The comments of referee #2 are repeated below in black. Our replies are in blue and changed text in the revised manuscript is in red.

This paper presents a further development of a CRD instrument previously presented, modified such that it can detect particulate NOy through thermal desorption after the use of a stripper to remove gas phase NOy. This is an interesting concept and offers an alternative method of integrated quantification of particulate NOy compared to other techniques such as semicontinuous IC and AMS. Two specific conceptual advantages this has over the AMS is the lack of a size cutoff and the fact that this has a more direct quantification of atomic nitrogen, rather than electron ionisation fragments. The advantages over IC is the reduced complexity of the measurement and ability to measure total organic particulate reactive oxidised nitrogen.

The technique is described in detail and some initial validation work comparing against an AMS is presented resulting in very favourable correlations, so this is within scope for AMT. While I have a number of technical queries and corrections, this should not detract from the fact that this presents a novel approach to what has always been a tricky measurement in atmospheric science, so I am happy to recommend publication with minor corrections.
We thank the referee for these comments and positive assessment of our manuscript.

Major comments:

One major conceptual point not directly addressed is the propensity for ammonium nitrate to dissociate to ammonia and nitric acid at low relative humidities, which may happen to an extent during the drying stage, after which they would then be removed by the denuder. This process has previously caused artefacts with HTDMA systems (e.g. https://doi.org/10.5194/acp-7-6131-2007) and seems dependent on how long the particles were subjected to a low RH for. This potential issue needs addressing the and the typical time particles spend at low RH after being sampled should be specified. Note that the time spent in the chamber at low RH prior to sampling is a separate issue, but this will affect the AMS and D-TD-CRDS equally.
The thermal decomposition of ammonium nitrate in the HTDMA that the reviewer refers to happens over a period of ~ 1 minute. This is much longer than the residence time in our drying system (a couple of seconds). We have added a statement (in section 2.4) to the paper.
"At a flow of 2.1 SLM, the residence time in the drying system and denuder is ~3 s and thus comparable to that found between the differential mobility analyser (DMA) and AMS-inlet in AMS calibration systems for which loss of ammonium nitrate through thermal decomposition to HNO3 and NH3 is known to be insignificant."

Minor comments:

Line 77: The other techniques summarised here only cover a specific subset of methods for the measurement of bulk oxides of nitrogen in aerosol, but other techniques are used, such as molecule-resolving techniques like CIMS, ESI and liquid/gas chromatography, or spectroscopic techniques such as NMR, FTIR and UV-Vis. While I would not expect a comprehensive review here, the authors should be more specific about the types of technology they are comparing their technique against.
We have specified "online" techniques that measure ambient air "in-situ" (i.e. we do not discuss methods in which samples are collected and stored (e.g. on filters) for later spectroscopic or spectrometric analysis.

"A number of online techniques are available to measure (in-situ) the nitrate content of ambient aerosol…."

Line 80: Are there problems with NO2 photolysis at 405 nm, or extinction caused by the O3? I'd like to assume not, but I couldn't find references to either in the Fridrich et al. (2020) paper, so it would be good to have these confirmed.
Photolysis of $NO_2$ is not an issue (for any of the CRDs that measures $NO_2$): The in-cavity light intensity is too low and this is well established. $O_3$ does not represent an interfering trace gas as its cross-section at 405 nm is too low.

Line 175: The technical details of the RH sensors must be specified, because the capacitance probes typically used for atmospheric work lose accuracy at lower humidities. Although it is noted that the RH thresholds referred to here are purely operational, so the absolute accuracy of the RH measurements probably isn't important.
The information has been added to the caption of Figure 1.
"The flow through the relative humidity (RH) sensors (iST, type-HYT939P, 0-100% RH, accuracy 1.8 %) was regulated by needle valves to ~ 20 sccm"

Line 283: What orientation was the t-piece in? If there was an asymmetric 10:1 split in flows (as implied) this could lead to an uneven transmission of particles over the two flows, as the centreline of a laminar flow could be exclusively following the major flow. If the minor flow was taken from the centreline using a subsampling tube (a common feature on AMS systems), this should be specified.
The T-piece was oriented such that the minor flow (to the AMS) was straight, whereas the 1 L flow was at a right-angle. This is mentioned in the text:
"…a stainless-steel T-piece was connected in front of the AMS inlet. The 100 sccm flow to the AMS passed straight through the T-piece with 1 SLM drawn through the T at 90°."

Line 298: A 10% error on the airbeam correction factor seems very high, especially for a discrete experiment such as this. How is this derived?
The reviewer is correct, this uncertainty was overestimated. We have modified the text and Figure 5.
"The uncertainty associated with the inorganic nitrate mass concentrations is ~16 %, which is related mainly to uncertainty in IE (16%) with a minor contribution from variability in the air beam correction factor (3%).."

Line 307: Strictly, the LOD should be determined based on filtered air, not the 'beam blocked' signal, as there may be gas phase interferences. Although this is likely to be minor for m/z 30, but if the authors have this data they should use that instead.
We do not have data using a filter for these experiments

Line 358: Not commented on is whether this instrument could be of use for more refractory nitrate species such as sodium or calcium nitrate, but given the desorption tube operates at a temperature higher than the boiling point of both, one might expect this to be the case. If it is able to, this would offer another potential benefit over the AMS, so should be mentioned.
As described in Friedrich et al, when operated at higher temperatures, the TD inlets detect $NaNO_3$ (albeit with low efficiency). With the lower TD-Inlet temperatures used here, we do not expect to detect sodium of calcium nitrate. This is now mentioned in section 2.1:
We note that the reduction in temperature will also make the instrument even less sensitive to sodium nitrate than the ~ 15 % conversion efficiency reported by Friedrich et al. (2020).

Technical corrections:

Line 25: Technically the biggest source of reactive nitrogen to the atmosphere is NH3. It would be truer to refer to combustion NOx as the biggest source of "reactive oxides of nitrogen". This terminology should also be used when referring to NOz further down.
We have modified the text appropriately:
"Combustion-related, anthropogenic emissions (mainly as NO) are the primary source of reactive oxides of nitrogen to the atmosphere."

Line 35: The statement about sodium nitrate is overly simplistic. Technically the nitrate salt is formed with other marine cations such as potassium, calcium, magnesium, etc. Furthermore, calcium nitrate can also be formed from nitric acid reacting with calcium carbonate minerals in wind-blown dust.
We have added some more information in line with this comment:
"…whereas in coastal regions with anthropogenic influence, sodium, potassium magnesium nitrates (formed from reaction of $HNO_3$ with sea-salt) is often present. Calcium nitrate can also be formed e.g. in the interaction of mineral dust with $HNO_3$ and mineral dust."

Line 64: The AMS is capable of sub-second time resolution in 'fast' mode, which has been successfully used in eddy covariance and aircraft measurements, among other applications.
We have adopted this statement:
"The time resolution of the AMS is usually a few seconds, although it can be used in "fast mode" with sub-second time-resolution for some applications."

Line 282: Different models of aerodynamic lens are now available, so it should be specified that this is using the 'Standard' Aerodyne design. Recommend citing http://doi.org/10.1080/02786820701422278
We have modified the text:
"The HR-ToF-AMS instrument we used samples ambient air with ~0.1 L min-1 through a "Standard" Aerodyne aerodynamic lens (Liu et al., 2007) that focuses particles into the vacuum system…"

Line 323: I wouldn't refer to the shrinkage of ammonium nitrate as "thermal decomposition" because this implies an irreversible chemical transformation similar to the desorption tube. What's more likely to happen is that the ammonia and nitric acid will be in dynamic equilibrium with the gas phase, but because the chamber walls are microscopically 'flat', the condensed phase will favour this surface over the particles in the chamber due to the Kelvin effect.
The decomposition of ammonium nitrate to $HNO_3$ and $NH_3$ is a thermal process that is enhanced in rate at high temperatures and slows at low temperatures. In line with this comment we now write:
"…substantial shrinkage of the ammonium nitrate particles (resulting from thermal decomposition and loss of $HNO_3$ and $NH_3$ to the chamber walls)….."