# Response to Reviewer 2

**Joughin et al. explored the sensitivity of projected 200-year mass loss from Pine Island and Thwaites Glaciers to the friction model, weakening of basal drag upstream the grounding line when ice approaches floatation, and the sub-shelf melt rates. They find relatively small differences between Weertman and Coulomb sliding laws but high sensitivity to the rate at which the basal drag is reduced when the ice approaches floatation. They also find the sea level contributions from both PIG and Thwaites glaciers are less sensitive to the spatial or temporal distribution of melt. The sea level contributions from these two glaciers is not likely to exceed 10 cm before end of 2200. Overall, the manuscript is well structured. However, the model setup section and some of the descriptions need improvements. There are quite a few typos in the texts and figures, especially the numbering of equations, which are misleading and need to be fixed. Information of some captions are insufficient and some statements in the discussion are a bit far-fetched.**

> *Summary of more specific comments that are addressed below.*

## Here are some general comments:

**Some of the methods description is not quite clear. I suggest more details in the way how you generate the basal melt distributions at four different levels.**

> *We provided a high-level summary of the melt procedure along with a reference to the original paper where the melt model is described in more detail. We did not see the need to repeat the detailed description here and refer the reviewer to the reference. With respect to the 4 levels of melt, the original text explicitly says "At each time step, each melt distribution is re-normalized to produce a specified level of melt <u>(e.g., 57 Gt/yr)</u>." In response to this comment, we added the "(e.g. 57 Gt/yr)" to make this point more clear.*

**Is it also not clear how you treat the basal drag and basal melt at the grounding line.**

> *Regarding basal stress, as the submitted manuscript states "Here we perform experiments using Equation (8) because it allows us to vary the amount of weakening so that we can study the resulting impact on ice loss." Equation 8 forces the basal shear stress to 0 at the GL (i.e., as the height above flotation goes to zero).*
>
> *To make the melt treatment clearer, we added the underlined text to the existing text. "For most of the experiments, we use a randomly generated ensemble of 30 melt distributions <u>applied to the floating nodes</u> (Joughin et al., 2021b).*

**One of the key findings here is the small difference between Weertman and Coulomb friction model. However, this statement has a very important premise that the**

**authors applied a linear weakening of the basal drag for both sliding laws. The statement will not hold without this premise, which should be mentioned in both abstract and the conclusion.**

> ***Coulomb friction and Weertman sliding are both friction laws that can be treated as a function of the effective pressure. If the effective pressure is well known then both should include the effective pressure dependence. Lacking such knowledge, a wide variety of treatments have been used by the modeling community ranging from completely dropping the dependence (Equations 1&2) to using an assumed value for the effective pressure (Equations 3, 4, and 5). And there is no reason that changes in effective pressure near the grounding line should not introduce similar weakening as for the Coulomb case (again when not simplified, both have a dependence on N – see Eq. 6 ).***
>
> ***A major focus of this paper is to try to systematically separate the effective pressure response (near grounding line weakening) from the friction type (Coulomb versus Weertman). So, we have pulled things apart in the way that we have to accomplish this objective. Readers can examine the results and draw their own conclusions.***
>
> ***The lead author has published more than half a dozen studies using this weakening approach. It works well for us. We do not, however, make the point this is the only way or best way to model the weakening. In fact, we conclude that both laws can be parameterized to produce similar results. "The fact that our empirically-derived value of $h_T$ agrees well with roughly equivalent values determined from consideration of effective pressure suggests that both types of models tend to reduce basal traction at rates that are approximately the right magnitude."***

**Figure 7 and 8 even show a clear difference under low melt cases between Weertman and RCFi, which was not well discussed in the paper. In Fig 8, the mass loss at the front of the Thwaites Glacier is clearly higher in Weertman than using RCFi.**

> ***We reconsidered and rewrote the explanation to say "Overall, our results indicate the choice of friction law yields relatively minor differences to the projected VAF losses (Figure 7), except for the PIG cases with low melt. These differences are consistent with the PIG re-grounding seen in the low melt simulations with Weertman sliding (Figure 9e&f). As noted above, there are limited areas ($h_0 < h_T$) where the bed can strengthen if thickening rather than thinning occurs. Such thickening rarely occurs because the region near the grounding line tends to nearly always thin. For some Weertman cases, however, thickening and advance do occur for sufficiently low melt, which should be reinforced by thickening-induced strengthening of the bed near the grounding line. This would explain why the losses decline as $h_T$ increases for the low melt Weertman cases on PIG, since the area subject to this type of strengthening***

*expands. Whether this should remain a feature of our model is a subject for future research."*

**This study found the ice mass losses are highly sensitive to the choice of $h_T$. However it's not clear about how to choose the best value of $h_T$ if we change to a different glacier? The best fit of $h_T$ = 41 m for the Pine Island Glacier may not be the best choice for other glaciers in Antarctic or even for the Whole Antarctica. $h_T$ may need to be adjusted based on the significant geometry change with time. This should be more discussed in the paper. Moreover, the study suggests a spatial variable Ht is feasible in future study. Then how about the changes of $h_T$ with time? In Joughin 2019, a 20 years simulation suggests that 46 m is the best choice. However, in a century scale simulation, the geometry near the updated GL may change a lot, which is not discussed yet.**

*The same can be said of all sorts of parameters used in ice sheet models. For example, an appropriate value of $\alpha^2$ must be selected and could also be glacier-dependent and time-varying. There are few if any model results that let the drag coefficient $\beta^2$. It's not an ideal situation, but making such assumptions is often the only way forward.*

*While we have only empirically evaluated our choice based on the time-dependent response of PIG that is a more quantitative evaluation of the parameter than often is the case. A major point of the study is to evaluate the sensitivity to the choice of this parameter. To make this point clear we added.*

*"Similarly, our best estimate for $h_T$ is based solely on the response of PIG over a decade and half. While it is likely that other glaciers can be modelled well with a value of $h_T$ of similar magnitude, further work is needed to establish the best value for other regions. Our results, however, do establish that choice of $h_T$ can have a substantial effect on projected losses as is the case for $\alpha^2$ (Barnes 2022)."*

**The last paragraph of Sect 4.2 is discussing the shortcomings of other sliding laws and conclude that 'any law that relies solely on the local height above flotation to govern changes in effective pressure, and thus, basal friction over the entire domain is likely oversimplified and incorrect'. However, the authors did not discuss the shortcomings of RCFi in this study, like how to better decide $h_T$ used in RCFi considering different glacier may have different sensitivity to the choice of $h_T$ as you show in Fig 7b and 7c for the high melt level case.**

*To be clear, this comment is directed only at Budd friction, which makes this assumption for the entire domain. This should be clear from the sentence, but we added. "(e.g., Equation 6)"*

*See comment above. In addition, we feel the strength of RCFi is that it applies Coulomb friction to areas of the bed where the ice motion is fast and where theory (see Schoof reference) and empirical work (see Zoet reference). Furthermore, RCFi has no parameter hT. That is introduced through weakening function $\lambda(h)$. We could retain RCFi and define a different weakening function with similar dependence as the other Coulomb friction laws as we clearly stated in the original submission. "There is no reason, however, that $\alpha^2$ in Equations (3) and (4) cannot be selected through a procedure like that used to derive our preferred value of $h_T$. On the other hand, Equation (8) can easily be modified to have a spatial variable $h_T$ that depends on effective pressure in a similar manner to Equations (3) and (4), which would allow the traction reduction to be decoupled from the form of the basal friction law."*

**Another statement in this paper is about the low sensitivity to the spatial or temporal distribution of melt rates. However, Fig 7and 9 did show sensitivity of GL retreat to the spatial distribution of melt as the author mentioned on Line 287-289. Recent studies indicate that the migration of the grounding line is extremely sensitive to how basal melt occurs adjacent to the grounding line (Arthern and Williams 2017; Reese et al., 2018; Goldberg et al., 2019). Modelling studies also suggest that ice sheet models are more sensitive to melt rates near the grounding line than to cavity-integrated melt rates beneath ice shelves, e.g. Gagliardini et al., 2010; Reese et al., 2018; Morlighem et al., 2021. Can you please justify it?**

*Simply put this is not something we find in our model.*

*Re Gagliardini et al., 2010 and another paper by Walker et al, 2008, we noted in the 2021 paper, "First, both earlier works used spatially dependent melt distributions rather than depth-dependent distributions. As a consequence, ice shelves in those models cannot respond by thinning to alter the melt intensity near the grounding line. Thus, high melt at a point near the grounding line can only be reduced by grounding-line retreat. By contrast, in our model and most other models, the simulated ice shelf can thin to redistribute melt without necessarily causing the grounding line to retreat. In addition, one of the earlier studies fixed the melt rate while allowing the total melt to increase as the shelf extent increased (Walker et al, 2008), so the total melt may have been an important factor for that model as in our results. The other of the two studies fixed the total melt by altering the shelf length (Gagliardini et al, 2010), which introduced an additional calving term that reduces the ice-shelf volume to a similar extent that more melt would have (Fig. 3). Last, both 1D models are fed by a fixed inflow of ice from a relatively short distance upstream of the grounding line. For PIG, however, a broad interior basin converges on the narrow main trunk, allowing inflow to the trunk to increase as the glacier speeds up. Unlike the case for models with fixed inflow, this extra inflow can moderate rates of grounding-line thinning and retreat (7)."*

*For the other references, there are several reasons for the differences. For example, Arthern et al used an order of magnitude coarser resolution than we did, which could easily explain the difference they get by applying melt to partially floating nodes. Goldberg et al get a variety of melt rates based on cavity geometry, which does not disagree with our result, which uses a fixed cavity-integrated melt rate. The Morlighem et al paper uses an automatic differentiation approach that linearizes the non-linear model. It's not clear to us that direct comparisons of that with the full evolving model is anything like apples to apples. The Reese et al model is diagnostic and does take into account the time-dependent response; the ensuing response to a change on the shelf can greatly modify the overall response.*

*Since we have already argued this point in our earlier work and provided a reference to that work, we have not made additional changes with respect to this point.*

**Lastly, I think it is very important to point out that the conclusion 'our work simulations suggest that melt-driven combined sea-level rise contribution from both glaciers is unlikely to exceed 10 cm by 2200' is under some assumptions, like the $h_T$=41m is the best fit for both glaciers in the coming two centuries.**

*For the largest fixed melt value, the 41 m value yields about 8 cm. Using hT=86 m, which is probably too large (we picked the larger value to examine sensitivity) the result not much over 10. So hT could still be substantially larger than 41 (almost double) and this statement would still be true. Nonetheless, it was a bit too strongly worded, especially in the abstract without the context of the discussion in the paper. We softened the statement with:*

*"Based on recent estimates of melt from other studies, our simulations suggest that melt-driven combined sea-level rise contribution from both glaciers may not exceed 10 cm by 2200, though the uncertainty in model parameters allows for larger increases. "*

Specific Comments:

**L66: what is the citation for typical value of  to be 0.5?**

*Changed to "(typically 0.5; e.g., Asay-Davis et al., 2016)"*

**L82: Why the  is much higher near GL (175 kPa) compared with Trunk (10 kPa) and inland tributary (100 kPa)? Same for title of Figure 3.**

*Because that's what the inversion for basal shear stress shows. We modified as follows (underlined text was added). "Figure 1 illustrates the sensitivity of these* *friction laws to speed for parameters meant to represent the near-grounding line region, central trunk, and outlying tributaries of PIG (see basal shear stress map in Figure 2a)."*

*We did modify the Figure 1a caption to indicate it is referring to the strong bed upstream of the grounding line.*

**L83: You mean haf =45 m here rather than hf right? If yes, I think you need to explain what haf is.**

*Fixed by making consistent. Dropped $h_{af}$ in figures and replaced with $h - h_f$ to make it consistent with the text.*

**I don't quite follow the legend on Figure 1 and this sentence. What's the meaning of different values of haf for Eq(3) and (4)?**

*For the parameters we picked, h-hf =45 m would be identical to the Weertman curve, which is overplotted multiple times already. So we didn't include it. To make this point clear, we added the following to the caption*

*"For the near-grounding-line case, the transition to Coulomb friction begins for $h - h_f < 39.2$ m using Equation (4), so the curve for this equation with $h - h_f = 45$ m is not shown to avoid an additional overplot of the Weertman curve."*

*We did add another curver (h-hf=40) to show the full Coulomb conditions. We removed the Equation (4) curves, since the didn't add much.*

**Why do you say the Weerman condition is not fond where hf = 45?**

*Inspection of the figure will show all of the Equation (3) and (4) results for the parameters we used over plot the Weertman result. The one exception is Equation (3) for near-grounding line case where h-hf=45 m. We reworded the sentence a bit to make this point more clear. It now says:*

*"). In these examples, Weertman conditions are found everywhere except for the case where Equation (3) is plotted using a height above flotation ($h - h_f$) of 45 m (transition to Coulomb) and 40 m (nearly full Coulomb; Figure 1a)."*

**L85: "transition to Coulomb sliding" how do you get the number of 67 m here?**

*Reworded to:*

*Thus, if we assume ~300 kPa as the maximum expected value for $\tau_b$ <u>with Coulomb friction</u>, then the transition to Weertman sliding takes place at locations where the elevation is less than 67 m above flotation for $\alpha^2$=0.5.*

*The value of 67 m comes from using the transition value $h - h_f = \frac{\tau_b}{\alpha^2 \rho_i g}$, given in the sentence above using 300 kPa (this relation follows easily from equating $\tau_b = \alpha^2 N = \alpha^2 \rho_i g(h - h_f)$ and solving for $h - h_f$.) This seemed too trivial a calculation to include and Reviewer 1 is pushing us to shorten not lengthen this section.*

**L87: please refer to the section you did the inversion for rather than just saying 'as described below'.**

*Replace "below" with "in the Methods Section"*

**L86: I still don't understand how you pick the four values 1, 41, 86, 176 m here. 1 m is easy but how about the rest three values?**

*We agree the numbers are a bit odd. The 176 number has been corrected to be 172. They are meant to be doublings of 41 (but as I recall the base value was originally slightly different). At some point, these values stuck, and it was not worth the re-running the simulations (weeks of computer time) with 41, 82, and 164. While somewhat inelegant, the slight deviations in no way affect the conclusions.*

**L119: The legend did not show Eq(7) at all. I guess the light blue line for RCF equation (6) should be RCF equation (7)?**

*Good catch. At some point, the text was updated, which changed the equation numbers, and the figure was not updated. The legend has been revised 6 to 7, and 8 to 9.*

**L121: I think it is worth mentioning that Gillet-Chaulet et al., 2016 used a power law rather that Coulomb law.**

*Modified to say:*

*Another study indicates PIG conditions are reproduced better with <u>a power law</u> using values of m in the range of 10–20, which produces a sensitivity of $\tau_b$ to speed that more closely resembles that of Equation (7) than that of Weertman sliding (Gillet-Chaulet et al., 2016).*

**L147: In the text, you refer to Eq 7 for pink line of Fig3, but the legend says the pink line is from Eq 9.**

*We changed the legend to "$\lambda(h)\tau_b$" since the same weakening is applied with RCF, RCFi, or Weertman.*

**L154: hT = 41 or 46 m**

*We specified a range because we are citing results from multiple papers with slightly different values.*

**L155: it should be 2021b rather than 2021a. hT = 123 m, m = 3**

*We added the units and exponents and put the range of values from the two earlier studies to be consistent with the RCF results.*

*"(Weertman sliding with m=3 produced best results with $h_T$ = 122–123 m). "*

**L178: Equation (7)**

*Fixed.*

**L186-189: It's not clear about the sequence of inversion here. Do you invert basal friction law parameters first and then A with a second inversion or invert both at the same time? Which sliding law do you use in the inversion?**

*The inversion iterations for the drag coefficient and A are interleaved. More details are in our earlier paper. We added a reference to Joughin et al, 2021b.*

*We amended as follows to make clear we inverted for the friction law used in the corresponding forward model (i.e., Weertman inversion for Weertman simulation).*

*We initialize the model by inverting for the basal friction law parameters ($\beta^2$ for Weertman or RCFi as appropriate)....*

**L193: It is not clear about how you generate the melt distribution until I further read through the whole text. I suggest you specify how you treat the melt distribution here. I suspect you run each experiment with 30 melt distributions and normalise it to four different melt levels (57, 75, 100, 125 Gt/yr), and then update this melt distribution with an updated grounding line position. What is the time step size?**

*There is more detail in the reference to the earlier work. But we did note there are 30 independent simulations each with its own melt rate.*

*For most of the experiments, we use a randomly generated ensemble of 30 melt distributions applied to the floating nodes (Joughin et al., 2021b), which are used to force 30 independent simulations. Unless otherwise noted, we present the results as the ensemble averages of these simulations.*

*Re time step. Changed to*

*"At each time step (0.01 years),..."*

**L269: I don't quite follow this. Do you mean poorer quality of the velocity used to invert the basal drag coefficient and A?**

*Yes, that's exactly what the sentence says:*

*"All the simulations have some thickening in the upper basin, which is likely due to the poorer quality of the velocity used to initialize the model there (i.e., speeds that are too slow)." The methods section makes it pretty clear this is what we mean by initialize the model.*

**L272-274: I think you need to specify the slight thickening and grounding line advance occur in PIG rather than both glaciers. I saw a few ensembles show more GL retreat in Weertman case (Fig 9f) compared with RCFi case (Fig 9b). Similar things also occur by comparing Fig 9h and Fig 9d. Why is it?**

*Changed from "…near the grounding line…" to "…near the PIG grounding line…"*

*Its not clear why: while we note the differences are small between sliding models and the melt explains much of the variance, there is still some variance due to melt/friction law.*

**L271-274: All of these are talking about PIG so it's better to specify it. I think the velocity contours in Fig8 is distracting to tell the VAF loss near the GL, which is important. When you say 'consistent with Fig7', it's hard to tell the thickening from Fig 7.**

*We added another "PIG" to make it clear that it's the PIG grounding line.*

*We feel the velocity contours are important for showing where the fast flow is, so we have left them in place.*

**L284-285: Then what is causing the lowest VAF loss from Weertman with $h_T$ = 172 at low melt level cases (57 Gt/yr) for PIG compared with other $h_T$?**

*It seems to be related to the fact that when thickening occurs, the bed gets stronger. We changed the text to say.*

*"As noted above, there are limited areas ($h_0 < h_T$) where the bed can strengthen if thickening rather than thinning occurs. Such thickening rarely occurs because the region near the grounding line tends to nearly always thin. For some Weertman cases, however, thickening and advance do occur for sufficiently low melt, which should be reinforced by thickening-induced strengthening of the bed near the grounding line. This would explain why the losses decline as $h_T$ increases for the low melt Weertman cases on PIG, since the area subject to this type of strengthening expands. Whether this should remain a feature of our model is a subject for future research."*

**L286: In Fig 8, we can clearly tell the difference between RCFi and Weertman for low melt cases (57 Gt/yr and 75 Gt/yr) at the front of Thwaites region (Fig 8a,b and Fig**

**8e,f). Similarly in Fig 7c, the dashed line from hT = 41 m and 86 m gave more mass loss than solid line for low melt case (57 Gt/yr).**

*This seems to be a comment rather than request for action.*

**L287: It will be good if you can show a map of the basal melt distribution for the Thwaites region. Just pick one of the melt realisations to prove what you said here.**

*We don't feel showing a single member of the ensemble will clarify much.*

**Does this sentence mean that the distribution of melt did affect the grounding line retreat in Thwaites, which conflicts with your statement that it is not sensitive to the spatial distribution of basal melt.**

*The r2 values give the sensitivity. In the case with the least sensitivity, absolute melt explains about 60% of the variance, which represents the majority.*

**L290-291: how do you get the 20% and 50%?**

*If you look at the figure, the RCF and Weertman results are generally within about 20% of each other. But if you look at the low melt PIG cases as is pointed out, clearly the differences can be greater than 50%.*

**L297-301: This comparison between this study and others in the same regions are important. I suggest a figure to compare the basal drag between their regularized Coulomb friction and RCFi in this study for the fast-flowing regions. Again, I don't understand how you decide they produce Coulomb friction for regions where $h-h_f$ < 86 m?**

*The 86 is not a precise number, we are just using that contour visual indicator of of the approximate extend where Coulomb behavior can occur. As noted in the basal friction section, the transition from Coulomb to transition should generally lie below about 67 m.*

**L304: Equation (6)?**

*Yes, this is a case of Weertman friction explicitly parameterized by effective pressure.*

**L310: basal drag of the area near the grounding line is weaker rather than 'area is weaker'.**

*The existing text seems fine. You can have a weak area or a strong area.*

**L317-318: From Fig 7, the diverge in ice loss for Thwaites is less compared with PIG at low melt values. It's not 'nearly the same' to me with a difference of 10 mm sle.**

*We clarified that we meant "PIG with RCFi". Those points are tightly clustered.*

**L334-335: Could you further explain how you translate the values of $H_t$ based on of Barnes and Gudmundsson (2022)?**

*"One way to obtain a rough equivalency is to determine the value of $h_T$ that yields equivalent area-integrated traction subject to reduction via the effective pressure dependency in Equation (4) for a given value of $\alpha^2$. "*

*Translating this into an algorithm note the original text said equation 3 instead of 4, which we fixed.*

1) *Given taub, determine area where Coulomb conditions and weakening will occur (ie., area where $h - h_f < \frac{\tau_b}{\alpha^2 \rho_i g}$).*
2) *Integrate taub over this area.*
3) *Now find $h_T$ such that when $\tau_b$ is integrated over area defined by $h - h_f < h_T$ the result equals the result from step 2.*

*As noted, it's not a perfect equivalent. But at least it provides a rough equivalency such that the total traction subject to weakening is the same.*

**L364: Equation (5) ? à Equation (6)**

*Fixed.*

**L373: you refer to Equation (7) here? so confusing.**

*Should have been Eq 6, fixed.*

**L408: it should be 0.96 or greater.**

*Fixed.*

**L796: basis boundaries à basin boundaries?**

*Figure 7.*

**L409: why the regression value for the ensemble data in Fig 10 (dashed lines) is not consistent with Figure 7 (solid lines)?**

*The dashed lines in Figure 10 should be the same lines as Figure 7 solid, though plotted over a different range. The r^2 values should be different. To make this clear, we added to the caption of Figure 10:*

*"The $r^2_{const.melt}$ values show the fraction of variance that the constant melt regression parameters explain for the depth-parameterized melt function simulations."*

*The point being that if you characterize the model as we did the constant melt simulations, the regression parameters do a pretty good job of predicting the results over a broad range of melt values.*

**L427: I think you refer to Figure 10 rather than Figure 9 here.**

*Fixed.*

**L455: citation please.**

*Added reference to Jourdain et al, 2022 ref.*

**L457: But it is also possible that PIG will have higher basal melt than 67+21 = 88 Gt/yr for the second century, which would exceed your 125 Gt/yr.**

*The math here seems unclear. The point though is that 125 Gt/yr is 2-century average. So if the melt in 2000 was 100Gt/yr and it increased by 25Gt/yr per century. Then it would be 150Gt/yr at the end, but the average would be 125 Gt/yr.*

**L464: You mean with $h_T$ =41 m? If this whole section 4.4 is talking about experiments with $h_T$ = 41, it's better to make it clear at the start of this section.**

*We added (ht=41) wherever there was an ambiguity. It was shorter than adding a sentence and we feel clearer.*

**L470: when did the melt reach 220 Gt/yr in Bett et al. (2023)'s model? The end of 2100 or 2200?**

*Neither, around 50 or 60 years. Based on personal communication with the authors, there are some issues in the preprint with the ice dynamics. But the ocean model should be fine. But for a given VAF loss, the cavity should be similar, and thus, the melting from the ocean model. So, the time is not important here (and likely will change for their final paper).*

**L476: the most aggressive parameterized melt rate function for Thwaites is B&G but is 160_700 when you talk about PIG. It's hard to tell it from Fig S2.**

*For comparison, the most aggressive parameterized melt rate function for Thwaites produces an average melt rate of 151 Gt $yr^{-1}$ ($h_T$=41 m; see B&G in Figure 10c).*

*There is also a slight deviation from the linearity. 160_700 produces a bit less melt but a bit more loss. So we added the parenthetical statement "(Note while 160_700 yields less melt, it produces a slightly large loss for Thwaites.)"*

**L795: Figure 8, for those who is not family with PIG and Thwaites, it's hard to tell the corresponding values of the velocity contours.**

*At the resolution of the plot, they are not intended to be completely discernible. Rather they are to show where fast flow is concentrated. For example, the thinning on Thwaites is much stronger to the east of the fast-flowing trunk, contrary to what one might expect.*

**L797: I guess you refer to Figure 9 here.**

*Yes.*

**L800: Figure 9, is the red line showing the location of the grounding line? What are the scattered points in Fig 9c and 9d?**

*It looks like "lakes" formed for a small number of ensemble members.*

**Figure S2: mr_4 in the legend but mr_2 in the caption?**

*Fixed.*

## References From Reviewer


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