

# Classical and Quantum Gravity

## Decision Letter (CQG-105358)

**From:** cqg@iop.org

**To:** pilondon2@gmail.com

**CC:** pilondon2@gmail.com, fauchon-jonesej1@cardiff.ac.uk

**Subject:** Our initial decision on your article: CQG-105358

**Body:** Dear Dr London,

Re: "On modeling for Kerr black holes: Basis learning, QNM frequencies, and spherical-spheroidal mixing coefficients" by London, Lionel; Fauchon-Jones, Edward  
Article reference: CQG-105358

We have now received the referee report(s) on your Paper, which is being considered by Classical and Quantum Gravity.

The referee(s) have recommended that you make some amendments to your article. The referee report(s) can be found below and/or attached to this message. You can also access the reports at your Author Centre, at <https://mc04.manuscriptcentral.com/cqg-iop>

Please consider the referee comments and amend your article according to the recommendations. You should then send us a clean final version of your manuscript. Please also send (as separate files) point-by-point replies to the referee comments and either a list of changes you have made or an additional copy of your manuscript with the changes highlighted. This will aid our referees in reviewing your revised article. Please upload the final version and electronic source files to your Author Centre by 20-Dec-2018.

If we do not receive your article by this date, it may be treated as a new submission, so please let us know if you will need more time.

We look forward to hearing from you soon.

Yours sincerely

Jane Hesling

On behalf of the CQG peer review team:  
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REFeree REPORT(S):  
Referee: 1

### COMMENTS TO THE AUTHOR(S)

This paper presents two algorithms for regression and applies them to create new fits for QNM frequencies and harmonic mixing coefficients for perturbed Kerr black holes. One of these algorithms has also been applied in other work to the modeling of QNM amplitudes excited by binary black hole coalescence. Having accurate fits for such quantities describing perturbed Kerr black holes is important for the success of gravitational wave astronomy, particularly as detectors improve in sensitivity (and especially looking ahead towards LISA). However, this paper does not show that the

fits it produces are more accurate than other fits in the literature (discussed further below). This seems to be mandatory if the authors want these new fits to be adopted in applications in preference to existing fits.

Since a good part of this paper is concerned with regression algorithms, it is possible that it would be better suited for a more general journal that deals with approximation methods. However, I think that the gravitational physics applications are a significant enough part of the paper that it is appropriate for CQG, and it presents enough new and potentially useful results to merit publication. I thus recommend that it be published in CQG after comparisons with the accuracy of existing fits are added (and the fits improved if they are not already significantly more accurate than existing fits) and the various other items I mention below are addressed.

In particular, this paper makes some statements that are unclear and also makes some incorrect claims of novelty. Most importantly, the paper claims in its abstract that it presents "the first model for QNM frequencies explicitly enforces consistency with the extremal Kerr limit." This does not appear to be the case: Cook and Zalutskiy <<http://adsabs.harvard.edu/abs/2014PhRvD..90I4021C>> present such fits in Sec. III A 1. Additionally, the paper's abstract claims that it extends previous models for harmonic mixing coefficients to dominant multipoles with  $\ell \leq 5$ . Thus, these statements (and the similar ones in the text) should be removed or appropriately qualified. I list other statements that need revision below.

Additionally, since the fits are only calibrated (and only compared with data) for spins of magnitude  $\leq 0.995$ , while Berti's webpage <<https://pages.jh.edu/~eberti2/ringdown/>> states that the calculations of Kerr QNM frequencies he presents lose validity for spins with magnitudes  $\gtrsim 0.999$ , it would be good to assess the accuracy of the fits presented in this paper for higher spin magnitudes than is currently done. One possibility is to compare to the results from Richartz <<http://adsabs.harvard.edu/abs/2016PhRvD..93f4062R>> in the extremal limit. Another is to compare with the fits from Cook and Zalutskiy. Of course, as mentioned above, it would be best to compare both the fits from this paper and the other recent fits in the literature (those from Cook and Zalutskiy and Nagar et al. for the QNMs and Berti and Klein for the mixing coefficients) to numerical calculations for spins with somewhat higher magnitudes. And even if one does not compare with higher spin magnitudes, it seems mandatory to discuss the accuracy of the new fits compared to the existing fits, as mentioned above.

\* Other comments (line numbers refer to those in the copy of the manuscript I was sent for review, and do not always match up well with the lines of text—I have tried to match them as best as possible):

p. 1, L16-17: You say that GMVR is a method for "interpolating rational functions." However, it seems rather to be a regression method like GMVP. I'm thus not sure why Runge's phenomenon (which refers to polynomial interpolation with equally spaced nodes) is applicable here.

The opening paragraph of the Introduction should cite the observing scenarios paper <<http://adsabs.harvard.edu/abs/2018LRR....21....3A>> for upcoming LIGO-Virgo(-KAGRA) observing runs.

p. 1, L32, left column: Is the GW150914 waveform systematics paper really the best reference here? This doesn't really discuss efficiency. I'm not sure that a reference is absolutely mandatory here, but the GW150914 PE paper (Ref. [2]) might be better if you want to include one.

p. 1, L44, right column: Kelly and Baker <<http://adsabs.harvard.edu/abs/2013PhRvD..87h4004K>> should probably also be cited for the mixing between spherical and spheroidal harmonics here.

p. 1, L46, left column: Is [20] really the best reference for spin-weighted spheroidal harmonics in addition to Leaver? I would suggest citing Berti, Cardoso, and Casals <<http://adsabs.harvard.edu/abs/2006PhRvD..73b4013B>> instead, since this studies those harmonics in detail.

page 1, L52, left column: It is unclear exactly what is meant by the term "effective completeness." From the context, I would presume that it refers to the statement about "gravitational radiation from generic perturbations ... be[ing] well approximated by" a sum over QNMs. If this is indeed the case, it seems clearer to avoid introducing the potentially confusing term "effective completeness." It would also be good to explain exactly what is meant by the statement about the QNM sum providing a good approximation (e.g., what norm is being used to assess the accuracy of the approximation, and whether this includes Price's power-law tail—I presume it does not) In Eq. (1),  $r$  is not defined (though it's fairly clear from context).

Third paragraph: The discussion of the EOB model's use of Eq. (1) should be clarified. For instance, while SEOBNRv4 and TEOBResumS indeed only include  $n = 0$  explicitly, SEOBNRv3, whose ringdown model is described in Babak, Taracchini, and Buonanno <<http://adsabs.harvard.edu/abs/2017PhRvD..95b4010B>>, includes higher overtones, as do earlier EOB models. It might also be worth pointing out that the EOB higher-mode model in Ref. [12] does not include the modes for which mode mixing is the most pronounced.

Additionally, when you describe the Phenom models' high frequency behaviour, you say that it

applies to “many of the Phenom models”; are there Phenom models to which this description does not apply? If so, this should probably be discussed. If you just mean that you are not citing all Phenom models (e.g., not older ones), this could be stated directly. You should probably also cite the nonspinning higher-mode Phenom model from the ICTS group here  
<http://adsabs.harvard.edu/abs/2017PhRvD..96l4010M>.

p. 1, L 49-50, right column: You might clarify here that PhenomD and the SEOBNRv\* models implemented in LALSuite interpolate tabulated results for QNM frequencies, which is not a “phenomenological model,” at least not to my thinking.

p. 1, L52-53, right column: One doesn’t have to compute QNMs by solving continued fraction equations—this is just the current standard procedure for high-accuracy work.

page 2, L30, left column: “aLIGO data analysis” -> “GW data analysis” (as this isn’t just restricted to aLIGO)

p. 2, L30, left column: The citation of [30] for zero-damping modes seems odd, since you’re concerned with the gravitational modes of Kerr here, while that paper primarily considers the scalar modes of Kerr-Newman (as well as the gravitational modes of Reissner-Nordstrom). Perhaps it would be better to cite Yang et al. <http://adsabs.harvard.edu/abs/2013PhRvD..88d4047Y> and also Cook and Zaltowski for a detailed numerical study.

p. 2, L39, left column: You say that Berti and Klein only present fits for mixing coefficients up to  $\ell = 3$ . This is not true. Their paper says that they present these fits up to  $\ell = 7$  online, and these fit coefficients are indeed available online  
[https://pages.jh.edu/~eberti2/ringdown/swsh\\_fits.dat](https://pages.jh.edu/~eberti2/ringdown/swsh_fits.dat).

In Eq. (6), it’s probably a good idea to define the superscript star notation for the adjoint.

p. 2, L26-27, right column: You mention formal series expansions of smooth functions here, but I’m not sure why this is relevant, since you are considering approximating via regression, not approximating via power series expansions (or Padé approximants thereof). This should thus be clarified or removed. Similarly, I am confused as to why it is useful to discuss the Taylor series expansion in Sec. II B in the context of constructing approximations by regression. As smooth functions with compact support (e.g., bump functions) illustrate, it is possible for the Taylor series expansion to arbitrarily high order (around a given point) to be an arbitrarily poor approximation to a smooth function in an arbitrarily small neighbourhood. Since you want to motivate being able to construct arbitrarily good approximations to smooth functions on compact sets using polynomials, the Stone-Weierstrass theorem (which is even applicable to functions that are only continuous) seems far more applicable here.

Algs. 2 and 5, step 2 and p. 3, L 58, right column: I think that the Cartesian product (i.e., set of ordered pairs) is likely meant here, instead of the Cartesian inner product (a.k.a. the dot product). Similarly, “power set” has a technical meaning in set theory (the set of all subsets), while I think what is meant here is the set of all powers of a given basis symbol.

Algs. 2 and 5, step 8: Shouldn’t the  $\lambda$ s be  $\epsilon$ s?

In fact, since it seems that Algs. 2 and 5 only differ by the action they call in step 3 (and some explanatory text in step 2 of Alg. 5), perhaps they can be combined into a single presentation, to make this similarity immediately apparent to the reader.

p. 3, L60, right column:  $\lambda_{\text{bulk}}$  seems to be missing the constant term and generally could present the structure more clearly (e.g., is “ $x_0, x_1 \dots x_N, \dots x_0^1$ ” supposed to be “ $x_0, x_1, \dots, x_N, \dots, x_0^2$ ” (or is “ $\dots x_0^1$ ” supposed to indicate something other than just  $x_0$ )).

p. 5, L3-4, left column: This statement is contrary to the Stone-Weierstrass theorem (since you are presumably not considering cases where there is a singularity in the domain over which you are attempting to approximate the function)—this is even true if you want to approximate the derivatives, as well (see, e.g., <https://math.stackexchange.com/questions/555712/on-finding-polynomials-that-approximate-a-function-and-its-derivative-extension>). As mentioned above, Runge’s phenomenon just refers to problems in interpolating with polynomials with uniformly spaced nodes, not approximating with them.

p. 7, L12, right column: Perhaps it would be clearer to write  $1 \leq \kappa \leq 0$ , to indicate that the  $j_f$  to  $\kappa$  mapping reverses the orientation (or leave the inequalities as they are and note this in words).

Eq. (20): Given Eqs. (22-28), it seems that this sum starts at  $j = 0$ .

p. 5, L52, left column: “signals” seems superfluous (and confusing).

p. 5, L15, right column: Presumably  $\|\cdot\|$  means the  $L^2$  norm, as in Alg. 3. This should be noted explicitly in the text, as well.

Alg. 3, step 3: The usual way of writing the name of this norm is the  $L^2$  norm (at least in mathematics).

Alg. 4, step 2: Shouldn't " $n = 1$ " be " $n = 0$ ", or am I missing something? Also, here and in step 11, I'm not sure what is the point of "Implicitly.")

Alg. 4, step 4: Should the "var" (and associated parentheses) be deleted? It's not defined, and not present in step 9.

Alg. 4, step 10: Shouldn't this compare the absolute value of the difference with tol?

p. 8, L7, left column: Ref. [16] doesn't seem like the appropriate citation here, as it's a technical numerical relativity paper about improvements to accuracy of evolutions when using 6th order finite differencing, and does not seem to discuss the physics of binary black holes at all. Is another Husa et al. paper, e.g., <http://adsabs.harvard.edu/abs/2016PhRvD..93d4006H>, intended instead?

p. 8, L11, left column: Is Ref. [31] (apparently arXiv:1510.08159) the appropriate reference here? I didn't see any discussion of asymptotic degeneracy in a quick skim through this paper.

p. 8, final line, left column: The "This" presumably refers to being consistent with the Schwarzschild limit, but not varying around unity, as in the previous sentence. This should be clarified.

Fig. 4: It might be good to say something about why the residuals have the shape they do. It might also be good to note explicitly that the bottom plots show more cases than the top plots. Additionally, it seems that the errors of the  $(\ell_{\text{ell}}, \ell_{\text{ell}}, \ell_{\text{ell}}, \ell_{\text{ell}}, n)$  mixing coefficients overall decrease with  $\ell_{\text{ell}}$ : This is very clear for  $n = 0$ , where there are four cases, but also is true for  $n = 1$ , though there are only two cases. Is there a good explanation for this? Similarly, it would also be good to give any intuition about why the cases that have the largest errors are less easy to model accurately. Of course, some of the large fractional errors are just because the magnitudes of some mixing coefficients are much smaller than others, but this doesn't seem to be all that is going on.

Eqs. (A1-A11): The parentheses in these equations seem superfluous. Also, it seems clearer to put the constant terms in the numerators as the left-most term, like they are in the denominator.

\* Bibliography: [12], [24], and [31] are missing bibliographic information.

\* Code:

The README for the code says to install positive using "pip install positive," but I get the error "Could not find a version that satisfies the requirement positive (from versions: ) No matching distribution found for positive" when I try this. However, I was able to run an example script using the code I downloaded.

Also, while seeing if there were implementations of the fits from this paper in the code provided, I noticed that positive/physics.py has some functions with comments that say "Copied from LALSuite Version." Additionally, the \*14067295\* functions in there seem to be verbatim copies of some LALSuite functions (from <https://git.ligo.org/lscsoft/lalsuite/blob/master/lalinference/python/lalinference/imrtgr/nrutils.py>) with changed names, though this is not noted. This should be noted, and the code released under the GPL (as LALSuite is), or the code from LALSuite should be removed. (Since the authors of this package do not seem to be authors of the nrutils.py code in LALSuite, this code presumably came from LALSuite into the positive package, not the other way around.)

\* Minor:

In general, there are some rather awkwardly/oddly worded sentences throughout the paper that might easily cause a reader to trip up while reading. For instance, p. 5, L6, left column: "Of the simplest examples are rational functions of the form...", which may be supposed to read "One of the simplest examples are rational functions of the form..." but could also have several other possible meanings. I thus recommend that you go through the paper and check wordings.

page 1, L39, right-hand column: "spins weighted -2" -> "spin- $(-2)$ -weighted" or something similar

In the pseudocode for the algorithms (and similar places in the text), it would likely be easier to read if the words given as sub- and superscripts were set in Roman.

p. 5, L32, right column: "broach" -> "breach"?

Referee: 2

COMMENTS TO THE AUTHOR(S)  
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