Dear Editor,

We thank the anonymous reviewer for their insightful assessment of our manuscript titled *Binaries drive high Type Ia supernova rates in dwarf galaxies*. We have taken their comments into consideration and updated the document accordingly. Changes to the text of the submitted manuscript are highlighted in red. We have copied the reviewer’s comments below alongside our responses with notes pointing to where we have updated the text where applicable.

We also apologize for our delay in responding to this report and thank the reviewer for their patience.

Sincerely,

James W. Johnson, on behalf of the authors

From the reviewer:

1. Some of the examples of the uncertainties that I think are a bit understated. To be clear, for all of these, all I really think is needed is 1-2 sentences, and maybe a summary paragraph in the conclusions mentioning them all, to convey to the reader how important the uncertainties are in these different aspects and how they play into the final conclusions:

- measured Ia rates: this is sort of acknowledged with the Baldry vs Bell MF -0.3 vs -0.5 argument, but that hides a bit. If you really consider a broader range of systematics, and different SMFs in the literature, you could get slopes as steep as -0.7, or as shallow as -0.1. For the argument here to work, you want a slope pretty close to -0.3. So quantify this: how close would it need to be to -0.3? I'm guessing from the plots, roughly 0.2-0.3 works, steeper than -0.3 becomes an issue. And clearly state in the conclusions this critically motivates better measurements of the completeness and underlying population MF (for LSB galaxies especially, as that's the key between Bell+Baldry and others), since this matters a lot as to whether this binarity trend is viable at all as the explanation of the Ia trend

- SFHs are also very uncertain in low-mass galaxies, and this isn't mentioned. Universemachine is not super-reliable at these masses as the observations it calibrates too are all over the place and in many cases (much of the mass range here, for example) it is actually just using extrapolated values from the "main sequence" SFR relation, not actual measurements of dwarf galaxies. And the extrapolations aren't great at 1e7 Msun. This is fairly important for how much you get from prompt Ias. If the low-M, LSB pops have very young stellar pops (e.g. Geha et al's argument), the need for metallicity dependence decreases quite a bit. If they are more quenched (as other observational studies have argued recently) and actually lie well \*below\* the "main sequence", then you need "more" metallicity dependence (akin to more like the M^(-0.5) models, which would be hard to fit with metallicity alone) - so there's a kind of "sweet spot" here. not an unreasonable one, but nonetheless it exists.

- the Maoz+ rates are reasonable, and the authors briefly mention some tests with different assumptions, but imply again it's more certain that it is. The slope varies a bit, but the cutoff at 100 Myr is really an upper limit (any cutoff <100 Myr works almost identically for their constraints). there is the one test with a cutoff at 40Myr mentioned, but no mention of how big a difference these assumptions actually make other than saying "not important". If the prompt rate were much higher/lower, presumably at some point it \*must\* move you out of the range (in either direction) where the binary scaling works, so when is this?

- the binary scaling of course is itself also uncertain. Maoz's paper is the state of the art but its an extremely hard measurement and debate still remains. Presumably theres a direct degeneracy between the slope of f\_close~Z^(-gamma) and your inferred "best" gamma that would fit the observations connecting to it. This is sort of summarized in your Fig 2 (right panel) but again it makes it look like there's not much uncertainty here.

- re: emphasizing in the conclusions that the steeper result from the Bell-like SMFs actually -doesn't- work

with the binary explanation

- GCE models, of course, have large uncertainties too -- here I think the authors do acknowledge this well in the text, just pointing to it as an example

- a big potential uncertain is non-linear back-reaction. This was discussed at length in the Gandhi paper the authors refer to a number of times, but merits mention here. Namely, changing the Ia rates changes feedback/masses/metallicities non-linearly which in turn changes the subsequent feedback and Ia rates in this picture, so its not as simple as "post processing" the SFHs as the authors do here. Again in the model here this isn't really possible, and that's ok for a rough conveying of the main idea, but the Gandhi paper argues that this non-linearity is quite a big effect, so it should be acknowledged here.

- the MZR is quite uncertain (more below)

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We have added clarifying statements to the beginning of section 2 and the conclusions to re-emphasize the uncertainties involved, with emphasis on more precise determinations of the SMF and how binarity varies with metallicity. We have also added a paragraph to the end of section 2 with a summary of the uncertainties involved, which acknowledges many of these points. In addressing the reviewer’s comments to the manuscript, this seemed like a more natural place for a summary of the uncertainties as opposed to the conclusions, so we simply reference this discussion there.

From the reviewer:

2. There's also some skipping over the details of the model which needs a bit more clarification. Most important, galaxies don't have a single metallicity, and the metallicity evolves with time. But your Eq. 1 makes it look like this is ignored, even though you show the z-dependent Zahid MZR, you only state its used to make predictions for the Ia rate observed at different redshifts, its not incorporated into your model. It's also not clear in your expressions if "Mstar" refers to "Mstar[tau]" (at a given time) or "Mstar[z=0]"

Even ignoring the "back reaction" or "non-linear" effects described above, the correct version of your Eq 1, within the assumptions of this model, I believe should be:

dot{N\_Ia}[z=0]/M\_star =  (\int[Mdot\_star[tau,Mstar[\tau]] \* R\_Ia[tau,\,Z[Mstar[tau]]] ]) / (\int[ dtau \*

Mdot\_star[tau, Mstar[tau]] ]) = (\int[Mdot\_star[tau,Mstar[\tau]] \* R\_Ia^0[tau] \* Z[tau,Mstar[tau]]^\gamma ) / (\int[ dtau \* Mdot\_star[tau, Mstar[tau]] ])

In other words, the "Z^gamma" should appear \*inside\* the integral, and should be Z of the stars formed at each "tau", which is the Z given by the adopted MZR as a bivariate function of both the mass \*at that time tau\* and the redshift corresponding to that time \*tau\*. Even if the MZR didn't evolve systematically with redshift (and your adopted one does quite strongly as you show!), because of the dependence on M[tau], the expression (1) written in the paper only works if the SFHs of galaxies are delta functions in time and Z.

The information needed to do this version of the calculation is provided by UniverseMachine + fits to the MZR. And perhaps the authors actually did this, but didn't state so clearly? If they used this, they should revise the text appropriately and say so. If not, they should try a version of this calculation and see what difference it makes. Naively I think it would actually be pretty significant, I think... If it's \*not\* a big difference, then I think that can only be true if the Ia rates today in the smaller galaxies are totally dominated by the prompt component (i.e. by stars formed recently, hence formed at very close to the present-day M\* and Z\*). If that's true, definitely worth stating that very clearly, and again it's another uncertainty of importance.

Another more subtle but important uncertainty to note about the modeling: something like UniverseMachine (UM) actually can't trivially be used to compare with observational relations calibrated using different stellar mass functions at z=0. The reason is that the whole infrastructure of UM is built explicitly by calibrating/fitting different measured SMFs and SFR data. The SFHs predicted by UM are by construction fit to give a specific SMF. If you say the z=0 SMF is different, and ran it properly through UM, the SFHs would also all be different. Now its noteworthy that the default UM model (the one adopted here) uses the Baldry SMF specifically at z=0. So this is consistent for that model. BUT if one were to consider the Bell SMF or something like it (i.e. a flatter SMF at low M\*) as correct, then a properly self-consistent UM re-calibration would necessarily give much lower low-redshift SFRs in dwarf galaxies (this is related to the uncertainties in the SFR vs mass of dwarfs from direct obs mentioned above, and allowed within those uncertainties). That exercise has been done for other science, by the way, in other UM papers. But that means here that there would be even less prompt Ia contribution for the dwarfs. So even "more" of the gap would need to be filled by metallicity dependence. So the "required" gamma would be even steeper than -1.5 (making a crude guess based on the slope of the SMF difference, I think you'd be forced to -2.5 or even a bit steeper). So it would be even-more outside the range the binary trend could explain.

Very minor model note: UM is typically described as a "semi-empirical" model, not "semi-analytic" (the latter usually refers to models which are predictively forward-modeling some physics equations for galaxy formation, UM strictly fits continuity equations to the observations statistically).

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There are indeed many uncertainties at play, and we wholeheartedly agree with the reviewer that equation (1) is simplified. In order to make these approximations clearer to the reader as they suggest, we have added a few sentences to section 2 following the appearance of equation (1).

It is true that one must marginalize over galactic enrichment histories to do these calculations most accurately. As Fig. 1 shows, low-mass field galaxies tend to assemble their mass at low redshift. This effect, combined with the steep nature of the DTD, indicates that the event rate in this regime should be dominated by young stellar populations anyway, so it’s possible that equation (1) is a fine approximation.

We did not use the UniverseMachine parameterization of metallicity as a function of stellar mass and redshift, which is adopted from Maiolino et al. (2008). This form has a similar shape as Andrews et al. (2013) and Zahid et al. (2014), so the differences in predictions should be minimal, since only shape matters due to our normalization of the rates to 1 at 1e10 Msun. However, even though it is the form that UniverseMachine uses, the Maiolino et al. (2008) form is not necessarily better for our purposes, because they focus on the MZR at z > 3, while we are primarily interested in understanding the observed rates at much lower redshifts. We have added a statement to this effect in section 2.

We have also corrected each instance of “semi-analytic” to “semi-empirical” to the best of our ability.

From the reviewer:

3. Regarding the MZR:

- the authors should acknowledge there are (large) uncertainties in the normalization, shape, and evolution. The comparison with Andrews+Martini 2013 sort of attempts to do this but it again hides the real uncertainties because it happens to be very close to Zahid (identical at <1e10 and 0.3 dex different at higher masses). But many have pointed out (e.g. Tremonti et al, Steidel et al., Strom et al., Kewley et al.) that the shapes can differ much more widely than this at low masses and the normalization differences for [O/H] can be as big as 0.7 dex in some regimes, depending on your estimator.

- It's also the case that Zahid+ only really measure the relation at > 1e9 Msun, where as you say, there's not much strong change in Ia rates. So the "most interesting" part of the relation is pure extrapolation here.

- more generally per the above, [O/H] gas-phase metallicities are super-uncertain, but they're also not really what matters. presumably, it's the stellar MZR that actually matters. why wasnt this used? its actually better measured over a wider mass range at least at z=0.

- related, some discussion of "which metallicity matters" is important. the MZR is quite different in [Fe/H] vs [alpha/H] for stars. And its easy to imagine either "mattering" for star formation theories, etc. But crucially, the Moe et al. relation being invoked by the authors is \*not\* based on [O/H] or [Z/H], but on [Fe/H] (because its measured in individual stars). Again, this means its much better to use the stellar MZR in [Fe/H], if you want to make an argument that they connect -- that's what will actually matter here. (e.g. use Kirby+ 2013 or Simon+2019, both of which can connect continuously to Gallazzi et al. and other stellar MZR estimates at >1e9 Msun). Note also Hwang et al. (admittedly for wide binaries) argued that there -wasn't- much correlation with [Z/H] and binarity, that it was indeed also physically driven by [Fe/H] (but again, even if [Fe/H] were not the driver, it should be compared since its the measured number for the binary statistics)

- that could make a significant difference (it's non-trivially shallower at these low masses: more like [Fe/H] ~ Mstar^-0.3, not -0.5).

- if redshift evolution of the stellar MZR is needed (the only reason I see why one would use the [O/H] MZR for much higher-mass galaxies), Leethochawalit et al. 2018/19, Zhuang 2021 can provide some examples

- related to above points re: modeling, emphasize ideally want [Fe/H] at formation of stars for scalings here

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The reviewer has raised insightful points about the nuances regarding metallicity and what is really meant by that term. We hope that the discussion in the paragraph we have added at the end of section 2 addresses their concern of transparency. As far as why we chose the gas-phase metallicity, we were simply interested in \*a\* mapping between mass and metallicity. Because this connection between stellar mass, binarity, and SN rates is quite new in the literature (mainly just this paper and Gandhi et al. 2022, to our knowledge), we elected not to contend with this question of the best way to parameterize metallicity and simply chose a form of the MZR that we had prior knowledge of through collaborators.

Although we are extrapolating to low masses with the Zahid et al. (2014) MZR, we have validated these calculations with the Andrews & Martini (2013) form at z = 0, which does have direct measurements down to 10^7.4 Msun. Related to point (2) from the reviewer above, this comparison is what validated our conclusion that only the shape of the MZR matters. Our statement about the Maiolino et al. (2008) MZR in section 2 immediately follows this point, so the additional discussion should clarify this a little more clearly to the readers.

From the reviewer:

4. Binary trend vs metallicity and flattening at lower masses/metallicities:

- see above re: [Fe/H]

- the authors very roughly refer to the Moe data being well-fit by Z^(-0.5), but actually moe's data is in their paper and shown here in Fig. 2 is fit by a log-linear expression: at [Fe/H]>-1, they fit f\_close = 0.2\*(1-[Fe/H]), which is closer to a f\_close ~ Z\_Fe^(-0.375). You see this in Fig 2, the 0.5 line actually non-trivially overshoots their lower-metallicity data. Worth a mention.

- more importantly, the data in and compiled by Moe et al. \*not\* shown in Fig 2, shows that this dependence \*flattens\* dramatically at lower metallicities. This is the opposite of the power-law-lines shown. Specifically, at [Fe/H]=-3 to -1, Moe+ fit a log-linear relation which connects to the one above, with the form:

f\_close = 0.335 - 0.065\*[Fe/H],

which is very close to a power-law of the form

f\_close ~ Z\_Fe^(-0.06),

which is much more shallow. For the stellar MZR in [Fe/H], you go below -1 at roughly Mstar ~ 2e8 Msun, and have a slope of ~0.25 or 0.3 below this. which predicts, below Mstar ~ 2e8 Msun, a relation:

f\_close ~ Mstar^(-0.02),

which is \*very\* weak/shallow.

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We had previously elected not to show Moe et al.’s (2019) measurement at [Fe/H] = -3 since it was simply off the plot in Fig. 2 and did not play a role in the story of the paper. However, this comment by the reviewer has made it clear to us that neglecting to mention it did indeed leave of a related and important piece of information, so we thank them once more for raising this concern.

We have marked the position of this measurement on the right panel of Fig. 2. We have also modified the second paragraph in section 3 to state that one would expect the metallicity dependence of the SN Ia rate to flatten off at this point if binarity is the driving factor, and that this impacts whether or not it can explain the effect across the full range of metallicities.