The manuscript by Sarah Albertin reported the measurements of diurnal variations in oxygen and nitrogen isotopes of atmospheric NO2 and NO3 collected at a mountain site, and from which they discussed the conversion mechanisms of NOx and nitrate, as well as the implications for NOx source emissions. Overall the manuscript represents a step forward in understanding the sources and transformations of NOx using stable isotopes, though I have some comments which should be addressed before the manuscript can be published.

Major comments:
1) the way to derive RO2 using measured D17O(NO2): I don't think it make any sense to use the other method (i.e., Case B) to estimate RO2 and compare the results with Case A.

    First of all, as shown in Table 1, in both SP1 and SP2, neither the derived RO2 concentration at each sampling period nor the averages can be considered as "consistent"; Don't get the points how can these values can be called "closeness" .

    Secondly, to derived RO2 from D17O(NO2) (i.e., Case A), one simply assumes that it is only RO2 completes with O3 to oxidize NO as indicated by Equation 13. This ignores the contributions of HO2. While in Case B, a RO2 /HO2 ratio of 0.859 was applied. So does this mean that in Case A the same ratio of RO2/HO2 can also be applied and then compared with Case B? In fact, it is highly doubtable that the same ratio in one study can be applied to another, given variations in concentrations of CO, CH4 and NMVOCs, don't even to mention the uncertainties associated with the empirical formula of HO2 calculation using O3.

So just saying from D17O(NO2) to estimate RO2 and note it is actually representing the sum of RO2 and HO2 is enough here. This provides a new methods to estimate RO2 and HO2 radicals, and can be verified with actual measurements in the future. The current way of Case B is just too uncertain and the results are not comparable.

2) the discussion on the D17O(NO3-) values of SP2: there are several issues, first, some values in Table 2 are wrongly calculated (this is an indicator that the manuscript should be better proofread):

The night average values of D17O(NO2) in SP2 night, is this really 29.8 permil? In Figure 2 it never exceeds ~25 permil at nights. This leads to doubt on the calculated D17O(NO3-) values at SP 2 night as you need D17O(NO2) to calculate D17O(NO3).

In this same table, the difference between the calculated and observed D17O(NO3-) values at nights are simply wrong, it is -0.4 and -2.1 for SP 1 and SP 2.

Now back to the discussions on the dynamics of D17O(NO3-), I agree that the high D17O(NO3-) values in SP 2 (both compared to SP1 and calculated values from D17O(NO2)) indicate no local surface NO2 oxidation, but it may also indicate no local nitrate production. Regional transport of nitrate could explain the disconnections between the observed D17(NO2) and D17(NO3-). It is problematic to attribute the anomalous D17O(NO3-) in SP2 (Only) to dynamics of boundary layer structure, as which may explain the high D17O(NO3-) at the daytime in SP 2, but how about at the night? Ass shown in Figure 2, in SP2, not only day time D17O(NO3-) are different,

but also the night values (apparently higher than D17(NO2) in SP2 night but not in SP 1 night). The authors paid too much attentions to the observations of Beijing and tried to make analogies between the two in order to explain their observations. However, the authors overlooked the differences in concentrations of PM2.5, PM10 and nitrate between the two days, and all of these concentrations in SP 2 are higher in SP2 than SP1, would this suggest a transport events and non-local sources of nitrate? Note the d15N(NO3-) values and its relationship with d15N(NO2) are also different in SP2 compared to SP1, this may indicate the same thing: a regional transport event occurred and which brought non-local nitrate. The d15N and D17O data should be combined to discuss at this point.

The last, references or other evidence should be provided when attributed the high load of PM10 in SP2 to Saharan dust, e.g., back trajectory analysis or something similar.

Technical comments:

1) Blank correction of isotope measurement of NO2: I wondered where the blank comes from, if it is coming from the chemicals used to trap NO2, there is no problem to correct as what was done here; however, if the blank NO2 was coming from absorption of atmospheric NO2 during the preparation and the installation of the coated denuder tubes, it won't makes sense to do the correction by assuming blank D17O(NO2) = 0 permil, which would over correct.

2) Equation (4) and relatives in the text: don't get the point why defines a new term T_{NO+O3} instead of using what has been long used in the literature, i.e., the A value to represent the fraction of O3 oxidation of NO and this "A" is almost reserved in the study of D17O of nitrate. I suggest to keep consistent with the literature and don not define new terms unless necessary so that peers can easily follow. I understand here the authors used "A*" later to define the lifetime differences, but it can be simply replaced with any other symbols.

3) Line 246: These latter, not "later"

4) line 409, I would not call 29.0 +/- 2.2 is consistent with the values of 26 +/- 1 in the literature. In fact, why not just use the relationship of D17O(O3*) = 1.5 D17O(O3)bulk? The latter would derive a lower D17O(O3)bulk which is more consistent with Vicars and Savarino 2014;

5) line 458: it should be $D17O_{NO2+O3}$(NO3) ? Again, if considering terminal O transfer when reacting with O3, why not simply using the relationship of 1.5? The transfer function D17O from O3 to NO and NO2 are different?