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Response to referees

Paper DyAl_2 O. Hartmann et al.

General:

The attempt to observe a μSR signal in the ferromagnetic regime of a heavy (high moment) rare earth compound of the Laves phase series DyAl_2 was a challenging undertaking. It was quite involved in terms of manpower and beam time. Since the task was successful and gives information on the reason why the observation of o a signal in the ferromagnetic regime is difficult, it deserves publication especially in a conference proceeding. This holds in particular in so far as a recent study in that direction was successfull in low moment SmAl_2 but failed in the high moment case of GdAl_2. The present data are the best one could obtain under the difficult circumstances.

Referee 1:

The referee's judgement is 'publish as is' except for a printing error

The printing error has been corrected; no further action needed.

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Referee 2:

In the Introduction.

--References to old works are provided, however I would suggest Authors to give some more and (a bit) deeper insights in the physics of these compounds, for the aim of clarity and contextualization.

The introduction has been extended. The basic aim of the earlier work has been explained.

It has to be realized that this early work was carried out at a time when the insight into magnetic relaxation as seen by μSR was not as well understood as is the case today. The seminal early paper is extensive and cleared the situation. To give finer details of its result exceeds the scope of the present contribution

In Results

-- The current presentation of experimental data is absolutely meager. I highly recommend Authors to present more carefully muon spin depolarization curves in both the T regimes (paramagnetic and magnetically ordered) at selected T value. Not only data should be

presented, but also fitting curves. As they stand, data in the current figure 2 are a bit confusing.

A new Figure showing a larger number of spectra wit fit curves is provided.

-- Concerning figure 1, it is not really easy to distinguish which data are new and which ones are taken from literature. ZF data are, according to the caption, relative to the current crystalline sample (triangles). However, this would imply that Authors measured up to at least 1000 K, at variance with that declared in the "Experimental" section. May Authors clarify these descriptions?

We have indeed measured up to 1000 K. This is now included in section 'Experimental'. The figure has also been enlarged, the differentiation between present and previous data should be without problems.

-- Again concerning figure 1, Authors refer to "reduced temperature" in the caption (relatively to x axis). However this is wrong, since the reduced temperature should be defined as (T-Tc)/Tc.

The x-axis has been changed accordingly.

-- The paragraph 3.1 is a bit hurried. The only argument brought by Authors in favour of a single μ site is questionable since two different sites in the paramagnetic regime could easily lead to very similar (hence non-distinguishable) relaxation rates as well. Moreover: is there any possible suggestion or speculation about the origin of the exponent -0.75 governing the T dependence of the depolarization rate?

A short discussion of the exponent is given in section 'Discussion' with added references.

The remark about two different sites is curious. One can always extend a single site fit with a two sites fit if the parameters of the two sites are (nearly) identical. But if the multiple sites model is not supported by independent data this is pure fiteritis. In the present case the opposite situation prevails. The single site approch is not contradictory to any of the published treatments of μSR data of magnetic C15-Laves phase compounds. And those are numerous. In fact in the REAl_2 compounds the single site approach is fully confirmed by the data of CeAl_2 and SMAl_2 in their LRO regime and also by Knight shift measurements for DyAl_2.

The data of support for a single stopping site have been mentioned in the text.

-- The description of paragraph 3.2 is too hurried as well. Authors should explicitly make clear which kind of fitting function they used, and show fitting results in both the T regimes where the fit works or does not work. It is crucial to show why (and how!) such fitting does not work for 10 K < T < 40 K (maybe more frequencies are distinguishable? What about FFT of the signal?)

This paragraph is also somewhat extended.

As mentioned in the text, the fitting function is just an exponentially damped single cosine oscillation. A fit to the spectra between 40 K and 15 K failed predominantly because of extensive damping in the signal (rapid decay of asymmetry). In this situation FFT analysis is also unable to extract meaningful frequency values.

The 'washed out' spectra for 15 K < T < 40 K are explained in text as a possible overlay of different subspectra occuring on account of a 'sluggish' reorientation transition.

In Discussion

-- For the aim of clarity, Authors should present some sketch for the position of the μ in the crystalline structure. If space is a problem they should - at least – try to make the explanation of sites and internal field splittings more sounding and clear.

Done

-- The main argument brought by Authors about the change in anisotropy between [100] and [111] is rather weak. In the absence of any information for 10 K < T < 40 K, having the main claim of the draft based on one single experimental point at 10 K is not sufficient to give any reliable conclusion about the splitting of the internal field due to a change in anisotropy. Some partial confirmation should come at least in the T > 40 K region but, also in this case, the fitting curve presented by Authors is systematically missing almost all the experimental points.

We would like to stress that the occurence of a reorientation of magnetic axis around 40 K is not a result of the present study, but has been established safely in previous bulk measurements (which are quoted). We only use this established feature for a sensible explanation of the high field value (which is fully supported by the data) at 10 K.

The situation has been explained in more detail in the text.

We hope that the extensions of the text according to the remarks by the referee has improved our paper as desired. We thank the referees for their attention.c