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Report of the thesis "General relativistic simulations of luminous and non-steady accretion flows", by Mr. Bhupendra Prakash Mishra.

The PhD thesis manuscript presented by Mr. Bhupendra Prakash Mishra deals with the analytical and numerical study of accretion flows around compact objects in general relativity. This is a fundamental field of research in modern astrophysics, a field in which the group of the PhD supervisor has made remarkable contributions along the years. Therefore, not surprisingly, the manuscript I have had the pleasure to review is of the highest quality. The results reported in the thesis have also been already published as four articles in prestigious, international, peer-review scientific journals, which provides enough evidence of the quality of those results. Indeed, most of the investigations pursued in this work are pioneer in the field, be them analytic (or semi-analytic) or fully numerical. Furthermore, for many of those investigations there is room to pursue natural extensions in the near future, by, say, improving the modelling of the accreting sources considered or extending the physical accuracy of the numerics by relaxing symmetry assumptions or the Cowling approximation.

In the following I provide a list of issues I have found in the text and that the author could perhaps address.

On page 2 at the introduction of the manuscript the author mentions the Bondi-Hoyle model of accretion flows in the context of spherical flows, with only the radial inward component of velocity. I wonder if this is correct since the Bondi-Hoyle model assumes an axisymmetric (or even 3D) motion (a wind). Perhaps the author had in mind the purely spherical model of Bondi (1952) or its relativistic extension by Michel (1974).

- Paper I

  The study of a viscous accretion disk around a spheroidal accreting source (modelled in the manuscript as a Maclaurin spheroid) is very interesting, and the implications that the changes in the eccentricty of the object (as opposed to the spherical case) may have in the spectra are remarkable. If the changes suggested in the spectra could be measured observationally they could be used to infer properties of the accretor, such as the eccentricity and the period.

It is interesting to note that the diffusion of the Gaussian shell of matter for $e = 0.0001$ is symmetrical when the shell is placed further away from the center (compare Figs. 7 and 8). Is this the expected behaviour? I miss some further comments about this in the paper.

Also, what is the motivation behind using a Gaussian shell in the first place? Does it represent an overdensity region in the accretion disc due to some perturbation? The results show that the variation in the spectra is more important the closer to the inner radius of the disc the Gaussian is located, but is there any physical motivation or intuition so as to where to place the shell? I guess my criticism is because I see the use of the shell as a handy way to produce variations in the eccentricity but I would like to have some physical support of this setup.

It would be very interesting to extend this model to more realistic sources such as a rotating neutron star, relaxing the strong assumption of a constant density accretor implicit to the Maclaurin spheroid. Indeed, the fact that the diffusion equation can accommodate angular velocities different to that of the Maclaurin spheroid employed in this work, is calling for additional investigations in this topic. As the author points out, the current model could potentially serve to constraint the EOS of neutron stars if the results were to also hold in that case.

Also regarding Paper I, I think that the manuscript would have benefited from the incorporation of a discussion about the validity of the model assumption, namely that the thin disc terminates at the surface of the accretor, the Maclaurin spheroid in his case. This assumption is key in this work, as changes in the eccentricity of the accretor imply changes in the inner radius of the disc and, in turn, changes in the spectra from the inner regions. Any comment on that?

One of the main pursuits of the author in Paper I is the numerical study of the transport of angular momentum in a viscous, non-stationary disc, by solving the diffusion equation using the Crank-Nicolson method. He states that the viscosity of the disc is kept constant as a first approximation. It would be important to assess the validity of this assumption and to find out the possible effects that a variable viscosity parameter might have on the results. In addition, it would also be important to compare the value of the physical viscosity the author employs in the disc with the numerical viscosity of the method as a result of the discretization of the diffusion equation. I am confident that the latter can be made in this case much smaller than the former (so as to not affect the results) since the author is essentially solving a 1D (radial) problem, but it would be informative to have such a comparison.

In page 1159 of Paper I, first paragraph, there is a "Q" symbol which seems like a leftover from the proofs, and it should have been removed.

- Paper II

  The results reported in Paper II are particularly impressive, as they have been obtained with challenging global GRMHD simulations that have allowed to assess for the first time the stability of thin accretion discs beyond simplistic shearing-box studies. It is reassuring to find that the gas-pressure-dominated model is stable and the radiation-pressure-dominated disc is unstable, as both should respectively be on theoretical grounds. In addition, the agreement with the shearing-box simulations of Jiang et al (2013) is also worth mentioning.

Is there any reason why rotating black holes have not been considered in this study? Since the code must surely be able to handle the Kerr metric in Kerr-Schild-like coordinates, it would have been interesting to know the effects of the black hole spin on the evolution. I am bringing this up because in other types of instabilities in discs (e.g. the runaway instability) the spin of the black hole is known to play some role.

I have failed to notice in the text what is the value of the strength of the magnetic field. This value is obviously important in connection with the growth of the MRI, and could help to validate the numerical requirements (resolution-wise) in terms of resolving the MRI, which is no doubt a major issue of these simulations.

I would also like to know if there is a deeper reason behind the choice of the radial coordinate span in the simulations, in particular why the innermost radius starts at four Schwarzschild radii, somewhat a bit far from the black hole horizon. I guess it should possibly be a bit closer (or even inside, as Kerr-Schild coordinates are well behaved at the horizon) since MRI drives accretion into the black hole.

What is the mass of the initial disc? Is it much smaller than the black hole mass to safely neglect its self-gravity and the dynamics of the metric potentials as a result of mass and angular momentum accretion during the evolution?

Regarding the grid setup, I would also like to know if the author has assessed the periodic boundary conditions he uses in the azimuthal direction (which are somewhat similar to those of shearing-box simulations but only in that direction) by, say, considering two different angular wedges and analyzing the dependence of the results on those choices.

Could the author comment on what should be the ultimate fate of the unstable disc? The simulation shows that it collapses in the vertical direction to a disc of very thin height. What would happen beyond that? Would the disc eventually disappear? Would a singularity form?

The author mentions that the magnetic pressure in the unstable model is not big enough to stabilize it. I suppose that this may depend on the initial strength of the magnetic field and of the ability to resolve the growth of the MRI in the code. Could the author comment on that?

- Paper III

  In Paper III the author studies the ejection of the corona in variable luminosity X-ray sources, using a toy-model based on solving numerically the equations of motion of test particles moving in a strong radiation field. Arguments in favour of the validity of this model in the context of actual accretion disc coronae, i.e. optically thin plasma, are provided and look convincing. (Nevertheless this work fully deserves to be extended and investigated in a more consistent way using a radiation-hydro code in general relativity, as the one used by the author in Paper II; I am glad to read in the manuscript that this will be attempted in future work.) An important result of this numerical study is that the radiation drag hampers the unbinding of particles/plasma in the innermost regions of the corona (near to the ISCO). The author also shows that the required luminosity to unbind the particles (the escape luminosity) strongly depends on the distance, which is a relativistic result with no Newtonian analogy, where the ejection luminosity does not depend on the radius.

  Regarding the study of the coronal ejection in variable X-ray sources, the author mentions that the equations are solved in the equatorial plane of the Schwarzschild metric. I am having some difficulties to understand this setup since it would seem that the author is not dealing with an actual disc but rather with a cylinder, since there is (slab) symmetry with respect to the theta coordinate. Am I wrong? Can the author comment on possible implications of this choice on the results?

The manuscript shows that unbinding particles near the ISCO requires the largest luminosities. Since the ISCO shrinks to lower radii with rotation, it might well be that for rotating spacetimes the required luminosities could be unrealistically high. Could the author comment on this?

In GR, an unbound particle moves outwards and reaches the condition $u_t < -1$ at infinity (or, equivalently, $hu_t < -1$, with h being the specific enthalpy). If I am not mistaken, this condition, which is mentioned somehow in the discussion around Eq. (9), is not general but requires of an *stationary and axisymmetric* spacetime. Could the author comment on that?

- Paper IV

In the last paper included in the PhD thesis the author studies quasi-periodic oscillations in tori around black holes. This is an important topic of research since no consensus has yet been reached to explain the variability in the frequency of low-mass X-ray binaries containing black hole candidates. There is a large amount of work in this field, observational, numerical, and analytical, as the author nicely summarizes in the introduction. The approach taken in the thesis to study this problem is highly interesting as it combines the results of hydrodynamical simulations of oscillating tori with synthetic light curves built from ray-tracing the hydrodynamical data. For the hydro simulations the author uses the same state-of-the-art code employed in Paper II, while for the ray-tracing he employs the public-domain code `GYOTO`, which is also a cutting-edge numerical tool. Upon the introduction of suitable perturbations (whose origin is yet to be explained) the author evolves many different torus models and computes PSD of the light curves to identify the dominant modes of oscillations. The discussion is fairly complete as well as the comparison with earlier numerical and analytical work.

Despite the approach followed in this study is modern, the fact that the author uses simplistic, and unrealistic, constant angular momentum tori is a bit disappointing. Especially given the fact that it is nowadays quite straightforward to build numerically non-constant, Keplerian, angular momentum tori. This fact diminishes a bit the interest of the results, particularly if comparisons to observations are to be drawn.

The numerical setup for the hydrodynamical simulations does not seem entirely obvious with the information that is provided in Section 2.1. It was not clear to me if the author was employing a low-density atmosphere surrounding the disc (which seemed the case telling from the very first figure of the paper). I had to wait to Appendix A to obtain the first mention to the fact that, indeed, an external atmosphere is being used. The comparison reported in this appendix is important and reassuring, as the modes are excited regardless of the value of the background density (although their particular amplitudes are obviously affected).

The discussion in this appendix is also very relevant in connection with the artificial increase of the mass of the central regions of the torus alluded to in Section 5.1. There is no physical reason it should grow secularly (it should simply oscillate around the equilibrium value) and therefore the effect is purely numerical, most likely as a result of the accretion of matter from the surrounding atmosphere onto the torus. I think the author could have done a deeper study of this effect. On the one hand he could have shown if the secular trend in the mass becomes smaller with increasing resolution (and at the expected rate given by the order of convergence of the code). On the other hand (and I am puzzled that this was not done) the author could have used the simulations he did in Appendix A to study the effect of the background atmosphere density to analyze the secular drift of the mass. Since the author must still be in possession of these data, I suggest that he prepares a plot like Figure 3 for the various values of the parameter “b_0” employed to produce Figure A.1. This should clarify if the secular increase of the mass is due to mass accretion from the atmosphere.

In view of all the discussion presented in this report it is my decision to consider the scientific merits of this thesis to be sufficient to meet the requirements to be defended.

Yours sincerely.

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