Review of "Inertia & viscosity dictate drop impact forces" by Sanjay et al.

The authors describe a combined numerical and experimental study on the influence of Weber and Ohnesorge number on the peak forces (and their associated timing) associated with droplet impact and rebound on a non-wetting, rigid, planar surface. Scaling arguments are proposed throughout to help rationalize the trends observed in the data. This work represents a direct extension of work previously published by the same group (Zhang et al. PRL 2022) wherein they identified the possibility of two peaks in the impact force evolution associated with the initial impact and retraction stages. Overall, the manuscript is well written and easy to follow. However I do have some major and minor concerns that are described in what follows.

I have several major concerns:

1. Apart from Figure 2 (which appears to be the previously published data from Figure 1 in Zhang et al. (PRL 2022)), there is very little *direct* comparison between experiment and simulation. Now that the parameter space has been significantly extended from prior work, I would have expected similar direct force versus time comparisons and droplet shape profiles spanning the new range of parameters to validate the simulation. Comparisons similar to that in Figure 2 should be included throughout when the various parameter various regimes are introduced and discussed.

2. Error bars (both horizontal and vertical) for the experiments should be included throughout whenever experimental data is presented. If smaller than the data points, this should be noted. Furthermore, a discussion of any error analysis is completely lacking. For the error bars that are included, do these account for multiple trials? Parameter uncertainties? Measurement uncertainties? Such a discussion, with errors quantified (and methods used to assess the errors), must be included. In particular, the resolution of the force sensor (reported as 0.5 mN) should be indicated when force measurements are presented, including in Figure 2.

3. Furthermore, the *dimensional* parameters of the experiments performed need to somehow be specified or indicated. The authors mention a range of droplet sizes, viscosities, and impact speeds, and so We and Oh do not uniquely defined the experimental realizations reported. This specificity is critical for repeatability and future comparisons that might be made.

4. Is gravity included in the simulations? If so, the Bond number in simulations should be specified, as We and Oh alone do not uniquely define the non-dimensional problem. The authors show that droplet rebound can be affected by finite Bond number effects in their very recent prior work (Sanjay et al. JFM 2023), so at least some discussion on the role of gravity should be included.

5. The title needs to be rewritten. The authors demonstrate that capillarity also plays a crucial role, and seem to ignore gravity throughout the present work anyways (i.e. gravity doesn't dictate impact forces here, because it is assumed). Furthermore, the title does not specify the nature of the substrate, which is a specific case. I would suggest something along the lines of "Impact forces of droplets on non-wetting planar surfaces" or similar.

And some more minor concerns:

6. What are the receding and advancing contact angles for the experimental surfaces used? The experimental subsatrate is not truly non-wetting (in contrast to the simulation), so this should be clearly described.

7. In the experiments, are the droplets arriving at the surface without residual oscillation? Particular for the small We, small Oh cases, I would expect this is not the case. This should be discussed.

8. How are the density, viscosity, and surface tension of the various water-glycerol mixtures determined? These also need to be reported.

9. Some details on the numerical implementation are left out. In particular, the computational domain size and initial conditions should be specified.

10. The authors ignore the role of the air film in the dynamics on the grounds that the air-based Ohnesorge number is small, however the air gap can become extremely small and thus it is not clear that the drop diameter is the relevant length scale in assessing the viscous forces in the air. Furthermore, this is contrast to prior work on non-wetting impacts (such as Kolinski et al. (EPL 2014)) that argues viscous effects in the thin gas layer *can* play a role in the liquid dynamics. This should be discussed.

11. Figure 5 does not include any experimental data. This should be explained. Perhaps the high Oh regime is not accessible with the droplet sizes and fluids used?

12. Figure 5(a) partially confirms the $\sqrt{Oh}$ proposed, but suggests a notable We dependence on the pre-factor that is absent from the scaling argument. This should be reanalyzed and discussed.

13. For the large Oh impacts (section 3.2) the authors use the Rayleigh oscillation time as the timescale for impact. This theoretical timescale is derived for $Oh \ll 1$ droplet oscillations, and so is not clearly justified here. If t_{max} is indeed independent of Oh for the cases considered here, the authors should demonstrate this with their data. Furthermore, a physical interpretation of the overall trends predicted by the scaling arguments (3.16) and (3.17) should be provided.

14. Access to the code is greatly appreciated. However, I would suggest a permanent identifier be assigned to the version applied in the present study. This can be accomplished with Zenodo (integrated with Github), for instance.

15. In the introduction, the authors describe water as a “low-viscosity” liquid. In comparison to what? As the authors note, viscous effects in the problem are best described by Oh which includes other dimensional parameters in addition to viscosity. Similarly, in the end of section 4, the authors describe their previous work demonstrating a critical viscosity whereas it should be a critical Oh (or indeed a critical viscosity when all other parameters fixed).

Overall, the manuscript provides valuable new data, interesting and important conclusions, and successfully extends their prior work in the area to consider a more complete parameter space. It will be of value to the droplets community and thus deserves to be published once the above concerns are addressed in detail.