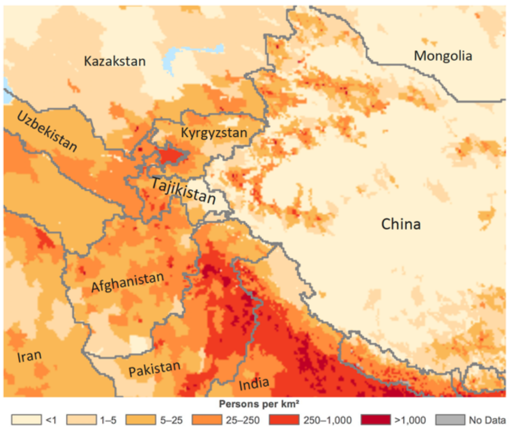
Thanks to the reviewer for his/her helpful advice. We appreciate the reviewer's help and effort in reviewing this paper. The answers to the reviwers' are listed below.

General comments

This paper reports the lidar observations in Kashi, China located west of the Taklamakan Desert. The location is very interesting, and the quality of the observations with a multi-wavelength Raman lidar looks high, and consequently the results merit publication. However, the discussion on dust characterization in the present manuscript is only conceptual and very ambiguous. No strong conclusions are obtained. The authors discuss polluted dust cases, but the definition of polluted dust is not clear. The location of the observation is relatively clean except for desert dust. Is the polluted dust a mixture with anthropogenic air pollution? Is it external mixture or internal mixture? Probably, it would not be possible to characterize it only with lidar data.

Answer: We have to admit that, with lidar measurements, we are not able to provide further information to tell the exact species of the pollutants nor the mixing state of dust and pollution, although they are very important information. To obtain such information and to get "strong conclusion", in-situ measurements are required.

According to long-term observations in <Li et al. 2018: Comprehensive study of optical, physical, chemical, and radiative properties of total columnar atmospheric aerosols over China: an overview of sun--sky radiometer observation network (SONET) measurements> and Figure 2, the role of anthropogenic aerosol is not negligible, so Kashi cannot be simply regarded as a 'clean site'. Kashi is a populated city in Xinjiang (see the figure below, referring to <Doxsey-Whitfield, Erin, et al. "Taking advantage of the improved availability of census data: a first look at the gridded population of the world, version 4." Papers in Applied Geography 1.3 (2015): 226-234.>), anthropogenic emission should be reasonably expected to occur. As to the mixing state, it is out of the scope of this paper and beyond what we can obtain on the basis of what we have.



Variability of the characteristics of "pure" dust is not well understood. Also, optical parameters are dependent on particle size distribution even if the composition is the same. The manuscript should be rewritten, in my opinion, with more focus on detailed comparison of the observed parameters (lidar ratio, particle depolarization ratio, Angstrom exponents) with previously reported results. The discussion with Table 3 in the present manuscript is not sufficient. Discussion on the change in optical characteristics by mixing with pollution should be given, if “polluted dust” is discussed.

Answer: The definition of ‘pure dust’ is given in the beginning of the ‘Discussion’ section. In this paper, the ‘pure’ Taklamakan dust is defined with PLDR >0.32 at 532 nm and the EAE(355-532) smaller than 0.1. The identification of Taklamakan dust is also confirmed with back trajectory. The definition of polluted dust is “PLDR <0.3 at 532 nm and EAE >0.2”. Again, back trajectory is used to support the identification. We agree that the optical properties are dependent on not only the composition but also the size distribution. For example, dust aerosol with different fraction of fine dust could present different optical properties, such as BAE, PLDR… This issue is added in the manuscript and the discussion part is improved.

Detailed comments

Line 19: T yr-1 -> Tg yr-1

Answer: Corrected.

Line 28-35: The authors should describe how the lidar data can be used as input and validation of models.

Answer: A common way of involving lidar data into models is to simulate lidar profiles (of lidar signal, backscatter coefficient profile, extinction profile or depolarization profile) with the output or description of models for a model-given atmospheric state. For example, Sekiyama et al. 2010 assimilated the backscatter coefficient and depolarization profiles of CALIPSO Level 1B data. In the model, the backscatter coefficient equals to the sum of backscatter coefficients of several aerosol component, such as sulfate, sea-salt and dust, whose concentrations are model prognostic variables. Zhang et al. 2011 and Campbell et al. 2010 chose to deal with the extinction coefficient of CALIPSO in mass transport model. Apart from satellite lidar, modelers also used ground-based lidar measurements as input of models. Wang et al. 2013 used AirBase lidar network data to simulate PM10 concentrations. As to model validation, it mostly depends on the output of models and the variables to be validated. In Yu et al. 2010, both vertical profiles, e.x. extinction profile, and integrated variable, e.x. AOD from CALIPSO are used to validate the GOCART model. However, the authors consider this detailed information is not so relevant to the topic of our paper. So, a brief description and a list of references given in the manuscript will be sufficient.

Line 75-77 “Moreover, there are populated cities in the neighboring countries such as Kyrgyzstan, Tajikistan and Pakistan. Under favorable meteorological conditions, various aerosol, for example, pollution, could be potentially transported to Kashi and mix with dust aerosols. “: This statement is not convincing, looking at the map.

Answer: In Figure 14, the air mass clustering indicates that the air mass arriving at the observation site may be originated from Kyrgyzstan, Tajikistan, Afghanistan… The air mass coming from Pakistan are not seen by the back trajectory, so we decide to exclude it from the manuscript.

Line 96-98: To my knowledge, the error analysis cannot be this simple. The error in extinction must be different from that in backscatter. Also, the error must be dependent on height and the background radiation. It should be mentioned that the Raman lidar measurement was limited in the nighttime, if so. In addition, it would be better to have some descriptions about the advantage of using rotational Raman instead of vibrational Raman at 532 nm.

Answer: A sentence presenting the advantage of rotational Raman channel has been added and one reference paper has been given. That measurements were made in nighttime has been added in the manuscript. The error estimate is presented in the appendix <Hu et al. 2019: Long-range-transported Canadian smoke plumes in the lower stratosphere over northern France>, so it is not repeated in this paper. The error is height dependent but here we selected typical values at a certain vertical level, calculated the error and then apply it to all the vertical levels. The 15% of error is a conservative value derived with 10% of error in the backscattering coefficient, volume depolarization ratio and 200-300% in the molecular depolarization ratio. We re-calculated the error more carefully and find that in some cases, the error at 355 nm exceeds 15%, for example, Case 3. The errors in the upper layer and lower in Case 3 and 4 are calculated separately. Two examples of the calculated errors are shown in the following tables:

Case 1:

Wavelength (nm)	R	VLDR	MDR	E_R	E_VLDR	E_MDR	PLDR	E_PLDR
355	2.6	0.19	0.015	10%	10%	200%	0.33	15%
532	9.80	0.31	0.020	10%	10%	300%	0.36	11%

Case 3:

Wavelength (nm)	R	VLDR	MDR	E_R	E_VLDR	E_MDR	PLDR	E_PLDR	
355	1.7	0.83	0.015	10%	10%	200%	0.21	21%	Upper layer
532	2.88	0.16	0.010	10%	10%	200%	0.24	13%	
1064	10.0	0.23	0.020	20%	10%	300%	0.26	11%	
355	1.64	0.11	0.015	10%	10%	200%	0.30	24%	Lower layer
532	4.58	0.25	0.010	10%	10%	200%	0.34	12%	
1064	28.0	0.30	0.012	20%	10%	300%	0.31	10%	

Figure 3: The periods of Case1, 2, 3 and 4 should be indicated in Figure 3.
Answer: Corrected.

Figure 5: Case1, 2, 3 and 4 should be indicated in Figure 5.
Answer: They were indicated in the caption of Figure 5, so we think it is not necessary to be indicated on the figure.

Figure 3: Legend “500 nm” should be AOD (500 nm).

Answer: Corrected.

Line 166-168: The backscatter coefficient at 1064 nm below 1800 m should be indicated in Fig. 6.
Answer: On 09 April 2019, the aerosol content was very high, so the signal at 1064 nm is not useable because of signal distortion. This is the reason why it was not plotted in Figure 6. The explanation has been given in the manuscript.

Line 169-170: "EAE" and "BAE" are not defined.
Answer: Thanks. It has been corrected.

Line 183-187: Is the description consistent with Figure 3?

L182-187: "… limit of the sun/sky photometer, so the AERONET and SONET retrieval can not be applied. A large and intense plume was first detected in the morning of 23 April 2019 (Figure 4). And on 24 April, a hot spot of UVAI appeared over the observation site. The daily average of AOD is 3.63 and Ångström exponent is about -0.01, according to the daytime sun/sky photometer measurements… "

Answer: Yes, it is consistent. I am not sure what inconsistencies you have observed in this paragraph. I guess maybe you mean the values of AOD and AE? The values we mentioned in this paragraph are daily averaged values, not the instantaneous values in Figure 3. If you were wondering why we say "an intense plume was detected on 23 April", but that was not reflected by Figure 3, the answer is that this plume was not over our observation site. I hope I got your question.

Line 227-228: What is the "clear evidence of polluted dust"?
Answer: It is the decrease of PLDRs and increase of EAE, which indicates the occurrence of fine particles and particles with more spherical shapes. The increase of BAE also corroborates that aerosols above 2200 m are not the same with those below 2000 m. You might want to point out that pollution may not be the only cause, the deposition of coarse-mode and giant particles could also lead to this effect. We agree, the manuscript has been improved with taking into account this issue.

Line 229-232: The structure at around 2500 m is interesting and should be studied further. Is the type of dust (or "polluted" dust) the same in 1000-2200 m and 2400-2800 m or different? Why relative humidity was high in 2400-2800 m?
Answer: They are different aerosol types since signatures in PLDR, EAE and BAE are different. The WVMR is also a tracer of air mass. The relatively higher WVMR or RH at 2400-2800 m indicates that the air mass at 2400-2800 m could have different origins with the air mass at lower altitudes. This is one reason why we supposed it is polluted dust. But the increase of WVMR is not significant enough to confirm that they are definitively different air mass.

Figure 9: Captions for (c) and (d)
Answer: The caption has been complemented.