Reply to Reviewer #1 comments on "Theoretical study of mixing in liquid clouds – Part 1: Classical concept" by A. Korolev et al.

General comments to all three parts (repeated in all three reviews). I read the papers with considerable interest mostly because this seemed to be a popular topic some time ago, in both observations and modeling. I was curious to see what new these manuscripts bring. Frankly, I was disappointed. First, the analysis concerns a highly idealized problem, with little applications to real clouds. Turbulent mixing in clouds is by far more complicated that situations depicted in Fig. 1 of part 1 (and then repeated in different shapes as Figs. 1 in Part 2 and 3). Second, I am aware of study in which the authors developed a fairly sophisticated model of microphysical evolution during turbulent stirring (Jarecka et al., JAS 2013) aiming at prediction of the homogeneity of mixing. They applied the model to LES simulations of shallow convective cloud field. The impact was surprisingly small and the authors of that paper argued why this might be so (the entrained air comes from the descending shell and is not far from saturation). So in a sense the subject is "old news". Finally, the lengthy discussions, full of unnecessary caveats and references to details of small multi-panel figures, made the reading frustrating. All three parts read like a student dissertation, not a concise scientific paper highlighting key points and leaving the rest for the reader to follow. Thus, I read the manuscripts with decreasing interest, and my comments are more detailed for the part 1, and get more general for parts 2 and 3.

Overall, I do not believe that the subject matter deserves close to 100 pages and close to 50 figures. I feel that the material deserves a single, short and concise manuscript, with new material clearly separated from what I feel has been discussed in the past, perhaps not at such a level of detail. Reading introductions to all three parts made me mad, because all three say basically the same thing with different language and organization. Part 1 is mostly trivial in my view, with some parts speculative and other repeating already published material (see detailed comments). Parts 2 and 3 have some aspects that perhaps deserve to be published, but it is not clear to me how useful these are (not very much in my opinion). References to aircraft observations are vague and missing the key aspect, which is the irrelevance of an idealized problem considered by the authors to low-spatial resolution observations of a complicated multiscale natural system.

Reply to general comments:

Authors appreciate the Reviewer's time and efforts to review our manuscript.

The overview sections, which were copied and pasted for all three different reviews, can be summarized by the following claims:

a) The problem of turbulent mixing in clouds "seemed to be a popular topic some time ago", but now "the subject is old news".

b) This study addresses a “highly idealized problem” and uses simplified models in order to describe cloud mixing.

c) The results presented in the papers are not new and are “repeating already published material”.

The authors strongly disagree with the above statements of Referee 1.

In response to the first claim: the mechanism of mixing is still not well understood and continues to be a highly relevant problem in the cloud physics community, especially given the high rate of recent publications on this topic. We believe that the three papers contribute significantly to the theory of interaction of cloud droplets with turbulent environment and present novel techniques of investigating the effect of mixing both from a theoretical standpoint and through in-situ observations.

Second, in contrast to the reviewer, we support the common practice of using idealized models of complex cloud processes, in order to investigate physical mechanisms without being bogged down by the multitude of other processes involved. Idealized considerations (e.g. adiabatic assumptions) are widely used in cloud physics community as well as in physics in general. The assumptions are clearly articulated at the beginning of each paper in order to let a reader judge about the level of idealization of the utilized approaches.

Third, as regards to novelty, the following new results have been obtained:

a) The first paper suggests a new technique for identifying type of mixing (homogeneous or inhomogeneous) based of the analysis of the moments of droplet size distributions. It was shown that homogeneous mixing breaks functional relationships between the moments. Nothing like that has been done before. The comments obtained by the authors from their colleagues showed that the proposed technique start to be utilized by other research groups.

b) The second paper considers *homogeneous* mixing. One of the important finding of this paper is an analytical universal solution describing the rate of evolution microphysical parameters as well as the final equilibrium state (mixing diagram). It is shown that in case of polydisperse droplet size distributions evolution of droplet spectra can lead to increase in characteristic size of droplets in contrast to widely accepted "classical" view, when the characteristic droplet size is decreasing.

c) The third paper is dedicated to *inhomogeneous* mixing. A theoretical framework for a time dependent mixing of two volumes that accompanies by cloud droplet evaporation is developed. A new turbulence-evaporation model of time evolution of ensemble of droplets under different environmental parameters is proposed. In contrast to previous studies the Damkoller number is introduced as a result of re-normalization of mixing-evaporation equation, rather than empirically. It is shown that any mixing leads to droplet spectrum broadening. For the first time the scientifically grounded demarcation between homogeneous and inhomogeneous mixing in the space of environmental parameters is performed.

The authors are confused that Referee 1 overlooked all these novelties.

The authors also believe it is impossible to follow the recommendation of Referee 1, to combine all papers into one single, summary paper. While the papers all consider the same subject, they perform completely different functions with regard to investigating the issues of mixing.

Comment:

A small technical comment: I think the terminology the papers use is not correct. The limiting cases should be referred to as homogeneous and extremely inhomogeneous mixing. Everything between the two is the inhomogeneous mixing.

Reply: Corrected.

Specific comments to Part 1:

1. The title should include “concepts”, not “concept”.

Reply: Corrected

2. I feel the proper start to the discussion is to recognize that bulk properties (moist static energy and total water) are sufficient to calculate the final thermodynamic state (i.e., once the mixing is completed). However, the transformation of the droplet spectrum may lead to different spectra with the same final liquid water. Extremely inhomogeneous mixing leads to the final spectrum as given by (1), that is, number of droplets in each bin is simple reduced in the same proportion. Homogeneous mixing leads to a shift of the spectrum towards smaller sizes. In such a case, the shift may lead to a complete evaporation of the smallest droplets in the initial spectrum. Note that such a simple interpretation makes the first sentence in the abstract to the Part 2 trivial.

Reply: The statements about independence of the final state of the bulk parameters on the type of mixing are scattered throughout the text of Part 1. One more statement was implemented in Section 2.1 following the reviewer’s comment.

3. The main problem with the observations is the insufficient spatial resolution. If the diluted cloud consists of filaments of cloud-free and undiluted cloudy air, averaging such a structure gives an impression of the extremely inhomogeneous mixing (this was pointed out long time ago, perhaps in on of the papers involving Charlie Knight). In fact, aircraft in-situ observations seldom allow looking at homogenized volumes, at least not at scales that the observations are

able to resolve. Moreover, there are additional processes that affect droplet spectra, such as updraft and downdraft, activation of additional cloud droplets, collision/coalescence, etc.

Reply: The problem here is not as much as in the particle probe resolutions, as in the identification of the stage of mixing. For example, Beals et al. (2015) demonstrated existence of cloud free zones in clouds down to cm scale. That's the highest possible spatial resolution available nowadays. However, the results of this study and other similar studies do not provide answer, whether this is a final stage of mixing and whether the mixing is extremely inhomogeneous, or it is an interim stage of homogeneous mixing. To address this question a collocated high spatial resolution (~1cm scale) measurements of temperature and humidity are required. Unfortunately, airborne instrument capable of such measurements are not available at that stage. The discussion about it is added in the text.

Yes, there are limited number types of clouds suitable for identification of type of mixing, which are free of "non-mixing" processes (i.e. collision-coalescence, mixed phase, activation of interstitial CCN). There is a discussion about it in the manuscript. Authors do not think that it should be expanded in the manuscript more than it is.

4. Reference to Jarecka et al (JAS 2013) needs to be included in the paragraph starting at line 20 on p. 30213. Note that the review by Davenish et al. was published prior to that paper.

Reply: The reference was added.

5. Section 2.2. Figure 1 shows processes occurring at a constant volume. Does it make the difference that atmospheric processes typically take place at a constant pressure?

Reply: Consideration of the effect of pressure (e.g.$u_z \neq 0$) is not included in the text. This was stated in section 2.2. A potential effect of the vertical ascent was discussed in section 6.

6. Section 2.3. Does the conservation of moist static energy and total water lead quickly to the answer?

Reply: The derivation of δq was done based on the mass and energy conservation. Yes, it leads quickly to the answer for q.

7. I do not understand the statement below Eq. 9. Latent heating is included if one follows what I suggest in 6 above.

Reply: The mentioned statement is misleading and it was excluded from the text of the revised manuscript. The original meaning of this statement was to indicate that the temperature in Eq.9 is used as a constant. The modified statement in the modified manuscript was moved to Appendix A.

8. Section 2.4. The initial paragraph provides information that needs to be stated at the onset of the analysis (see 2 and 6 above).

Reply: The sequence of sections was rearranged in order to improve the flow of the text.

9. Eq. 15. The phase relaxation time scale goes back to Squires.

Reply: The reference to Squires was added.

10. Section 3. First, I do not think there is anything to model. Is the comparison between a specific model used by the authors (no details provided) and the analytical solutions the purpose of this section? Sections 3.1 to 3.4 should be compressed into a short section and a single figure should be selected. These sections are exactly what I mean by my statement that the paper reads like a student dissertation.

Reply: The sections were shortened and rearranged. Figs.4-6 were converted into one figure.

11. Section 3.5 is perhaps a good start to a follow-up investigation. At the moment, it does not belong to this paper.

Reply: This section has a strong link to the subject of the paper, which might not be well articulated in the original text. The text and the sequence of the sections were rearranged in order to address this issue.

12. Section 3.7. This is really not a summary.

Reply: The title of the section was changed. This text of this section was moved into Sections 5 in the revised manuscript.

13. Section 4 is long and does not bring anything new in my view. What is the point of having it here? I was not able to follow detailed discussion in section 4.1 and references to the specific figures. Section 4.2 can be omitted. I question the link between in-cloud observations and the results of theoretical analysis that the previous sections provide.

Reply: The results of this section create a basis for a new approach for identification of type of mixing from in-situ measurements. All previous attempts of identify homogeneous mixing were based on the comparisons of measurements with the $N - r_v$ calculated for the first stage of mixing. Such attempts have a limited success and may be misleading. More explanations were added in the text in order to clarify the results of this section and link it to in-situ measurements.

14. Section 5 discusses aspects that have been beaten up in other papers. Just a short paragraph with proper references would be sufficient.

Reply: The entire section on time scales was removed in the modified manuscript to make the paper more focused.

15. Conclusion section is short, perhaps not surprisingly.

Reply: Nothing to comment.

Replies to the Reviewer 2 comments on "Theoretical study of mixing in liquid clouds – Part 1: Classical concept" by A. Korolev et al.

Overview:

The main contribution of this paper apparently is to demonstrate the relationship between different moments of the size distribution for the limits of homogeneous and extreme inhomogeneous mixing. Analytical results are compared with the results from a parcel model. The conceptual picture of inhomogeneous and homogeneous mixing is well illustrated in Figure 1 and the central analytical expression is validated in Figure 2. Figures 3-8 then show the response of different moments of the cloud droplet size distribution to idealized mixing processes. Figures 9-11 describe a conceptual model of a cascade of mixing events between a dry parcel and the cloud, which is a step toward making comparisons between the theory and observations within an evolving cloud environment. Section 4 and Figures 12-15 provide a brief analysis of observational data in the context of the conceptual models developed in the previous sections. The analysis is useful in attempting to connect the concepts and idealized models to the more complex situation observed in real clouds. The paper ends with a discussion of characteristic time scales, which seems somewhat disconnected. It is not clear how this integrates with the previous sections, and perhaps it should be either moved closer to the introduction or separated as an appendix. If kept in this location, its logical flow with the rest of the paper needs to be improved. Overall, my sense is that the expanded view to consider different moments of the size distribution is a valuable contribution, especially for the experimental cloud physics community, but perhaps also for applications to radiative transfer, remote sensing, etc. I am not aware of other papers where different moments are considered thoroughly as here, so this seems to be original. Comment regarding disconnect of the section 5 discussing the characteristic time scales.

Reply: Authors highly appreciate the Reviewer's comprehensive comments and time to read our manuscripts. Special thanks for thoroughly going through equations and revealing numerous typos.

The manuscript underwent major revision and modification. The text was shortened to make it concise, sections were rearranged, some of them were re-written, the variable names were modified to be consistent with part 2 and 3. We agree that Section 5 is in many ways disconnected, and it was excluded from the manuscript.

General criticisms:

1. The application to size distribution moments is original, as far as I am aware (Jeffery gave a brief discussion of how the second moment is affected by mixing, but the treatment here is much more thorough and covers all typical moments). But much of the conceptual model is written more like a textbook. Maybe this is nice for readers new to the field, but the authors take a risk in expanding the length of the paper, especially when combined with the other two parts. Much more important, and definitely missing from the introduction as it currently stands, is some kind of overview of how the three part series fits together. What are the different levels of complexity treated? Why are two specialized papers on homogeneous and inhomogeneous mixing needed if part 1 already treats both cases? Now that I have read all three parts I have an idea, but this needs to be clear from the outset. It is especially important to motivate why part 1 should be connected at all. Currently it is disconnected in its approach, in its use of observational data, and even in its notation. The use of observational

data is nice, but it is somewhat confusing given the title "theoretical study…" The notation is a major problem that needs to be corrected… the physics is difficult enough by itself, without having to translate symbols from one paper to the next.

Reply: The authors shortened several pages of the text in order to reduce the size of the manuscript and make it concise. A number of cross references were added in all three parts in order to link them together. As it is seen now, part 2 is closely related to part two and it uses the same approach. Part 3 utilizes the results of part 2. The first part uses experimental data to demonstrate the how the theoretical outcomes could be verified from in-situ measurement. In our opinion such comparisons with experimental results are natural, and if it is not there, it probably would be requested by reviewers. So, since

We also checked Jeffery's works on mixing. However, no discussions of the effect of mixing on the DSD second moment were wound. We appreciate if this reference could be provided.

2. After a long preliminary discussion, the most important paragraph in the introduction is on page 30214 starting at Line 26: "Besides the effect on N and r the type of mixing is anticipated to manifest itself in relationships between other moments of the droplet size distribution…" It should be further explained in that paragraph why it is valuable to analyze different moments. Are they expected to be more insightful than the traditional mixing diagram methodology; is it making applications of mixing to other fields clearer; etc?

Reply: The paragraph explaining importance of the effect of mixing on the DSD moments was added in the introduction following the Reviewer's comment: "It is shown that the newly obtained relationships between the moments provide a more robust identification of type of mixing from in-situ measurements as compared to conventional $N - D_v^3$ relationships used in mixing diagrams. Relationships between moments may be useful for parameterization of mixing in numerical simulations of clouds and climate, interpretations of remote sensing measurements."

3. In Fig. 9 and after, a multiple-step mixing process is envisioned. The approach is to consider mixing between a cloud and the dry environment, and then to consider subsequent mixing events between that parcel and the cloud again. Why did the authors choose to take this view instead of considering a cloud parcel progressively mixed with clear air? Some motivation for that choice is needed and some discussion of how the results would be expected to differ. For example, if one were to focus on the dry air first, dots should be concentrated at lower end in Figure 10.

Reply: The modeling of the progressive mixing presented in the paper corresponds to the case when the entrained dry air is interacting with the cloud environment. The final state of this interaction is diluted cloud, which continues its life cycle. The progressive mixing of the cloud environment with the environmental dry air corresponds to detrainment, which ultimate state is dry cloud free air. It can be show that during detrainment the relationships between moments will be the same as during primary mixing. The authors consider that the case of detrainment is less interesting, and left it outside the frame of the manuscript in order to keep it concise. Following the reviewers suggestion a paragraph was added in the text in order to explain the motivation of our choice: "It is worth noting that progressive mixing with the dry air does not break the functional relationships between the moments. This case is equivalent to detrainment of cloudy environment into dry air. It can be shown that Eq.(14) remain valid at any stage of progressive homogeneous mixing with dry air only, i.e. $N_j/N_1 = \mu^{(1)} \cdots \mu^{(j-1)}\mu^{(j)}$ where $\mu^{(j)}$ is the mixing fraction at the j-th stage of mixing. Eqs. (15)-(24) also remain valid for the progressive mixing with the dry air only. ”

4. There are many mistakes in the paper, including errors in the equations, at least according to the derivations as I am able to follow them. Again, the physics is difficult enough by itself, without having to make corrections. Please thoroughly check all results and the typesetting.

Reply: The authors highly appreciate the Reviewers efforts to improve our paper and pointing out numerous typos. All specific comments listed below were addressed and the text of the manuscript was thoroughly checked.

Specific comments

1. Eq. 1, page 30218: As monodisperse cloud droplets are used in this part of the study, the droplet size distribution f(r) will confuse people. Especially Equations 2 and 3 only work for monodisperse droplets theoretically. Please explain and be consistent.

Reply: The relationships between moments are valid for relatively narrow non-monodisperse droplet size distributions. However, the modeling was performed for monodisperse size distributions. The confusion about assumption of monodisperse droplets during deriving relationships between the moments is probably coming from mentioning monodisperse size distributions in section 2.2. This section was modified in order to avoid possible confusion about the assumption about monodispersity.

2. Eq. 5: prefactor should be (cpRvTmo2/L2)? Tmo not T2?

Reply: Corrected.

3. It is difficult to connect Eq. 8 to Eq. 5. How do you prove Eq. 5 is (1-μ) Eq.8, when T1=T2=Tmo?

Reply: The term (1- μ) appears as a result of expansion in series. Appendix B was added to clarify the derivation of this equation.

4. Line 21, page 30218: q is liquid water mixing ratio (g/kg), not liquid water content (g/m3).

Reply: Corrected

5. Line 6, page 30220: The neglect of latent heat is a strong assumption that removes possible important factors such as negative buoyancy production. It is valid in the range specified by the authors, but the limitation should be discussed. Does it restrict the results to certain environments or cloud types (e.g., shallow convection)?

Reply: If fact the latent heat was accounted during derivation of Eq.3 (old Eq.8) (see Eq.A7 in Appendix A). The confusion regarding disregarding the latent heat is coming from inaccurate statement on page 30220 as indicated by Reviewer. The original purpose of this statement was to indicate that the temperature remains constant. In order to demonstrate that δq^{*} and δq_m allow accurate depiction of the temperature depression during mixing-evaporation process, the air temperature formed after mixing calculated from the analytical expression Eq. 6a,b was compared with the modelled temperature in Figs. 4h and 6h.

6. Line 7, page 30220: “comparisons of with numerical…” needs to be corrected.

Reply: Corrected.

7. Line 13, page 30220: missing space between “on” and “delta_q”

Reply: This sentence was deleted.

8. Line 17, page 30220: the volume change due to temperature change should not affect liquid water mixing ratio, because it’s connected to mass not volume as mentioned in point 4.

Reply: This paragraph was deleted.

9. Eq. 8: prefactor should be (cpRvT22/L2)?

Reply: Corrected.

10. Eq. 13: left side should be r33/r303

Reply: Corrected.

11. Eq. 14b: I think the right side should be (q/q0)2/3(q+delta_q*/q0+delta_q*)1/3

Reply: Corrected.

12. Eq. 16: I believe the exponent should be -1/3, and inside the parentheses should be N_0/N.

Reply: Corrected.

13. Eq. 20: right side should be q2/3(q+delta_q)1/3/q0

Reply: Corrected.

14. Fig. 3: it looks like panels a and b are mixed up. Also the caption refers to liquid water mixing ratio but the axis label states LWC; needs to be consistent.

Reply: Corrected.

15. Figs. 3 and 4: should use same format for S through the whole paper (e.g. 20% as in Fig.4 or 0.2 as in Fig. 3)

Reply: Corrected. In the revised manuscript S is replaced by RH.

16. Lines 12-15, page 30224: Lots of problems here. Where are the black stars in Fig. 4? Do you mean the stars in panels a and b of Figure 3, or should there be stars in Figure 4 too? And by the way, the stars in Figure 3 are very difficult to see… I had to search for them. And again, regarding text on line 14, the question of LWC versus q comes up. Finally, on line 15 it is not obvious to be that the statement is for Figs. 3 and 4. Do you mean to include Fig. 2 also?

Reply: The problems with the figures numbering and incoherency of the associated text were fixed.

17. Line 25, page 30226: q0 is not liquid water content.

Reply: Corrected.

18. Line 9 page 30227: Fig.17 should be Fig. B1?

Reply: Corrected.

19. Fig. 7: why changes from r0=10um (Fig. 4,5,6) to r0=5 um. And also changes the S from 50% to 90%?

Reply: The sizes 10μm and 5μm were selected to demonstrate mixing for the cases $T_1 = T_2$ and $T_1 \neq T_2$ in a most pronounced way. For the case RH_2=50% no supersaturation will be formed. Positive supersaturation may occur only at RH_2>80% and ΔT<15C. Larger ΔT seems to be uncommon for the tropospheric clouds.

20. Fig. 8: My understanding is that homogeneous and inhomogeneous mixing coincide with each other for Smo>1? It's hard to see this phenomenon in Fig. 8 (might use different colors or symbols?) also line 5 page 30228: unclear, should be "exceed those for inhomogeneous mixing for delta_T=0 and delta_T=5…?"

Reply: Corrected. Inhomogeneous mixing for ΔT=10C was indicated by the grey circles.

21. Line 5, page 30228: in Fig. 8, Delta_T is negative, here it's positive.

Reply: The sign of ΔT was corrected.

22. Line16, page 30228: could you explain why "the effect.. is more pronounced when T1>T2 compared with T1<T2."

Reply: When the entrained air is colder (T_1>T_2), it results in additional condensation of the cloudy air due to its cooling compared to the case when the dry air is warmer (T_1<T_2). This statement is supported by the results of numerical simulations. This explanation was not included in the text for the sake of conciseness.

23. Line 27, page 30229: "becomes denser towards the top right corner" Is it because the mixed volume is mixed with cloud volume, not environmental volume?

Reply: Yes. The mixing with the cloud environment results in approaching of the properties of mixing environment to the cloud properties. Eventually the entrained air is dissolved in the cloudy environment.

24. Fig. 11: why use r0=5 um, not 10 um. It's better to use the same radius through the paper, except you want to do the sensitivity test.

Reply: During the paper preparation the authors tried different r_0. Unfortunately is does not work well for the same r_0. Different r_0 (5μm and 10μm) were used in order to demonstrate the most pronounced effect of mixing on microstructure. A relevant comment was embedded in the text to address this issue.

25. Line 13, page 30231: missing space between "q" and "beta"

Reply: Corrected.

26. Line 14, page 30231: define Sc, Ac, Cu, Cb

Reply: This sentence was deleted in the revised version.

27. Line 1, page 30232: missing space between "N" and "q"

Reply: Corrected.

28. Fig. 13: caption T=-12 not -120

Reply: Corrected.

29. Line 13, page 30233: how does sample averaging affect homogeneous versus inhomogeneous mixing?

Reply: This is a good question. It was debated over years: how the averaging scale affects identification of the type of mixing, i.e. homogeneous versus inhomogeneous? The single instrument approach used in this and the majority of previous studies does not allow judgement about type of mixing at scales smaller than the averaging scale L_{av}. In part 2 it was shown that for typical cloud environmental conditions the upper spatial scale of homogeneous mixing is limited by few m. Inhomogeneous mixing depending on the conditions may caver a wide range of scales from cm to km. A discussion of spatial scales oh homogeneous and inhomogeneous mixing is provided in parts 2 and 3.

30. Fig. 14a: y axis unit (g/m3) not (km-1)

Reply: Corrected.

31. Fig. 14: what's the dash line in a,b,d

Reply: The explanations for the dashed lined was implemented in the caption for Fig.14.

32. Line 9, page 30237: Da>>1 is for inhomogeneous mixing, while Da<<1 is for homogeneous.

Reply: This section was removed in the revised manuscript.

33. Line14, page 30237: Andrejczuk is misspelled both here and in the reference list.

Reply: Corrected.

34. Lines
35. Lines17-22, page 30238: lambda_ev, lambda_v, and lambda_DeltaV need to be defined, and the assumptions in calculating them clarified (e.g., evaporating distance assumes droplet always falling at terminal speed corresponding to time-dependent radius?).

Reply: This section was removed in the revised manuscript.

36. Line 6, page 30240: S2 approximate 1 not 0?

Reply: This section was removed in the revised manuscript.

37. Line 6, page 30240: missing space between "concentration" and "nev"

Reply: This section was removed in the revised manuscript.

38. Fig. 16: define A and B in the text or caption

Reply: This section was removed in the revised manuscript.

39. Line 13, page 30240: missing space

Reply: This section was removed in the revised manuscript.

40. Line 15, page 30240: missing space

Reply: This section was removed in the revised manuscript.

41. Eq. B4: left side should be Tmo not Tm

Reply: Corrected.

42. Eq. B8: There seem to be mistakes here. I believe the prefactor should be (cpRvTmo2/L2) and Tmo not T2?

Reply: Corrected.

43. Line 14, page 30244: "is hold" should be "holds"?

Reply: Corrected.

44. Line 15, page 30244: Figure B1 is Figure 17.

Reply: Corrected.

45. Table A1: there are two \tao_ev

Reply: The variable related to the time scale section were removed from the table.