We thank the editor and the reviewers for their kind feedback. We have made a number of minor and some major edits inside the manuscript in response to these, as well as took the time to reread and further polish the text and presentation style. The most major edits have been highlighted in the revised manuscript in yellow for easy identification.

Hereafter, we include our point-by-point responses to the reviewers' comments.

Reviewer A

We thank the reviewer for the very helpful and detailed feedback. We respond to the various points raised as follows:

1)
"Together with references 19 and 20, the authors should add and comment the rate-independent linear damping model defined in Meccanica 50 (3), pp. 617-632."

Thank you for pointing out this relevant recent work. It has been included in the relevant discussion of the literature.

2)
"The authors should show the substantial rate-independent damping of the proposed model for different g values (see Fig. 9, g=1,5,10)."

Loss factor values of g=1, 5 or 10 (corresponding to loss of 100%, 500% and 1000% of elastic energy per cycle respectively) would be quite extreme and we are not aware of real-life analogues, except some highly viscous rubbers, where >100% of the elastic energy is lost per cycle. Fig. 9 already shows results for two values g=0.01 and g=0.1 (1% and 10% respectively), spanning a range of one order of magnitude. We believe it already demonstrates what the reviewer is requesting, albeit with an order of magnitude closer to actual structures. Fig. 13 further illustrates the dependence of the response and the manifest loss factor on frequency.
If we misinterpreted the reviewer's comment, please advise.

3)
"Sect. 4. The proposed recursive solution should be numerically instable. Some comments should be made."

In the relatively brief simulation runs that were performed in the context of this work no numerical instabilities were observed –although it is conceivable that such will start to manifest, if the simulation will continue for sufficiently long time. Because the instantaneous pseudo-frequency correction factor is calculated based on the immediately prior states, the solution is always bound, in a sense, by said prior states. In addition, in a time-domain simulation the damping also tends to delay the onset of numerical-error-induced instabilities.
These comments have been added to section 4, as per the reviewer's recommendation.

4)
"Sect. 5. The propose model of the rate-independent damping is globally nonlinear. The following period should be explained and expanded “This line of thinking will prove to be essential for the generalization of this approach for multi degrees of freedom, where the benefits of linear modal superposition will be convenient. This is going to be the topic of a soon future paper, which cannot be swiftly presented here.” "

We readily recognise the global non-linearity of the model at the start of section 5. Upon reviewing this period, we felt that it was poor style to allude to our next paper in this way. This statement did not add anything essential to the present paper and therefore we opted to remove it entirely. As originally indicated, we shall be visiting the matter in more depth in a next publication.
At the same time, we modified the wording to more explicitly clarify that the differential equation of motion is locally linear (in other words, it is linearised at each time step), as the value of ω_{ins} remains the same for each time step.

5)
"Page 13. The following period should be explained “A better still solution is to use the neighbourhood of the fixed points to initialize the recurrence sequence until convergence is achieved. A more rigorous mathematical treatment for this issue will be a topic for a future paper, as we are here demonstrating the validity of the concept.” "

As in point 4, upon reviewing this period, we felt that it was poor style to allude to our next paper. At the same time, the elaboration of this statement will have to be quite involved and a simple expansion will generate more questions than it will answer. Lastly -and importantly-, from our own simulations during the making of this work, we found that the initialisation of the recurrence sequence is not a sensitive issue and it has been exceptionally easy to achieve stability without too much attention to the initialisation. For all these reasons, we elected to remove this sentence altogether. As originally indicated, we shall be visiting the matter in more depth in a next publication.

6)
"Sect. 6. The following period should be explained and expanded “Thus a final judgement would be left to the precise experiments to decide on which one is the most accurate. However, the proposed approach has the privilege of simplicity and generalizability over the other solutions for any kind of loading scenario, not only harmonic as will be shown later on.” "

The corresponding part of section 6 has been reformulated as follows:
"...the proposed method produces results that are pretty similar to the other methods, such as Collar [7], Neumark [6], Chen [37] and Ribeiro [38], although small differences in their predictions exist. At this time, however, there are no available sufficiently precise experimental data sets from ideal SDOF systems that could allow a final judgement as to which prediction is the most accurate. Moreover, while in reasonably good agreement with the rest, the proposed approach has the privilege of simplicity and generalisability..."

The point we wanted to make is that the minor differences in the predictions between all these phenomenological models, including our own, cannot be assessed in terms of their realism/ accuracy in the absense of sufficiently detailed experimental data and thus the comparison is mainly on simplicity and generalisability.
We believe the reformulated text, as per the reviewer's suggestion, better conveys this meaning.

7)
"Check eqs. (16), (17)"

A clear unambiguous notation was adopted, using v(t), x(t) etc as opposed to x|t. Also, the abiguous word "response" was replaced by "displacement", literally corresponding to x(t). Lastly, a consistent notation style d/dt was used for the derivatives, replacing the casual mix of this form with dots/ primes, leading to a clearer style.

8)
"Eq. (26) is not present in the paper."

This was a typographic error. The reference should have been to Eq. (25). This has been corrected.
Equations after Eq. (25) have been renumbered.

Reviewer B

We thank the reviewer for the candid and detailed feedback, thanks to which we are now able to clarify and solidify our narrative.

1)
"I do not think that this paper gives any valuable contribution to the understanding of a new hysteretical damping model. This would be the main achievement and as that it would be enough. But that is not the case (in my opinion) and therefore its practical use is quite
doubtful."

Firstly, let us clear the confusion about what the model we propose is and what it does:
It is essentially a formulation of Collar's model mx''+(kg/ω_ins)x'+kx=F, where we have actually proposed a real-time adjustment of the instantaneous value ω_ins, using the available state information x(t) (namely: we calculate ω_ins from immediately prior and current state data).
Unlike our work, everyone before us would consider ω (in place of our ω_ins =instantaneous) as a constant parameter that must be known a priori and would try to infer its value from the forcing excitation, or from a system eigenfrequency, etc We, on the other hand, do not requre prior knowledge of anything, which is what makes our model far more versatile and generally valid. From a causality/ mathematical point of view, our model is just as plausible as those by Kussner, Kassner, Collar, Bishop, Neumark et al.

Just like in the works by our predecessors, our model does give some obvious predictions in the cases of a forced vibration, i.e. $\omega_{ins}=\omega_0$, and in a free vibration ω_{ins} converges to the system natural frequency. This simply proves that our predictor function for ω_{ins} works as intended. We have also demonstrated the predictions of our model for a step load as well as various other scenarios, where we have discussed how it compares to all other models and the results are plausible and non-trivial.

We believe reviewer A made a very insightful succinct summary of our work, which in our opinion identifies all the above points splendidly:
"In the paper authors propose a simple modification to the viscous damping model such that the resulting behaviour is hysteretic (rate-independent damping). They propose to achieve this by applying a correction to the viscous model. The correction factor, which is called an instantaneous pseudo frequency, is computed based on the system local state variables at
each time-step. The authors found that the new model predicts consistently the weak variation in the loss factor as a function of frequency. In addition to its simple mathematical formulation, it does not require knowledge of the past history of motion neither the knowledge of the excitation frequency and is extensible to any type of loading."

We have carefully reviewed our narrative in section 3 and have made some changes to let there be no confusion about the above. We believe that the hypotheses and novelty/ distinguishing characteristic of our model is now even more clear and explicit.
Specifically addressing the reviewer's comment, we have added a dedicated paragraph at the beginning of section 3 to clearly lay out the main concept, beyond doubt.

Having explained the above, we address hereafter the reviewer's other specific comments:

2)
"Even when discussing previous works from other authors, there is a mix of free vibration versus forced vibration that makes things really messy. For instance, in section 2.3, page 7, the authors state that "… the solution for free vibration of hysteretic damping is…" (eq. 12), but they keep "omega". What is "omega" in eq. (12)? Certainly not the same of equations 8 and 9…, where it is the forced frequency."

We agree with the reviewer that there is quite a bit of debate and in some cases confusion in the literature and indeed some of this "mess" has to do with intended or unintended confusions in several of the cited works between free and forced vibration responses. We have carefully reread sections 1-2 and made a few minor changes to try to improve clarity, but we think that our narrative is already quite clear. If anything, we have brought to light a controversy and sense of "mess" that is well-established in the literature. However, we see no instance where our own presentation of this state of the art is unclear or "messy".
Wrt Eq. (12), it is clearly stated in the immediately preceding paragraph that ω_{Ch} is an overall "effective" natural frequency and this is perfectly consistent with free vibration. In fact, we have carefully reread the several sources that we cite

and could not find any error or misrepresentation. As for the selection of the symbols, we tried to be consistent with our sources, in this case Chen et al (1994).
We also do not think that splitting the discussion into separate sections on forced and free vibration respectively would help with the clarity of the presentation or the flow of the narrative. We think it is much more informative to examine each model or compare two models in terms of all the possible solutions (forced, free, etc) in the same place in the narrative, rather than having to re-discuss them in different sections simply because of a change in boundary conditions. In fact, our own approach is founded on the premise that a model should be valid regardless of the specifics of the excitation and boundary conditions, so fragmenting the discussion of models would be counter-productive.

3)
"In section 3 the authors conclude that ”gama” coincides with g. But this is obvious, since omega*c = g*k!"
Presumably by "gamma" the reviewer is referring to the symbol "Y", which was introduced in Eq. (18) in the form of an a-priori-unknown analogy coefficient, which was after some deductions shown to be identical to the loss factor g in Eq. (20). Although there is nothing wrong with this approach and in mathematical terms it is appropriately rigorous (when we originally developed the model -admittedly independently of Eq. 5 at that time- we followed this exact reasoning to prove to ourselves that Y is indeed identical to g), we can see how, having the hindsight of Eq. 5, this seems very redundant at this point in the paper. We agree and have eliminated the use of the symbol Y, directly replacing it with g in Eq. 18. We still retain Eqs. 19-20 and argue why g in the context of our model still exhibits the characteristics of the loss factor.

4)
"The authors talk about an instantaneous frequency omega_ins, which in page 10 they say that converges to the driving frequency of motion omega_0. Then they obtain eq. (21), representing the free vibration equilibrium equation, where omega_ins appears! This does not make sense, does it?"

Wrt the specific passage, we clearly state that "In case of forced vibration with purely harmonic excitation... By virtue of its definition ω_ins converges to the driving frequency of motion ω_0". Then we proceed with considering a different case: "In case of free vibration, we will have a look at what happens" and then under this premise obtain Eq. (21), where omega_ins appears. We are not sure why it would not make sense. We are not able to find any inconsistency in any of this.

5)
"Then the authors follow a rather intrincate iterative procedure that I do not understand, because it seems to me that there is a big confusion between the forced and free responses."

We may have been too mathematical, concerning ourselves in section 4 about the theoretical convergence of successive estimations of ω_ins. We cannot find logical fault with our calculations and reasoning on convergence in section 4. One thing that we can clearly state is that there is absolutely no confusion in our treatment of subcases of free and forced vibration and we think it it is highly warranted to use these to study various behaviours (also in terms of convergence) of our system.

Even though we recognise that the mathematical treatment is somewhat involved and may be difficult to follow (even though this is not our fault but simply the nature of the problem), we should point out in any case that we have seen and demonstrated this convergence in all the time-domain simulations that we conduct in section 6, where we carefully compare the predictions of all models, including our own and show what we think is meaningful, interesting and non-trivial results.
So, from either the mathematical or the practical standpoint, we have clearly demonstrated the convergence and stability of our model.

6)
Overall, we are grateful to the reviewer for inciting us to take another critical look at our paper. We could not find much fault with what we have, but we did polish and make explicit some of our wording and argumentation, to avoid any similar misunderstanding in the future.

In our narrative, we have been very careful to attribute properly to all our predecessors their due credit for the concepts that we use. Much of the discussion in the paper is dedicated to comparisons with other preceding models. It appears an unintended side-effect has been that it's been confusing and easy to miss our contribution.
Now that we have clarified the specific contribution of the presented model, we think it remains important to discuss and compare it with its predecessor models. For this reason, we chose to keep all the relevant discussions, as we feel that none of them are redundant, even though discussing 3-4 similar models under various excitations (forced/ free vibration etc) does entail some risk of confusion. We have even discussed and warned about this confusion in sections 1-2.