Review of Gas isotope thermometry in the South Pole and Dome Fuji ice cores provides evidence for seasonal rectification of ice core gas records." by Jacob D. Morgan et al, The Cryosphere.

General:

The manuscript presents new very interesting high-precision nitrogen and argon data for disentangling thermal from gravitational effects and come up with an improved interpretation of the vertical temperature gradients and firn thickness changes obtained from this measurement partitioning. The latter being partly influenced by snow accumulation, which is dependent on the topography along the ice flow line upstream at flank sites like South Pole. The authors state that observed temperature gradients in the firn cannot be explained by annual-mean processes alone and they therefore propose that there is a seasonal bias term present, rectifier effect, which strength itself is again dependent the topography upstream.

Major points:

Line 110ff: This conversion from isotope ratios to the firn physical properties assumes that the isotope ratios occluded in bubbles at the base of the firn column are in diffusive equilibrium with the local environment and that the only fractionating processes occurring are gravity and thermal gradients. This is generally true for the firn column at an ice core site, although we discuss in Sect. 5.2.4 reasons why this might not be the case at South Pole, Dome Fuji, and potentially other ice core sites.

What are the implications when the equilibrium will not be established? As the authors state the rectifier, effect is something that violates this assumption. How valid are then the results obtained in a first step assuming equilibrium conditions and then in a second step using this results and stating that there must be a rectifier effect at work. Is it somewhat a circular argument that can lead to such a statement, i.e. wrong assumption (equilibrium state reached) leads to a wrong partitioning of temperature gradients and firn thickness changes, which may then lead to a wrong interpretation (→ rectifier). Please clarify that this is not the case.

Line 134ff: How and when is the hydrogen content measured?

Besides 28N2+ also 40Ar+ will react with H! This leads to a negative peak in 40Ar! Of course this happens also to 36Ar therefore the ratio 36/40Ar should remain rather stable in contrast to 29/28 where the mass/charge ratio decreases in contrast to 29 which loses 29N2 through the reaction with hydrogen but gains it from the same production using 28.

Have you looked into the stability of argon isotopes with varying hydrogen amounts in the sample?

Such reactions as mentioned are manifold in mass spectrometry. Have you looked into ArN2 formation and how it influences the isotopes of N2 and Ar?

4.1 Reproducibility: The authors did an excellent job in measuring the isotopic composition of nitrogen and argon with highest precision. Yet, there is still to be investigated, at least in my opinion, what kind of uncertainty is adequate to assign to a single depth measurement. In many publication so-called pooled standard deviation calculations have been used. Yet, this corresponds to a mean standard deviations based on replicated measurements on several depths. Whether this is an adequate measure is not clear to me. Maybe the authors can add some argument why they think it is justified to use eq. 9. To be on the safe side one could argue to take the largest standard deviation of replicates or weight it according to the quality of the ice as bubble ice behaves differently than ice from the brittle zone or clathrate zone.

Table 1: The fact that the pooled standard deviation of d15Nexcess for La Jolla air is lower than the d15N standard deviations shows that there is an instrument dependence present. Or is there an thermal diffusion fractionation expected during sampling of La Jolla air? I guess not since the inlet should be an aspirated intake (→ R. Keeling publications).

Why is this standard deviation of d15N and d15Nexcess for ice core measurements smaller than for La Jolla air? Is it due to the lower number of samples?

Line 186ff: What about the possibility of instrumental influence that affect both nitrogen and argon isotopes? Can you exclude this? It would be worthwhile to report the reproducibility of standard gas admissions of both isotope ratios and report whether or not a co-variation exists. Furthermore, it would be good to mimic ice core measurements with aliquots of standard gas on bubble free ice.

Line 1990f: I agree if the assumption of a co-variation is true and not to be assigned to the instrument!

Minor points:

Line 146f: Can you explain why you choose a density correction of 15 kg/m^3?

Line 150f: Give a reference to this statement about the surface density

Line 156ff: This shortcoming is not directly addressed in the paper or do I miss something. Therefore, either skip this statement or add a statement how this shortcomings are addressed in the paper.

Line 255f: rewrite? The mechanism is that katabatic winds accelerate on steeper slopes and decelerate on less steep slopes.

Line 260f: add reference. Is this based on an ice sheet model study?

Figure 3: It is not clear from Fig. 3 which process is driving the DCH change (increase) in between the grey zones (i.e. from 19 kyr to 12kyr). The temperature is increasing. This should lead to a higher accumulation rate but this is not seen. Only a strong accumulation change is obvious between 14 and 13 kyr without a corresponding signal in DCH, only in ΔTz! Why?

Line 307ff: A similar study could be made with the uncertainty in the 40Ar measurements. What kind of uncertainty increase is necessary to be in agreement with the REF model output?

Line 424f: not clear? there must be a forward and backward movement possible. Need more explanation.

Line 451f: What means rapid. Here we talk about the enclosure time of several hundred years! If it changes from year to year, there must be a consistent regime over a very long time range at work to maintain this rectifier effect.

Line 484: This section Broader impact … could be combined with the conclusion section!

Supplementary material

Figure S1: Left panel, do the same for 40Ar!! It should remain constant, but it needs to be shown!!

Line 65f: What kind of splining function was used?

Line 67ff: This is very arbitrary

Figure S2: This is a very vague approach also indicated by a rather questionable slope calculation. What is the error of dAr/N2?

Would it not be more straight-forward to look at correlations of the d15Ntherm to d40Artherm reaching a slope as expected from laboratory thermal diffusion experiments, and approach it such that a loss of Ar is assumed following the measured Ar/N2 measurements.

Line 125: shallowest? Correct here, it corresponds to the deepest firn depths!