Peer Review File

Manuscript Title: Spherical symmetry in the kilonova AT2017gfo/GW170817

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

This manuscript presents an analysis of the geometry of the "kilonova" AT2017gfo, based on its early optical spectroscopic features and early luminosity. The authors argue that - contrary to earlier published claims - the source is spherical to a very precise (few percent) degree. They substantiate this statement with 2 independent methods: one compares the photospheric velocity components perpendicular and tangential to the line of sight, and the other examines the Sr II absorption line profile. Assuming spherical assumption, the authors then derive a precise distance to the source, and speculate on mechanisms that can make the explosion highly spherical. The manuscript is clear, well written and cites all necessary references.

This paper contains numerous pieces of valuable technical analysis of AT2017gfo spectral properties and hints at fundamental considerations and problems. However, I have some reservations against the approach and the results. Assuming supernova explosion as a reasonable analogue of kilonova, a rather isotropic degree in the outer ejecta of the AT2017gfo explosion is not unexpected, as the outer layers are - similar to supernova - less affected by the inner anisotropies. In supernova, the central asymmetries are customarily estimated via late-epoch spectroscopy, that probes the central regions of the explosion, as attested by a wide range of published studies of supernova nebular spectra observations and models. The best diagnostic of AT2017gfo isotropy is therefore not its early photospheric phase, but rather its nebular (unfortunately unobserved) phase.

This said, I believe the uncertainty associated with the asymmetry parameter (page 2, line 35) is unrealistically small. The authors do not report explicitly the errors on the individual velocity components, however, the error affecting the radial velocity component, estimated from the Sr II line blueshift, cannot be less than 20%, considering the many assumptions that enter this estimate: the line identification is still not 100% secure (as admitted by the authors in their Methods section and as stated in the original Watson et al. Nature paper, ref. 8); a description of the ejecta structure and a model for the evolution of the photospheric speed are not available. This comment affects also the asymmetry estimate from the absorption line profile, which, in addition, disregards the possible blending with other atomic species. The uncertainty of the tangential velocity component, estimated via the EPM method, is of the same order of magnitude (~20%), considering the assumptions, see e.g. line 370 of the Methods.

With individual uncertainties of ~20%, the propagated error on the asymmetry parameter is substantially larger than 0.02, and its power of symmetry diagnostic is thus significantly reduced, which weakens any speculation about scenarios responsible for high spherical symmetry. As a consequence of this serious underestimate of uncertainties, I do not think this paper meets the requisites for publication in Nature.

Referee #2 (Remarks to the Author):

This paper, "The Kilonova AT2017gfo Was Spherically Symmetric" presents a novel analysis and interpretation of very early optical/near-infrared flux spectra obtained for this still-unique gravitational-wave event with an electromagnetic counterpart. In short, as the title suggests, the authors contend that there is evidence to support that both the electron-scattering photosphere and elemental distribution of strontium was spherically symmetric. If true, this would be a very significant finding, as sphericity is not a generic outcome of many (any?) merger models of the coalescing neutron stars that are generally agreed to have been the progenitor of the event, and so would necessitate a rethink of the theoretical models to fit this constraint.

I find this paper to be, on the whole, compelling, as the essential idea behind the sphericity claim is straightforward: The Baade-Wesselink method ("EPM") of distance determination, in its simplest form, appears to work for the object. For this to be so, the authors contend that it demands that the electron-scattering photosphere that presents to the observer along the line-of-sight (R_parallel) must be at the same radius as the one that presents orthogonally to it -- i.e., in the plane of the sky (R_perpendicular). If these two radii are equal, it suggests sphericity of the electron-scattering photosphere. A further claim is made that the specific observed absorption-line profile of the sole spectroscopic feature available at very early times (attributed to Sr+) demands sphericity of the strontium distribution in the ejecta. Coupled together, these two separate findings appear to make a strong case for overall sphericity of the ejecta.

I do have concerns, though, that a skeptical reader may seek to dismiss the finding due to a couple implicit -- but unremarked upon – fundamental assumptions that are being made in the (simple) application of the EPM being employed here. I believe it would serve to forestall such criticism and strengthen the overall paper for the authors to directly address/comment on the following assumptions, both in the text of the paper, and in detailed response to this report:

1) The assumption that the photospheric radius (i.e., where photons are finally released) is precisely equal to the radius at which the blackbody is being produced (i.e., at the "thermalization depth"): I.e., that R_phot / R_therm = 1.000 . Put another way: Are the authors absolutely certain that the location in the atmosphere where the total inward integrated radial optical depth equals tau=2/3 is the *same* location in the outflow where photons are thermalized and the blackbody is itself being released? This assumption is fundamental: When deriving v_parallel, one uses the P Cygni absorption minimum to locate R_phot (see more on this assumption in concern [2], below); when deriving v_perpendicular, one employs R_therm. If these two quantities are not fundamentally pinpointing the same location in the atmosphere, then the argument that EPM working (where "working" == gives the correct distance) necessarily demonstrates sphericity does not hold.

By way of background, I remind that for the class of objects for which EPM has historically been most applied -- Type II-Plateau supernovae -- it is decidedly *not* the case that R_phot / R_therm = 1.000 at early times. In these objects, as demonstrated initially by Eastman et al. (1996, ApJ, 466, 911) and later by Dessart et al. (2005, A&A 439, 671), R_therm < R_phot because the electron-scattering opacity contributes significantly to the total opacity at the photosphere, especially at early times. As written in the latter paper: "Although this gray-opacity preserves the relative wavelength distribution of the Planck functon(i.e., the black-body Planck function is preserved), set at R_therm, it introduces a global 'flux dilution'." Further, in a follow-up study on stripped-envelope SNe (Dessart et al. 2015, MNRAS, 453, 2189), it was found also that SNe that lack hydrogen (perhaps somewhat more comparable to what is being tested here with a kilonova) also produce a "dilute blackbody" as well, requiring flux-dilution factors to "correct" naively derived EPM distances.

So the basic question becomes: Are there compelling theoretical reasons to believe such "correction" (or, "dilution") factors are uniquely unnecessary for a NS-NS merger (kilonova) -- i.e., that R_therm necessarily equals R_phot -- when this has not been the case for all other classes of transients for which EPM has been studied in detail?

2) That the velocity measured by the Sr+ line is, in fact, a perfect indicator of the photospheric velocity. Again, studies including the ones cited above suggest that great care must be taken with this assumption, as a particular line feature in a transient's spectrum may vastly over (or, even, under) estimate the actual velocity of the photosphere due to optical depth effects. For instance, in SNe II-P it is well known that the H-alpha line can overestimate photospheric velocity by up to 40% at early times.

Put simply: Are there compelling theoretical reasons to believe that the Sr+ line in a NS-NS merger is an accurate indicator of photospheric velocity?

To be sure, Occam's razor would suggest that since a spherical photosphere is the *simplest* explanation for why the basic EPM application works so well in this case (which it clearly does), it should obtain. But directly and forthrightly addressing the validity of both of the above assumptions will assist in turning a compelling argument for sphericity into a convincing one, and in allaying fears that a confluence of multiple false assumptions -- coupled with a non-spherical photosphere -- have conspired to produce the result. Should the authors capably provide such assurance, I would recommend consideration by Nature for publication of this very interesting -- and, even, clever -- finding.

David Branch (Remarks to the Editor):

An interesting paper. The identification of Sr II in the early spectra has been pretty convincingly established in previous papers. In supernovae, inferring the degree of asymmetry from P Cygni profiles is difficult because of the degeneracy with the orientation relative to the line of sight, but here they have independent constraints on the inclination angle, so they can measure asymmetry. I have no reason to doubt that the inclusion of asymmetry in the reference-8 work of Ulrich Noebauer (an expert in line formation in moving atmospheres, as is coauthor of this paper Stuart Sim) is done correctly, so no reason to think that their profile-fitting technique isn't, in principle, a valid indicator of sphericity.

The epochs of the five spectra should be shown in Fig. ED 1.

As you may recall, my referee reports were "never anonymous". If you want to share any of this with the referees or the authors, I have no objection.

David Branch

Author Rebuttals to Initial Comments:

We would like to thank the referees for their comments and questions. Addressing these questions has emphasised key assumptions that are important to clarify for the reader, forestalling potential misconceptions and improving the argumentation on the robustness of the results.

We provide below a point-by-point response to each issue raised.

Referee #1 (Remarks to the Author):

This manuscript presents an analysis of the geometry of the "kilonova" AT2017gfo, based on its early optical spectroscopic features and early luminosity. The authors argue that - contrary to earlier published claims - the source is spherical to a very precise (few percent) degree. They substantiate this statement with 2 independent methods: one compares the photospheric velocity components perpendicular and tangential to the line of sight, and the other examines the Sr II absorption line profile. Assuming spherical assumption, the authors then derive a precise distance to the source, and speculate on mechanisms that can make the explosion highly spherical. The manuscript is clear, well written and cites all necessary references.

This paper contains numerous pieces of valuable technical analysis of AT2017gfo spectral properties and hints at fundamental considerations and problems. However, I have some reservations against the approach and the results. Assuming supernova explosion as a reasonable analogue of kilonova, a rather isotropic degree in the outer ejecta of the AT2017gfo explosion is not unexpected, as the outer layers are - similar to supernova - less affected by the inner anisotropies.In supernova, the central asymmetries are customarily estimated via late-epoch spectr oscopy, that probes the central regions of the explosion, as attested by a wide range of published studies of supernova nebular spectra observations and models. The best diagnostic of AT2017gfo isotropy is therefore not its early photospheric phase, but rather its nebular (unfortunately unobserved) phase.

We agree; that we are here probing the outer layers of the kilonova. However, hydrodynamical models of kilonovae predict non-spherical photospheres in the outer layers (and are actually typically more anisotropic in the outer layers, where dynamical ejecta processes dominate). We explicitly point this out and comment on how it affects the interpretation of the kilonova in the methods section "The expanding photospheres method".

This said, I believe the uncertainty associated with the asymmetry parameter (page 2, line 35) is unrealistically small. The authors do not report explicitly the errors on the individual velocity *components, however, the error affecting the radial velocity component, estimated from the Sr II line blueshift, cannot be less than 20%, considering the many assumptions that enter this estimate: the line identification is still not 100% secure (as admitted by the authors in their Methods section and as stated in the original Watson et al. Nature paper, ref. 8); a description of the ejecta structure and a model for the evolution of the photospheric speed are not available. This comment affects also the asymmetry estimate from the absorption line profile, which, in addition, disregards the possible blending with other atomic species. The uncertainty of the tangential velocity component, estimated via the EPM method, is of the same order of magnitude (~20%), considering the assumptions, see e.g. line 370 of the Methods.*

With individual uncertainties of ~20%, the propagated error on the asymmetry parameter is substantially larger than 0.02, and its power of symmetry diagnostic is thus significantly reduced, which weakens any speculation about scenarios responsible for high spherical symmetry. As a consequence of this serious underestimate of uncertainties, I do not think this paper meets the requisites for publication in Nature."

We have taken into account the concerns raised here and used them to improve the presentation of the robustness of our constraints. These improvements and their arguments are laid out below.

Overall:

A 20% uncertainty in v_phot is incompatible with the data. This would correspond to a constraint on the absorption centre with a 500–600Å uncertainty for epochs 1 and 2. The line is not saturated, so this is definitely ruled out. We have revised *Fig. 1* to emphasise this point. A point to note here is the much greater velocity of the KN compared to typical SNe, so that a 2000km/s uncertainty is only a ~2% velocity uncertainty in a KN, but ~25% for a SN.

Regarding the uncertainty in the tangential velocity from EPM: while for *supernovae* this is, as the referee states, often of order 20% due primarily to uncertainty in estimating the dilution factors, this is fundamentally different for kilonovae. The electron scattering dilution is expected to be negligible in kilonovae because the total opacity is much higher and dominated by bound-bound opacity and because the number of free electrons per unit mass is lower by two orders of magnitude. We now address this explicitly in the main text and in the methods section *"Robustness of the spectral modelling and epoch selection"*. For this reason, we are confident of our estimate of the tangential velocity uncertainties.

Our statistical uncertainties are consistently below 1% for the velocity and normalisation. These are, as the referee infers (and as we now state explicitly in the text), much less than the main systematics: flux calibration, extinction, and peculiar velocity. We discuss the systematic uncertainties further, below.

It is perhaps worth pointing out several differences between kilonovae and supernovae which make the EPM uncertainties much smaller for kilonovae. First, as mentioned above, the dominance of electron scattering in SNe requires large dilution factors that need to be estimated. Second, separate epochs are not independent for SNe because the initial explosion time is found by combining multiple epochs. In KNe, the merger time is known precisely. In conjunction, these effects make the EPM uncertainties much larger for SNe.

Line identification:

We have already tested the systematic uncertainty associated with a misidentification by allowing a line at arbitrary wavelength to fit the data instead of SrII (see Methods "PCygni modelling"). Note that this fit still supports spherical expansion. Additionally, it is worth emphasising, that the one-line fit is worse than the SrII line fit despite having an extra free parameter.

Line blending:

We have now added a section on the line blending uncertainty in the Methods section that addresses the arguments below. A full radiative transfer model with a complete atomic data set for the relevant elements would be the ideal case. However, the spectra do not require additional lines - and while one can increase the complexity with a multitude of lines, this is unlikely to be a major concern for several reasons. First, a blend of lines conspiring to make an aspherical geometry look highly spherical is not probable. Second, it would require that the blending lines matched the SrII lines in strength over time, despite major changes in density and temperature, (as well as possible composition) that we are observing at each epoch. In addition, since we get the similar distance/symmetry for each epoch, this hypothesis requires these lines to mimic a SrII P Cygni profile, *coherently* shifting in wavelength and receding deeper within the ejecta at *exactly* the rate predicted by the blackbody continuum.

Were we to accept the unlikely coincidences required for line blending for arguments' sake, to address the magnitude of the possible systematic error in such a case, we mapped the change in the inferred asymmetry that a P Cygni line of freely varying tau would induce as a function of the blended line's wavelength. This reaches a maximum at about 9900Å and shifts the asymmetry index by at most 0.02 and 0.03 respectively for epochs 1 and 2, which is within our margin of error. Adding additional blending lines, while obviously increasing the uncertainty due to additional free

parameters does not increase the asymmetry shift by more than this. However, as stated above, line blending affecting the spectrum in such a specific, tuned way is not likely.

Overall, we make a general remark on the validation of our derived uncertainty from the small spread in the inferred distance (or in asphericity) for the full model across epochs 1–5. While we are using the same model, the physical conditions at each epoch change substantially, so that potential systematic effects such as line blending, continuum fluxes, and dilution should not necessarily all act in the same way. Each epoch is statistically independent because the merger time is provided by the gravitational wave signal. The standard deviation in the central values of asymmetry index for the full model is $\sigma_{\Upsilon} = 0.02$ **, within our errors for epochs 1 and 2. This is despite the fact that a large fraction of this variation is set by the unmodelled and poorly-understood spectral features emerging at late times. This is remarkably consistent and reinforces the robustness of the results.**

"Referee #2 (Remarks to the Author):

This paper, "The Kilonova AT2017gfo Was Spherically Symmetric" presents a novel analysis and interpretation of very early optical/near-infrared flux spectra obtained for this still-unique gravitational-wave event with an electromagnetic counterpart. In short, as the title suggests, the authors contend that there is evidence to support that both the electron-scattering photosphere and elemental distribution of strontium was spherically symmetric. If true, this would be a very significant finding, as sphericity is not a generic outcome of many (any?) merger models of the coalescing neutron stars that are generally agreed to have been the progenitor of the event, and so would necessitate a rethink of the theoretical models to fit this constraint.

I find this paper to be, on the whole, compelling, as the essential idea behind the sphericity claim is straightforward: The Baade-Wesselink method ("EPM") of distance determination, in its simplest form, appears to work for the object. For this to be so, the authors contend that it demands that the electron-scattering photosphere that presents to the observer along the line-of-sight (R_parallel) must be at the same radius as the one that presents orthogonally to it -- i.e., in the plane of the sky (R_perpendicular). If these two radii are equal, it suggests sphericity of the electron-scattering photosphere. A further claim is made that the specific observed absorption-line profile of the sole spectroscopic feature available at very early times (attributed to Sr+) demands sphericity of the strontium distribution in the ejecta. Coupled together, these two separate findings appear to make a strong case for overall sphericity of the ejecta.

I do have concerns, though, that a skeptical reader may seek to dismiss the finding due to a couple implicit -- but unremarked upon -- fundamental assumptions that are being made in the (simple) application of the EPM being employed here. I believe it would serve to forestall such criticism and strengthen the overall paper for the authors to directly address/comment on the following assumptions, both in the text of the paper, and in detailed response to this report:

1) The assumption that the photospheric radius (i.e., where photons are finally released) is precisely equal to the radius at which the blackbody is being produced (i.e., at the "thermalization depth"): I.e., that R_phot / R_therm = 1.000 . Put another way: Are the authors absolutely certain that the location in the atmosphere where the total inward integrated radial optical depth equals tau=2/3 is the *same* location in the outflow where photons are thermalized and the blackbody is itself being released? This assumption is fundamental: When deriving v_parallel, one uses the P Cygni absorption minimum to locate R_phot (see more on this assumption in concern [2], below); when deriving v_perpendicular, one employs R_therm. If these two quantities are not fundamentally pinpointing the same location in the atmosphere, then the argument that EPM working (where "working" == gives the correct distance) necessarily demonstrates sphericity does not hold.

By way of background, I remind that for the class of objects for which EPM has historically been most applied -- Type II-Plateau supernovae -- it is decidedly *not* the case that R_phot / R_therm = 1.000 at early times. In these objects, as demonstrated initially by Eastman et al. (1996, ApJ, 466, 911) and later by Dessart et al. (2005, A&A 439, 671), R_therm < R_phot because the electron-scattering opacity contributes significantly to the total opacity at the photosphere, especially at early times. As written in the latter paper: "Although this gray-opacity preserves the relative wavelength distribution of the Planck functon(i.e., the black-body Planck function is preserved), set at R_therm, it introduces a global 'flux dilution'." Further, in a follow-up study on stripped-envelope SNe (Dessart et al. 2015, MNRAS, 453, 2189), it was found also that SNe that lack hydrogen (perhaps somewhat more comparable to what is being tested here with a kilonova) also produce a "dilute blackbody" as well, requiring flux-dilution factors to "correct" naively derived EPM distances.

So the basic question becomes: Are there compelling theoretical reasons to believe such "correction" (or, "dilution") factors are uniquely unnecessary for a NS-NS merger (kilonova) -- i.e., that R_therm *necessarily equals R_phot – when this has not been the case for all other classes of transients for which EPM has been studied in detail?*

Yes, indeed there are important theoretical reasons to believe dilution is far less prevalent in kilonovae than when comparing with supernovae. In short, in kilonovae the total opacity is dominated by bound-bound transitions from the ions rather than electron scattering because of the much greater line opacity of the r-process elements, and because the number of free electrons per unit mass is lower by two orders of magnitude relative to ionized hydrogen. The ratio of electron scattering opacity to total opacity is therefore 3-4 orders of magnitude lower in kilonovae than in SNe. Overall this means that the relative electron scattering opacity contribution is negligible, so R_{phot} **is a good measure of** R_{therm}**.**

We have added this argument to the main text and to the Methods; "Robustness of the spectral modelling and epoch selection".

2) That the velocity measured by the Sr+ line is, in fact, a perfect indicator of the photospheric velocity. Again, studies including the ones cited above suggest that great care must be taken with this assumption, as a particular line feature in a transient's spectrum may vastly over (or, even, under) estimate the actual velocity of the photosphere due to optical depth effects. For instance, in SNe II-P it is well known that the H-alpha line can overestimate photospheric velocity by up to 40% at early times.

Put simply: Are there compelling theoretical reasons to believe that the Sr+ line in a NS-NS merger is an accurate indicator of photospheric velocity?

While there is little prior theoretical constraint on the spatial distribution of Sr+ within the ejecta, there are good observational reasons to believe that Sr+ is an accurate indicator of photospheric velocity.

First, while line velocities may overestimate photospheric velocities, this is typically due to their high optical depth detaching the line from the photosphere. However, the optical depth of Sr+ over all epochs analysed is moderate, with $\tau_{Sr^+} < 1$**.**

Second, we measure the sphericity at multiple epochs with quite different photospheric velocities. We have to invoke a considerable coincidence for the velocity of the line to decrease at a rate that

precisely matches the decrease in the continuum flux. Note, that we can derive constraints for each epoch individually is only possible due to gravitational wave timing.

We have now added these observational constraints and considerations to the main text.

*To be sure, Occam's razor would suggest that since a spherical photosphere is the *simplest* explanation for why the basic EPM application works so well in this case (which it clearly does), it should obtain. But directly and forthrightly addressing the validity of both of the above assumptions will assist in turning a compelling argument for sphericity into a convincing one, and in allaying fears that a confluence of multiple false assumptions -- coupled with a non-spherical photosphere -- have conspired to produce the result. Should the authors capably provide such assurance, I would recommend consideration by Nature for publication of this very interesting -- and, even, clever -- finding."*

Lastly, we have added the epoch numbers of the five spectra to the Fig. ED 1 as David Branch suggests.

We again express our gratitude for all these comments. We found that addressing the issues raised have significantly improved clarity and robustness of the arguments presented.

Thank you for your consideration and attention,
Albert Sneppen, Darach Watson, Andreas Bauswein, Oliver Just, Rubina Kotak, Ehud Nakar, Dovi Poznanski and Stuart Sim.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 left only remarks to me, in which he endorsed publication.

Referee #2 (Remarks to the Author):

I thank the authors for their consideration of the points raised in my first report, and I find their response -- both to the report and in their revision of the paper -- to satisfactorily address my concerns. Given this, I recommend this paper for consideration for publication by Nature -- the findings are compelling and of broad interest to the astronomical community.