Several previous referee comments were not sufficiently addressed. I agree with the main referees' comments that many arguments and conclusions are hand waiving and based on generic statement taken from the literature. These criticisms were only partially addressed.
In addition, I have numerous other comments that need to be addressed before the manuscript may be accepted for publication.
If you consider revision, please upload a track-change manuscript where all changes (adding/deleting/moving text) can be clearly seen.

I. Previous referee comments.

Please improve upon the responses to the following comments by Referees 1 and 2. The numbering refers to the reviewer reports by reviewers 1 (R1) and 2 (R2) and their comments as numbered in their original reports.
If you choose not to change the manuscript text according to the comments, please justify this decision.

R1, C2: reasoning for 10 min blank samples – where is the information as given in your response found in the manuscript?

R1, C4 iii: Where was this information added in the text? How do you define 'good enough'?

R1, C6: Please provide more reasoning why you limited your discussion to ozone as oxidant only. I understand that you do not have measurements of other oxidants; however, what is the extent to which this limitation influences your conclusions?

R1, C11: Can you be more specific and quantitative here? The potential influence of long range transport requires more discussion than just half a sentence. How would it affect your results if indeed the effect of long-range transport had affected the C2, C3, C4-content of the aerosol samples?

R1, C11: The textbook by Seinfeld and Pandis does not explicitly discuss the conversion of S(IV) in aerosol water, only in clouds; however, it shows that the S(IV) oxidation rate is a function of LWC, which is smaller by several orders of magnitude for aerosol water than clouds.

Thus, the comparison of lifetimes (proportional to inverse loss rates) cannot be easily done based on the discussion in the textbook.
Please provide a more detailed discussion on the various S(IV) oxidation rates in gas and aqueous phases (also considering pH) for the conditions at your measurement location.
Your previous response is contradictory in itself:

"The SO4 2- formation is considered to be mainly via gas-phase reactions at relatively low atmospheric RH levels (RH < 30 %) and aqueous-phase reactions at relatively high atmospheric RH levels (RH > 60 %) (Li et al., 2017; Liu et al., 2020). RH in this study ranged from 29.8% to 86.9% with an average of 52.2 ± 10.2 %. It can be concluded that SO4 2- in this study was dominantly from the aqueous oxidation."

R2, C1 i: Where is this information stated in the manuscript? I could not find it. Apparently, it was not sufficiently clear for the referee. Please clarify.

R2, C3: The referee had suggested that parameters in columns, parameters in rows and sequence in both should be same in the panel that shows before lockdown and the panel that shows after lockdown. Please use this advice and rework the figures accordingly.

Currently they are still too confusing as the reader does not have a clear comparison of the various dependencies and parameters.

R2, C4: Can you comment on the outlier? I agree with the referee's comment and your revision to remove the regression line. However, you should add a comment on the outlier either in the figure caption or main text.

R2, C5: Given that you base a large part of your discussion on the ozone concentrations, you should discuss in more detail the reasons for the differences in ozone concentrations before and during LCD. I agree that the MLR/random forest analysis might get too far in the present manuscript; however, uncertainties associated with the various factors influencing the differences in ozone concentrations should be mentioned.
R2, C5 i: Where is this comment addressed in the revised manuscript? Even in the new text S3, I could not find any discussion on strong and weak species.
ii: Please add information on how you decided which acids to keep out of the models and why.
iii and iv: Is this information added to the manuscript? Where?

R2, C7 ii: Please improve the captions of Tables S2 and S3. Did you use all, i.e. day and nighttime values in Table S2? How meaningful is such statistics given that there are significant differences in day vs night values as shown in Table S3?
Add also sufficient information, either in the data base referred to in the data availability statement or in the supplement that the reader understands which statistical test was performed for which compounds and why.

R2, C8: I am not sure that the authors understood the referee's comment correctly. In the revised manuscript, the pH is now stated as 3.2 ± 3.0 which would correspond to an H+ concentration of 6.3e-4 M ± 1e-3 M – but yet the reported range of measured values is 2.3 – 4.4, i.e. [H+] = 4e-5 – 5e-3 M. Please clarify.

II Additional editor comments

Major comments

1. In the title and text throughout the manuscript, you imply that the lockdown led to differences in the concentrations of diacids before and during the lockdown. However, given that the meteorological conditions (T, RH) were also vastly different during the two periods, the diacid levels were likely also affected by these conditions. Your statement in lines 173/174 point to this but this is too brief.
Please emphasize this in the discussion and possibly adjust the title accordingly.

2. Throughout the manuscript, you refer to ozone concentrations that may be responsible for different patterns of the diacid concentrations. Section 3.4 is even named 'O3-dominated formation of SOA'. This is misleading as most likely many oxidation pathways of the organics are by the OH radical and/or other oxidants. Only in line 321, you state that OH measurements were unavailable. Please rephrase all instances where you refer to O3-dominated pathways and O3 as the main oxidant.
Can you refer to any OH measurements before and during the lockdown? Could it be simply possible that OH during the lockdown was higher because fewer sinks were present?

3. The formation and ratios of diacids have been discussed in numerous previous studies for a wide range of conditions and locations (work by the same and other authors). How do the results from such studies compare to those found here?
Please pay attention to ACP's aims and scope that 'Articles with a local focus must clearly explain how the results extend and compare with current knowledge.' https://www.atmospheric-chemistry-and-physics.net/about/aims_and_scope.html

2. Minor comments

l. 40: I assume that the 14% refer to WSOC in PM, not total (gas + PM) (?) – Please clarify.

l. 43: High solubility and hygroscopicity are not necessary for good ice nuclei.

l. 53: what do you mean by 'oxidative capacity of aerosols'? This expression is not used in any of the cited references and not clear in this context.

l. 58 – 67: (i) There are many lab and model studies that have explored the formation of oxalic acid, e.g. (Carlton et al., 2007; Crahan et al., 2004; Perri et al., 2009; Warneck, 2003). In these studies, the main oxidant was OH. Please comment on this and include appropriate references. (ii) Given the small proportion of oxalic acid to total aerosol loading, its effect on air quality can be assumed negligible.

l. 83/4: Also aqueous phase processes can be photochemical. Do you mean 'oxidation in the aqueous vs gas phase'?

l. 168: SOA formation can be also a strong function of NOx level. Thus, enhanced ozone concentrations do not necessarily lead to higher SOA.

l. 175 – 185: What is the extent to which the differences in EC, OC, WSOC, PM2.5, levoglucosan etc can be ascribed to different air masses rather than different emission strengths? (How) Can the back trajectories help in answering this?

l. 191 ff and Figure 2: (i) The text states that TDOC/CO is discussed whereas the figure caption only says TDOC. Please correct. (ii) Why do you expect the increase of diacids to follow Arrhenius Law? While the formation of diacids might follow such a temperature dependence, so does also their loss by further oxidation. (iii) If the loss may be negligible, why should the temperature dependence during and before LCD have a different exponent?

l. 271: Aqueous phase formation of C2 is also photochemical as it is driven by OH (and less o by NO3 or other dark processes). What is the basis of your assumption of an accelerated formation of C2 during night?

l. 291: Why should Gly and mGly show a positive correlation with RH and LWC? Wouldn't one expect the opposite, i.e. more efficient oxidation in the presence of water? They are formed by gas phase processes, i.e. independently from the presence of water.

l. 306ff: Both the oxidation of glyoxylic acid and the oxidation of oxalic acid are pH dependent since the rate constants of the anions differ from those of the undissociated acids. Could these differences in formation/loss rates explain the different concentrations at different pH?

l. 308 vs 338: These lines are contradicting each other. In the former, you write that the degradation of longer acids is not a source of C2 whereas in the latter you state that it is likely a major source.

l. 349 – 356: This text is very unclear. What is the relevance of the R^2 = 0.49 for O3 and C2/Gly in relation to the lab study by Carlton who used OH as an oxidant?

Sections 3.5.1 and 3.5.2. Given that the latter subsections repeats quite a bit of information from the former one, I suggest merging both sections into Section 3.5.

Section 3.6: I do not fully follow the categorization into the different factors: (i) Given that sulfate may be also formed by gas phase oxidation and/or originates from long range transport, it seems questionable to based the 'aqueous phase factor' solely on the presence of sulfate. (ii) Again, I do not understand why you distinguish between 'aqueous oxidation' and 'photochemical oxidation' , (iii) biomass mass burning emissions might be also processed by oxidation processes in the gas and/or aqueous phases. Why is biomass burning a separate factor?

3. Technical comments

a. Please carefully proofread your manuscript. There are many grammar and language mistakes as well as numerous typos. I list a few below but this list is by no means complete. (line numbers refer to the manuscript version without annotation).

l. 15: remove 'the' before eastern China
l. 16: 'evolutionary process' could be replaced by 'formation'
l. 17: Reword this sentence, e.g. 'Samples of ... were taken...' (samples are not measured)
l. 19: replace 'element' by 'elemental'
l. 45: replace 'activity' by 'sources'
l. 49: replace 'aids' by 'acids'
l. 115: should 'dervied' be 'derivatized'?
l. 116: replace 'quantificationally' by 'quantitatively'
l. 127: remove 'and' in 'and the derivatized'
l. 169: do you mean 'solar'?
l. 170: concentration
l. 262: replace 'makers' by 'markers'

b. Round numbers to significant digits. E.g., 22.7 ± 8.9 should be 23 ± 9 etc

c. Please carefully check all units in the text and tables for consistency and correctness. Individual acids are certainly not present in concentrations on the order of micrograms (e.g. l. 234)