

Peer Review File

Manuscript Title: The Radcliffe Wave is Oscillating

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

Nature Manuscript MS 2023-06-10928 by Konietzka et al. analyzed position and kinematic data from molecular gas and star clusters to investigate whether the gas and star motions for this spatially oscillating structure are actually consistent with coherent wave-like motion. The manuscript concludes by using the oscillation parameters to place constraints on the mass content of a disk-like component for dark matter. A full review follows.

-- Erik Rosolowsky

Overall, the manuscript is a notable piece of analysis, which would be suitable for publication. Prior to recommending acceptance for publication in Nature journals, I recommend a few clarifications and extensions of the analysis to make the work complete.

- The results of the work depend on the location of the Sun with respect to the midplane of the Galactic potential. This is addressed in part in two locations and, overall, the authors argue that this effect is small. However, in fixing to $z=0$, the credible intervals for the derived parameters do not include the possible variation. I recommend including $z_{\text{gal,sun}}$ as a nuisance parameter with reasonable distribution and marginalizing over it in the dynesty analysis. The Reid et al determination of $Z_0 = 5$ pc reflects referencing to the midplane though that paper admits there may be warping locally, which could mean that the Z_0 is larger (though likely <30 pc) in the Solar neighborhood. Marginalization would obviate any concerns about the importance of this variation and clearly demonstrate that the changes in derived parameters are small. It also wasn't clear whether manuscript considered the deceleration of the Sun if it were above the midplane.

- The evaluation of a traveling vs. standing wave model is a reasonable question but the manuscript omits considering intermediate cases. In wave analysis, this would be framed as considering two counter-propagating waves with frequencies of $\pm\omega$ but different amplitudes or other equivalent formulations. The manuscript is correct that a strict 0 degree phase offset between position and velocity components is not consistent with the data, but the manuscript does not consider intermediate cases. A proper, generalized analysis would likely introduce too many free parameters to constrain since the vertical velocity is not uniquely connected to the vertical position. However, the traveling wave provides a clear extremum for the wave motion with a standing wave providing the other. Hence, the manuscript should be clear that this is a limiting case and reframe conclusions as limits as appropriate. In particular, reframing this as a limit may reduce the constraints on Solar motion but the manuscript is impactful and important without the section on dark matter and

vertical oscillation constraints. Alternatively, the authors could invert the reasoning and assume the parameters of the local potential to show that the amplitude of the $-\omega$ wave would be small showing this is a traveling wave.

- The adopted model is shown to be consistent with the data and at least as representative as the previous spatial model, up to an intrinsic scatter term. However, there are a series of ad hoc model choices that are adopted but not well evaluated to determine if alternative choices are more appropriate. Notably the choice of a Lorentzian damping term for the amplitude and the linear scaling of the wavelength with parametric shape are introduced but not critically evaluated. While exploring all possible models is beyond the scope, the manuscript could better motivate these choices and either dismiss or suggest alternatives that could be explored in future work.

- While more physically grounded than the original Radcliffe Wave work, the manuscript has a strong empirical basis and the manuscript should be expanded on phenomena that would lead to strong variations in the oscillation wavelength over a what the authors (correctly) contend is a relatively uniform region. Of note, this implies a strong variation in the phase velocity of the wave. Checking some numbers, suggests a phase velocity of 320 km/s (!) on one end of the wave and 20 km/s at the other (though the domain of the parameter s was unclear). The model proposed in the manuscript would be more compelling if it could explain these strong variations in phase velocity and propose a physical model (and associated estimates of group velocity) that explained these variations. It is challenging to develop a clear physical understanding of something that would lead to such strongly varying wavelengths in just a couple periods without a dramatic change in the underlying potential. The authors should expand the manuscript to propose a possible physical model.

Minor comments:

- "Elliptical integral of the first kind" -> "Elliptic integral of the first kind"

- In Equation 4, the notation in the manuscript suggests that angular velocity ω should be a function of z, r and not ζ, r .

- Are the weights in Equation 7 set to sum to unity? The methods by which the weights are determined is vague and should be expanded.

- The Schönrich et al. values for the peculiar solar motion are given as (11.1, 12.24, 7.25) km/s. Were these translated to a different frame before use in this calculation (e.g., LSRD->LSRK)? The values are more consistent with the older Kerr et al. (1986) values.

- It would be good to confirm to the reader that the scaling of v_z by $1/\omega_0$ to place it on a common basis as the spatial terms in Equation 8 uses the variable ω_0 . In reading, I found that this might be made clearer by explicitly including this factor in Equation 8. Related, is the time derivative in Equation 8 a full time derivative or a partial? It wasn't clear, given the complexity of the model, that these were equivalent.

- The origin of the prefactor of 3 in front of the variance term in Equation 9 was not clear particularly

when referencing Equation 7.

- The statement that $C(x_k) = |x_k|$ for small $|x_k|$ presumably means the Euclidean norm for the vector but in a vector valued sense this might be better denoted $||x_k||$ since $|x_k|$ is sometimes used to denote the L1 norm.

- In the section on Line of Sight Gas Velocities:

"model the 6D motion" -> "model the 6D structure"

"taking advantage of the fact that the Radcliffe Wave" -> "taking advantage of the model of the Radcliffe Wave as"

Referee #2 (Remarks to the Author):

The paper presents a kinematic model for the Radcliff wave. The RW is a feature in the local Galaxy traced out by molecular clouds and star clusters. Its properties will certainly be of interest to a wide range of astronomer and a paper that ties together spatial and kinematic data is certainly worth considering for publication. However, I have a number of concerns about this work that prevent me from recommending this paper for publication in Nature in its current form.

My main concern with the paper is that it arrives at strong conclusions based on very specific models that make simplifying assumptions. In my view, this may lead to over-interpretation of the data. I would add that the assumptions are not always explicitly stated.

Let me be more specific. As I understand, the basic assumption is that the RW is born when star clusters and molecular clouds are either displaced in z or given a kick in v_z as well as in v_R and v_{ϕ} . The individual objects along the wave then evolve kinematically -- oscillating in z and drifting in R and ϕ . Furthermore, the motion is determined solely by gravity and the z and in plane motions decouple. Do I have this right? All these assumptions should be stated explicitly. The authors comment about deceleration of the wave as it moves through the ISM so already, there is a question about the last assumption.

The core of the paper is the kinematic model, which begins with an ansatz for the potential, equation 1. The authors include an anharmonic term, citing Antoja et al. Now in Antoja et al, one is observing thousands of stars at the same R and we see the phase mixing due to the anharmonicity of the potential directly in the famous Gaia phase spirals.. Here, we have a 1-D object with essentially a single R and z as a function of s along the RW. The RW is within 250 pc so the effect of the anharmonic term is at most 4%. It looks like the radial extent of the RW is on the order of 1-2 kpc. The effect on the oscillation period is probably smaller. Also, the effective ω_0 is changing with galactocentric radius through the exponential term on the potential. This seems to be a bigger effect than the anharmonic term. Finally, ω_0 is only ever determined by 10%. Thus, it seems like the anharmonic term will be within the noise for this study.

As for the model, I would also suggest that the authors quote the Oort constants A and B along with

v_{circ} and the midplane density for their equation 1 and assumed values for the model parameters and include references to literature values, with uncertainties. (Minor point: Since ω_2 has units of $1/\text{length}$, I suggest using a different symbol from ω . As written, one is tempted to think that ω_0 and ω_2 have the same dimensions.)

I found Sup Fig 1 a bit difficult to interpret even in interactive form. How about a figure that shows z and v_z as a function of s along the wave. To my eye, the z and v_z data have clear systematic differences with the data, but the 3D plots can lead to unwanted projection effects.

Each point along the RW acts as an oscillator about the midplane. So, the model should have two terms --- a right moving wave and a left moving wave. The question of standing vs travelling wave then comes down to the relation between the two...travelling and one of the two terms is zero; standing, and they are the same or have opposite signs. It would be good to fit the two terms and then let the data determine the admixture of the two solutions. Then, a standing wave would have $A = -B$ and travelling waves either $A=0$ or $B=0$. I would suggest that the authors use their data to constrain A and B .

It is worth stating that they are treating the RW as purely kinematic structure and the gravitational potential of the galaxy stationary. There's been quite a bit of research lately on vertical oscillations of the disc itself. Xu et al and Schonrich and Dehnen found evidence for vertical waves near the Sun. There's the work by Antoja et al, Widrow 2012, the Gaia DR2 and DR3 kinematics papers and many more that discuss vertical oscillations of the disc itself. How would a changing vertical potential affect the authors conclusions?

Finally, I'm puzzled by the focus on the dark disc and dinosaur extinction. These ideas are on the fringe of Galactic dynamics and have already been widely discounted. For example, the authors can translate their results into a constraint on the total density in the midplane by using the potential and Poissons equation. This could then be compared with previous determinations such as the one in the McKee paper that they cite or many others. Dressing it up in a discussion of the dark disc hypothesis and dinosaur extinctions seems gratuitous.

Referee #3 (Remarks to the Author):

A.1 Summary of the key results

The authors reconstruct the Ratcliffe wave in space and kinematics and show coherence of motion across the structure. They propose that the Sun lies on that structure and measure a period of vertical oscillation for the Sun based on a fit to the Ratcliffe wave. They use their obtained value to argue that it is higher than the value needed for a dark disc (with properties that have already been ruled out two years ago thanks to Gaia data) to kill the dinosaurs. However, this is not an outstanding result given that prior work already came up to similar conclusions (details below).

B. Originality and significance: if not novel, please include reference

The results/conclusions are not original or significant (details below).

It is nice to see that the authors have been able to map the Ratcliffe Wave in 6-D phase-space. However, the significance of the results is not major, but rather incremental since its discovery. We also know that coherent structures in the Milky Way are expected to exhibit 90 degree offsets between kinematics and space, that can be naturally appreciated from the epicyclic approximation (see Binney & Tremaine 2008). That said, it is beautiful to see things come full circle and I congratulate the authors for it.

Figure 2 which is the main figure of the paper, looks quite dramatic, but the most stringent limits on a thin dark disc were already set two years ago by Widmark et al. (2021), with an upper limit of 1.96 Msun/pc^2 at 68% confidence level for a thin dark disc with scale height of 25pc. The Widmark+21 result is consistent with there being no dark disc since for comets to destroy dinosaurs would require a dark disc with a surface density above 10 Msun/pc^2 !

Given the result from Widmark+21, this makes the main and only science plot/result of this paper rather anecdotal and a mere corroboration of Widmark et al.'s work (<https://ui.adsabs.harvard.edu/abs/2021A%26A...653A..86W/abstract>).

Because this paper solely rests on one Figure/result which is not novel and a bit outdated now, I cannot recommend this paper to Nature as it is not an original or novel result... However it may do well in another type of journal.

On a more subjective note, I do not think this result will be of immediate interest to people in my field (Galaxy Formation/Galactic Dynamics/Galactic Archaeology) or to paleontologists...

C. Data & methodology: validity of approach, quality of data, quality of presentation

The approach is fine. The Galactic dynamical modelling is somewhat simplistic, but I understand that the authors wanted to make "order of magnitude" estimates...

D. Appropriate use of statistics and treatment of uncertainties

The methods are fine-ish, but they do not discuss how they determine basic quantities such as distances from parallaxes for example (or deal with negative parallaxes), there are a number of things that are just under the hood... Also why did the authors opt to leave out their error bars on any of the different extended figures (except the CO velocities figure)?

There are systematics which they do not have any control over that they do not discuss (e.g. uncertainty in the baryonic model for the Galaxy...), would have been nicer to see a more thorough discussion here.

E. Conclusions: robustness, validity, reliability

Reasonable and fine-ish with speculative bits which the authors acknowledge are beyond the scope of their paper.

F. Suggested improvements: experiments, data for possible revision

Show your error bars and give a more thorough discussion of the methods... I would recommend a better review of the prior literature and possible extended discussion w.r.t. past results.

G. References: appropriate credit to previous work?

Should have cited Widmark et al. 2021:

<https://ui.adsabs.harvard.edu/abs/2021A%26A...653A..86W/abstract>

H. Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

The paper is well written in general, though Figure 2 is a bit over the top since a lot of the parameter space has already been ruled out thanks to Gaia. Perhaps in a future submission they could discuss the result and show the complementarity between the two studies, that could be useful/interesting.

I am sorry I cannot recommend this paper, but I am sure that with a few little revisions, it will do quite well in any of the main astrophysical journals (MNRAS, ApJ, A&A)!

Author Rebuttals to Initial Comments:

Response to Referee Comments on “The Radcliffe Wave is Oscillating”

We thank the referees for their thorough and constructive review of our manuscript. Given the additional analysis performed and discussed in this referee report, we wish to make this entire report and our response public alongside the manuscript as part of *Nature’s* protocol for publishing the peer review file.

To account for the comments of the referees we performed the following main changes to the manuscript.

1. Removal of the dinosaur discussion:

Since the key part of this work is the Radcliffe Wave’s oscillation and not the analysis of the Galactic potential, we removed the discussion of the dinosaurs from the text. In this context, we also removed Figure 2. Based on the referee’s comments, we now only show how we utilize the Wave to measure properties of the local Galactic potential and the Sun’s vertical oscillation period.

2. New Figure 2:

To emphasize that the key part of this work is the Radcliffe Wave’s oscillation, we incorporate a new Figure 2 showing the oscillation of the Radcliffe Wave in WorldWide Telescope at different phase snapshots.

3. More detailed modeling

In response to the referees’s comments about the possibility of a traveling versus standing wave, we modified our wave model to be more general and included more detailed descriptions and motivations of the underlying assumptions. Here we summarize the key changes. A full mathematical description of the modeling is included after this executive summary. In brief, we present a formalism that allows for superpositions of a standing and traveling wave, damped vertically and horizontally that can explain any observed wave shape seen in Figure R1. While considering alternative models has significantly strengthened the manuscript we find that our original model is still an excellent fit to the data. We have added a discussion about the new generalized model being in favor of a traveling wave in the Methods section and added a brief reference in the main text.

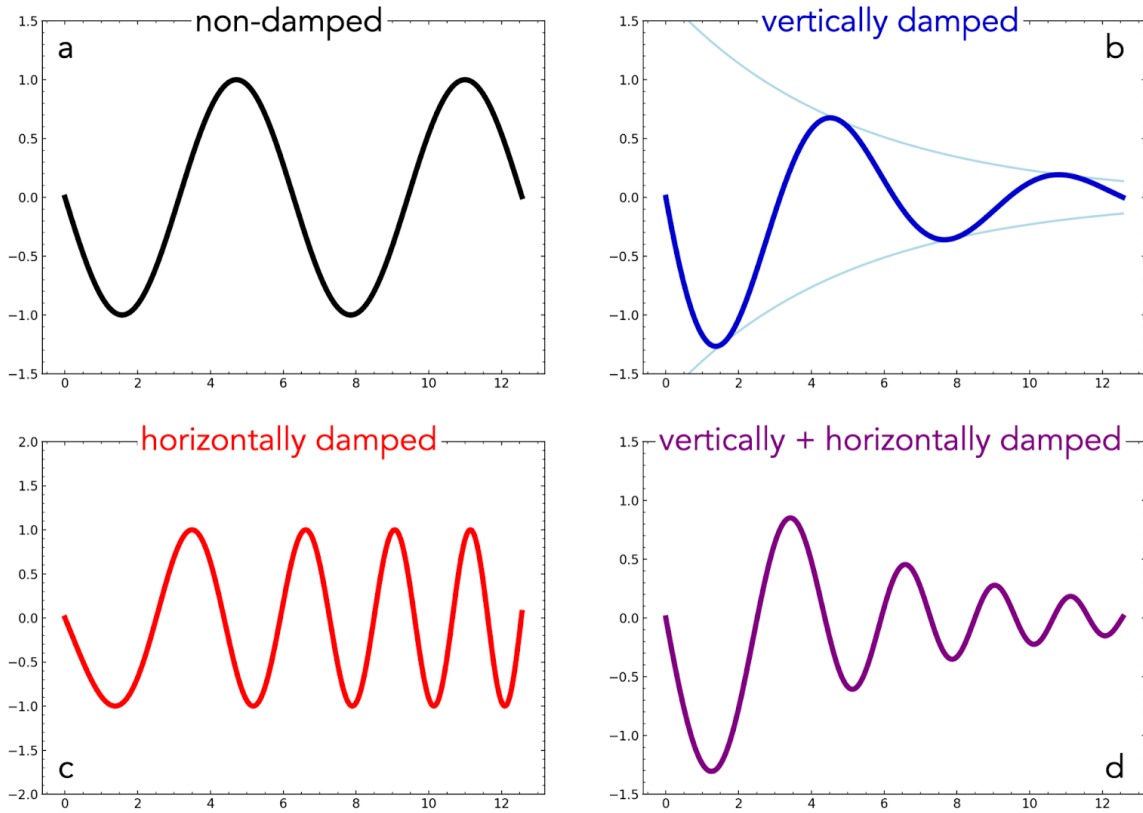


Figure R1: Different forms of wave “damping” used in this work. **Panel a:** We show a non-damped classical “sine”-wave in black. **Panel b:** We show a vertically damped wave in dark blue and the corresponding damping function in light blue. **Panel c:** We show a horizontally damped wave in red. **Panel d:** We show the combination of horizontal and vertical damping in purple.

3.1. New generalized model:

We now consider two counter-propagating waves with frequencies of $\pm\omega$ but different amplitudes to allow for a traveling or standing wave and all mixtures of the two. We fit this model to the stellar cluster data and find that the more general model is consistent with the Radcliffe Wave being a traveling wave.

3.2. Modeling the vertical damping

We motivate our vertical damping model in more detail by computing the vertical energy along the Wave. We show what we refer to as vertical damping in Figure R1b.

3.3. Modeling the horizontal damping

We motivate our horizontal damping model in more detail by computing the change of the phase along the Wave, which is equal to computing the change of the wavelength along the Wave. We show what we refer to as horizontal damping in Figure R1c.

3.4. Decoupling of xy and z :

We motivate our choice on decoupling the in-plane (Galactic x,y -coordinates) and vertical (Galactic z -coordinate) motion and now discuss this point in the Methods section.

3.5. Dominating force:

We discuss the validity of the assumption that the gravitational pull is dominating over hydrodynamic influences in the context of the Radcliffe Wave kinematics. This point was added to the Methods section.

3.6. Stationarity and shape of the Galactic potential:

We motivate our choice of treating the underlying Galactic potential as stationary. In the revised Methods section, we discuss the possibility and effect of a perturbation of the potential around the Radcliffe Wave.

3.7. Effect of the Solar position

To investigate the effect of the Sun's current vertical position with respect to the Galactic plane, we rerun our analysis including the vertical position of the Sun as a free parameter. After marginalizing over the vertical position of the Sun we find that our conclusions are unaffected by the variation of the Sun's vertical position. We added a discussion of the effect of the Sun's height in the revised Methods section.

3.8. Model sensitivity to the scale height

In the revised Methods section, we now emphasize that our constraint on the vertical scale height needs to be treated with caution for large scale heights, especially for >300 pc, as our sensitivity to the scale height becomes too low to be distinguishable from the noise level of this study.

4. Analysis of the Wave's phase velocity:

Motivated by the referee's comment to investigate the change in phase velocity along the Wave, we perform an analysis of the variation in the wavelength and phase velocity in the Methods section and added a brief reference in the main text.

5. Discussion about the origin of the Wave:

We added an additional paragraph to the main text where we discuss possible Wave origins given the new insights obtained in this work.

We respond in detail to each comment below.

Referee #1 (Remarks to the Author, *referee's original comments in gray and italics*):

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Overall, the manuscript is a notable piece of analysis, which would be suitable for publication. Prior to recommending acceptance for publication in Nature journals, I recommend a few clarifications and extensions of the analysis to make the work complete.

- The results of the work depend on the location of the Sun with respect to the midplane of the Galactic potential. This is addressed in part in two locations and, overall, the authors argue that this effect is small. However, in fixing to $z=0$, the credible intervals for the derived parameters do not include the possible variation. I recommend including $z_{\text{gal,sun}}$ as a nuisance parameter with reasonable distribution and marginalizing over it in the dynesty analysis. The Reid et al determination of $Z_0 = 5$ pc reflects referencing to the midplane though that paper admits there may be warping locally, which could mean that the Z_0 is larger (though likely <30 pc) in the Solar neighborhood. Marginalization would obviate any concerns about the importance of this variation and clearly demonstrate that the changes in derived parameters are small. It also wasn't clear whether the manuscript considered the deceleration of the Sun if it were above the midplane.

We thank Prof. Rosolowsky for raising this point. We rerun our analysis on the stellar cluster data (which is the key data set of this analysis as it combines the spatial and kinematic information of the Wave) including the vertical position z_0 of the Sun as a free parameter. We set a uniform prior with a lower limit of $z_0 = -10$ pc and upper limit $z_0 = 25$ pc. After marginalizing over z_0 we find that the amplitude A , the phase φ , and the damping parameter δ change by less than a third of the 1σ uncertainty reported in the case where z_0 is fixed. All remaining parameters in Θ_1 change by less than 10% of the 1σ uncertainty reported in the case where z_0 is fixed. This is now reported in the text. As for the Sun's deceleration, our analysis of the Wave's kinematics is only based on stellar velocities but not on second order changes of the stellar positions with time, so we do not expect the Sun's deceleration to affect our results.

The evaluation of a traveling vs. standing wave model is a reasonable question but the manuscript omits considering intermediate cases. In wave analysis, this would be framed as considering two counter-propagating waves with frequencies of $\pm\omega$ but different amplitudes or other equivalent formulations. The manuscript is correct that a strict 0 degree phase offset between position and velocity components is not consistent with the data, but the manuscript does not consider intermediate cases. A proper, generalized analysis would likely introduce too many free parameters to constrain since the vertical velocity is not uniquely connected to the vertical position. However, the traveling wave provides a clear extremum for the wave motion with a standing wave providing the other. Hence, the manuscript should be clear that this is a limiting case and reframe conclusions as limits as appropriate. In particular, reframing this as a limit may reduce the constraints on Solar motion but the manuscript is impactful and important without the section on dark matter and vertical oscillation constraints. Alternatively, the authors could invert the reasoning and assume the parameters of the local potential to show that the amplitude of the $-\omega$ wave would be small showing this is a traveling wave.

The adopted model is shown to be consistent with the data and at least as representative as the previous spatial model, up to an intrinsic scatter term. However, there are a series of ad hoc model choices that are adopted but not well evaluated to determine if alternative choices are more appropriate. Notably the choice of a Lorentizan damping term for the amplitude and the linear scaling of the wavelength with parametric shape are introduced but not critically evaluated. While exploring all possible models is beyond the scope, the manuscript could better motivate these choices and either dismiss or suggest alternatives that could be explored in future work.

We thank Prof. Rosolowsky for bringing up these very important two points. In the following we will address the below points.

1. Introduction of a more generalized model
2. Motivation of the vertical damping
3. Motivation of the horizontal damping
4. Application of the entire model to the data

We find that the more generalized model is consistent with the Radcliffe Wave (RW) being a traveling wave.

1. Generalized wave model

In the revised manuscript, we extend our model to allow for a traveling or standing wave and all mixtures of the two. As suggested, we do this by considering two counter-propagating waves with

frequencies of $\pm\omega$ but different amplitudes. In this context, we introduce the parameter B which determines the mixture between a standing and traveling wave model. We note that based on the position of the RW in the Galactic plane (pitch angle $\neq 0$) the radius r can be parametrized as a function of distance s along the Wave, implying $\omega(z_{\max}(s), r(s)) = \omega(s)$, such that we can write down the model as

$$z(s, t) = \zeta(s) \left(B \sin(\omega_0 t + 2\pi \Lambda(s) + \varphi) + (1 - B) \sin(-\omega_0 t + 2\pi \Lambda(s) + \varphi) \right) \quad (1)$$

$$v_z(s, t) = \zeta(s) \omega(s) \left(B \cos(\omega_0 t + 2\pi \Lambda(s) + \varphi) - (1 - B) \cos(-\omega_0 t + 2\pi \Lambda(s) + \varphi) \right) \quad (2)$$

where t denotes the time, s the distance along the Wave, ω is the vertical oscillation frequency (defined by equation (3) in the original work and by equation (16) in this manuscript), ω_0 is the zeroth order oscillation frequency, φ is the phase and $\Lambda(s)$ and $\zeta(s)$ describe damping functions along the Wave.

In the following we will motivate our choice of the vertical and horizontal damping functions.

2. Modeling the vertical damping ζ :

Using the vertical gravitational potential introduced in this work (see Full Spatial and Kinematic Model) we can calculate the vertical energy of the Wave and from that the maximal vertical deflection of a single star cluster k under the vertical pull of the Gravitational potential as

$$z_{\max, k} = \Phi^{-1} \left(\frac{1}{2} v_{z, k}^2 + \Phi(z_k) \right). \quad (3)$$

Following equations (1) and (2), we can approximate $z_{\max}(s)$ as follows:

$$z_{\max}(s) \approx |\zeta(s)| \left(1 + 4 \cos^2(2\pi \Lambda(s) + \varphi) (B^2 - B) \right)^{0.5}. \quad (4)$$

Ultimately, we find that B is close to 1 and in particular larger than 0.5. This implies that the second part in equation (4) will always be larger than zero and can be seen as a scatter term perturbing $\zeta(s)$. In other words, the overall behavior of $z_{\max}(s)$ is encapsulated in $|\zeta(s)|$. In particular, in the case of $B = 1$ (i.e. in the case of a traveling wave) $z_{\max}(s)$ is equal to $|\zeta(s)|$.

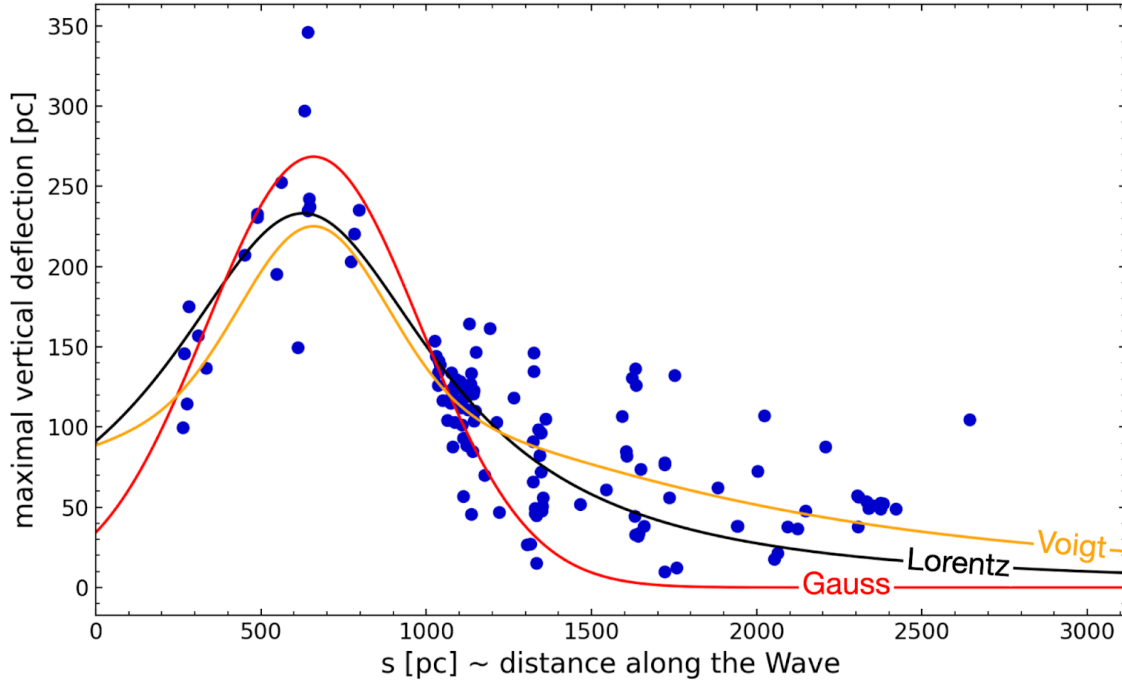


Figure R2: The maximal vertical deflection of the stellar clusters as a function of distance s in pc along the Wave is shown in blue. In addition, we show three different models: a Lorentzian in black, a Gaussian in red and a mixture between both realized by a pseudo-Voigt profile in orange. We choose the Lorentzian because it describes the data well while keeping the number of free parameters low.

The simplest functions for $\zeta(s)$ to describe the data are a Gaussian and a Lorentzian damping function (see Figure R2).

We find that a Lorentzian describes the behavior of $z_{\max}(s)$ minimally better than a Gaussian, motivating our choice of the former. However, especially around the peak region, the data favors the Gaussian model. This makes us conclude that probably a mixture model with an additional free parameter (as the case for a Voigt profile), is the true underlying damping. Since our data is too limited to constrain an extra free parameter properly, we adopt the Lorentzian damping profile. However, we emphasize that the evidence we find for a mixture model might imply that the RW originated from two distinct structures whose original damping profiles might still be imprinted into the data.

The final form of $\zeta(s)$ can therefore be described by the following equation:

$$\zeta(s) = -A \left(1 + \left(\frac{s-s_0}{\delta} \right)^2 \right)^{-1} \quad (5)$$

Note the additional minus sign compared to the full model from the original work. In the original work we dropped the sign since we could always absorb it in the definition of the time t , as our model was somewhat independent of time. Now, considering the possibility of a standing wave, the sign needs to be included.

3. Modeling the horizontal damping:

In principle $\Lambda(s)$, the horizontal damping function, is a function of the time t .

In particular, since ω is a function of the distance s along the Wave, we can define damping functions $\Lambda_{+/-}(s,t)$ as

$$\Lambda_{+,-}(s,t) = D(s) \pm (2\pi)^{-1} f(s) \omega_0 t \quad (6)$$

where we decomposed $\omega(s)$ in its zeroth order term ω_0 and higher order terms $f(s) \omega_0$.

In the equations (1) and (2), we did not use the decomposition of $\Lambda(s)$ in $\Lambda_{+/-}(s,t)$ because of the following reason.

The dependence on t results from the second term in equation (6). Using the introduced gravitational potential, we can compute the function f to show that f becomes at most ~ 0.25 for any distance s along the RW. Ultimately, we find $|\omega_0 t| \sim 0.4$ (implying that $|f(s) \omega_0 t| \sim 0.03\pi$) which allows us to neglect the dependence of time t and approximate $\Lambda(s) \approx \Lambda_+(s) \approx \Lambda_-(s) \approx D(s)$. Note that this approximation breaks down, meaning that the time dependence of $\Lambda_{+/-}$ does have an effect, as soon as we evolve our model with phase / time, implying that we cannot place an upper bound on $|\omega_0 t|$ anymore.

The effect of $\omega_0 t$ becoming significant will start to be observable above the noise level of the data after around 40 Myr. Therefore, we let the data in the interactive Supplementary Figure 5 fade out with time. We evolve the model by keeping the horizontal damping independent of time, in order to demonstrate the key kinematic feature of the Wave, i.e. that its kinematic behavior is consistent with a traveling wave. We added a caption on the side of interactive Supplementary Figure 1 to emphasize this approximation.

With that, the entire horizontal damping of the Wave as a function of s distance s along the Wave can be described by a single function $\Delta(s) + \psi$ where $\psi = \omega_0 t + \phi$.

In order to analyze the horizontal damping of the Wave, we define the phase ϕ_k of a cluster k along the Wave as follows:

$$\phi_k = \arctan \left(\frac{z_k \omega_k}{v_{z,k}} \right). \quad (7)$$

3.1 Case $B = 0.5$ (standing wave):

For now, let's imagine $B = 0.5$, as if the RW were a standing wave. From equations (1) and (2) together with equation (7) we obtain the following behavior for the phases ϕ :

$$\phi = \omega_0 t \quad (8)$$

where we see that ϕ is in this case independent of s .

Even if we included the higher order terms $f(s)$ of $\omega(s)$, we would find that $|f(s) \omega_0 t| \sim 0.03\pi$ (as discussed above). So for $B = 0.5$, the observed phases ϕ_k should vary with a spread in phase of at most $\sim 0.03\pi$.

In contrast, we find that the phases ϕ_k change over the whole range of $-\pi/2$ to $\pi/2$, such that **$B = 0.5$ (a pure standing wave) is inconsistent with the data.**

We therefore concentrate on $B \sim 1$ from this point forth.

3.2 Case B ~ 1 (traveling wave):

In the case of B ~ 1 (the traveling wave case), we can conclude from equations (1) and (2) the following behavior of the phases as a function of the distance s along the Wave:

$$\phi(s) = \omega_0 t + 2\pi \Lambda(s) + \varphi \quad (9)$$

where the function $\Lambda(s)$ encapsulates the change of $\phi(s)$ with distance s along the Wave while the other terms $\omega_0 t + \varphi = \psi$ are independent of s .

By examining the overall distribution of stellar clusters in position and velocity space (see Figure 1 in the manuscript), we can conclude that the horizontal damping function as a function of s , i.e. $\Lambda(s)$, is a monotonically increasing and convex function of the distance along the RW, s .

Since the arctan function only projects to phases between $-\pi/2$ to $\pi/2$, we need to shift each phase ϕ_k by $n_k\pi$ (with n_k being an integer for a given cluster k) such that the change of phases ϕ_k with distance s_k (where s_k is the distance along the Wave s for a cluster k) is consistent with the fact that the change in phase is a monotonically increasing and convex function of s .

The final spectrum of phases is shown in Figure R3.

While the damping model from Alves et al. 2020 captures the overall behavior of the stellar clusters, it is unable to explain the change in phase of the data in detail.

We find that $\Lambda(s)$ with

$$\Lambda(s) = \frac{s}{p - \gamma s} \quad (10)$$

describes the data better (see Figure R3).

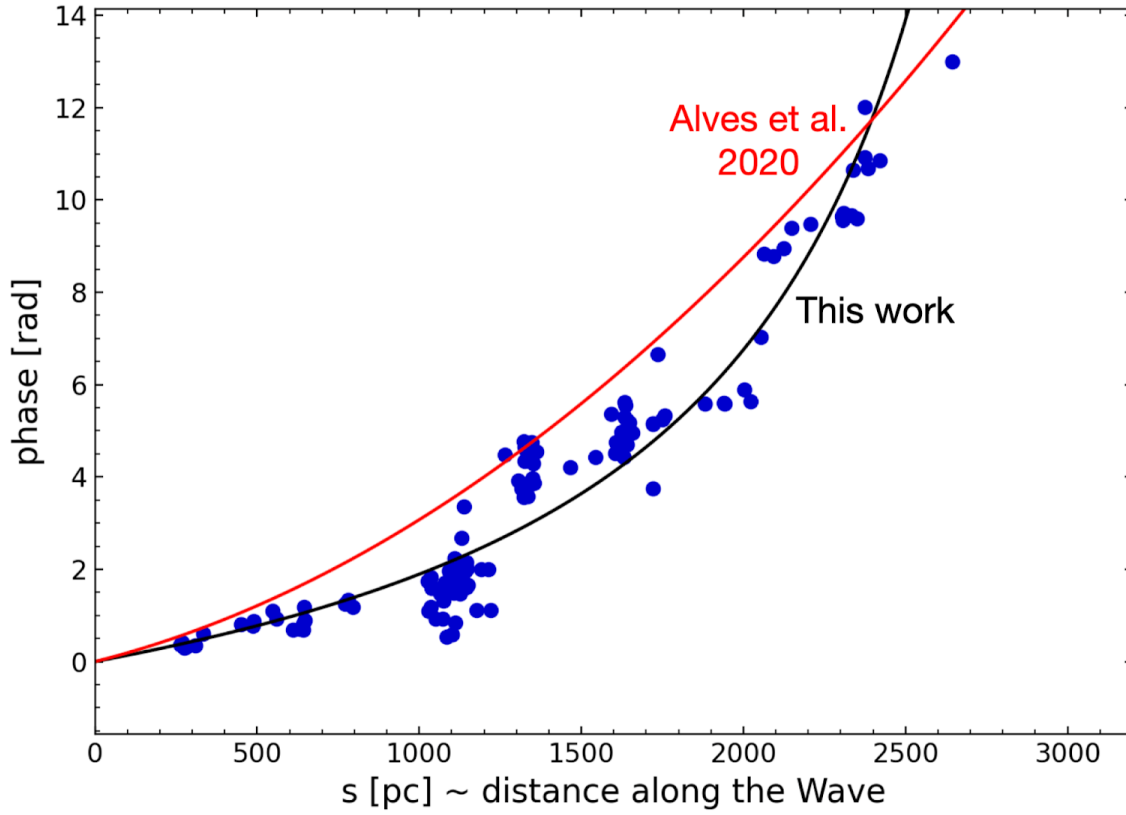


Figure R3: The phases of the stellar clusters as a function of distance s in pc along the RW are shown in blue. We also show two different models. The model from this work is shown in black. The model from Alves et al. 2020 is shown in red.

Given the new stellar cluster results, we could in principle refit the data with the parabolic model introduced in Alves et al. 2020. However, the first order period and the second order scaling factor both go to infinity, meaning that the first order term in the model vanishes and the wavelength $\lambda(s)$ – determined by equation (11) – goes to infinity for s going to 0. For the damping model introduced in this work (see equation (10)), the wavelength $\lambda(s)$ is well defined for $s = 0$ pc (see equation (12)), so we adopt equation (10) for $\Lambda(s)$ instead.

However, the distribution of the phases allows for variation in the model. With the advent of even more precise data, future work should be able to narrow down the change of phases with distance along the Wave.

4. Fitting the generalized model

Given the above derivations, we fit our full model, defined by the equations (1) and (2).

For the newly introduced parameter B , we apply a uniform prior from 0 to 1, allowing for a traveling wave, a standing wave, and intermediate cases. We adopt a uniform prior between $-\pi$ and π for the phase $\omega_0 t$.

Next, we fit our model to the stellar cluster data. We infer the values of $\Theta_1 = (x_0, y_0, x_1, y_1, x_2, y_2, A, p, \varphi, s_0, \delta, \gamma, \omega_0 t, B)$ in a Bayesian framework, sampling for our free parameters using the nested sampling code *dynesty* by adopting pairwise independent priors on each parameter (see Extended Data Table 1). We run a combination of random walk sampling with multi-ellipsoid decompositions and 1000 live points. The results of our new sampling procedure were added to Extended Data Table 2.

We find a best fit value for B of 0.84, meaning that the model still favors a traveling wave.

In addition, we redo the same fit with the same free parameters Θ_1 while fixing $B = 1$ and $\omega_0 t = 0$. By comparing the logarithm of the Bayes factor of the two realizations (B fixed = 1 and B varying between 0 and 1) using their evidences Z_1 and Z_2 , we find $\Delta \ln(Z) = \ln(Z_2) - \ln(Z_1) \approx 1$. Thus, **both models are equally good fits to the data while the realization with fixed $B = 1$ is slightly preferred.**

Keeping in mind that the traveling wave represents a clear extremum for the Wave's motion, while a standing wave forms the other, we therefore can conclude that the Wave's motion is at first order consistent with a traveling wave, implying that we can set $B = 1$ in all further performed calculations.

We added the key points of the above discussion to the Methods section.

While more physically grounded than the original Radcliffe Wave work, the manuscript has a strong empirical basis and the manuscript should be expanded on phenomena that would lead to strong variations in the oscillation wavelength over what the authors (correctly) contend is a relatively uniform region. Of note, this implies a strong variation in the phase velocity of the wave. Checking some numbers, suggests a phase velocity of 320 km/s (!) on one end of the wave and 20 km/s at the other (though the domain of the parameter s was unclear). The model proposed in the manuscript would be more compelling if it could explain these strong variations in phase velocity and propose a physical model (and associated estimates of group velocity) that explained these variations. It is challenging to develop a clear physical understanding of something that would lead to such strongly

varying wavelengths in just a couple periods without a dramatic change in the underlying potential. The authors should expand the manuscript to propose a possible physical model.

This is a very important point. We thank the referee for pointing this out. Given the change of $\Lambda(s)$ with the distance along the Wave s , we can use the following equation to compute the change in wavelength $\lambda(s)$:

$$\lambda(s)^{-1} = \left(\partial_{s'} \Lambda(s') \right) \Big|_{s'=s} \quad (11)$$

Using equation (10), this leads to the following equation for the wavelength:

$$\lambda(s) = p^{-1} (p - \gamma s)^2 \quad (12)$$

From this we can conclude that the wavelength is changing between around 400 pc and 4000 pc, leading to a phase velocity change between 5 to 40 km s⁻¹ and a mean wavelength of around 2000 pc.

This change of the phase velocity may provide clues to the origin of the Wave. Since, in addition, a two component damping function fits the vertical damping of the Wave the best, we speculate that the Radcliffe Wave may have originated from two structures. Future simulations should be able to evaluate if the interplay of two distinct physical processes could create structures that are kinematically consistent with a traveling wave. We added the derivation of the phase velocities as well as our interpretation of their variation to the Methods section and included the final values and more discussion on potential Wave origins in the main text.

Minor comments:

"Elliptical integral of the first kind" -> "Elliptic integral of the first kind"

Thanks for pointing this out. This was adopted.

In Equation 4, the notation in the manuscript suggests that angular velocity omega should be a function of z, r and not ζ, r .

Thanks for raising this question. The oscillation frequency depends on the maximal vertical deflection $z_{\max}(s)$ and the Galactocentric radius $r(s)$. In the case of a traveling wave ($B = 1$), it follows

from equation (4) that $z_{\max}(s) = |\zeta(s)|$ which is the reason why we wrote ω as a function of ζ and r . To avoid this confusion, we decided to change to $\omega(s)$ instead, since both $z_{\max}$ and r can be expressed as a function of s .

Are the weights in Equation 7 set to sum to unity? The methods by which the weights are determined is vague and should be expanded.

We agree with the referee that traditionally weights are summed to one. However, because we included the weights within the sum of the log-likelihood, the weights need to sum to the length of the underlying data set. We added two sentences clarifying that the weights sum to the length of the datasets.

The Schönrich et al. values for the peculiar solar motion are given as (11.1, 12.24, 7.25) km/s. Were these translated to a different frame before use in this calculation (e.g., LSRD->LSRK)? The values are more consistent with the older Kerr et al. (1986) values.

There was no additional translation performed. We changed the citation to *Kerr et al. (1986)*.

It would be good to confirm to the reader that the scaling of v_z by $1/\omega_0$ to place it on a common basis as the spatial terms in Equation 8 uses the variable ω_0 . In reading, I found that this might be made clearer by explicitly including this factor in Equation 8. Related, is the time derivative in Equation 8 a full time derivative or a partial? It wasn't clear, given the complexity of the model, that these were equivalent.

Thanks for pointing this out. The equation now includes ω_0 . To avoid confusion about the time derivative, we included an equation where we write out the expression for the velocity v_z .

The origin of the prefactor of 3 in front of the variance term in Equation 9 was not clear particularly when referencing Equation 7.

For the first fit, the dimension of the problem is equal to 2 (equation (7) in the original work), since we fit x, y, z with the one constraint, which is that we are determining the distance from a line. In the case of the three additional fits we fit x, y, z and v_z , such that the dimension of the problem is equal to 3 (equation (9) in the original work).

The statement that $C(x_k) = |x_k|$ for small $|x_k|$ presumably means the Euclidean norm for the vector but in a vector valued sense this might be better denoted $\|x_k\|$ since $|x_k|$ is sometimes used to denote the L1 norm.

This was adopted.

In the section on Line of Sight Gas Velocities:

"model the 6D motion" -> "model the 6D structure"

"taking advantage of the fact that the Radcliffe Wave" -> "taking advantage of the model of the Radcliffe Wave as"

We revised the sentences in line with this comment.

Referee #2 (Remarks to the Author *referee's original comments in gray and italics*):

The paper presents a kinematic model for the Radcliff wave. The RW is a feature in the local Galaxy traced out by molecular clouds and star clusters. Its properties will certainly be of interest to a wide range of astronomers and a paper that ties together spatial and kinematic data is certainly worth considering for publication. However, I have a number of concerns about this work that prevent me from recommending this paper for publication in Nature in its current form.

My main concern with the paper is that it arrives at strong conclusions based on very specific models that make simplifying assumptions. In my view, this may lead to over-interpretation of the data. I would add that the assumptions are not always explicitly stated.

Let me be more specific. As I understand, the basic assumption is that the RW is born when star clusters and molecular clouds are either displaced in z or given a kick in v_z as well as in v_R and v_{ϕ} . The individual objects along the wave then evolve kinematically -- oscillating in z and drifting in R and ϕ . Furthermore, the motion is determined solely by gravity and the z and in plane motions decouple. Do I have this right? All these assumptions should be stated explicitly. The authors comment about deceleration of the wave as it moves through the ISM so already, there is a question about the last assumption.

We thank the referee for bringing up this very important point. In the following we will motivate the assumptions made in this work in more detail below.

Decoupling between in-plane (x,y) and z motion

In our analysis we decouple the in-plane and z motion of the Radcliffe Wave. This assumption is satisfied by the fact that the maximal vertical deflection of regions along the Wave is usually less than or similar to the scale height of the Galactic disk (see Binney and Tremaine 2008).

We also examined the effect of a misalignment of the orientation of the Galactic disk we are using in this work (meaning Galactic cartesian coordinate $z = 0$) and the “true” underlying Galactic disk which in principle could be slightly tilted with respect to the Galactic cartesian coordinate frame.

To describe the possible tilt between the two planes, we introduced two additional parameters μ_1, μ_2 . In this context, μ_1, μ_2 can be seen as angular spherical coordinates which rotate the z -axis in the “theta”-direction described by μ_1 (meaning the angle between the z -axis and the x - y -plane) and “phi”-direction described by μ_2 (meaning the angle in the x - y -plane). We find a best fit angle of μ_1 of around $\sim 1^\circ$ - 4° , implying a slight tilt between the Cartesian x - y -plane and the underlying plane of the Gravitational potential. However, by comparing the two realizations (including μ_1, μ_2 as free

parameters and fixing μ_1, μ_2 to zero) we found that the two models are an equivalently good fit to the data and that the introduction of μ_1, μ_2 had no effect on our results. Therefore, we decided to remove μ_1, μ_2 from the final analysis.

Dominating forces

In this work we demonstrate that the observed spectrum of the Wave spatially and kinematically can be explained solely by coupling the position and velocity space with the Gravitational potential of the Galactic disk. In other words, we found that there is no need for an additional acting force to explain the observed spatial and kinematic pattern.

While this is expected for the stellar clusters's trajectories – as they can be described by treating each cluster as a “point” particle evolving in the Gravitational potential – the molecular cloud component might be additionally decelerated through the surrounding ISM.

This deceleration should be dominated by ram pressure from the surrounding medium, meaning

$$P \approx \rho_{\text{ISM}} \Delta v \quad (13)$$

where ρ_{ISM} is the density of the surrounding ISM and Δv is the relative velocity between a molecular cloud of the Wave and the surrounding medium, implying that $\Delta v \approx v_{cl}$, where v_{cl} is the modular cloud velocity.

This leads to a change of the cloud trajectory with starting velocity $v_{cl,0}$ which can be described by the following equation:

$$v_{cl}(t) \approx \frac{v_{cl,0}}{1 + v_{cl,0} \alpha t} \quad (14)$$

where we defined α with

$$\alpha = \frac{\rho_{\text{ISM}}}{\Sigma_{cl}} \approx c \frac{1}{r_{cl}} \frac{\rho_{\text{ISM}}}{\rho_{cl}} \quad (15)$$

where Σ_{cl} is the surface density, ρ_{cl} is the density and r_{cl} is the radius or thickness of the Wave. The constant c encapsulates the local shape of the structure in the direction of motion and is of order 1. From equation (14) it follows that the parameter α defined in equation (15) describes the strength of the deceleration as a function of the intrinsic properties of the Wave and the surrounding medium.

For the Radcliffe Wave we find that $r_{cl} \sim 50$ pc. From the 3D measurements of the molecular clouds (from Leike et al. 2020) we further find that the density ratio between the Wave and the regions below or above is $\sim 10^{-4}$.

This implies a value for α of $\sim 2 \cdot 10^{-6} \text{ pc}^{-1}$. We interpret the value of alpha as an effective declaration length around which the declaration starts to dominate. In this case we get an effective length of $\sim \alpha^{-1} \sim 500$ kpc.

In contrast, the length of one oscillation period is at most ~ 1 kpc, much smaller than the effective deceleration length. This implies that for the current oscillation of the Wave the deceleration can be neglected.

This calculation changes when we consider the drift of the Wave through the ISM. If we assume that the Radcliffe Wave emerged from sweeping up material from the surrounding ISM while drifting through it, the density contrast must have been much higher during this formation process (otherwise there would have been no material to be swept up). This leads to a density ratio of $\sim 10^{-1}$ to 10^{-2} . In addition, the Wave's thickness can be assumed to be $r_{cl} \sim 25$ pc. This leads to an effective deceleration length of ~ 250 to 2500 pc.

Assuming that the Radcliffe Wave emerged while drifting for ~ 20 Myr radially through the Milky Way with $\sim 10 \text{ km s}^{-1}$ (corresponding to ~ 200 pc), the deceleration starts to matter and needs to be taken into account.

Therefore, we argue that an exact traceback of the Wave's position since the birth of the Local Bubble would require modeling the deceleration of the Wave. However, the exact traceback modeling is beyond the scope of this work.

We expanded the Methods section to clarify the assumptions made including the key parts from the above discussion.

The core of the paper is the kinematic model, which begins with an ansatz for the potential, equation 1. The authors include an anharmonic term, citing Antoja et al. Now in Antoja et al, one is observing thousands of stars at the same R and we see the phase mixing due to the anharmonicity of the potential directly in the famous Gaia phase spirals.. Here, we have a 1-D object with essentially a single R and z as a function of s along the RW. The RW is within 250 pc so the effect of the anharmonic term is at most 4%. It looks like the radial extent of the RW is on the order of 1-2 kpc. The effect on the oscillation period is probably smaller. Also, the effective ω_0 is changing with galactocentric radius through the exponential term on the potential. This seems to be a bigger effect

than the anharmonic term. Finally, ω_0 is only ever determined by 10%. Thus, it seems like the anharmonic term will be within the noise for this study.

This is a very important point. We thank referee #2 for raising this question. Following from the Galactic potential used in this work, we derived the following expression for the vertical oscillation frequency (Note that we renamed ω_2 with μ_0 here based on the comment of referee #2 further down):

$$\omega(z_{\max}, r) = \frac{\pi}{2} \omega_0 \exp\left((r_\odot - r)(2r_0)^{-1}\right) (1 - \mu_0^2 z_{\max}^2)^{0.5} K^{-1}\left(\mu_0 z_{\max} (1 - \mu_0^2 z_{\max}^2)^{-0.5}\right) . \quad (16)$$

To get a better understanding of this equation, we now Taylor expand equation (16) in $\mu_0 z_{\max}$ and make use of the relation $\mu_0 = (\sqrt{6} z_0)^{-1}$ with the vertical scaleheight z_0 in order to rewrite ω in the following way:

$$\omega(z_{\max}, r) = \omega_0 \exp\left((r_\odot - r)(2r_0)^{-1}\right) \left(1 - \frac{3}{4} \mu_0^2 z_{\max}^2\right) = \omega_0 \exp\left((r_\odot - r)(2r_0)^{-1}\right) \left(1 - \frac{1}{8} z_0^{-2} z_{\max}^2\right) . \quad (17)$$

In addition, we can derive from equation (17) the effect of the anharmonic term on the oscillation period as:

$$T(z_{\max}, r) = 2\pi \omega_0^{-1} \exp\left((r - r_\odot)(2r_0)^{-1}\right) \left(1 + \frac{1}{8} z_0^{-2} z_{\max}^2\right) . \quad (18)$$

Anharmonic term

For both the period of oscillation and the oscillation frequency, it follows from equations (17) and (18) that the effect of the anharmonic part of the potential can be approximated by $\sim 1/8 z_0^{-2} z_{\max}^2$. The Radcliffe Wave is best described with an amplitude of ~ 220 pc.

From the measurements of the local baryonic density (McKee et al.), namely $\Sigma_0 = 47.1 \pm 3.4 \text{ M}_\odot \text{pc}^{-2}$ as well as $\rho_0 = 0.084 \pm 0.012 \text{ M}_\odot \text{pc}^{-3}$ we expect the vertical scale height to lie between 230 and 350 pc. For a vertical scaleheight varying between 230 and 350 pc we find that the effect of the anharmonic term is between 5% and 11%.

In comparison, we determine the amplitude A with a $\sim 5\%$ accuracy. Therefore, in the general wave fit where we keep the underlying potential fixed using the values for ω_0 , r_0 and μ_0 as reported in the main text (see Full Spatial and Kinematic Model), we need to take into account the effect of the anharmonic term in the potential, as it is greater than our accuracy in the amplitude A .

However, we agree with the referee that in the case where we leave the parameters describing the Galactic potential as free parameters, meaning that we fit for ω_0 , r_0 and μ_0 (which is a function of the scale height z_0), we need to compare the effect of the anharmonic term with the error in the zeroth order oscillation frequency ω_0 which is $\sim 10\%$.

This implies that for large scale heights, especially for >300 pc, the sensitivity for the vertical scale height becomes too low to be distinguishable from the noise level of this study.

Therefore, we now caution in the text that our constraint on the scale height of $z_0 \sim 294$ pc is tempered by the fact that a scale height of ~ 294 pc leads to a 7% change of the total oscillation frequency. We emphasize that for $z_0 > 300$ pc, the sensitivity of our model to z_0 would be indistinguishable from the noise level of this study.

Radial term

The change of ω with Galactocentric radius through the exponential term in the potential is as important as the anharmonic term in the equations (17) and (18). Since the Galactocentric radius varies by ~ 1200 pc, meaning $\sim +900$ pc and ~ -300 pc with respect to the Galactocentric radius of the Sun, we find that the exponential term changes the vertical frequency by up to 12%, implying that we need to consider this change in ω for all fits performed in this work.

This also implies that the Radcliffe Wave is suited to constrain the radial scale length r_0 .

As for the model, I would also suggest that the authors quote the Oort constants A and B along with v_{circ} and the midplane density for their equation 1 and assumed values for the model parameters and include references to literature values, with uncertainties. (Minor point: Since ω_2 has units of $1/\text{length}$, I suggest using a different symbol from ω . As written, one is tempted to think that ω_0 and ω_2 have the same dimensions.)

We thank the referee for asking this point. The Oort constant A_{Oort} for our adopted potential (see Irrgang 2013) is $15.1 \text{ km s}^{-1} \text{ kpc}^{-1}$. The Oort constant B_{Oort} has a value of $-13.7 \text{ km s}^{-1} \text{ kpc}^{-1}$. The circular velocity at the position of the Sun is 240 km s^{-1} . From our adopted values of $\omega_0 = 74 \text{ km s}^{-1} \text{ kpc}^{-1}$ and $\mu_0 = 1.3 \text{ kpc}^{-1}$, we infer the corresponding values for the local matter density as $\rho_0 = 0.1 \text{ M}_{\odot} \text{ pc}^{-3}$ and the local scale height as $z_0 = 314$ pc. This is consistent with literature values (McKee et al.) which are $\rho_0 = 0.097 \pm 0.013 \text{ M}_{\odot} \text{ pc}^{-3}$ as well as $z_0 = 280^{+70}_{-50} \text{ pc}$.

We added the Oort constants to the Methods section. We also added our assumed values for the model parameters, including references to literature values, with uncertainties. We renamed ω_2 with μ_0 in order to avoid confusion with the dimensions.

I found Sup Fig 1 a bit difficult to interpret even in interactive form. How about a figure that shows z and v_z as a function of s along the wave. To my eye, the z and v_z data have clear systematic differences with the data, but the 3D plots can lead to unwanted projection effects.

This is a very important point. Here, we clarify our definitions of distance and position along the Wave.

With position along the Wave we refer to the x-axis of a coordinate frame in which the Galactic coordinate x-y-frame has been rotated anticlockwise by 62° . This is illustrated by the purple line in Figure R4. However, due to the non zero curvature of the Radcliffe Wave we can also define the so-called distance along the Wave which is defined to be zero at the left side of the Wave (as observed from the Sun) and to increase the further we go to the right of the Wave. The distance along the Wave is shown in black in Figure R4.

To get a sense of the effect of the curvature of the Wave on the “side view” of the Radcliffe Wave, we now redo Figure 1 from the work in both coordinate frames (see Figure R5 and Figure R6). We find no significant difference between the two realizations, meaning that the curvature does not matter when plotting the Wave from the side. Therefore, we decided to keep the version showing the 3D plot in the main text.

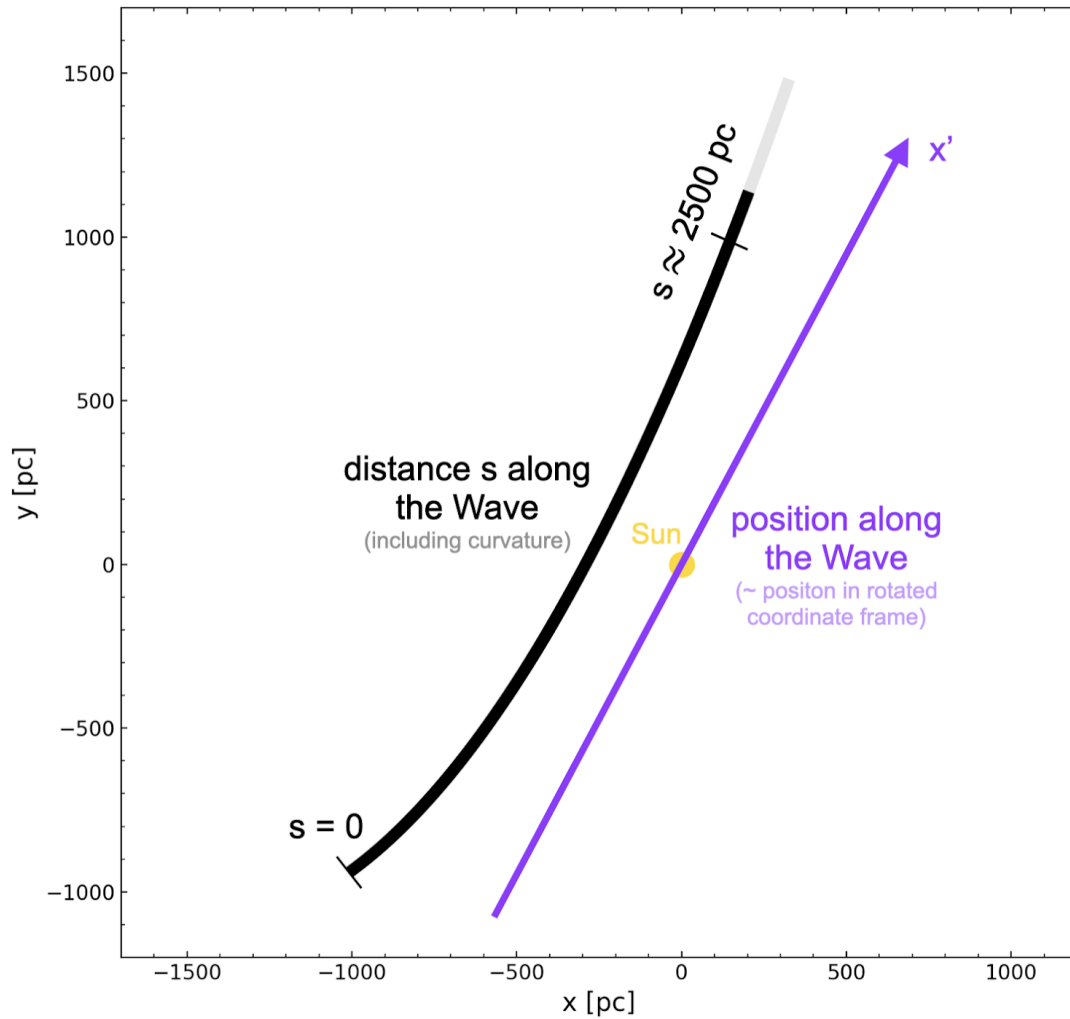


Figure R4: Difference between the distance and the position along the Wave. With the black line we show the best fit of the Radcliffe Wave projected to the x - y -plane in Galactic coordinates. The distance s along the Wave follows this projected line from the left (viewed from the Sun) to the right where $s = 0$ is defined to be on the left side of the Wave. The position along the Wave corresponds to the x -axis of a coordinate frame in which the Galactic coordinate xy -frame has been rotated anticlockwise by 62° . We show this axis in purple.

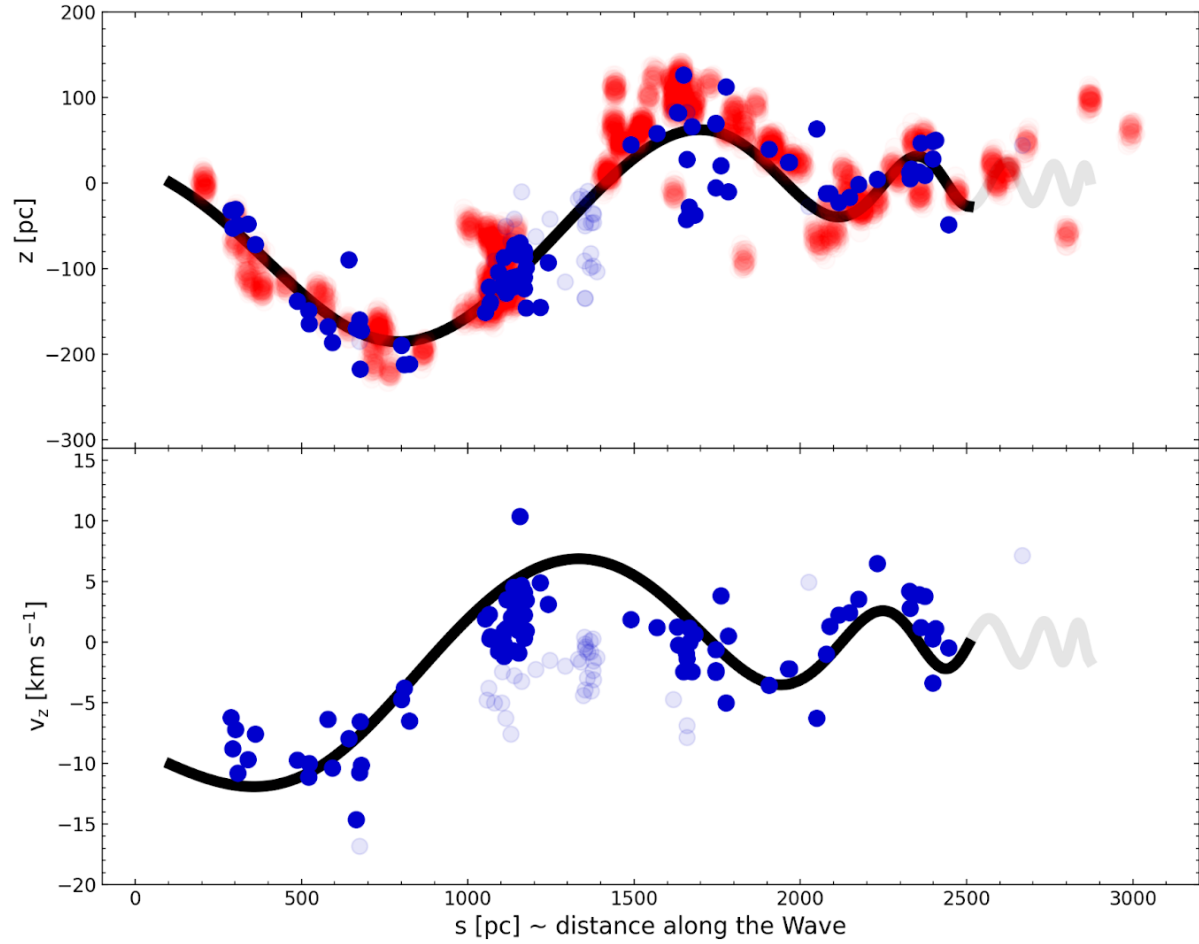


Figure R5: Figure 1 from the main text redon using the distance s along the Wave as the x-axis. The stellar clusters are shown in blue, the molecular clouds in red as well as the best fit in black.

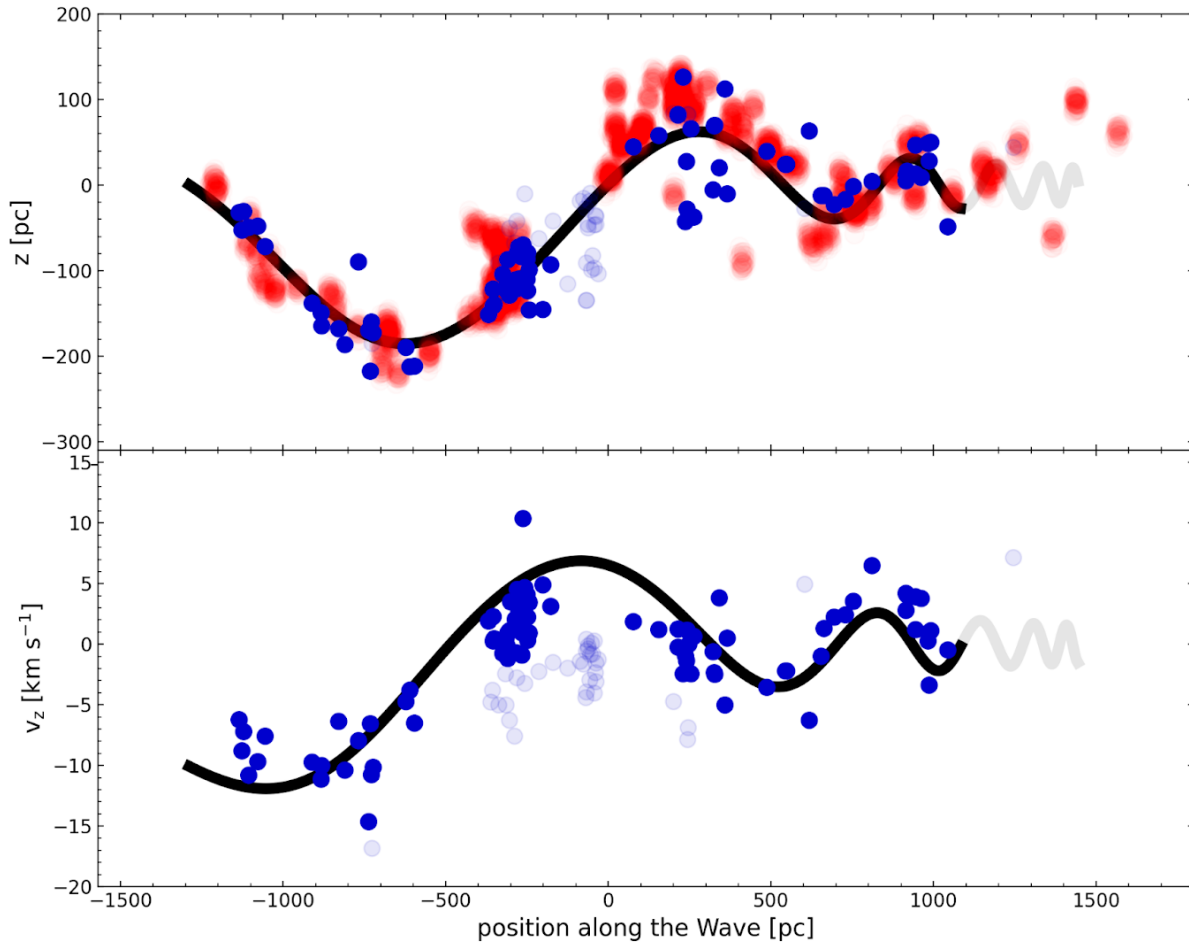


Figure R6: Figure 1 from the main text redone using the position along the Wave as the x-axis. The stellar clusters are shown in blue, the molecular clouds in red as well as the best fit in black.

Each point along the RW acts as an oscillator about the midplane. So, the model should have two terms --- a right moving wave and a left moving wave. The question of standing vs travelling wave then comes down to the relation between the two...travelling and one of the two terms is zero; standing, and they are the same or have opposite signs. It would be good to fit the two terms and then let the data determine the admixture of the two solutions. Then, a standing wave would have $A = -B$ and travelling waves either $A=0$ or $B=0$. I would suggest that the authors use their data to constrain A and B .

We thank the referee for bringing up this very important point, which was also asked by referee #1. We refer here to our answer we gave in the discussion of the comments of referee #1.

It is worth stating that they are treating the RW as purely kinematic structure and the gravitational potential of the galaxy stationary. There's been quite a bit of research lately on vertical oscillations of the disc itself. Xu et al and Schonrich and Dehnen found evidence for vertical waves near the Sun. There's the work by Antoja et al, Widrow 2012, the Gaia DR2 and DR3 kinematics papers and many more that discuss vertical oscillations of the disc itself. How would a changing vertical potential affect the authors conclusions?

We thank the referee for bringing up this very important point. We expanded the Methods section to clarify the assumptions made regarding the potential including the key parts from the below discussion.

Stationary potential

We indeed treat the underlying Gravitational potential as stationary, which is an appropriate assumption to describe the Galactic potential based on the literature (Binney and Tremaine 2008). This choice is furthermore motivated by the distribution of the old stellar component around the Radcliffe Wave, which is expected to dominate the local Galactic potential (Binney and Tremaine 2008). In the old stellar component, there is no evidence (see Tu et al. 2022, Thulasidharan et al. 2022) for perturbations with wavelength < 10 kpc. In the vicinity of the Radcliffe Wave, a perturbation of the potential with wavelength > 10 kpc would correspond to a local tilt in the potential. The Galactic warp also constitutes a large-scale change in the potential. However, we already examined the inclusion of a tilt in our motivation of the decoupling between the motion along the x-y plane and the z-direction and found that including two extra angle parameters has no effect on our results. In addition, perturbations with wavelength > 10 kpc are expected to change on timescales larger than the vertical oscillation period of the Radcliffe Wave (Poggio et al. 2020).

Radially evolving vertical waves

We thank the referee for pointing us to the recently discovered vertical waves near the Sun (Xu et al. 2015 and Schönrich and Dehnen 2018) with wavelengths < 10 kpc. However, these waves appear radially (pitch angles $\sim 90^\circ$), while the Radcliffe Wave evolves predominantly tangentially (pitch angle $\sim 0^\circ$). Therefore, due to their radial nature, these waves should not have a significant effect on the Radcliffe Wave. Even in the case of a wavelength ~ 2 kpc (Schönrich and Dehnen) where the radial wave might become detectable due to the non zero pitch angle of the Radcliffe Wave, the amplitude of these waves would be only $\sim 0.3 \text{ km s}^{-1}$ between ~ 900 pc and ~ 300 pc with respect to the Galactocentric radius of the Sun (Schönrich and Dehnen 2018) which is within the noise of our study.

Vertical density perturbations

Antoja et al. and Widrow 2012 find vertical (z-direction) density perturbations in the stellar component near the Sun. Particularly in the case of Widrow 2012, the vertical wavelength of these vertical perturbations (~ 2 kpc) is large compared to our vertical region of interest, where $|z| < 300$ pc. In addition, in our region of interest the amplitude of the vertical density perturbations is $\sim 5\%$ (Widrow 2012), implying a change of the vertical oscillation frequency of $\sim 2.5\%$ which is within the noise level of our study.

Effect of underlying potential on the phase velocity

Since we find a change in phase velocity along the Wave of 5 to 40 km s^{-1} we could ask if a change in the Galactic potential could explain this change in the phase velocity. From the definition of the phase velocity and the vertical oscillation frequency defined in equation (16), we find that the local matter density ρ_0 would need to change by at least two orders of magnitude which is in disagreement to any observations or models of the Milky Way (see Binney and Tremaine 2008, McKee et al. 2015).

Finally, I'm puzzled by the focus on the dark disc and dinosaur extinction. These ideas are on the fringe of Galactic dynamics and have already been widely discounted. For example, the authors can translate their results into a constraint on the total density in the midplane by using the potential and Poissons equation. This could then be compared with previous determinations such as the one in the McKee paper that they cite or many others. Dressing it up in a discussion of the dark disc hypothesis and dinosaur extinctions seems gratuitous.

We decided to remove the entire discussion about the dinosaurs from the text. As suggested we translate our results into a constraint on the total density in the midplane, the vertical scale height as well as the solar oscillation period by using the potential and Poisson's equation. We then also compare this constraint with previous determinations from the literature.

Referee #3 (Remarks to the Author *referee's original comments in gray and italics*):

A.1 Summary of the key results

The authors reconstruct the Ratcliffe wave in space and kinematics and show coherence of motion across the structure. They propose that the Sun lies on that structure and measure a period of vertical oscillation for the Sun based on a fit to the Ratcliffe wave. They use their obtained value to argue that it is higher than the value needed for a dark disc (with properties that have already been ruled out two years ago thanks to Gaia data) to kill the dinosaurs. However, this is not an outstanding result given that prior work already came up to similar conclusions (details below).

B. Originality and significance: if not novel, please include reference

The results/conclusions are not original or significant (details below).

It is nice to see that the authors have been able to map the Ratcliffe Wave in 6-D phase-space. However, the significance of the results is not major, but rather incremental since its discovery. We also know that coherent structures in the Milky Way are expected to exhibit 90 degree offsets between kinematics and space, that can be naturally appreciated from the epicyclic approximation (see Binney & Tremaine 2008). That said, it is beautiful to see things come full circle and I congratulate the authors for it.

Figure 2 which is the main figure of the paper, looks quite dramatic, but the most stringent limits on a thin dark disc were already set two years ago by Widmark et al. (2021), with an upper limit of 1.96 Msun/pc^2 at 68% confidence level for a thin dark disc with scale height of 25pc. The Widmark+21 result is consistent with there being no dark disc since for comets to destroy dinosaurs would require a dark disc with a surface density above 10 Msun/pc^2 !

Given the result from Widmark+21, this makes the main and only science plot/result of this paper rather anecdotal and a mere corroboration of Widmark et al.'s work (<https://ui.adsabs.harvard.edu/abs/2021A%26A...653A..86W/abstract>).

Because this paper solely rests on one Figure/result which is not novel and a bit outdated now, I cannot recommend this paper to Nature as it is not an original or novel result... However it may do well in another type of journal.

On a more subjective note, I do not think this result will be of immediate interest to people in my field (Galaxy Formation/Galactic Dynamics/Galactic Archaeology) or to paleontologists...

C. Data & methodology: validity of approach, quality of data, quality of presentation

The approach is fine. The Galactic dynamical modelling is somewhat simplistic, but I understand that the authors wanted to make "order of magnitude" estimates...

D. Appropriate use of statistics and treatment of uncertainties

The methods are fine-ish, but they do not discuss how they determine basic quantities such as distances from parallaxes for example (or deal with negative parallaxes), there are a number of things that are just under the hood... Also why did the authors opt to leave out their error bars on any of the different extended figures (except the CO velocities figure)?

There are systematics which they do not have any control over that they do not discuss (e.g. uncertainty in the baryonic model for the Galaxy...), would have been nicer to see a more thorough discussion here.

E. Conclusions: robustness, validity, reliability

Reasonable and fine-ish with speculative bits which the authors acknowledge are beyond the scope of their paper.

F. Suggested improvements: experiments, data for possible revision

Show your error bars and give a more thorough discussion of the methods... I would recommend a better review of the prior literature and possible extended discussion w.r.t. past results.

G. References: appropriate credit to previous work?

Should have cited Widmark et al. 2021.

H. Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

The paper is well written in general, though Figure 2 is a bit over the top since a lot of the parameter space has already been ruled out thanks to Gaia. Perhaps in a future submission they could discuss the result and show the complementarity between the two studies, that could be useful/interesting.

I am sorry I cannot recommend this paper, but I am sure that with a few little revisions, it will do quite well in any of the main astrophysical journals (MNRAS, ApJ, A&A)!

We have already addressed referee #3's comments in our response to referees #1 and #2 and in our executive summary. However, we added a reference to Widmark et al. 2021 in the text.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

Thank you again for the opportunity to review the revised version of Nature manuscript 2023-06-10928A. I appreciate the care with which the manuscript author team of Konietzka et al. responded to my original review of their submission. Overall, I find that their responses are well laid out and address several of the concerns that were raised in the original report. In consider these responses and the responses to the other referees, I think the manuscript would be suitable with publication after a minor revision, mostly surrounding the presentation of the results.

In particular, the manuscript notes that the Radcliffe wave is "oscillating traveling wave driven by the gravitational acceleration of the Galactic potential." (abstract). This remains challenging to accept as a complete explanation for the wave motion given the order-of-magnitude variation in wavelength / phase velocity of the wave across the extent of the wave. The authors carefully work through a set of possible explanations as to why the changes seen along the wave cannot be explained by the Galactic potential alone, ultimately attributing the peculiar structure of the wave to arise since "the Radcliffe Wave may have originated from the interplay of two different physical processes." Hence, it seems that vertical oscillation is broadly consistent with the gravitational potential, but it requires a conspiracy of physical effects to establish the spatial variations along the wave. While understanding that nuance is usually deferred to the Supplementary sections of the article, it would be good to subtly alter the abstract / forward presentation of the article. The authors may be able to inject this subtlety by downplaying the traveling nature of the wave at the abstract level "... oscillating wave driven by the gravitational acceleration." If the authors would like to retain the forward presentation of the traveling nature of the wave, they should also include a discussion of the damping terms at the abstract level and the lack of a satisfying explanation.

Sincerely,
Erik Rosolowsky

Referee #2 (Remarks to the Author):

I commend the authors on addressing the comments and suggestions by me and the other referees. In my view, the paper is now acceptable for publication in Nature Astronomy.

Author Rebuttals to First Revision:

Response to Referee Comments on “The Radcliffe Wave is Oscillating”

We thank the referees for reviewing our manuscript a second time. We reply to any remaining issues raised below.

Referee #1 (Remarks to the Author, referee’s original comments in gray and italics):

Thank you again for the opportunity to review the revised version of Nature manuscript 2023-06-10928A. I appreciate the care with which the manuscript author team of Konietzka et al. responded to my original review of their submission. Overall, I find that their responses are well laid out and address several of the concerns that were raised in the original report. In consider these responses and the responses to the other referees, I think the manuscript would be suitable with publication after a minor revision, mostly surrounding the presentation of the results.

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*Sincerely,
Erik Rosolowsky*

Following the recommendation from referee #1, we have removed the word “traveling” from the abstract in order to alter the forward presentation of the article.

Referee #2 (Remarks to the Author, *referee's original comments in gray and italics*):

I commend the authors on addressing the comments and suggestions by me and the other referees. In my view, the paper is now acceptable for publication in Nature Astronomy.