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Interactive comment on "Precambrian faulting episodes and insights into the tectonothermal history of North Australia: Microstructural evidence and K–Ar, ^{40}Ar–^{39}Ar, and Rb–Sr dating of syntectonic illite from the intracratonic Millungera Basin" *by* I. Tonguç Uysal et al.

Roelant van der Lelij (Referee)

roelant.vanderlelij@ngu.no

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General comments

Uysal et al present a large dataset of geochronological, mineralogical and petrographic data from fault rocks and host rocks in the Millungera Basin in North-Western Queensland, Australia. The geological history of the basin is largely unknown and therefore the data represent a potentially good contribution on the geological history of the area.

The text is generally clear and well written with some occurrences of imprecise or unclear terminology, which I attempted to address case by case, see the line numbered comments below. Although the data appears to be generally of good quality, there are items missing which prevent detailed evaluation or further use of the data – IE a data table for 40Ar/39Ar geochronology (in the appendix is OK) stating irradiation time / J values and signal volts for the different isotopes; the used decay constants in all isotopic methods, the uncertainties in the 87Rb/86Sr ratios (necessary for calculating isochrons), and the uncertainties for 40Ar* and K2O determinations for K-Ar. Furthermore, in text, sometimes errors are discussed as 2σ, sometimes as 1σ for a single method (IE K-Ar, further discussion in the line numbered comments below). Three different dating methods (Rb/Sr isochrons, K-Ar and encapsulated 40Ar/39Ar) have been used to extract age information from numerous clay samples, which may or may not be mixtures of different generations of illite. In the present dataset, when various illite types are identified by XRD, their presence in different proportions appears to yield similar K-Ar ages, which suggests that the K-Ar isotope system was closed (IE by dropping temperature) or started (due to neo-crystallization) at the date recorded by the samples. Unfortunately illite polytype data are not provided for most of the samples, presumably because it requires a substantial sample amount compared to K-Ar analysis. Conceivably the Kübler and Arkai indexes can provide some insight but these data are not significantly discussed in text. It is not likely that 2M illite is formed in the "diagenetic" zone according to the Kübler index, for example.

Generally, it seems like the host rock has a similar illite age as the fault gouges. Then the interpretation of the illite ages from fault gouges, as ages of faulting needs an expanded discussion. Is it not possible that the faults were formed in a first stage, and that both fault and host rock experienced fluid flow and illite growth at a later stage (IE 1100-900 Ma)? Also, one of the existing hypotheses is that this basin is Paleozoic-Mesozoic based on regional considerations (Korch et al 2011). Thus, if the illite is authigenic, then the Mesoproterozoic age of the basin is a major conclusion, and its implications should be further explored.

In my opinion therefore, the dataset could vastly benefit from more detailed, well structured discussions on what is actually being dated, both from a mineralogical and macroscopic point of view. A section evaluating age vs. polytypes, when available, and age vs. crystallinity is currently lacking.

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Method specific comments 40Ar/39Ar with 39Ar encapsulation for recoil correction:

An implicit assumption of 39Ar recoil measurements is that the sensitivity of the measurement of 39Ar is the same as during the measurement of the rest of the sample, because we are measuring volts as an independent variable during the 39Ar release from the vial and combining them with voltage measurements during step heating, where normally only voltage ratios are used (which is typical for the 40Ar/39Ar method. However, in most mass spectrometers, ionization efficiency is a function of total gas pressure in the ion source. For example, Burnard and Farley (2000) show that 40Ar* sensitivity increases by 20% when increasing the argon pressure by a factor of $\sim$50 on a MAP mass spectrometer. Normally 40Ar/39Ar irradiation durations are planned to have a 40Ar*/39Ar ratio of 100-300. It follows that the gas pressures during 39Ar recoil measurements may be different by a factor of >100 from the gas pressures during step heating, and therefore it is not permissible to do recoil corrections simply by adding volts together, without tuning the sensitivity in the expected pressure ranges and correcting the volts for this calibration before calculating the total gas age. This problem may be exacerbated for old samples with lots of 40Ar*, particularly if irradiation times are chosen for a rather conservative J value. More generally, have illite ages this old ever been calibrated between K-Ar and 39Ar recoil corrected 40Ar/39Ar analyses? I am not sure, which leads to the more general question – are recoil-corrected 40Ar/39Ar ages really equivalent to K-Ar ages? Perhaps the attempt to base geological constraints on the 40Ar/39Ar ages is not sufficiently warranted.

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Method comments - Rb/Sr

In the data tables 1 and 3, it should be clearly marked which samples were used for which isochron age calculation. Furthermore, the error of the 87Rb/86Sr ratio should be specified as it is required to reproduce isochron calculations, and the referenced constants used to calculate the ages In line 513 you mention discrepancy between the Rb/Sr and K-Ar ages are possibly due to heterogeneous samples. Are you suggesting that the sample homogenization before splitting between Rb/Sr and K-Ar was not effective? Or do you mean something else by heterogeneity at the micro scale? Homogeneous sample material is a prerequisite for K-Ar geochronology. Clay size fractions have millions of particles in a typical argon aliquot size. If the samples were adequately homogenized during preparation, within size-fraction heterogeneity seems like an unlikely problem. Alternative suggestion - as is documented by XRD with the crystallinity, the polytypes (when available) and the K-Ar ages of these fractions, there may be different generations of illite. It is possible that the Rb/Sr "isochrons" are mixing lines between older and younger components. A common test for mixing is to plot the 87Sr/86Sr ratios vs. 1/(total Sr). Colinearity suggests mixing (e.g. Faure 1977, Zheng 1989, Chemical Geology). In the case of these samples, there is a clear correlation between 87/86 for most samples. Is it not plausible, then, that the "isochrons" you plot are in fact mixing lines instead? A discussion on this possibility is lacking from the current manuscript but is necessary in evaluating isochron data. If, as the authors suggest, the Rb/Sr system is more resistant to thermal resetting than the K-Ar system, is it possible that the K-Ar ages are variably reset near 1 Ga, whereas the Rb/Sr systematics display two component mixing? Together, it seems that the data suggest several illite generations, which suggests that the components in the samples are not co-genetic, but rather mixtures, and thus this very real possibility that the isochrons are instead mixing lines, should be discussed.

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Line numbered comments

Line 43 – “cores from borehole shaft or tunnel sites” – do you mean borehole shafts? Or boreholes, shafts or tunnel sites?

Line 49 – “reveal” is maybe better than “define”.

Line 60 – do you mean previous studies in north-central Australia as suggested by the flow of the text? Or worldwide?

Line 73 – is the potential reactivation history relevant to the discussion? This general statement may not be necessary. Also, Viola 2013 is missing from the reference list.

Line 131- relative errors of each of the 1M and 2M polytypes? Or the relative error on the %2M1/total illite? Or the absolute error?

Line 173 – 1 or 2 σ?

Line 181- this is repetition of line 179

Line 182 – The error of which air shots is 0.2??? The 16 40/36 measurements you report have a standard deviation of 1.23, and clearly these values do not overlap within error (excess scatter is common in argon geochronology). It is not clear what your quoted uncertainty represents.

Line 183 – you quote 2 σ uncertainties of 1% for argon measurements, yet your HDB1 analyses yield concentrations with a 1 σ standard deviation of 1.1% relative, for a material which is far more ideal than most samples. Are you sure you don’t mean 1 σ uncertainties? Is your standard deviation of spike calibrations (σ(X) in the error equation from Cox and Dalrymple) better than 0.5% RSD (which it has to be if the total uncertainty is better than 1% 2 σ)? Also, please use the same uncertainty level (1σ OR 2σ) throughout the paper. 1σ is generally fine to use for K-Ar.

Figure 3 is at present too low resolution for the reader to be able to follow your descriptions referring to this image throughout the text. Please provide higher resolution images.

Figure 4 – Muscovite should be abbreviated by Ms. It looks like the muscovite is oriented along the S shears, is it possible that the muscovite is authigenic with deformation? This whole section is very nice otherwise.

Line 266 – “an ultrafine grained oriented at a small angle” – it appears that a noun is missing here.

Line 281 – I don’t understand what you mean by “to compare their metamorphic grade with that of the fault gouges”.

Line 292 – SEM analyses on the bulk rock?

Line 296- on the right-hand side (as opposed to site). Also, this sentence is not very clear – are you implying that the detrital illite has straight edges in this study or not? In this sentence, do you mean to say, “unlike those presented in this study”?

Table 1, Please mention that the quoted uncertainties are 1σ (as you say ine line 312). Also, the illite percentages are relative to total illite, not the total percentage in the bulk rock. This should be specified in the caption.

Table 2 – please show the reference values you use for comparison. Generally, I think it is good practice to show the uncertainties on the individual measurements (K, 40Ar*). The uncertainties on the ages are not visible here (the table is cropped).

Line 311 – Obviously from the graphs in figure 6, F(recoil) is the fraction, not the percentage, which would mean 5-12% recoil loss.

Line 312 - “data” is a plural term, so “data are”, not “data is”

Line 329 and further on in this section – you document varying amounts of 2M vs 1M in sample JC360.7. Taking a weighted mean of different ages implies that you are dating the same material, which is obviously not the case for this sample. If you suggest that the 2M and 1M were formed simultaneously this is permissible, but then this should be more clearly discussed in the discussion section and such averaging should be moved to the discussion section, as it is an interpretation.

Line 333 – total gas age of fraction <2μm (specify the fraction). What does this discrepancy mean, it is present for half the 40Ar/39Ar analyses? And if you trust the K-Ar data from that sample, why do you compare the discordant 40Ar/39Ar age from one sample to K-Ar ages from other samples? Clauer et al 2012 present numerous comparisons of K-Ar vs encapsulated 40Ar/39Ar data and there often (if not to say usually) is a discrepancy.

Line 336 – total gas age.

Line 339 – Same as before. I realize it is impossible to do polytype analyses on some very small fractions, however XRD data from coarser samples suggest that ages get younger with decreasing 2M. Therefore, it is plausible, that the youngest ages (without polytype quantification) have more low-crystalline 1M illite which is younger.

Supplementary data – you have several XRD plots for the same samples which are not grouped together, please group them. Also, you have several analyses for the same fractions with different scale, but the graphs are different, please clarify what those graphs represent.

Line 369 – 371 – this is an interpretation, move it out of the results section.

Line 376- the lines in figure 8a are technically "reference isochrons" as they only approximate the data and are not formed by regression. It is a good idea to show this, but it should be labeled appropriately. The other graphs in this figure are appropriately labelled.

Line 377 – which samples?

Figure 8 – please provide higher resolution graphs –in vector format if possible.

Line 475 – This is indeed the case for micas formed during UHP metamorphism, in which case white micas recrystallize below the 40Ar* closure temperature and/or in-

corporate 40Ar* from the host rock; however I never heard of this being a problem in "normal" settings.

Line 508 – Although I largely agree with the discussion, I disagree that the K-Ar age of finer fractions of 922.2±21.2 is consistent with the Rb/Sr age of 1000±12. My own cursory reading of the (limited) data on the Millungera basin shows that the thermal gradient is extraordinarily high (60-70 degrees/km, Korsch 2011). Is it possible that high thermal gradients partially reset the illite K-Ar system at ~900-1000 Ma?

Line 509 – I disagree when half of the 40Ar/39Ar ages are very different from K-Ar ages from the same fractions.

Line 536 – Granites with a thickness of 5 km ? do you mean, granites 5 km below the surface?

Section 5.4 – Korsch et al 2011 tentatively suggest that this is a Mesozoic basin. This hypothesis appears to be clearly contradicted by the data in this paper.

Line 597 – no tectonic event preserved after 905 Ma – this conclusion is only true if i) you consider 1M and 2M illites to form simultaneously, or ii) if the youngest sample has no older illite.

I hope that the authors take my comments constructively.

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