Reply to the Referee's Report and Summary of Changes

Dear Editor and Referee:

In light of the Referee's evaluations, we are resubmitting the manuscript "TheHaloMod: An online calculator for the halo model" (ASCOM-D-20-00082) to Astronomy and Computing.

We attach this response addressing the concerns raised by the Referee, with relevant changes to the manuscript. For convenience, we have also returned a manuscript in which all new modifications are marked with red/blue highlighting (via the `latexdiff` tool). Please see the summary of changes and the detailed response to the report as follows. We hope that the amended version of our manuscript meets with the standard of the Referee as well as the criteria of the journal for publication.

Sincerely,

The authors

To the Referee:

We want to first express our sincere gratitude towards the Referee for acknowledging our work and offering comments and suggestions that greatly improve our paper and associated package. The comments were extremely thoughtful and detailed, and we are very thankful for the reviewer's efforts.

1. SUGGESTIONS

a. At the moment, I can use halomod to produce 'tracer-tracer', 'tracer-matter' and 'matter-matter' power via 'power_auto_tracer', 'power_cross_tracer_matter' and 'power_auto_matter'. However, I can imagine that one might want to compute 'tracer1-tracer2' type cross power spectra (e.g., galaxies-HI intensity, or the cross spectrum between two different galaxy samples) and I don't think that this is currently possible. Allowing the user to define multiple tracers to cross correlate would be a very useful feature.

halomod indeed supports cross-correlation via the `CrossCorrelations` framework. While this is documented in the online documentation, we did only mention it in passing in the paper. We have now added a brief section devoted to this framework (section 4.2.4). We have also added a standalone tutorial to the online documentation on how to use this feature.

b. Dark Quest (Nishimichi et al. 2019;) has recently been publicly released, which also has an HOD halo model. It might be worth comparing your code with theirs. I believe that CCL (Chisari et al. 2019;) also has a halo model implementation, but I am not sure how advanced it is.

We thank the referee for these suggestions, which will indeed be useful for precision unit tests in future versions of halomod. We have additionally added references to these codes in our introduction.

c. Many users might be interested in dark energy, which I don't think you support at the moment. I think that adding this would be relatively simple, since all that really changes is the scale-independent growth of the power spectrum (ignoring small perturbative dark-energy effects). CAMB already supports this, so you could extract the growth factor from there, but if you wanted to support it within halomod directly then I think that you would only need to add an ODE solver for the growth equation with dark energy (wCDM or w(a)CDM).

Arbitrary dark energy models are already fully supported in terms of cosmography (i.e. expansion rate) via astropy. However, the important component of the growth factor is only partially supported for dark energy models, via using CAMB, as the referee suggests (to use, this simply set `growth_model='CAMBGrowth'` in the constructor). We have added a note to this effect into the paper. We also intend to fully support these models directly from halomod/hmf in the future.

d. The mass function/bias of Tinker et al. (2008/2010) comes defined for different halo-overdensity thresholds. I think that currently you only have the M200 coded up. It would be interesting/useful to add the other definitions and maybe to compare the results to what you get if you 'translate' between halo-mass definitions via the NFW profile.

In fact, the Tinker+08 function as implemented in hmf comes with the parameters from all mass definitions, and the parameters are interpolated for intervening definitions. This is not made clear in the paper, as we do not list all the mass functions in a table as we do for other components (because it is highly redundant with Murray+2013). Translating between halo definitions for the mass function is possible within HMF, and we have made the algorithm for doing so more clear in section 4.3.5 (along with necessary caveats outlining the errors involved).

e. Regarding the Hankel transforms being the bottle neck for some computations: have you considered using FFTlog?.

We thank the referee for this suggestion. Indeed I had considered it previously, but at the time the available library was difficult to integrate into the python library. It seems that now this hurdle has been overcome (there is an `fftlog` library on PyPI). It is not clear to us that FFTLog will outperform the (reasonably sophisticaed) hankel transform algorithm we are using, but it certainly may, in which case we would be crazy not to shift to it. We have created an issue on the halomod repo to check this out.

2. MODERATE CRITICISMS

a. The mix of Fourier and configuration-space around equation (11) is very difficult to understand. It reads as if P(k) is a function of both k and r (!?) I suppose that you keep 'r' in the equations to allow for possible halo exclusion and/or scale dependence in the bias terms (equation 13), but then surely this Fourier transforms along with everything else [$\delta_h(r) = b(M,r)\delta(r) \rightarrow \delta_h(k) = b(M,k)\delta(k)$]? In any case, in their current form I do not understand equations (11) through (15)..

This notation is indeed slightly confusing, but is consistent with the literature (see eg. Tinker+2005). While it would be reasonable to specify the (scale-dependent) bias as a function of k, one should keep in mind that this would require denoting $P_{\rm hh}$ as a *convolution* of the (k-space) bias function with the linear (or halofit) power spectrum. This is currently done implicitly by performing the fourier-transform to real-space and then back to fourier space, as explicitly noted in the paragraph after Eq. 15. We have added additional text around Eq. 11 that clarifies this for the reader.

b. Around footnote 10: "Typically, empirical non-linear corrections on the linear matter power spectrum are used for this quantity". I'm not sure I agree with this: recent galaxy-galaxy lensing work (e.g., Dvornik et al. 2018) uses pure linear theory; as do many cross-correlation studies (e.g., Hill & Spergel 2014; Padmanabhan et al. 2017; Yan et al. 2021); Dark Quest (Nishimichi et al. 2019) uses an emulated model for the halo power spectrum, negating the need to go via the matter. Halo model implementations that try to model the matter auto correlations (e.g., Mohammed & Seljak 2014; Mead et al. 2015, Seljak 2015, Philcox et al. 2020) cannot use HALOFIT (or similar) in the two halo term since this would be completely circular; instead some use perturbation theory to improve the match in the quasi-linear regions.

We agree with the referee that "typically" was the wrong choice of words here. We have updated this paragraph with a little discussion of various approaches to modelling P_m and P_{hh}, and cite some further references.

c. Following on from the previous point: It seems to me that using HALOFIT (or any non-linear prescription for the matter-matter power) in the two-halo term is only common practice by some authors who use the halo model for galaxy-galaxy correlations. While this might improve the model in the slightly non-linear regime (k 0.1 h/Mpc) where 'linear halo bias times non-linear matter' is a reasonable model for the halo density field, this then leads to a large mess in the deeper parts of the two-to-one-halo transition region because 'one-halo' information is entering twice: once from HALOFIT in the two-halo term and once from the one-halo term of the halo model directly (discussed in Cooray & Sheth 2002, and more recently by Garcia et al. 2020; Mead & Verde 2021). This then needs to be fixed by various fudges that people often call 'halo exclusion' or 'scale-dependent bias' but that actually do two jobs: first damping the excess one-halo power that is now erroneously in the two-halo term and second accounting for the genuine effect that a linear bias model cannot be valid to arbitrarily small scales because haloes cannot overlap. If a proper model of the halo-halo power spectrum was incorporated in the two-halo term this wouldn't be necessary. In principle a more complicated biasing prescription can be used within the halo model (e.g., Smith et al. 2007; Nishimichi et al. 2019; Mead & Verde 2021).

In accordance with the previous suggestion, we have updated this paragraph to include much of this discussion.

d. Following on from the previous point: I note that when running halomod and requesting the matter-matter power (via hm.power_auto_matter) the default behaviour is that the halo centre power comes from HALOFIT, which means that the one-halo term is double what it should be for $k > 1$ (you can see this by plotting the ratio of hm.power_auto_matter to hm.nonlinear_power). This is gratuitously incorrect, since HALOFIT is known to match simulation results at the 10% level. I know of one user who stopped using halomod because they could not understand this behaviour (they were trying to adopt halomod to calculate a trispectrum contribution to a covariance matrix). I strongly suggest that you change the default halo-centre spectrum to be linear (this is the least-worst option, and is correct at large scales), and have a warning if a user tries to evaluate the halo model matter-matter spectrum with the halo-centre spectrum as HALOFIT.

We thank the referee for being candid on this issue, and regret that it has driven away potential users. We have changed the default to 'linear' in a new version of HALOMOD. Now, when setting `hc_spectrum` to 'nonlinear', a warning is raised if the matter correlations are computed. Correspondingly, the default for `hc_spectrum` in Table 1 has been updated.

e. Section 2.3: Note that translation between different halo-mass definitions is not always possible via an NFW profile. For example, with an FoF finder run with different linking lengths the density field will generally be partitioned into completely different haloes. This translation is only possible for the halo mass reported by some implementations of SO finders, and only then if the halo centre is fixed while different overdensity spheres are grown, and only then if the halo uniqueness criterion is independent of mass definition (often not the case). I realise that these translations are common in the literature, but do you know of any references that state the accuracy of these translations for the relevant quantities (halo mass function, bias, concentration)?

We've added a small bit of text to this section, making it clear that this is always approximate. We cite figures from Diemer and Kravstov (2015) concerning the concentration-mass relation errors upon translation. We also cite Bocquet et al (2016) concerning errors in the mass function upon translation. While we did not see fit to include a figure in the paper showing the accuracy of the mass function translation, we refer the referee to the final figure of our online tutorial concerning mass definitions: `https://hmf.readthedocs.io/en/latest/examples/change_mass_definition.html`, which shows the errors associated with simple translation using the Bocquet (2016) function. We note that in HMF, the ability to translate between halo definitions for the mass function is OFF by default (instead an error is raised if the definition doesn't match the measured one), but can be switched on (and even then, a warning is raised indicating some of the problems with doing so).

f. Equations (23) and (28): How do you treat mass functions where the normalisation condition is not met (discussed in detail in Schmidt 2016)? This is (probably) not a problem for galaxy correlations, where a lower-mass limit appears by virtue of having a minimum halo mass that hosts a galaxy, but when you compute 'matter' clustering statistics you need to integrate over all halo masses in order to recover linear theory at linear scales, and if equation (28) is not satisfied this will not happen.

See response to point (h) below.

g. Equation (28) is generally satisfied as long as equation (23) is satisfied and the bias is derived from the mass function using the peak-background split approach. Equation (23) can also be satisfied manually, (e.g., Tinker et al. 2010). Do you allow users to use mass function/halo bias pairs that are 'incompatible' in this sense? If so, do/should you warn them?

See response to point (h) below.

h. Following on from the previous two points: I noticed when using halomod that the two-halo term for the matter-matter power is identical to linear theory (when hc_spectrum='linear'), this sidesteps the issue discussed in the previous points, and is not what you have written in equations (14) or (15). I think I should see the two-halo power drop below linear on small scales when the values of the integrals in equation (15) drop below unity.

Here we respond to this and the previous two points. The referee's points here are very well taken – it has always been a goal to make the choosing of "compatible" mass functions and bias functions more easy within halomod. To this end, we have updated hmf and halomod (in v3.4.0 and vXXX, now referenced in the paper) with the following features:

- Each fitting function now has an attribute `normalized` that indicates whether that model integrates to the mean density over all halo masses. This allows the user to quickly check if their model is normalized. Models such as Sheth-Tormen further allow adjusting the shape parameters and automatically re-normalizing (this was already a feature).
- Each bias model now has an attribute `pair_hmf`, which points to the relevant HMF that is a "compatible" pair (in the halo model ansatz sense). This HMF will then be chosen by default within halo model calculations.
- The behaviour the referee was noticing for the 2-halo matter-matter spectrum was indeed short-circuited in halomod to ensure that the large-scale matter spectrum lines up with linear theory, regardless of the HMF and bias models chosen. This was a poor choice on our part, as it obviously means that the transition region is not getting any of the halo modelling benefits. We have updated the code to do the halo modeling for the matter-matter 2-halo term, including any appropriate halo exclusion etc. However, we now provide a new option, `force_unity_dm_bias` which manually renormalizes the 2-halo term so that it tends to a bias of unity at $k \to 0$ if set. This is independent of whether the HMF/bias models are chosen to be compatible. We make this by default to be switched on, but leave it as an option since some halo models (eg. those for WDM) explicitly do NOT require all DM to be in halos. The fact that this renormalization occurs for any pair of HMF/bias is perhaps slightly confusing – the renormalization is implemented by manually computing the integrated "effective bias" at $u(k,m) = 1$ (over the limited mass range set in the code) and normalizing by it, which can be done for any mass function (even divergent ones). The user may expect (and desire) a non-unity bias at large scales when using incompatible HMF/bias functions, which can of course be achieved by setting the `force_unity_dm_bias` to false. However, the value of the bias in this case is still likely not accurate – while part of the reason for not obtaining a unity bias is due to the incompatibility, another part is that the integration range is finite. For diverging integrals, the regions outside the mass range may significantly contribute to the true effective bias. In future versions, we may attempt to approximate the full integral by extrapolating to masses of zero (or until convergence).

We have updated the text in several places to explain these updates to the code:

- The HMF component description (S4.3.5) now explains the new `normalized` attribute.
- The Bias component description (S4.3.6) now explains the new `pair_hmf` attribute, and the relevant paired HMF is listed in Table 5.
- The `force_unity_dm_bias` option is now listed in Table 1, with a very brief explanation of what it does.
- We have added a new standalone paragraph to the discussion of "features" of the halo model framework in S4.2.1 discussing the new `force_unity_dm_bias` option.
- We have updated the discussion after Eq. 14 to introduce this behaviour.

i. The authors should explain the origin of $\langle N(N-1)\rangle$ above equation (57) and in Section 3, i.e., why is this not $\langle N^2\rangle$? It seems to me if the factor $\langle N(N-1)\rangle$ appears in the one-halo term then this will always be zero for a tracer with occupation number either 0 or 1 (e.g., central galaxies, but also haloes themselves), but the one-halo term of such tracers should be a pure shot-noise term in Fourier space (obviously this vanishes in configuration space, except at r=0).

We have significantly expanded the arguments in this section and the following section (where it applies to the 1-halo term). The short answer is that halomod returns power spectra without shot-noise intentionally – the correct shot-noise (under the assumptions) is simply a constant $1/\bar{n}_g$, and this can be manually added back in by the user.

The reason we do not leave it in is because there is a spurious shot-noise-like behaviour in the 1-halo term that we attempt to remove (via the `force_1halo_turnover` option). However, doing so would also remove the true shot noise.

j. When I use halomod to evaluate power spectra I see that for 'matter-matter' and 'matter-galaxies' the one-halo terms are constant at large scales, as I would expect. However, the one-halo term for 'galaxies-galaxies' drops to zero on scales only slightly larger than the transition scale. This is despite no 'one-halo turnover' being set. Why is this? I think that there should be a shot-noise term originating from the auto correlation of discrete galaxies.

By default, the `force_1halo_turnover` is true, which is probably why the referee is seeing this behaviour when it is "not set". On the other hand, the fact that it was *not* turning over for matter-matter was a bug, and this is fixed in an updated version of HALOMOD. We thank the referee for noticing this.

k. When measuring a concentration-mass relation from numerical data it is necessary to have defined halo mass, be it M200, Mvir, or whatever. I do not see any discussion of this in your paper, so please can you add some text describing how you address this. Is the user warned if they attempt to use a concentration-mass relation that is not compatible with their halo definition? Are halo concentrations 'translated' in this case? Are they warned if they use a concentration relation that is inappropriate for their choice of cosmology?

We have added some text to the concentration-mass section (4.3.8), making it clear that (i) halomod does not warn users about inappropriate choices of cosmology (we expect the errors from this to be less significant than other modeling errors), (ii) halomod does NOT translate c(m) relations between halo definitions, but instead issues a warning to the user, but (iii) if the user adopts c(m) relations from COLOSSUS, the c(m) relation is automatically translated.

3. MINOR CRITICISMS

a. You should add some more galaxy-galaxy lensing references in your introduction (e.g., Cacciato et al. 2012; Dvornik et al. 2018).

We have added these references to the introduction, along with a couple of extra references.

b. Some of the other references in the third paragraph of the introduction are a bit dated, and could be usefully updated.

We have added many more newer references to this section – still certainly not covering the full breadth of current research, but more representative.

c. There are recent commits on AUM, so I'm not sure that it should be labelled as discontinued.

Thank you for noting this – there were no recent commits when that particular part of the text was written. We have removed the 'discontinued' label.

d. The chomp link you provide did not work for me, but I found this: `https://github.com/morriscb/chomp`

We have updated the URL to the Github repo, though it still appears to be discontinued (last commit in 2015).

e. Section 2, point 1: To calculate an n-point function you actually need the n-th moment of the distribution of halo density profiles at fixed mass. This is usually ignored and the n-th moment is taken to be the n-th power of the mean, with the assumption being made that there is negligible scatter at fixed halo mass. Accounting for this scatter in the matter-matter spectrum has been investigated (e.g., Giocoli et al. 2010) and in cross correlations (e.g., Koukoufilippas et al. 2020). I think that you are effectively taking this into account with your assumption of Poisson statistics in Section 3.1 and the fact that you keep the expectation value in the expressions.

We thank the referee for this clarifying point. We have amended the introduction text to indicate that in general the n-th moments of the distribution of halo density profiles are required (but that we will be ignoring this). We believe that while the Poisson statistics of the HOD account for this to some degree for galaxies, this distinction independently affects DM statistics, which is clearly ignored in our approach.

f. Section 2, points 3 and 4: These two points could be combined to just say "A model for the halo distribution", since this is all that equation (1) requires.

While the referee is correct here, we find it more clear to leave this as two points, as we introduce both of these concepts further in the text.

g. You have two points '4.' in your list under equation (1).

We made the corresponding correction, thank you!

h. What you have written in (the first) point 4 is only correct for linear halo bias models. More advanced bias models model the halo-halo spectrum via perturbation theory or directly.

We have removed the unnecessarily non-general 'linear' from point 4 here. It is made clear later that in fact HALOMOD does typically assume the linear density field, but it is not necessary at this point.

i. Footnote 10: HALOFIT does not use the halo model. HALOFIT is a fitting function, which happens to be heavily inspired by the halo model, but it does not use the halo model directly. There is no halo bias, mass function, or halo profiles in HALOFIT.

To clarify this point, we have amended the footnote to read: "This is slightly circular, as the HALOFIT model itself is inspired by the halo model (and is tuned to emulate the clustering of dark matter within and between halos)."

j. Figure 2: How are the linear spectra with the different T(k) normalised? It does not look to me like they all share *sigma_8* (BBKS is low across the range compared to CAMB, for example).

In fact, they are normalized by σ_8 – though BBKS is low across most of the range, it is higher in the centre where the integral is more highly weighted. We have made an addition to the caption of this figure to make this clear.

k. Table 3: BBKS model: Your equation for 'q' differs from that in BBKS (their equation G3). Their definition of 'q' is dimensionless, whereas yours carries the units of 'k' (I think you are missing a division by h/Mpc). Your 'q' has an Omega_b dependence, which is absent in BBKS. Also you have 5.47 whereas they have 5.46 for one of the c's.

Thank you for pointing this out. The 5.47 was indeed a typo (both in the code and the paper). The Ω_b dependence is drawn from later work, which is now cited in Table 3. We have also added the appropriate units to q. Furthermore, we have updated Figure 2 to include the fixes. Finally, the referee's comment prompted us to also check the Bond-Efstathiou fit, which had an extra h than what is required. This has now been fixed as well.

l. Please specify if the $\xi_m(r)$ appearing in equations (30) and (31) is linear or non-linear.

We have clarified that this is the non-linear matter correlation function from (eg.) halofit.

m. Throughout you switch between using single and double subscripts for two-point functions (e.g., either ξ_g or ξ_{gg} for the galaxy auto correlation). Please stick to one convention; I would suggest the double-subscript convention since it most easily generalises to cross correlations (e.g., ξ_{gm} for the galaxy-matter cross correlation).

Thank you, this is an excellent suggestions. We have made the corresponding correction.

n. Is there not a factor of 2 missing in the final term of equation (57), (58), ...?

Indeed, there is a factor of two missing. This was a complete oversight in the text. It has been fixed.

o. Where does the factor of 1/2 come from in equation (62), and the factor of 2 in equation (63)?

Yes, these are also typos (the code itself is correct). We have fixed these equations.

p. Fig. 7: Please label the z and Mmin of each curve.

We have updated Fig. 7 with these changes.

q. In halomod you have a 'takahashi' parameter, but you do not specify what happens if this is set to False. I guess that in this case you get the Smith et al. (2003) HALOFIT model, but you should state what happens explicitly somewhere.

We have updated Table 1 in the paper to explicitly mention this, and also the docstring for the takahheter in the HMF code.

r. For the Duffy et al. (2008) c(M) relation, please specify if you provide that for the 'full' or 'relaxed' halo sample, and for which overdensity criterion/ia.

HALOMOD in fact includes both 'full' and 'relaxed' halo samples, and all overdensity criteria (and there is in-built flexibility to adjust any of the coefficient parameters). We have update the caption of Table 7 to reflect this.

s. Final paragraph of Section 2.2: You say that HALOFIT modifications are "applied as ratios of $P_{m,lin}$". Is this correct? The 'quasi-linear' term in HALOFIT can be expressed as such a ratio, but it is my understanding that the 'halo' term cannot be expressed in the same way.

The referee is quite correct. We have amended the relevant text.

t. For which halo-mass definition are the curves in Fig. 3 presented?

We have updated the caption of Fig. 3 with the following:
"Note that the mass definitions and cosmologies assumed by the fits are rather disparate – this is not meant to be an apples-to-apples comparison. While $\nu f(\nu)$ is almost independent of cosmology [**?**], and in principle independent of mass definition (peak-height is based on the initial Lagrangian field), in practice $f(\nu)$ is measured from simulations which must employ some halo-finding algorithm and mass definition."

u. Final paragraph of Section 2.3: you say that the translation is possible "to first order" - is this correct in a formal, mathematical way?

This was not intended to be a formal use of the phrase. We have amended it to "approximately".

4. MINUTIAE

1. The first sentence of your abstract could be read as if the halo model can *only* predict '2-point' functions, you might want to change this to 'n-point' functions, or remove '2-point' altogether. We have changed this to 'n-point' functions.
2. Both 'two-point' and '2-point' are used throughout the paper - the authors should pick one of these and stick to it. We have changed them to '2-point'.
3. 'Python' should be formatted with small capitals along with other software. Thank you, we have made the corresponding correction.

4. Nested brackets in the Bond & Efstathiou (1984) reference on p7, Murray et al. (2013b) reference on p14, Skibba & Sheth (2009) on p28, and elsewhere. Thank you for noticing this. We believe we have fixed all instances of this.
5. You sometimes incorrectly have new paragraphs starting directly underneath equations when in fact the preceding paragraph should continue (e.g., below equation 18). Thank you, we have checked every equation and following paragraph, and have amended as necessary.
6. The authors could replace $<\sim$ and $>\sim$ with $\lesssim$ and $\gtrsim$. We made the corresponding correction.
7. 'sin' should be 'sin' in equation (38), (40). We made the corresponding correction.
8. The 'd' in any derivative should be Roman formatted (e.g., equation 22). We believe we have checked and converted every instance of this in the text, thank you.
9. Schneider (2014) has now been published and is Schneider (2015). We have updated this reference.
10. Equations (84), (86) overflow the boundary. We made the corresponding correction.
11. The word 'Figure' is missing before '11' just before Section 4.3.9. We made the corresponding correction.
12. 'figure' → 'Figure' if you reference a figure in the paper. We made the corresponding correction, as well as homogenizing all references (equations/figures etc.) with `\cleveref`.
13. 'may used' → 'may be used' in the second paragraph of Section 8. We made the corresponding correction.
14. Many of your subscripts on variables ought to be Roman formatted (e.g., 'hh' in P_hh) if they refer to names rather than variables. This is a good suggestion, thank you. We have made the appropriate changes.
15. You need lower limits on the integrals in Equation (46). This has been rectified.

5. OTHER CHANGES

Additionally, some slight changes have been made to our manuscript...

- New authors have been added, who were instrumental in developing the new more interactive version of TheHaloMod.
- Figure 1 – a screenshot of the web-application – has been updated to the newest online version (which is quite different from the version available at time of submission).
- We have updated the version of halomod/hmf referenced in the paper to v3.4.1 and v2.1.0. These new versions were mostly the result of the very helpful comments from the referee.