Reviewer #1: The reliability of molecular dynamics (MD) simulations critically depends on the accuracy of the empirical interatomic potentials employed. Therefore, it is essential that these potentials be thoroughly validated against more accurate methods such as density functional theory (DFT) calculations. Unfortunately, this important aspect, raised in the first review of this manuscript, remains insufficiently addressed after revision. The authors assert that the angular dependent potential (ADP) used for the U-Mo system has been shown to reasonably reproduce several material properties for both pure U and U-Mo alloys. However, the supporting references they provide (Refs. 38-41) predominantly focus on applications of the ADP potential in MD simulations, rather than on rigorous validation against DFT or experimental benchmarks. Notably, Ref. 38 does not employ the ADP potential for U-Mo at all and is therefore not relevant in this context; it should be removed from the list of supporting references. Furthermore, in Figures 1A and 1B, the formation energies of both interstitials and vacancies exhibit a significant variation over the temperature range of 600 K to 1200 K. Such behavior may be an artifact of the empirical potential rather than a reflection of true physical trends. Without additional validation or discussion, it is difficult to assess the physical realism of these temperature-dependent results. To strengthen the manuscript, the reviewer recommend that the authors provide validation of the ADP potential through direct comparison with DFT data, either from the literature or new calculations.

Reviewer #3: [Since some of the equations are difficult to read, I have attached a PDF version for clarity. The content remains the same.]

In this paper, the authors investigate various properties related to point defects in U-Mo alloys under pressure using molecular dynamics simulations. U-Mo alloys are important fuel materials, and the analysis conducted in this paper is also essential. The employed analysis methodology appears generally reasonable, except for the issues listed below. In addition, the manuscript contains several inappropriate analyses and lacks sufficient discussion in critical areas, as outlined below. Therefore, I do not recommend this manuscript for publication in the Journal of Nuclear Materials in its current form and suggest major revisions.

1. Throughout the manuscript, various properties are evaluated without distinguishing between U and Mo atoms. However, properties such as formation energies and diffusion coefficients are different for U and Mo and should be evaluated separately. If such a distinction is not made, the authors should clearly explain why it is appropriate to treat U and Mo as indistinguishable in their analysis.

2. U-Mo alloys exhibit a miscibility gap, and phase separation is expected at high Mo concentrations. Nevertheless, the authors carry out all calculations assuming a completely homogeneous solid solution, even at high Mo concentrations. The significance for using this assumption should be clearly explained.

3. The formation energy is defined in Eqs. (1) and (2), but the reference states used in these definitions are unclear to me. For instance, if the formation energy of a U interstitial is defined as E\_f^(U\_int )=E^(U\_int )-E^bulk-E\_U, it is understood that a U atom has moved from a pure U reservoir into the U-Mo alloy as a interstitial atom. Alternatively, a reasonable definition would be E\_f^int=E^int-〖(1+N)/N E〗^bulk, where one atom in U-Mo alloy is taken from the bulk to form an interstitial. Here, the energies on the right-hand side are directly obtained from MD, not as defined by Eq. (2). The definitions given in Eqs. (1) and (2) appear more complicated and less transparent. A more detailed explanation is necessary to clarify these points.

4. If my understanding is correct, the formation volume is calculated as (∂E\_f)/∂p instead of (∂H\_f)/∂p in this paper. This approach is incorrect, as E\_f=H\_f only holds at zero pressure. Since finite pressure conditions are considered in this work, the formation volume should be determined typically via NPT-MD simulations of defective structures, which does not appear to have been performed in this study. If this understanding is incorrect, a clearer explanation of how the formation volume is computed should be provided.

5. The elastic dipole tensor is defined in Eq. (4) as the first-order coefficient in the strain expansion of defect energy. Under this definition, the elastic dipole tensor is independent of strain or pressure. However, in this study, the pressure dependence of the elastic dipole tensor is analyzed, probably suggesting a generalization of the above definition. The authors should explicitly define the version of the elastic dipole tensor used in this work and clarify how it differs from the conventional definition (Eqs. 4 and 5).

6. The authors claim that the elastic dipole tensor for interstitial defects is isotropic. However, in bcc structures, interstitials typically adopt dumbbell configurations, which result in anisotropic elastic dipole tensors. If the dipole tensors are averaged without accounting for specific interstitial configurations, isotropy may appear artificially. The authors should consider the actual geometry of interstitials in their calculations. It is possible that dumbbell orientations fluctuate rapidly at high temperature, leading to effective isotropy. In such a case, the authors should verify and demonstrate whether this dynamic averaging occurs in their simulations.

7. The temperature used in the pressure gradient simulations is not specified. This information should be included.

8. In the simulations involving pressure gradients, the analysis of defect travel distance and spatial spread is insufficient. Given that pressure-dependent diffusion coefficients, the evolution of defect density could be estimated by solving the diffusion equation　∂ρ/∂t=∂/∂x D(x) ∂ρ/∂x. 　Such an analysis should be included to support the discussion.

9. In the radiation damage simulations, the number of defects is reported to be the largest at 1200 K. However, it is generally expected that defect recombination becomes more efficient at higher temperatures, leading to a lower net defect population. The results presented here contradict this common understanding, yet no sufficient explanation is provided. A more thorough discussion is necessary to reconcile this discrepancy.

Reviewer #4: In the revised manuscript, the authors has addressed the majority of the reviewers' comments. While the general topic is of interest, there are still a few issues that the authors should address:  
1) If possible, please compare the calculation results of this article, especially the formation energy of defects, with the previously published DFT or experimental results.

2) Fig.1 is taken from a published paper Ref.[23] by the same authors, and the according discussion is similar. The author should expound on its necessity although the authors mentioned in the manuscript that This figure is updated from [23].

3) The defect formation energies initially decrease and subsequently increase with the increasing Mo content. However, the underlying physical mechanisms and contributing factors responsible for this trend were not discussed by the authors.

4) In cascade displacement simulations, excess kinetic energy is removed through temperature rescaling. However, this approach may influence the defect evolution process and the number of surviving defects, both of which are temperature-dependent.