The present study is concerned with the magnetic ground state properties of AReO$_4$ (A = Mg, Zn), a system in which Re^{6+} ions decorate a quasi-one-dimensional zig-zag network of distorted ReO$_6$ octahedra along the crystallographic c-axis that are bridged by distorted AO$_6$ octahedra along the other directions. The authors outline a high-pressure synthetic procedure for the preparation of polycrystalline samples, suggest possible magnetic ground states for both compounds and suggest a 6+ valence state of the Re ions. These conclusions are drawn from neutron powder diffraction data, muon+ spin spectroscopy, muon stopping site calculations, and bond valence sum calculations.

Overall, I think the musr data were well described and analysed, but beyond that, I do not think that the manuscript merits publication in SciPost Physics, and it would require significant changes for publication in SciPost Physics Core. Some of the reasons follow:

1- The study does not contribute any concrete conclusions or open new pathways for understanding the title compounds. Instead, it mostly presents analysed data with minimal insights. Additionally, as most of the study's conclusions (musr stopping sites calculations, bond valence calculations) depend on the crystallography done by the authors, more work is necessary to confirm the average structure of the compounds. In a previous study on MgReO$_4$ done by the same group (E. Nocerino et al. J. Phys.: Conf. Ser. 2462 012037), for example, through bulk property measurements, the authors have suggested that the material undergoes a structural transition at 280 K. Could symmetry lowering be the source of the impurity peaks observed in both compounds? And if that is the case, do any of the calculations hold? This could be easily confirmed through a few simulations.
2- The neutron absorption cross section of Re is relatively large, and the sample size (0.5 g) used for the experiment could explain not seeing any magnetic intensity. The subtracted diffraction patterns were also collected at least 80 K apart, which would hide intensities behind artefacts caused by the thermal contraction. The authors could perhaps provide upper estimates on the moment size and peak positions for the suggested magnetic structures.
3- The symmetry analysis for the magnetic structures begins with assuming propagation vectors similar to other 3d metal-based wolframite compounds. This assumption does not hold as the electronic structure of a $5d^1$ oxide could significantly deviate from that of 3d metals, and the study does not consider any symmetry lowering introducing other possible irreps.
4- It is generally assumed throughout the paper that both systems have a small ordered moment and that this stems from a non magnetic Jeff = 3/2 state with M = L-2S = 0 (L = 1, S = ½). This assumption, however, only holds for a cubic crystal field, and any slight deviations, even in the well ordered Re6+ perovskites (please see Re6+ double perovskites), introduces an appreciable moment (<0.2 mub) that can be seen using PND. This is especially not the case in AReO$_4$ where the octahedra are strongly distorted and manifold mixing is to be expected. The implications of this should be discussed in the manuscript. If the orbital moment is quenched due to this distortion, for example, could the small ordered moment be a result of a 1D Heisenberg chain AFM model?
5- The reasoning for the loss of one of the observed muon precession frequencies in MgReO4 should be expanded on as it is currently unclear.

6- Bond valence sum calculations require very high quality diffraction data and refinement that the study does not have. I'm also surprised that the R0 value obtained for Re6+ is smaller, rather than larger as is usual, than that of Re7+.

Minor points:

1- The current structure figures are unclear, and the zig-zag chains should be clear.
2- New figures are necessary for the diffraction data with zoomed in versions for the difference plots and an explanation for the observed quality of the fits
3- The crystal structure parameter table should be expanded to include the U parameters, occupancies, refinement goodness of fit parameters, etc, with the appropriate error bars that are not currently defined. The bond lengths should also be presented.
4- A list of the temperature dependent parameters for the phase parameter of the TF fits that are not at 0 as one would expect should be presented.
5- The AC susceptibility data in appendix A is inconclusive and is only shown from 40 – 250 K.
6- The impurity phase was presented as a paramagnetic impurity, but is then discussed as a ferromagnet in appendix section A.