long-term field trial should be a great way of trying to address this hypothesis. However, a major limitation of the study is that there is no compost-only treatment, so there is no way to separate the effect of compost alone from the interactive effect of compost and cover crops together. There is nothing the authors can do about this except recognize this as a limitation of the study design.

Response: We have added a sentence at the end of section 2.1 clarifying that there is no compost-only treatment, and restructured the titles of the various discussion sections to remove all references to the impact of compost alone.

A major feature of the author's hypothesis is that cover crop roots have created greater porosity that facilitates greater water flow down the soil profile. The data simply do not support this notion. The authors find no difference in saturated hydrologic conductivity at 35 cm (although there was a trend for much greater variability in the compost + cover crop treatment) and no difference in soil aggregates across treatments.

Response: We have extensively edited section 4.1 to include data from previous studies at Russell Ranch that support our hypothesis of increased water storage and movement in cover cropped plots. We have ensured that any discussion of increased porosity in the manuscript is clear that this is an inference, and supported this inference with previous literature where appropriate.

The only significant difference was greater water content in the two treatments with cover crops but the authors did not measure bulk density in the 2018 samples and they did not measure porosity so it is difficult to come up with an explanation for this observation.

Response: We have included a discussion of several previous studies at Russell Ranch that showed little to no change in bulk density, increased infiltration and increased moisture holding capacity under winter cover crops. We have also included a discussion of the impact of small Ksat sample size, cover crop root type, and the lack of support for other potential hypotheses for increased moisture content.

The next major component of the hypothesis is that compost leads to greater soluble C and N. The authors use salt-extractions of soil samples at four time points during the 2018 season to generate supporting data. Salt-extractable C is an interesting carbon pool (a potentially soluble pool of C) but there is ample evidence that this lab-extracted pool has little relationship to DOC when collected in lysimeters in the field. Without direct collection of DOC diffusing and advecting down the soil profile it is difficult to say whether the differences in the extractable pools are actually leading to more DOC flux to the subsoil under compost addition.

Response: We have changed all references of DOC in the manuscript to EOC, and provided support for our decision to use EOC instead of DOC via lysimeter in section 2.5. We have also noted that measurements of EOC are common in the literature, and though they are not exactly equivalent to DOC, EOC measurements can still be used to draw inferences about the presence and movement of soluble carbon.

We have supported our conclusion of increased soluble C flux to subsoils in the ORG systems by highlighting our multiple lines of evidence:

1) More EOC in ORG subsoils
2) Observations of higher water storage, and potentially increased infiltration under cover crops
3) Greater amounts of soluble organic carbon in compost, and
4) Reduced subsoil microbial stress indicators under ORG systems (attributed to more soluble C and nutrient availability)

The third component of the hypothesis relates to the preferential partitioning of DOC chemistry down the soil profile. The evidence here is particularly weak. Mid infrared FTIR spectroscopy is not a quantitative analytical tool for determining abundance of specific compounds. If it were, labs wouldn't spend millions of dollars on more precise equipment. FTIR spectroscopy is good for identifying compounds in simple mixtures but not for quantifying their abundance in simple or complex mixtures (and soil is one of the most complex there is). Peak features depending on if they are due to vibrations, wiggles, combinations or overtones all have different relationships between abundance of the specific bonding environment and absorption – basically, you would have to prove that there is a linear relationship between "aromatics" and those two peak features in order to do a spectral subtraction and have any confidence that the difference spectrum represents real differences in chemistry.

Response: We have modified the introduction, results and methods (section 2.8) sections to support our approach using FTIR spectral subtractions for pseudo quantification of SOM functional groups. We have replaced the previous Figure 8A and

B with a Kubelka-Munk corrected spectra on a common y-axis to correct for non-linearity of concentration and absorbance in our spectra.

We have also included a table of peak intensity ratios of aromatic to carboxyl moieties [v(C=C):vas(COO^-) (1662 cm^{-1}:1631cm^{-1})] to support our observations of a change in SOM composition over systems and depth.

I also find it problematic that all treatments have showed the same increase in carboxylate functional groups over 25 years – wouldn't we expect the conventional treatment to be more or less at steady state, so we shouldn't see the same changes as seen in the cover crop and compost + cover crop treatments?

Response: When shown with the KM correction, the spectral contributions for the change in carboxyl groups in CONV are no longer similar to those in CONV+WCC and ORG systems.

Lastly, what is the actual magnitude of the "increase" in aromatic features in the compost treatment over the conventional treatment? There are no units on the y-axis. The authors have replicates so they could run statistics to see if this increase was significant.

Response: We have included figure A6 to highlight the very small amount of variation between spectra that were averaged for subtraction. We have also included a section for the caption of figure 8A to highlight that the spectra are shown on a common y-axis scale and that they are only offset for ease of comparison.

Finally, the microbial data is not well integrated into the hypothesis. Would lower microbial stress result in greater carbon stabilization via increased carbon-use efficiency or would it result in greater priming and potential loss of older SOM? Regardless of what microbial stress means for carbon cycling, the data were non-significant across treatments. The only significant difference was in Gram+:Gram- ratio but the ecological significance of this difference was not described.

Response: We have better integrated both our explanation of the microbial stress indicators and their integration into the manuscript. We highlighted that the microbial stress ratios are primarily meant to support our inference of increased soluble C and nutrient at depth, but have refrained from using them to draw detailed conclusions about the microbial community.

Just to reiterate, I think the hypothesis laid out here for subsoil C accumulation under compost and cover crops is entirely plausible but the evidence in this study to support the hypothesis is not particularly strong.

Response: We again thank the reviewer for their constructive comments, and hope that the edits we have made have provided a more convincing argument.

Reviewer 2

Summary:

This manuscript leverages data from a long-term agricultural experiment at the Russell Ranch in California and a year of more detailed measurements to explore interacting cover crop and compost effects on subsurface soil carbon dynamics. Authors blend historical measurement of carbon stocks with present day analyses of carbon (bulk C, FTIR), nutrients (Mehlich-III), soil physical properties (aggregation, moisture content, and hydraulic conductivity), and microbial biomarkers (PLFA) at four sampling dates. An ANOVA was used to assess the effect of time, depth, and management, with subsequent separate analysis of differences between management treatments at each of three depths.

Although the experimental design and methods are sound, there is a disconnect between the objective to assess interaction of cover crops and compost and the data analysis. The discussion ties in interesting concepts such as the 'cascade theory' and microbial stress indicators that must be brought up further into the introduction to create a threat throughout the paper.

RESPONSE: We appreciate the reviewer's comments, and have extensively rewritten the introduction, results and discussion to take advantage of their comments.

Below please find my recommendations to reframe the paper and utilize historic data, specific questions and a few line edits for authors. Most edits occur in the first half of the paper, which may help to connect the methods and results into the compelling discussion.

Title: I recommend making this more specific. The final sentence of the discussion states that "care should be taken when applying these results to different soil types and climates"; therefore, adding the soil type or climate (or both) into the title seems prudent.

RESPONSE: We have changed the title to "Synergy between compost and cover crops in a Mediterranean row crop system leads to increased subsoil carbon storage"

Abstract:

Throughout the paper, can authors use the treatment names as in the original experimental dataset (Wolf, 2018 page 6): CONV = CMT conventional maize-tomato, ORG=OMT organic maize-tomato, and (page 5) WCC – winter cover crop? I understand that Tautges and Chiartas 2019 used the CONV, ORG notation, but a brief explanation would be helpful.

RESPONSE: We have included a short sentence in section 2.1 to highlight the difference in treatment names.

The theory of cover crops providing a macropore system for transport of DOC is interesting, but the data do not support this theory (no measurement of porosity, change in bulk density, or changes in soil hydraulic properties). It is appropriate for a discussion, but I might exclude this as a main finding from the abstract.

RESPONSE: We have removed this inference from the abstract and replaced it with a more general statement.

Introduction:

I appreciate that the abstract and introduction mention soil health, but there is no clear definition or explanation of its importance to the paper. Either simply remove this term and focus solely on soil carbon and microbial processes, or please directly connect soil health and often associated shallow sampling regimes to this "outsized perceived role in ecosystem services".

Response: We have removed any mention of soil health from the manuscript and clarified that surface soils should not be used to answer questions about the entire soil profile.

This is a good argument and dataset to support deeper sampling. Authors may also include references summarized by Mobley et al 2015 in their article "Surficial gains and subsoil losses of soil carbon and nitrogen during secondary forest development": Post & Kwon, 2000; West & Post, 2002 review 360 articles on land use change, with only 10% sampling below 30cm.

Response: We have included the Mobley reference, as well as including several other references on depth of soil sampling.

In this paragraph, please clarify, at what depth are the authors designating topsoil v subsoil for this study?

Response: We have included a paragraph discussing our decision to label 0-15 as surface soil, 15-60 as a transition zone, and 60-100 cm as subsoil.

This first paragraph of the introduction discusses "longer C residence times" of deep soil C, which requires further explanation.

Response: We have included a sentence in the introduction highlighting that subsoil C can be as old as 10^3-10^4 years, as opposed to younger surface C.

Overall, the introduction structure can be strengthened by clarifying topic sentences (e.g., specify cover crops L51) and adding updated references. Can you support the Jenny citation with more modern references, even Brady and Weill Nature and Properties of Soils, or USDA technical information "Designations for Horizons and Layers" in Soil Survey Manual – Ch 3 (https://www.nrcs.usda.gov/wps/portal/nrcs/detail/soils/ref/?cid=nrcs142p2_054253#designations).

Response: We have added the suggested updated references, and clarified topic sentences throughout the introduction and discussion.

The introduction structure may flow better using paragraphs separated into chemical, physical and biological controls or layered as (1) depth; (2) chemistry of C inputs and stabilization at depth; (3) management impacts at depth – specifically cover crops; and (4) management interaction with other factors (microbial).

Response: We restructured the introduction into sections highlighting

1) Importance of deeper soils

2) Definition of subsoils

3) Depth and C chemistry

4) Depth and microbial C processing

5) Support for methods used

6) Depth and the impact of agricultural practices

The introduction touches upon stoichiometry, a critical highly manipulated factor in managed conventional systems that effects soil C storage. To go further in depth on soil chemistry (e.g., at L40), authors can address changes over time in stoichiometric constraints on decomposition (e.g., see Soong et al 2019 "Microbial carbon limitation: The need for integrating microorganisms into our understanding of ecosystem carbon cycling").

Response: We have included the reference and inserted a short discussion of the importance of C and nutrient stoichiometry in microbial biomass formation into the discussion.

Also, authors can mention higher physical disturbances in surface soils (L55), and the types of management associated with cover crops, such as crimping/rolling.

Response: We have included additional disturbance with cover crops as a potential impact in the introduction.

Please also include specific soil type, climate and cropping system when comparing to other studies, otherwise direct comparisons are not particularly informative.

Response: We have added data about soil type and cropping system when making direct comparisons (section 4.4), and have clarified the cover crop mix used when comparing results from studies with cover crops.

Can also cite McClelland et al 2020 "Management of cover crops in temperate climates influences soil organic carbon stocks: a metaanalysis" that analyzed soils only down to 30cm.

Response: We did not include this citation, but instead added Singh et. al (2021) to better focus on cover crops and lysimeter measurements.

Singh, G., Kaur, G., Williard, K. W. J., & Schoonover, J. E. (2021). Cover crops and tillage effects on carbon–nitrogen pools: A lysimeter study. Vadose Zone Journal, 20(2), e20110. https://doi.org/10.1002/VZJ2.20110

As for the sampling strategy by depth, can the authors please describe why they separated out into these depths 0-15, "intervening", and the subsurface as 60-100cm? How do these depths compare to the horizons in these two soils? (Looking up the series descriptions Yolo has A horizons down to 66cm and then C horizons, and Rincon has A down to 20, B 20-100cm. Should the analysis be completed on A and B horizons rather than depth profiles?) How do these depths relate to roots of corn (100cm+), tomato (60cm+) and cover crops (variable)?

Can authors please justify why 15-60cm is combined into a single sample in 2018, when historical data had an additional delineation? (Is it simply limited time/costs or another reason?)

Response: We have included a paragraph in the introduction highlighting our reasoning behind using the depth intervals we have chosen (previous RR work, lack of horizonation).

Please stay consistent with the terms "subsoil" versus "subsurface soil", as depth is a major component of this study.

Response: We have removed all reference to subsurface soil in the manuscript and replaced them with subsoil, except when referencing subsurface drip and subsurface flow.

The overarching question and hypothesis require further editing to clearly lead into the results and discussion. There seems to be a disconnect between the main question and the methods of this paper. The main question includes "carbon formation" (does that mean microbially processed C? or stabilizedC formation?) and "storage processes" (that obviously includes aggregation, but the carbon content of these size classes was not measured). Also, what is meant by the term "SOC-related indicators", does that mean SOC stability or reactivity-related indicators? As written the hypotheses are just predictions, there is no description as to the mechanisms behind the described expected results.

Response: We have reworded the hypothesis and removed references to carbon formation and stabilization processes, as we do not measure any processes in the manuscript.

An interesting hypothesis arises in the discussion around cascade theory, can authors pull that into the introduction? This can provide a way to integrate the study of carbon chemistry (FTIR) and microbial biomarkers that otherwise are not included in the hypotheses.

Response: We have introduced the cascade theory in the introduction section.

Finally, I agree with the previous reviewer comment, that the treatments CONV (fertilizer), CONV+WCC (fertilizer + cover crops), and ORG (compost + cover crops) do not disentangle the effect of compost. I don't think there is there a treatment in the Century Experiment that was maize-tomato plus compost only or fertilizer + compost, but this should be mentioned as a limitation in the study, particularly in the subtraction of FTIR spectra.

Response: We have added a sentence at the end of section 2.1 clarifying that there is no compost-only treatment, and restructured the titles of the various discussion sections to remove all references to the impact of compost alone.

This manuscript covers many aspects of deep soil C and management, no need to emphasize the complicated factors of global change (L87) at the end of the introduction, unless those are also analyzed over time.

Response: We have removed this sentence from the introduction.

Materials and Methods:

Thank you for a concise description of the site and experiment. I recommend authors also add basic climate data such as climate type, mean annual mix and max temperature, mean annual precipitation, and also specific 2018-19 climate data for comparison.

Authors write that the 'horizon information' is available from Wolf et al, but I only can find soil chemistry by depth, not the soil description in that dataset (horizon delineations are online). Can authors add in the horizon depth into the methods for both the Yolo and Rincon soils, and key chemistry such as pH and texture? A table in the materials and methods section could organize all of this soil and climate information for quick reference.

This could also include the other key management notes that will impact DOC transport, such as the conversion from furrow to drip in 2014, as well as information from the 2018-2019 season such as crop planting/harvest dates, total irrigation amount, and the anomalous compost application in September 2019. These details can then be incorporated smoothly into the discussion.

Response: We have included a graph of temperature and rainfall (supplementary figure A5) and a table of soil horizon variables (Supplementary Table A4). We have also included climate and management notes in the Methods section under section 2.1 and 2.3, and in Supplementary Table A5.

The differentiation between the sampling and analysis of the older data and 2018-2019 methods is now clearer. Thank you for the new methods section. However, without hypotheses asking seasonal questions over time – why sample at four time points in a single year? Particularly as the authors state that a single year of data is not sufficient to look at differences at depth (L81-82) to justify use of the historical data. Perhaps authors can create one or two hypotheses for the 2018-19 season, and other for the long-term effects and historical data.

Response: We have included a paragraph referencing the seasonal variation in the data in the discussion, as well as support for sampling at multiple time points during the year in the methods section.

The use of PLFA and FTIR is not justified from the hypotheses or introduction. The use of these techniques, particularly stress ratios for PLFA needs to be explained within a wider context in the introduction.

Response: We have included a paragraph supporting the use of PLFA and FTIR into the introduction.

2.7 Please clarify the statement that 9 out of 18 plots were sampled for hydraulic conductivity (those under tomato). Were half of the plots under corn and the other under tomato during this 2018 sampling? That needs to be included in the methods section. Or are you referencing the full 18 plots of all the Century experimental treatments? Finally, why are 8 dates included for soil moisture content, when soils are sampled only 4 times?

Response: We have clarified that half the plots were under corn, and half were under tomato in the methods section, and highlighted that we only sampled the plots under tomato for hydraulic conductivity sampling. We have also corrected the methods section to state that soil moisture content was sampled 8 times.

2.8 I have some concern over the use of averaging and subtraction of the spectra. What was the variance between the historic soils of 15-30 and 30-60 cm?

Response: We have included figure A6 to highlight the very small amount of variation between spectra that were averaged for subtraction.

What information is provided via subtraction of the conventional plus cover crop from the organic spectra? I am unfamiliar with this subtraction analysis, so I am curious, what information is revealed from subtraction as the reflectance intensity does not represent quantity, but rather soil chemical signature?

Response: We have modified the introduction, results and methods (section 2.8) sections to support our approach using FTIR spectral subtractions for pseudo quantification of SOM functional groups. We have replaced the previous Figure 8A and

B with a Kubelka-Munk corrected spectra on a common y-axis to correct for non-linearity of concentration and absorbance in our spectra.

We have also included a table of peak intensity ratios of aromatic to carboxyl moieties [v(C=C):vas(COO^-) (1662 cm^{-1}:1631cm^{-1})] to support our observations of a change in SOM composition over systems and depth.

2.9 Can the authors please describe the details of the ANOVA. Was this a mixed effect model accounting for the block design? Was there an effect of block? (That difference would be interesting to see due to the two soil types). It would be helpful if the authors state that they checked normality of the data prior to ANOVA.

Response: We have included details about the statistical analysis in section 2.9, including our observation that there was no effect of block, and that we checked for normality.

If variability was high for certain metrics (hydraulic conductivity), it seems there may be some outliers, how were those assessed?

Response: We have clarified that these outliers are likely a function of the low number of Ksat cores and the effect of cover crop roots in our systems.

The lack of differences in the field may simply be due to low power with only three field replicates. Rather than splitting the data by depth to do comparisons between treatments, can the authors run an analysis that accounts for autocorrelation over depth? On that same note, do authors need to account for repeated measures across sampling dates in 2018-2019 and within the historical data?

Response: We were not able to run the suggested analysis due to time constraints, but believe that the results split by depth provide support for our hypothesis. When comparing the historical data and 2018-2019 samples, we only compared data from the August timepoint to avoid issues with repeated measures.

I appreciate access to the data and code used for this analysis. Thank you for supporting transparency in data analysis.

Response: Thank you!

3.1 The cumulative inputs over 25 years are useful, but would be more comparable to other studies if averaged per year. This data also may be well suited for a table including all C inputs and nutrient inputs over the 25 year period (transform Fig 1 to Table 1 using Mg/ha/yr). Perhaps with the level of detail from the Century Experiment on all organic inputs, the statistical analysis could incorporate the treatments as continuous variables (amount of mineral/organic N input) rather than categorical variables?

Response: We have kept the data as a figure, as we believe it aids in interpretation, but have included Supplementary Table A6 which lists average C and nutrient inputs per year over 25 years. We were not able to run the suggested analysis due to time constraints.

L227 If a result is non-significant, than I would remove any interpretation of ‘increase’.

Response: Where appropriate, we have removed the term “increase”, but have also highlighted datas that we believe are indicative of larger trends.

Fig 2. Extremely clear pattern here. Can the significant differences be noted in some way on the figure? I would remove the lines between the points, as there are no actual measurements there, and the trends are obvious.

Response: We have included significance indicators for C change for Figure 3, and kept the lines between the points for figure 2 as we believe they aid in interpretation, but modified the line widths.

Fig. 4 I would change the layout of this figure. You can zoom in on the y-axis and add precipitation and irrigation events. Otherwise, a simple average across the time and bar graph or box plot would tell the story more clearly, since the statistical analysis was not over time.

Response: We have zoomed in on the y-axis, and included precipitation events in figure A5. There were no irrigation events.

L270 Why do authors state “largest seasonal variation” in nutrient data was in June, when only mineral N and DOC were highest in June? S and P were higher in August.

Response: We have corrected this statement to state that DOC and mineral N were highest in June, whereas S was slightly higher in August.

3.7 Authors must introduce microbial stress indicators earlier in the introduction and hypothesis. How does this relate to stoichiometry and soil C stability?

Response: We have included a discussion of PLFA in the introduction, methods and discussion, but have refrained from relating our observations to stoichiometry and C stability as our measurements do not give clear indications for those trends.

Discussion:

Authors list the key finding of increased SOC and then write what I perceive as the hypothesis of the paper: “that high concentrations of mobile C and essential nutrients for microbial activity provided by the compost, combined with the easier movement of water downward associated with a history of cover-cropping, helped

transport the material needed to build C in the subsurface." Having this in the introduction will help to set up the statistical analysis, results, and discussion. However, this hypothesis was not supported by the aggregation data or the hydraulic conductivity data.

Response: We have included this hypothesis in the introduction.

We have also extensively edited section 4.1 to include data from previous studies at Russell Ranch that support our hypothesis of increased water storage and movement in cover cropped plots. We have included a discussion of several previous studies at Russell Ranch that showed little to no change in bulk density, increased infiltration and increased moisture holding capacity under winter cover crops. We have also included a discussion of the impact of small Ksat sample size, cover crop root type, and the lack of support for other potential hypotheses for increased moisture content.

Finally, we have supported our conclusion of increased soluble C flux to subsoils in the ORG systems by highlighting our multiple lines of evidence:

5) More EOC in ORG subsoils
6) Observations of higher water storage, and potentially increased infiltration under cover crops
7) Greater amounts of soluble organic carbon in compost, and
8) Reduced subsoil microbial stress indicators under ORG systems (attributed to more soluble C and nutrient availability)

Please go into more detail on how no differences in aggregation "rule out" increased pore space as the increase in water content. What is the alternative explanation? Is this just an issue with statistical power?

Response: We have removed this sentence and clarified that the increase in moisture content was likely due to an increase in root-related macropores.

L335 This also seems like a great candidate sentence for another hypothesis: "Due to the fact that tillage in all systems would likely eliminate differences among them in the top 30 cm, we would expect any differences in macroporosity and infiltration among treatments to be most affected by those roots that extend below the 30 cm plow layer". This is the first mention of tillage depth. Please specify the depth of disking in the methods, and if this was applied to the conventional fields as well.

Response: We have edited this sentence and included a description of tillage depths and number of events in table A5.

L340-345 This paragraph on cascade theory describes why FTIR analysis was necessary. This also should be included, or at least alluded to, in the introduction. This is a really interesting discussion (L350-355), and could also be a good place to bring up the variability in the conductivity data.

Response: We have included this paragraph in the introduction.

L371 Figure referenced should be Fig 9.

Response: We have corrected this reference.

L375 Support with values from the results. The nutrient values may all be better represented by tables, although the graphs show dynamics across the season, I would argue that depth, not season, is the key factor in this analysis.

Response: We have rewritten the referenced section. We have kept graphs showing dynamics across seasons to highlight that the increased EOC and P presence in ORG systems is not limited to a single timepoint, while mineral N and S levels are dependent on timepoint. We have also highlighted the seasonal variation in a paragraph in our discussion.

L380 Consider rewriting this section title, as there was no direct comparison to a compost treatment alone.

Response: We have reworded this section title to remove references to compost alone.

L382 Is the microbial processing near the surface based on the FTIR data? Please reference.

Response: We have clarified that our inference of increased microbial processing near the surface is based on biomass and the increased presence of more oxidized carboxylate C.

L388-L390 This paragraph seems speculative. Please input FTIR data that supports these ideas (C chemistry from this dataset).

Response: We have clarified that this paragraph is our attempt to lay out a hypothesis for the differences in C stocks, and inserted references to the results.

L388 What does "high variability of soil C measurements" refer to? Dry combustion measurements of total C are very consistent.

Response: We have removed this reference.

Conclusion:

L406-407: "This was facilitated by increased soil macropores created by cover crop roots leading to higher rates of transport of soluble C". Macropores were not analyzed in this study, and no increases were found in hydraulic conductivity or aggregation, please clearly delineate quantified results versus hypotheses in this conclusion.

Response: We have reworded the conclusion to better differentiate between our results, hypothesis and inferences.

We would like to thank the reviewer for their detailed, helpful comments, and hope that we have addressed their concerns in our response.