General: Following the reviewers' comments, we deferred addressing the revision until Paper I was submitted, both to arXiv (https://arxiv.org/pdf/2507.14433) and ApJ. During this process, we identified a minor bug in the polarization modelling, which led to slight adjustments in the best-fit solution, though without significant impact on our overall interpretation. We have updated Table 1 accordingly. Additionally, we removed the m=1.5 solution, as its results were nearly identical to the m=2 case. This simplification improves the paper's clarity without compromising the scientific conclusions.

- my main comment is the following: this paper relies heavily on paper I (e.g., L40, ...), it uses the model developed in paper I and applies it to a (from what I understand) additional dataset to that used in paper I. This manuscript also does not provide any extra conclusions or findings compared to paper I (at least those are not made clear, but also I did not see paper 1). So I am wondering what the argument is for this to be an extra paper, and not one chapter in paper 1.

Several sentences in the paper underline my statement, for example:

+ L40 and following: "... (hereafter Paper I) based on a more comprehensive dataset and improved modeling". Also the rest of the paragraph, and the next one, leave the question how this work adds to new findings about this systems that are not (or cannot be) incorporated into paper I, especially given that paper I is apparently in prep (and not submitted).

+ L86 and following: this paragraph describes the model in paper I, but several aspects remain unclear. For example, how were the best-fit parameters found, and which of the solutions given in Table 1 actually correspond to those, there are two solutions in Table 1.

+ L162 and following: the authors state that "similarly to Paper I, where the model successfully reproduced some observational features but failed to match others, the same pattern is evident in Figure 2.". So what are the new findings of this work that were not already shown (or could not easily and logically incorporated) in paper I? Given the lack of a clear conclusion of this paper, I would argue that it can be made part of paper I.

Additionally I cannot comment on several aspects of the paper (i.e., the model) as the main information is provided in a (so far unpublished and not accessible) work that I do not have access to.

So as a summary, unless there is a clear reason I am missing, I suggest to incorporate this manuscript into paper 1.

We appreciate the reviewer's comments and understand their concerns. While the current dataset was originally part of Paper I, as suggested, we opted to separate it for the following three main reasons: 1) Due to a mismatch between the model and the observations, no combination of parameters can fully reproduce the observed behavior. This highlights an important limitation of the current modeling approach and suggests the need for further refinement. However, the model used in Paper I is already quite sophisticated and provides valuable insights into the system. Within this context, the BV photometry appears to be

the main challenge for the model and thus should serve as the main key observable for guiding future improvements. 2) Paper I is already quite lengthy (approximately 30 pages), and many journals have been adopting stricter page limits, for instance, Astronomy and Astrophysics is currently limited to 12 pages. Separating the content helps meet these editorial guidelines while preserving the clarity and focus of each study. 3) Most importantly, we wanted to properly acknowledge and highlight the work of the non-professional astronomers involved. These collaborators invested significant time and effort to produce the observations, and the current paper is part of a broader initiative to strengthen and formalize collaborations between professional and non-professional astronomers in Brazil. While the result discussed here is scientifically significant (point 1), it was somewhat overshadowed in Paper I by the other findings. Giving it a dedicated manuscript allows us to better value their contribution. Given these points, we felt that separating this section was justified, especially considering that this paper is part of a special issue being organized by participants of the Hot Stars 2024 meeting in Kazakhstan. The expected page limit for contributions to this special issue is only 7 pages, which aligns well with the scope of this study. In response to the reviewer's feedback, we have taken the following steps: i) Paper I has now been submitted to ApJ and posted to arXiv, allowing the reviewer to compare both manuscripts directly. ii) We have revised the phrasing in the abstract and discussion to more clearly highlight the limitations discussed in point (1), ensuring that the scientific motivation for this study is fully evident.

- L125: do the TLUSTY models really cover the parameter space in which subdwarfs are found (both in terms of Teff and logg?). I did not think this was the case.

Well, it does, depending on the radius of the subdwarf. The O+B grids published by Ivan and Thierry (the TLUSTY developers) cover logg from 2.25 to 5 (for 20 kK) and Teff from 10 kK to 50 kK. For instance, V658 Car has a logg of approximately 4.3 (many other Be + sdOB are also below the limit of 5). The main issue with this grid is actually that they lack the helium enhancement expected in subdwarfs formed through binary interactions.

- Figure 2 and corresponding text: it remains unclear why the lightcurve was cut in the way it was cut. Why are there two different parts after the primary eclipse, but not before the eclipse? For an untrained eye, this blue part looks very similar. Also, in the secondary eclipse, why was the red fraction selected to be so wide and why does it not only include the eclipse itself?

We understand that the choice of colors and cuts may been confusing. We initially planned to discuss differences between the symmetric and non-symmetric components of the TESS light curve. However, due to insufficient photometric accuracy to reliably distinguish between them in the BV photometry, we have opted to uniformly color all attenuation parts in green, the flat portion in cyan and the eclipses in red. With this adjustment, it becomes even more evident that our coverage of the second eclipse is insufficient.

- Figure 2: the captions mentions that "the m=2 and m=1.5 models are represented by black and colored lines". What are those lines? why do they evolve and what exactly do they cover? the orbital evolution? this is probably stated in paper I, but without paper I it remains unclear.

These curves represent the model-predicted behavior throughout an orbital cycle. Now, that we plot only one model this may be more clear. For instance, the red solid line in the top-left panel shows the model-predicted CMD trajectory during the first eclipse.

Minor comments:

- L2: out of curiosity: can you also estimate disk masses from your model?

Yes, the disk mass can in principle be calculated by integrating the power-law density distribution. For fixed values of m and beta, the total mass becomes proportional to the base density rho0. However, this estimate is highly sensitive to the chosen m and beta parameters, even small variations in these exponents lead to significantly different mass estimates, making the result unreliable for quantitative analysis.

- L66: can you please clarify the sentence about the cadence - I do not understand the meaning of it.

We agree that the sentence was somewhat awkwardly phrased. We have revised it, and we believe it is now clearer and easier to understand.

- L124: given the rest of the reference formatting, I think "Coelho" should be removed here and only the reference number should remain.

Thanks for spotting that. It was a typo, and we have now corrected it.