Author's response

We want to thank both referees for carefully reviewing our manuscript and giving helpful and precise comments to further improve it. We found it highly beneficial to, on one hand, critically observe and, on the other hand, to justify the decisions made during the writing process. In the author's response, the responses to each comment are written using the blue font and text added to the manuscript is **bolded**.

Referee #1

General comments

This study explains the development of two aerosol decay functions, one with ventilation and deposition and the other with an additional coagulation term, and explores their application to cooking events across several sites in Tampere, Finland in 2022. The paper clearly indicates the novelty of the functions, the gap in the literature it is addressing and how, and the scientific and health implications of its findings. The main concern I have is the use of 'aerosol' and 'particle' interchangeably – while they can be treated similarly in the equations the authors use due to their size and dynamics, they are not the same thing, which should be made explicit in the paper and in the title for readers who may not be as familiar with these topics and concepts. Once this (and a few other minor suggestions, listed below) has been addressed, I think this paper can be accepted for publication.

Thank you for carefully examining and commenting our manuscript. We really feel that the manuscript was improved via your comments. We have taken seriously the concern about the usage of terms 'aerosol' and 'particle'. To solve this issue, the usage of those terms was check throughout the manuscript. Before that, we further investigated, how these terms are utilized in literature. Our findings show that, for example, both 'aerosol dynamics' (Nazaroff and Cass, 1989; Whitby and McMurry, 1997; McGraw, 1997; Holmes and Morawska, 2006) and 'particle dynamics' (Nazaroff, 2004; Zhang, 2006) are used in literature. Our conclusion is that, in general, the term 'particle dynamics' is used when considering transportation and deposition. However, when coagulation is included in the consideration the term 'aerosol dynamics' is the most commonly used. All in all, the consideration of coagulation as an aerosol process is not as clear as of condensation and evaporation. Based on these findings, we decided to still use the term 'aerosol dynamics' throughout the manuscript. From our point of view, it is more explicit for the reader, if we use that term in our manuscript also when only considering ventilation and deposition. The same applies to the term 'aerosol process'.

Yet, considering our study as a whole, it is evident that the study is focusing mainly on aerosol particles, which are also be referred in the manuscript simply as 'particles'. The usage of the term 'aerosol particle' especially at the beginning of the paper is an effort to make the reader familiar with the fact that particles are a component of aerosol. The term 'aerosol' is solely used only when considering both the particle and the gas phase. According to the above explained principles, the title of was reformulated as follows:

"Decay phase aerosol dynamics of an indoor **particle** source has a significant role in exposure analysis"

References:

Holmes, N. S. and Morawska, L.: A review of dispersion modelling and its application to the dispersion of particles: An overview of different dispersion models available, Atmospheric Environment, 40, 5902 – 5928, https://doi.org/10.1016/j.atmosenv.2006.06.003, 2006

McGraw, R.: Description of Aerosol Dynamics by the Quadrature Method of Moments, Aerosol Science and Technology, 27, 255 – 265, https://doi.org/10.1080/02786829708965471, 1997.

Nazaroff, W. W.: Indoor particle dynamics, Indoor Air, Supplement, 14, 175 – 183, https://doi.org/10.1111/j.1600-0668.2004.00286.x, 2004.

Nazaroff, W. W. and Cass, G. R.: Mathematical Modeling of Indoor Aerosol Dynamics, Environmental Science and Technology, 23, 157 – 166, https://doi.org/10.1021/es00179a003, 1989.

Whitby, E. R. and McMurry, P. H.: Modal aerosol dynamics modeling, Aerosol Science and Technology, 27, 673 — 688, https://doi.org/10.1080/02786829708965504, 1997.

Zhang, Z., and Chen, Q.: Experimental measurements and numerical simulations of particle transport and distribution in ventilated rooms, Atmospheric Environment, 40, 3396 – 3408, https://doi.org/10.1016/j.atmosenv.2006.01.014, 2006

Specific comments

My specific comments are broken down into Introduction, Methods, and Results/Discussion rather than according to the sections defined in the paper because I do not have very many; the main themes are: 1) adding some relevant context to the introduction, 2) including more details on experimental design and data treatment, and 3) clarifying a few figures and points in the results/discussion.

- Introduction
    - It would be worthwhile to add a couple of sentences describing intake fraction as well as the relationship between concentration, exposure, and dose, as not all readers may be familiar with these concepts or how they fit together.

      Thank you for this suggestion. We agree that it would be helpful for the reader to gain explicit understanding of these terms. Unfortunately, straight definitions of the terms would compromise the telling of the introduction substantially. To balance between these competing interests, following modification was made:

"To prevent the negative health effects, the exposure to particles resulting from spending time at elevated particle concentrations should be reduced. One approach, currently adopted, is to regulate particle concentrations by guidelines (WHO, 2021) and legislation (EU, 2008; EPA, 2013)."

The definition of 'dose' is not included in that modification, but, from our opinion, the definition is made clear through Eq. (22). Additionally, all the occurrences of the terms 'exposure' and 'dose' were revisited to ensure the consistent use of the terminology.

- There is a heavy focus on developed countries, but developing countries should at least be mentioned, especially when discussing cooking (lots of women and girls spend disproportionally more time cooking on stoves that may not be the same as those used in developed countries, which can lead to even higher exposure to PM).

We agree that in developing countries the cooking-related exposure to aerosol particle, especially for females, is potentially a major health issue. However, considering the decay phase of cooking-generated particles, the exposure is likely to be greater in developed countries, as study of Pacitto et al. (2021) shows. In that study, airing of the buildings, different time activity pattern related to cooking and eating, and different cooking habits were given as possible contributors leading to such differences. All in all, to acknowledge this comment following sentences were added to the manuscript:

"Pacitto et al. (2021) have reported that the contribution of cooking and eating activities to daily particle exposure varies from 13 to 59 % in western cities and from 7 to 14 % in cities located in low- and middle-income countries. The same study also shows that, in general, women are receiving higher doses of cooking-generated particles."

"…buildings equipped with mechanical ventilation are usually airtight which slows down the decay process of indoor generated particles. **This offers one explanation, why cooking-related LDSA exposure has more significance in developed countries, considered in this study, as Pacitto et al. (2021) report. Altogether, these findings** emphasize the need for characterization of aerosol dynamics of the decay phase in mechanical ventilated dwellings."

References:

Pacitto, A., Stabile, L., Morawska, L., Nyarku, M., Torkmahalleh, M. A., Akhmetvaliyeva, Z., Andrade, A., Dominski, F. H., Mantecca, P., Shetaya, W. H., Mazaheri, M., Jayaratne, R., Marchetti, S., Hassan, S. K., El-Mekawy, A., Mohamed, E. F., Canale, L., Frattolillo, A., and Buonanno, G.: Daily submicron particle doses received by populations living in different low- and middle-income

countries, Environmental Pollution, 269, https://doi.org/10.1016/j.envpol.2020.116229, 2021.

- Additionally, it would be useful to add a sentence or two focusing more on cooking (how some stoves are worse than others, some ingredients or styles of cooking cause more PM, etc.)

  We added the following sentence to the manuscript:

  "**Additionally, it has been shown that the particle dose received from cooking varies greatly depending on ventilation style, ingredients, cooking style and stove type (Kang et al., 2019; Abdullahi et al., 2013; Wallace and Ott, 2011).**"

- Please define the size range of UFP for those who may not be familiar with it.

  We added the definition of UFP was added to the manuscript as follows:

  "Ultrafine particles **having a diameter of 100 nm or below** have been concerned as a health risk..."

- Methods
  - Can the authors clarify the frequency of measurements?

    With the following addition, we clarified to the manuscript that there was one measurement in each dwelling resulting in four measurements in total:

    "**A total of four measurements**, designated as Cases I to IV, **were conducted** in four different dwellings..."

  - Were any correction factors developed and applied after co-location? Was any calibration of the sensors needed? What data processing or QA/QC steps were implemented after the measurements were finished?

    Based on the results of zero and background measurements, no correction factor were applied. However, Partector sensors were previously calibrated with following correction factors: 1.20 (living room Partector, 25.10.2021), 1.52 (backpack Partector, 22.11.2020) and 1.40 (outdoor Partector, 26.10.2020).

    After the measurements, Partector data, with initial time resolution of 1 s, were averaged to the time resolution of 4 s using Partector Data analysis tool. Next, both Partector and DiSCmini data were imported into Matlab. The data processing and QA/QC steps performed in Matlab, e.g. computation of plotted mean particle size and the fitting procedure of the decay function, are already described in the manuscript.

- Only one experiment was conducted per site, correct? Can the authors justify why only one was enough or why they chose not to conduct repeat experiments?

  It is true that only one experiment was conducted at each dwelling, which clearly adds uncertainty to the results. However, the main purpose of the experiments was to validate the developed decay functions, which should be doable with only one experiment per site. As mentioned in the *Summary and conclusions* section of the manuscript: "more extensive studies of indoor LDSA concentrations including the decay phase of indoor particle emissions are required".

- Can the authors include additional information on experimental design, including: any utensils and their materials, who was wearing the backpack (resident? Research team member?), how the sites were chosen, etc.?

  The measurement sites were primarily chosen based on their availability. The second objective was to include different kinds of dwellings with mechanical ventilation common in Finnish dwellings. If the main aim of the study had been to gain a comprehensive overview of cooking-related indoor particle exposure, measurement sites should have been chosen more carefully.

  During the cooking events, the utensils ordinarily used in each dwelling were utilized, which weakens the comparability of the results between the dwellings but reflects the real-world scenario better.

  The backpack was worn by research team members and, excluding Case I, there were only the backpack-wearing research team member present at the measurement site. In Case I, there were two research team members present at the dwelling.

- Line 251: can the authors elaborate on what they mean by 'cooking styles and stove models'? What stove models were used and how were they different? Why were the cooking styles not standardized?

  In all dwellings, the cooking was done using an electric stove, but the stove technologies included both induction cooktops and a ceramic radiant cooktop, as new Table 2 shows. In addition, the models of the cooktops of the same type varied. Following additions were made to the manuscript to provide information about stove models:

  "In all Cases, the stove was electrical, **but both induction and ceramic radiant cooktops were included as Table 2 shows.**"

Table 2. The specification of cooking measurements in Cases I to IV. Considering extra ventilation during cooking, range hoods were divided into two types. Type 1 range hood has its own exhaust channel, whereas type 2 range hood is connected to the exhaust ventilation system of the dwelling

Case	Date	Cooking start time	Decay end time	Cooking duration (min)	Stove type	Extra ventilation during cooking
I	13.9.2022	10:53:28	14:03:07	8.43	Induction	Not used
II	20.9.2022	10:40:00	13:50:00	9.00	Radiant	Range hood, type 1
III	26.9.2022	17:10:12	20:14:22	13.54	Radiant	Range hood, type 2
IV	13.10.2022	10:32:00	13:37:51	5.85	Induction	Range hood, type 1

The differences in stove models are likely to lead to differing cooking styles making the standardization of the cooking style challenging. As mentioned in the previous answers, the main purpose of measurements was to validate the decay functions. Thus, the additional value of conducting measurements as standardized as possible compared to the workload needed was assessed to be rather low from the perspective of this paper's scope. We acknowledge that in future studies focusing on the factors effecting the amount of the cooking-related particle dose standardization should be highly prioritized.

- Just to clarify, did the authors use the air change rate in Lines 69-70 or calculate rates for each site?

  The computation done in the study did not require air change rates and due to that those rates were not calculated. The value given in Lines 69–70 is only presented for contextualization of Finnish ventilation compared to ones in other countries.

- Did whoever was wearing the backpack give an accurate account of their movements during the measurement period?

  Yes, the movements of the backpack-wearing researcher were documented, but they were not reported in the manuscript since backpack measurements were not used in computation. However, our decision that the existence of the backpack measurements is still briefly reported in the paper was made to enable the utilization of the date for further analysis. For that purpose, the logbook was published as a part of the data publication. To explain this decision, the following statement was added to the manuscript:

  "After the cooking, the decay phase of indoor particles was measured for 3 hours, during which the measurement backpack was carried around in different rooms of each dwelling. **However, the backpack measurement data were not further utilized in the computation of this study. For that reason, the movement information of the backpack measurement is only presented in the field log of the data publication to enable further analysis (Vesisenaho et al., 2025).**"

- At what time and on what day did the experiments at each location occur?

This important information was added to the manuscript in a new table, which is now numbered as Table 2:

Table 2. The specification of cooking measurements in Cases I to IV. Considering extra ventilation during cooking, range hoods were divided into two types. Type 1 range hood has its own exhaust channel, whereas type 2 range hood is connected to the exhaust ventilation system of the dwelling

Case	Date	Cooking start time	Decay end time	Cooking duration (min)	Stove type	Extra ventilation during cooking
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III	26.9.2022	17:10:12	20:14:22	13.54	Radiant	Range hood, type 2
IV	13.10.2022	10:32:00	13:37:51	5.85	Induction	Range hood, type 1

- Results/Discussion
  - Can the authors indicate in Figure 2 or in a table in the SI what periods were spent in which room with respect to the backpack measurements?

    As discussed in second to last answer, we decided that the movements of the backpack measurement are excluded from this paper. In the case of Fig. 2, there is a risk that the figure would become unclear if the movement information was added.

  - This is not a major sticking point, but is it possible for the authors to provide information on 'typical' or 'expected' concentrations when cooking chicken and/or when cooking with rapeseed oil to help contextualize the results a little?

    We agree that it would benefit the reader to have the suggested information to contextualize the results. A literature search was done to respond to this need, but the task was proven to be difficult. The only cooking event corresponding to ours, pan-frying chicken using rapeseed oil and induction cooktop, was measured by Tang et al. (2024), but the results were reported as mass concentrations, which hinders the comparison. In that study, the emission of pan-frying was greater than ones for stir- and deep-frying of chicken. The second closest study design was conducted by Yeung and To (2008) with same ingredients but using electric griddle. In that study, the total number concentration of pan-frying chicken fillets during 5-minute SMPS scan varied between 5.14 and 8.58 · 10^5 cm^{-3}. As a comparison, in our measurements, the peak number concentrations measured with DiSCmini were 1.05 · 10^6, 3.06 · 10^4, 6.12 · 10^4 and 4.09 · 10^4 cm^{-3} in Cases I to IV, respectively. This comparison indicates that, excluding Case I, the cooking emissions of this study may have been relatively low. This confirms the already included message of the manuscript:

    "Compared to previous studies, the LDSA concentrations of the cooking event are relatively low in Cases from II to IV whereas in Case I the concentration is comparatively high (Geiss et al., 2016; Pacitto et al., 2018; Scungio et al., 2020; Pacitto et al., 2021)."

To provide the results of the above comparison to the reader, following modifications were made to the manuscript:

"However, in Cases from II to IV, the highest concentrations in the living room were observed after the cooking event. **The peak number concentrations measured with DiSCmini were 1.05 · 10^6, 3.06 · 10^4, 6.12 · 10^4 and 4.09 · 10^4 cm^{-3} in Cases I to IV, respectively. These values can be compared to ones determined by Yeung and To (2008) for pan-frying chicken fillets in rapeseed oil using electric griddle. The comparison to the values of that study, ranging from 5.14 · 10^5 to 8.58 · 10^5 cm^{-3}, confirms that, apart from Case I, the cooking emission was relatively low in this study. Additionally, the** phenomenon **that the peak concentration is reached after the cooking event** indicates that"

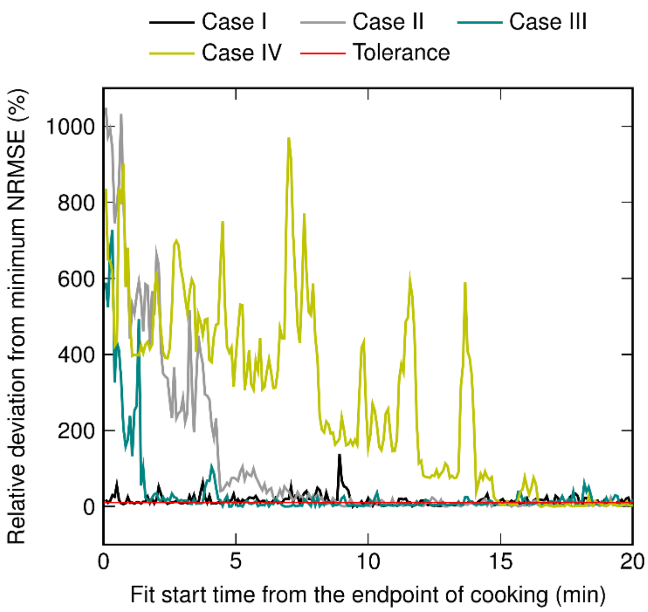
References:

Tang, R., Sahu, R., Su, Y., Milsom, A., Mishra, A., Berkemeier, T. and Pfrang, C.: Impact of Cooking Methods on Indoor Air Quality: A Comparative Study of Particulate Matter (PM) and Volatile Organic Compound (VOC) Emissions, Indoor Air, 2024(1), 6355613, https://doi.org/10.1155/2024/6355613, 2024.

Yeung, L. and To, W.: Size distributions of the aerosols emitted from commercial cooking processes, Indoor and Built Environment, 17, 220 – 229, https://doi.org/10.1177/1420326X08092043, 2008.

- Figure 3: it is difficult to distinguish between case 3 and case 4, please use a different color scheme.

That is true and due to that the colour scheme has been modified as follows:



- Figure 4: too much text, it might be better to put this information in a table or in the main text.

  The amount of text in the figure is more than the usual, but our opinion is that for the reader it is helpful to have the values right next to the graph. We agree that table could be another option, but there is a risk that the table will be further away from the figure.

- Line 345: why, physically (not just in the equation), does it matter more?

  With the monodisperse approximation, coagulation depends quadratically on the particle concentration whereas linearly on the coagulation coefficient. The quadratic dependence results from the fact that collision probability of two particles increases quadratically as a function of particle concentration. The sentence in question is included in the manuscript to avoid the possible misunderstanding that, for example, the effect of coagulation could be stronger in Case II compared to Case I. Through this, it also becomes easier to understand that it is possible that the concentration, and thus effect of coagulation, might not be high enough to determine the coagulation coefficient accurately in Case II. To clarify the above discussed points, the manuscript was modified as follows:

  "The values of coagulation coefficient KLDSA, presented in Fig. 4, vary widely between Cases. It has to be noted that **with the monodisperse approximation the effect of coagulation on LDSA concentration** depends more strongly**, quadratically,** on particle concentration as stated in Eq. (16). For instance, in Case I, the **effect** of coagulation is approximately 40 times **greater** at the beginning of the decay phase compared to Case II despite of the two orders of magnitude lower value of the coagulation coefficient."

- Were the ages of the 'chefs' taken into account in dose calculation (inhalation rate)?

  In the manuscript, it is stated: "The daytime IR of 16.3 l min^{-1} is computed as the average IR excluding sleep or nap time for individuals aged from 21 to 60 years based on the statistics of EPA (2009)." All of the research team members cooking during the measurement belong to this age category. It is true that, the age differences are likely to induce differences in inhalation rates. This could be considered more preciously if the main aim of this study was to calculate doses received by each individual.

- Line 435: can the authors clarify what is meant by 'cooking-related'? Does this entail both the blue bar and the dark gray bar?

Yes, cooking-related includes both, i.e. cooking and mixing phase together with extrapolated decay phase. Thank you for raising this issue up, because it was not well defined in the paper. Now, the definition is added as follows:

"With both decay functions, the cooking-related particle exposure fraction, presented in Fig. 6, varies greatly between Cases. **In this study, the cooking-related particle dose considers cooking and mixing phase together with extrapolated decay phase.** The highest **cooking-related** fraction of..."

- How was the influence from other activities that could potentially emit PM minimized?

  In the dwelling, there were only one member of the research team present in Cases II to IV and two persons in Case I during the measurement. The researcher team members were avoiding actions, such as vacuum cleaning, candle burning, using laser printer or generally doing activities involving burning or thermal processes, that might induce indoor particle emissions. From the decay graphs presented in Fig. 2, it can be seen that the particle concentrations are decreasing pretty constantly, which indicates that there were no other major particle emission sources present during the measurements.

- Line 273: What might be some other explanations?

  Most likely, an important reason for lower concentrations compared to Case I is the temperature of the frying pan. That does not become clear from the initial manuscript, so thank you mentioning this issue. The following sentence was added to the manuscript to resolve this issue:

  "The difference between the concentration levels of Case I and other Cases is **most probably explained by higher frying pan temperature related to cooking style in Case I and** the fact that extra ventilation was not used in Case I."

- Did the authors conduct a comparison between outdoor measurements at city sites and at experimental sites? If not, why not? If so, what did it yield?

  During the data analysis outdoor data of our measurement was compared to the measurement data of Tampere. During our measurements, the relative differences of the outdoor Partector's mean LDSA concentrations compared to the mean LDSA concentration of the city sites were found to be +17, –38, +16 and –16 % in Cases I to IV, respectively. Although the concentration do not exactly match, the difference is, from our point of view, on acceptable level for dose assesment.

- Figure 6: it's a little difficult to see, but is the indoor background in Case 1 similar to the others? If not, why not?

We agree that it is difficult to see from the figure due to the higher y-axis scale. Nevertheless, both indoor and outdoor backgrounds are exactly the same in Case I as in other Cases.

Technical comments

Overall, this paper is very well-written and structured. I have highlighted a few places where there were typographical errors or where the sentence was a little convoluted, as well as some minor suggestions for the structure. I also noticed that the use of past tense vs. present tense is not consistent throughout the paper – my general rule (to be applied as the authors see fit) is to use the past tense to indicate what the authors actually did (experimentally and mathematically) and the present tense for the results themselves. I may not have caught everything, so please review before resubmission just in case.

We want to thank you for these technical comments. Especially, the comment about the tenses in the paper was very valuable and improved the text notably. An effort was made to check and improve the consistence of the tenses. The new Sections, 3.2 and 3.3, for mixing phase algorithm and dose calculation seemed to also help with the more consistent use of tenses. The main principles of the tenses should now be as follows:

1. Measurements and data analysis are described using the past tense
2. The decision made, such as nighttime hours, are also described using the past tense
3. The results are described using the present tense

- Typographical errors:
    - Lines 58-60: sentence is unclear

      We agree that the sentence was unclear. Due to that, effort was made to reformulate the sentence clearer:

      "Lepistö et al. (2023) and Salo et al. (2021) have also found that **when LDSA is used** as a metric instead of PM2.5**,** the differences in **the strength of** the health response to particles between different geographical regions **become narrower**."

    - Line 204: 'With a population' instead of 'With population'

      Suggested correction was made. Thank you for pointing this out.

    - Line 219: '...estimate of the geometric...' instead of 'estimate of geometric'

      Suggested correction was made. Thank you for pointing this out.

    - Line 227: 'underestimation' instead of 'underestimate'

      Suggested correction was made. Thank you for pointing this out.

    - Line 286: 'in the living room' instead of 'in living room'

Suggested correction was made. Thank you for pointing this out.

  - Line 331: change conjugation from singular to plural

    Suggested correction was made. Thank you for pointing this out.

- Restructuring suggestions:
  - The explanation of what the mixing phase is and how it was determined in Section 4.2 and the derivation of equations for dose calculations in Section 4.4 should be moved into the methods section.

    Thank you for this suggestion. We agree that the structure of the manuscript would benefit from the suggested modifications. Thus, new Sections 3.2 *Algorithm for determining the length of the mixing phase* and 3.3 *Dose calculation* are introduced. Due to this modification, some minor changes in text were made to keep the text flowing.

  - Given that the measurements took place only in September and October of 2022, I'm not sure I understand how Figure 5 and Section 4.3 add to the conclusions or novelty of the paper. Can the authors either make the connection more explicit or move them to the SI?

    We agree that presenting the outdoor air quality data of Tampere does not notably add to the novelty of this study. Still, we think that it is necessary to show the outdoor air quality results utilized in dose calculation, at least at some level. The decision of using the annual mean values in dose calculation, was made to provide LDSA dose results that would be as applicable as possible throughout the year. Yet, we admit that using only the data of September and October 2022 could have provided slightly more accurate, but not quite that applicable, results from our data.

    Based on this valuable comment, the Section 4.3 concerning the outdoor air quality was shortened. Additionally, effort was made to clarify the need of these results from the perspective of the dose calculation using following sentences:

    "The outdoor air quality data of Tampere was utilized in the dose calculation described in Sect. 3.3."

    "The above described mean daytime and nighttime LDSA concentrations of Tampere were utilized in the dose calculation."

Referee #2

General comments:

The paper presents an approach to estimate the LDSA dose with and without coagulation considered during decay phases. While the paper is interesting and of utility for the IAQ research community, it requires considerable improvement on technical and writing fronts.

Most of the sentences used in different sections are redundantly lengthy and can be made crisp. The paper seems well-connected, but it is not easy to read and can be improved substantially. Some critical details about the study are missing. For example, more details about the activity time diary, the number of times the cooking was done, and the number of occupants should have been explicitly mentioned. A few of the numbers and sentences were arbitrarily mentioned and should be justified with references.

Some specific comments are as follows.

Thank you for carefully commenting on our manuscript. We admit that our writing style includes fairly long sentences. There are two main reasons for this: 1. to communicate the details of the study as accurately as possible and 2. to ensure that the sentences are well-connected so that the text 'flows' smoothly. As you mentioned, the downside of this approach is that some sentences might become unnecessarily hard to read. To address this issue, some sentences were shortened or clarified otherwise. The issue was also noted when writing additions during this review process. However, the main style explained above was not dramatically changed, because of the encouraging comments of the other referee regarding the writing style and structure of the manuscript. The answers to the specific comments, for example regarding the study design and measurements, are included under each comment.

Specific comments:

Line 30: The Sentence may be reframed for more clarity.

It is true that the sentence is long, but from our point of view it is important to communicate the message with precision. An effort was made to clarify the sentence otherwise by removing non-finite clauses and changing some of the terms as follows:

"Scungio et al. (2020) have proposed that **the particle dose** could be underestimated by 35 % if **assessed using city-scale** outdoor particle concentrations, rather than **personal-scale measurements that account for** both indoor and outdoor exposure"

Line 43: Since the exposure represents the area under the cocn. Vs time curve, it is well understood and documented that decay phases may dominate the overall exposure.

We agree that the major role of decay phase in indoor particle exposure analysis is easily understood. This fact is somewhat mentioned in several previous papers (e.g. Wan et al., 2011;

Isaxon et al., 2015; Pacitto et al., 2018; Zhao et al., 2021). However, the actual contribution, e.g. numerical fraction, of the decay phase versus the active indoor particle emission event is not presented in the listed studies. Based on this comment we made an additional literature search but were not able to find more-in-detail information about the contribution of the decay phase. Anyway, to be more precise, the sentence in question was modified as follows:

"Due to the long decay time, it has been **observed** that cooking-generated particles **can** induce significant exposure after the cooking action, for instance, during a night's sleep (Pacitto et al., 2018; **Zhao et al., 2021**)."

References:

Isaxon, C., Gudmundsson, A., Nordin, E., Lönnblad, L., Dahl, A., Wieslander, G., Bohgard, M., and Wierzbicka, A.: Contribution of indoor-generated particles to residential exposure, Atmospheric Environment, 106, 458 – 466, https://doi.org/10.1016/j.atmosenv.2014.07.053, 2015.

Pacitto, A., Stabile, L., Moreno, T., Kumar, P., Wierzbicka, A., Morawska, L., and Buonanno, G.: The influence of lifestyle on airborne particle surface area doses received by different Western populations, Environmental Pollution, 232, 113 – 122, https://doi.org/10.1016/j.envpol.2017.09.023, 2018.

Wan, M.-P., Wu, C.-L., Sze To, G.-N., Chan, T.-C., and Chao, C. Y.: Ultrafine particles, and PM2.5 generated from cooking in homes, Atmospheric Environment, 45, 6141 – 6148, https://doi.org/10.1016/j.atmosenv.2011.08.036, 2011.

Zhao, J., Birmili, W., Hussein, T., Wehner, B., and Wiedensohler, A.: Particle number emission rates of aerosol sources in 40 German households and their contributions to ultrafine and fine particle exposure, Indoor Air, 31, 818 – 831, https://doi.org/10.1111/ina.12773, 2021.

Lines 45- 50: More recent references should also be included

Considering the translocation of particles to the brain, a more recent reference (Kanninen et al., 2020) was added to the manuscript. Otherwise, we want to note that there are multiple references published between 2019 and 2021. The older references are also referred because our effort was to include the original reference that are cited in more recent papers.

References:

Kanninen, K., Lampinen, R., Rantanen, L., Odendaal, L., Jalava, P., Chew, S., and White, A.: Olfactory cell cultures to investigate health effects of air pollution exposure: Implications for neurodegeneration, Neurochemistry International, 136, 104 729, https://doi.org/10.1016/j.neuint.2020.104729, 2020.

Line 120: The use of β(LDSA) to replace the (vA/V) should be justified, as the deposition velocity will depend on the particle size

That is a valuable point and, actually, β_{LDSA} is also dependent on the particle size, which is not made clear in the initial manuscript. Of course, during the fitting of the decay functions, it is assumed that $D_{LDSA,}$ including also $\beta_{LDSA,}$ is assumed to be constant. In practice, it means that particle size is assumed to be constant. This assumption is commented in Section 4.2 as follows: "In the fitting, the coefficients has to be assumed constant throughout the decay process, although in reality they would vary due to the slow increase of GMD over time." However, it would also be possible to fit the decay functions separately for each particle size during a time step. In that sense, it is important to clarify that β_{LDSA} is dependent on the particle size and, thus, the following modification was made:

"Using the **particle size dependent** deposition coefficient of LDSA β_{LDSA}, Eq. (3) can be simplified to..."

Line 120-125: The reason somehow does not support the replacement of the use of other metrics than the deposition coefficient

The deposition coefficient of LDSA was utilized to simplify Eq. (3). An effort could be made to convert the deposition coefficient form LDSA to number or mass concentration. However, this would require an assumption of the particle size. Consequently, considering that deposition coefficients are not directly comparable due to the dependence on the room geometry, it was decided not to be derived in this study. Additionally, the deposition coefficient is included in the dilution coefficient D_{LDSA}. Because the ventilation flows and the supply air filter efficiency were not determined in this study, we were not able to separate the effects of ventilation and deposition from each other. Thus, there would have not been use for the deposition coefficient conversion.

In equations 1, 3, and 4, subscripts must be used to indicate that the material balance corresponds to specific processes.

Thank you for this comment. The clarity of the manuscript was improved by adding the suggested subscripts. Additionally, Eq. 9, 15 and 16 were modified according to the same principle.

Equation 5: Some of the terms from Eq.1 are missing in Eq. 5.

In the manuscript, it is explained that the 'missing' terms are actually included in constant background source term S_{LDSA}, as already indicated in the manuscript: "...including the supply ventilation term of Eq. (1) to the constant background source S_{LDSA} assuming that outdoor LDSA concentration stays constant..."

Line 134: Looking at Eq (6) should be 'Looking at Eq (7)'

Thank you for pointing out this mistake. The mistake is now corrected.

Line 139: What is the difference between initial and background concentration? Are they the same, or is the initial concentration the one that is at the start of the decay phase?

The initial concentration is the concentration at the start of the decay phase, which equals the end of the mixing phase. The background concentration is the concentration in the indoor space without any indoor particle sources. In this study, it is determined using the background measurement before the cooking event. These points are already described in Section 4.2: "The background concentrations were computed as the mean concentration of the background measurement in each living room, whereas the initial concentrations were defined as the concentration of the first data point in each decay phase." However, to further clarify the definition of the initial concentration in Section 2 following statement was added to the manuscript:

"...where $C_{LDSA,i,0}$ is the initial indoor LDSA concentration **at the start of the decay process**."

Line 147-149: The monodisperse assumption seems crude, considering that coagulation physics is well understood. Any background checks were performed before assuming that the studied aerosols are equal in size, at least GSD could have been calculated to know the polydisperse nature and how far this assumption would hold?

It is true that the monodisperse assumption is crude considering coagulation. Yet, based on the results of this study, it seems to provide sufficient results. The consideration of the polydisperse emission in the decay function would complicate the already complex from of it. And actually, numerical modelling could be better option than the analytical solution in that case. All in all, the crude assumption was made to simplify the situation to provide an as simple as possible decay function that would still result into sufficient results.

Considering the measurements, the devices utilized in this study are not able to provide information about the GSD. We agree that this is unfortunate, because it would have been helpful to demonstrate the capabilities of the decay functions more accurately.

Line 153: "and −1, that is the change of particle number when two particles collide and adhere forming larger particle." is a wrong statement. The negative sign indicates that the number conc. is reducing with time.

We agree that the negative sign indicates reducing number concentration. However, when two particles collide and adhere, the change of number concentration is –1 ($\Delta N = -1$). In the case of LDSA, the change in the same situation is different as stated in Eq. (13) ($\Delta LDSA \approx 2\pi a\,(2^{-2/3}-1)\,d_{p,1}$). It is true that explaining the minus sign that way is not the easiest to understand, but it is made to make the derivation of the Eq. (15) more understandable. The –1 term can be derived from the coagulation part of Eq. (5) in Whitby and McMurry (1997) applying the monodisperse assumption as follows:

$$d\,C_N = \frac{1}{2}\int_0^\infty \int_0^\infty \left(d_{p_1}^3 + d_{p_2}^3\right)^{k/3} K\left(d_{p_1}, d_{p_2}\right) C_{N,d_{p_1}} C_{N,d_{p_2}}\,\mathrm{d}\,d_{p_1}\,\mathrm{d}\,d_{p_2}$$
$$-\frac{1}{2}\int_0^\infty \int_0^\infty \left(d_{p_1}^k + d_{p_2}^k\right) K\left(d_{p_1}, d_{p_2}\right) C_{N,d_{p_1}} C_{N,d_{p_2}}\,\mathrm{d}\,d_{p_1}\,\mathrm{d}\,d_{p_2} \qquad |\text{ for } C_N,\ k=0$$

$$= \frac{1}{2}\int_0^\infty \int_0^\infty 1 \cdot K\left(d_{p_1}, d_{p_2}\right) C_{N,d_{p_1}} C_{N,d_{p_2}} \,\mathrm{d}\, d_{p_1} \,\mathrm{d}\, d_{p_2}$$

$$-\frac{1}{2}\int_0^\infty \int_0^\infty 2 \cdot K\left(d_{p_1}, d_{p_2}\right) C_{N,d_{p_1}} C_{N,d_{p_2}} \,\mathrm{d}\, d_{p_1} \,\mathrm{d}\, d_{p_2} \qquad | \text{ from monodisperse approximation } d_{p_1} = d_{p_2}$$

$$= \frac{1}{2} \cdot 1 \cdot KC_N^2 - \frac{1}{2} \cdot 2 \cdot KC_N^2$$

$$= \frac{1}{2}(1-2)KC_N^2$$

$$= \frac{1}{2}(-1)KC_N^2$$

$$= \frac{1}{2}\Delta N\, KC_N^2$$

The corresponding derivation for mass concentration, in which case k = 3, gives zero as the change of mass when to particles coagulate. Also, it has to be noted that the Eq. (13) gives negative values, but the negative sign is hidden in the term $2^{-2/3}-1$. Following equations are further explaining the situation:

$$\frac{\mathrm{d}\, C_N}{\mathrm{d}\, t} = \frac{1}{2}\Delta N\, KC_N^2$$

$$= \frac{1}{2}(-1)KC_N^2$$

$$= -\frac{1}{2}KC_N^2$$

$$\frac{\mathrm{d}\, C_{\mathrm{LDSA}}}{\mathrm{d}\, t} = \frac{1}{2}\Delta \mathrm{LDSA}\, KC_N^2$$

$$= \frac{1}{2}2\pi a\left(2^{-\frac{2}{3}} - 1\right) d_{\mathrm{p}} K \frac{1}{(\pi a)^2} C_{\mathrm{LDSA}}^2 d_p^{-2}$$

$$= \frac{1}{\pi a}\left(2^{-\frac{2}{3}} - 1\right) d_{\mathrm{p}}^{-1} KC_{\mathrm{LDSA}}^2$$

References:

Whitby, E. R. and McMurry, P. H.: Modal aerosol dynamics modeling, Aerosol Science and Technology, 27, 673 — 688, https://doi.org/10.1080/02786829708965504, 1997.

Equation 10: The equation seems dimensionally inconsistent if LDSA and N are concentrations as in the previous text. Please clarify.

To address this comment, the usage of LDSA and N in the manuscript was checked. The check showed that LDSA and N are only used when referring to *lung deposited surface area* (μm^2) and *particle number* (dimensionless), respectively. The LDSA and particle number concentrations are always written as C_{LDSA} and C_N, respectively. According to this notation, the dimensions of Eq. (10) are consistent.

Line 164: Please quantify 'reasonable accuracy'. Also, what about PM not lying in 30-300 nm range?

We agree that the expression was unclear. To solve this issue, following modifications were made to the manuscript:

"In the size range of 30 to 300 nm, the model can be parameterized using the expression $DF_{al} \approx a d_p^{-1}$, where a is 14.37 nm. **The coefficient of determination (R^2) for the parametrization is 0.987.**"

Outside the given particle size range, the accuracy of the parametrization decreases, which also decreases the accuracy of the entire decay function. Thus, it is recommended only to use the decay function that considers coagulation in that size range. Considering that most of the sensors measuring LDSA, including Partector and DiSCmini, are utilizing a parametrization of the same type, the limited accurate size range should not become an additional issue. The Section *Summary and conclusions* includes following statement emphasizing the recommended size range:

"The decay functions could also be utilized to describe the decay processes of other indoor particle sources noting that, in the more complex decay function, the consideration of coagulation includes the assumption that the size distribution is dominated by particles with diameter in the range of 30 to 300 nm."

Table 1: The mentioned floor area is of the entire apartment or just the kitchen.

The mentioned floor area is the total floor area of the dwelling. From our point of view, this is indicated in the following sentence: "The measurement sites included one apartment, two terraced houses, and one detached house with floor area ranging from 22 m^2 to 128.8 m^2." This was further clarified to the Table 1 by changing the column header 'Floor area (m^2)' to 'Total floor area (m^2)'.

Table 1. Further, it would be good to mention what type of ventilation was there in the kitchen

This information was added to the new table 2 using range hood types 1 and 2 that are explained in the caption:

Table 2. The specification of cooking measurements in Cases I to IV. Considering extra ventilation during cooking, range hoods were divided into two types. Type 1 range hood has its own exhaust channel, whereas type 2 range hood is connected to the exhaust ventilation system of the dwelling

Case	Date	Cooking start time	Decay end time	Cooking duration (min)	Stove type	Extra ventilation during cooking
I	13.9.2022	10:53:28	14:03:07	8.43	Induction	Not used
II	20.9.2022	10:40:00	13:50:00	9.00	Radiant	Range hood, type 1
III	26.9.2022	17:10:12	20:14:22	13.54	Radiant	Range hood, type 2
IV	13.10.2022	10:32:00	13:37:51	5.85	Induction	Range hood, type 1

Line 227-229: It would be for a specific set of aerosols; the results can vary for different emission sources. Therefore, it would be good to mention more details about these comparative studies.

In the manuscript, it is mentioned that Todea et al. (2017) tested the sensors "with a large variety of test aerosols" calculating the presented averages from those results. We acknowledge that the measured aerosol has an effect on the accuracy of the measurement devices. Still, our opinion is that the current description of measurement accuracy is enough from the perspective of our study, especially because the main aim of the study is to validate the decay functions.

References:

Todea, A. M., Beckmann, S., Kaminski, H., Bard, D., Bau, S., Clavaguera, S., Dahmann, D., Dozol, H., Dziurowitz, N., Elihn, K., Fierz, M., Lidén, G., Meyer-Plath, A., Monz, C., Neumann, V., Pelzer, J., Simonow, B. K., Thali, P., Tuinman, I., van der Vleuten, A., Vroomen, H., and Asbach, C.: Inter-comparison of personal monitors for nanoparticles exposure at workplaces and in the environment, Science of the Total Environment, 605–606, 929 – 945, https://doi.org/10.1016/j.scitotenv.2017.06.041, 2017.

Line 238-239: What was the number of occupants in each of these dwellings? The person carrying the backpack was moving in which direction and to which room. Brief details needed

The backpack was worn by research team members and, excluding Case I, there were only the backpack-wearing research team member present at the measurement site. In Case I, there were two research team members present at the dwelling. The movements of the backpack-wearing researcher were documented, but they were not reported in the manuscript since backpack measurements were not used in computation. However, our decision that the existence of the backpack measurements is still briefly reported in the paper was made to enable the utilization of the date for further analysis. For that purpose, the logbook was published as a part of the data publication. To explain this decision, the following statement was added to the manuscript:

"After the cooking, the decay phase of indoor particles was measured for 3 hours, during which the measurement backpack was carried around in different rooms of each dwelling. **However, the backpack measurement data were not further utilized in the computation of this study. For that reason, the movement information of the backpack measurement is only presented in the field log of the data publication to enable further analysis (Vesisenaho et al., 2025).**"

Line 248: Given the quantity of oil relative to the chicken, the frying mode of cooking seems to be implausible. Maybe 'pan frying' or something more appropriate.

Thank you for this suggestion. The term 'frying' was substituted for the term 'pan frying' to be more explicit.

Line 254: Please briefly describe the movement of the cook or occupant? Also, was there any activity diary maintained in each of the cases

Please check the second to last answer.

Figure 2: What is the reason for the higher standard deviation in LDSA in case 1? Why is there such a difference in values between case 2 and case 4? Both are spacious apartments per floor area in Table 1.

In Fig. 2, the given standard deviations are for the outdoor air as the subscript indicates. In Case I the dwelling was located in an urban area, whereas the dwellings of other Cases are located in suburban areas. Because of that, Case I was expected to have the highest standard deviation as indicated in the manuscript:

"The dwellings of Cases II to IV are located in a suburban area, whereas the apartment of Case I is located in an urban area, which might have an effect on background particle concentration."

Figure 2: Apart from case 1, for the rest of the cases, I think the same y scale could be done, so that the subplots are easier to compare.

The suggestion was tested, but the especially in the Case II the graph became too small to read accurately. Due to this, as the y-scale of Case I would anyways differ from other Cases, we decided to keep the original y-scales.

Line 272: I think as the cooking time varies, the peak concentration might depend on that too, so cooking time should be mentioned in Table 1.

This information was added to new Table 2 describing the cooking measurements.

Table 2. The specification of cooking measurements in Cases I to IV. Considering extra ventilation during cooking, range hoods were divided into two types. Type 1 range hood has its own exhaust channel, whereas type 2 range hood is connected to the exhaust ventilation system of the dwelling

Case	Date	Cooking start time	Decay end time	Cooking duration (min)	Stove type	Extra ventilation during cooking
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IV	13.10.2022	10:32:00	13:37:51	5.85	Induction	Range hood, type 1

Line 303: A comment about internal mixing characteristics, such as fan or recirculation, would be helpful for readers.

Thank you for pointing this out. In the studied dwellings, mixing was not enhanced using a fan or recirculation. This information added to the manuscript using following expression in Section 3.1:

"After the cooking, the decay phase of indoor particles was measured for 3 hours, during which the measurement backpack was carried around in different rooms of each dwelling **and no extra ventilation or mixing was applied**."

Line 310-311: Explain in brief. The algorithm used and the type of fitting done.

The algorithm is already described in detail in Section 4.2: “The length of the mixing phase is determined by an algorithm based on the normalized root mean square error (NRMSE) of a fitted decay function following Eq. (21). The normalization is performed using the mean value of data points, and the decay function including coagulation is selected for the algorithm because the highest concentrations, where coagulation might have a substantial role, are typically measured in the mixing phase. In the algorithm, the decay function is fitted to the time series of LDSA concentration using a non-linear least squares method and with a varying starting point. In the first fit, the starting point is the first measurement point after the cooking event and then it is shifted 5 seconds forward in every fit covering the first 20 minutes following the cooking event. The relative deviation from minimum NRMSE in each Case is presented in Fig. 3, which shows that the NRMSEs stabilize near the minimum at a certain Case dependent time indicating the length of the mixing phase. In the algorithm, the starting point of the first fit having NRMSE within 10 % of the minimum NRMSE in each Case is defined as the end point of the mixing phase.” The fitting method, that is non-linear least squares, is also mentioned in the previous extract.

Line 312: What is the frequency of data logging in these instruments, and did you do any other averaging other than that 5-minute one?

The initial time resolution of Partector sensor was 1 s. After the measurements, Partector data were averaged to the time resolution of 4 s using Partector Data analysis tool. However, in Figures

Line 350: How can both be compared, as both of them have different units!

The coagulation coefficient for number concentration, determined by Zhao et al. (2021), was converted to the coagulation coefficient for LDSA using Eq. (17). This conversion allows us to compere the literature value to ones computed in our study.

References:

Zhao, J., Birmili, W., Hussein, T., Wehner, B., and Wiedensohler, A.: Particle number emission rates of aerosol sources in 40 German households and their contributions to ultrafine and fine particle exposure, Indoor Air, 31, 818 – 831, https://doi.org/10.1111/ina.12773, 2021.

Line 360: Case 1 had a higher concentration and no ventilation, so the decay period should have been greater than in the other cases. Was the surface-to-volume ratio high here? Please explain.

We agree that the decay period is short in Case I compared to other Cases and considering the higher initial concentration. Thus, the ventilation and wall losses have to be greater than in other Cases as the higher D_{LDSA} value indicates. Please note that the in Case I no extra ventilation during cooking was applied but the apartment was mechanically ventilated throughout the measurement. In addition, the floor area of the dwelling in Case I is low compared to other Cases, which probably leads to the higher wall loss rate. However, the more effective ventilation is most likely the main contributor to the high decay rate.

Line 394-395: Is there a way to see the difference between exposure across all the periods?

The cooking-related particle dose is divided into *cooking and mixing phase* and *extrapolated decay phase* in Fig. 6. The reason for not dividing the former further into the cooking and mixing phase is that the cooking emission most likely spreads into the living room mostly during the mixing phase. This makes the cooking phase of the living room measurement relatively irrelevant.o

Line 405: Formatting/grammatical error

The sentence in question was revised as follows:

“Despite integrating to infinity, the total dose of **a single** cooking event is suitable for assessing a daily dose**,** because in all Cases the integral increases **by less than** 1 ‰ after 12 hours of decay.”

Line 418: Where did the 21 hr 26 min come from?

The value is from Hussein et al. (2012) as the previous sentence indicates: “…and the average time spent indoors in Finland provided by Hussein et al. (2012).” To make this clearer, following modification was made:

“The sleep time **assumed in this study,** 8 hours**,** is subtracted from the indoor-spent time of 21 hours and 26 minutes.”

References:

Hussein, T., Paasonen, P., and Kulmala, M.: Activity pattern of a selected group of school occupants and their family members in Helsinki – Finland, The Science of the total environment, 425, 289 – 292, https://doi.org/10.1016/j.scitotenv.2012.03.002, 2012.555