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No 321/DZ/17: Report for the doctoral thesis of Mr. Pavel Čížek

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Dear Prof. Jan Kratochvíl,
dear board members,

the PhD thesis presented by Mr. Pavel Čížek "Stationary fields in black hole spacetimes" tackles a long standing and unsolved problem of general relativity: Which spacetimes describe properly a black hole surrounded by a thin disk or a ring. Indeed, many astrophysical black holes are surrounded by additional matter like accretions disks. Their presence is often even vital to observe the black holes and their properties in the first place, which implies that deviations from the Kerr black holes, which describe isolated black holes, must in principle be taken into account. Still, in contemporary astrophysics any distortion caused by the additional matter is assumed to be negligible, see, e.g., (arXiv:1510.00394 [astro-ph.HE]). In order to test this assumption the effects of possible distortions must be modeled at least in a few relevant cases. Mr. Pavel Čížek takes one step in this direction by allowing for a thin disk or ring surrounding the black hole in general relativity.

The difficulty of generating solutions describing such systems is the non-linearity of the corresponding field equations, namely, the Einstein equations, which do not allow a superposition of solutions. Mr. Pavel Čížek explores such systems in two well-known ways: A perturbative approach and the Belinskii-Zakharov method.

The *perturbative approach* seems fitting since astrophysically the mass of a possible accretion disk is assumed to be small compared to the black hole mass such that their ratio can serve as an expansion parameter. An additional multipole expansion completes the scheme and decouples the equations entirely. The surprising and nice result found by Mr. Pavel Čížek is that the Green's function for that system can be found and even summed over the multipole expansion yielding a closed form expression. This is only possible because of technical and non-trivial mathematical results, which are interesting in their own right, regarding the special functions involved. These are summarized in the Appendices.

This avoids the usual numerical approach and improves the convergence behavior of the perturbation scheme. This allows to explore the solution space much more efficiently than with numerical methods and to identify the physically relevant solutions faster.

As a side note, the discussion of the angular momentum induced by a perturbation is (at least in the ApJ paper, which is part of the thesis) very clear and treats this standard problem in perturbation schemes in a concise and clean way. For example, similar problems appear also in the Post-Newtonian approaches to equilibrium figures and, of course, in the full theory.

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The *Belinskii-Zakharov approach* is investigated in the third chapter. The used seed solution can in general be chosen arbitrarily although the Minkowski spacetime is a common choice. Mr. Pavel Čížek used instead a Bach-Weyl Ring as seed as did de Castro and Letelier in (CQG **28** 225020 (2011)). He can again obtain the spectral function in closed form in contrast to the previous authors. He exploits the advantages of the analytical over the numerical approach fully and is able to show that there is no choice of the parameters entering this solution such that there are no further sources present. Although this rules such solutions essentially out as solution describing the desired systems, it is a feat in itself to be able to show this rigorously.

These results show clearly his ability to solve creatively scientific questions in particular not shying away from involved problems that appear at first glance completely mathematical in nature instead of resorting to numerical methods right away. I believe that his closed form Green's function will find several applications in the future and will lead to interesting solutions. However, the presentation of his results leave room for improvement. Subsequently, I will address the critical points in the presentation with a few examples each. The list of examples is in all instances not exhaustive.

As the biggest shortcoming of the thesis, I see the lack of *references*. As example, Sec. 1.2 does not refer to *any* previous works although no new results are presented, which is definitely not the scientific standard. In particular, these results were partially derived by people at the Charles University and would have been easily accessible. Although he does not mention the previous works here, he does also not claim that these are his results. Moreover, in the ApJ paper, which is part of the thesis, this point is rectified and the appropriate references are given.
Some – not all – other instances are:

- p. 30, 4 §: "Perturbations of black-hole space-times are a "classical" GR problem, recently mainly studied in a non-stationary regime in connection with collisions of compact objects and a consequent gravitational emission." That is certainly true but references to the recent works would be helpful.
- p. 79, last sentence of the conclusion: "Our main future plan is to study the black-hole–disc solution found in the second chapter in more detail and compare it with the results of numerical treatment of similar configurations which have appeared in the literature." To which literature and to which solution is here referred?
- After Eq. 3.65: "Such metric is well known and does not require further discussion here." Again no reference is given.

However, the lack of references yields also some additional problems. For example, one finds in the thesis several claims that are in dire need of some references if not further explanations. To name some instances:

- p. 10, 3 §: "This property is standardly called "orthogonal transitivity" (see section 1.1) and the metrics without this property are only very little known." Here some more context should be given. I would immediately agree that there is less known for those without this property than for those with this property. "Very little" is, however, maybe not the best term to describe the works in these directions. At least there are such solutions known, e.g., (E. Ayon-Beato et al. PRD **74** 024014 (2006)) even though their approach was purely kinematical and the resulting stress energy tensor would probably be rather unphysical. More details or a few references would clarify this statement.
- p. 28, 1 §: "Most of the (infinite families of) metrics obtained by generating techniques still remain to be interpreted, and it is very probable that almost none of them has a reasonable

physical sense. In this thesis we nevertheless include a section on a possible usage of the inverse-scattering method." Without an explanation of the meaning of "physical sense" (does it mean, e.g., astrophysical, in principle measurable in the next N years, the energy conditions are satisfied, certain types of matter content, no struts, ...) this is certainly wrong. For example, the inverse scattering techniques was used in (arXiv:1108.4854 [gr-qc]) for the first complete uniqueness proof of the Kerr-Newman black hole spacetime describing axially symmetric and stationary black holes. In fact, this proof is also *constructive*. Since Kerr black holes are used to model the end state of binary inspirals and supermassive black holes, they are physical sensible, i.e., they are applied to astrophysical phenomena. The inverse scattering technique was also used to generate the Neugebauer-Meinel disk (arXiv:gr-qc/0302060) of rigidly rotating dust, which is also physical in the sense that the energy conditions are satisfied. To be fair, this was also mentioned in the introduction as exception but not at this point. The aforementioned solutions are obtained in a direct way, i.e., properties of the solution are already prescribed. But even the indirect generating techniques produce physical sensible solutions (in one of the above mentioned senses). For example, why is the solution describing a source for the Kerr metric found by J. Bicak and T. Ledvinka (PRL **71** 111669 (1993)) not "physical sensible"? This warrants at least some explanation.

- p. 41, 4 §: "We assume this ring is located in the equatorial plane, $\theta' = \pi/2$, on some radius $r = r'$ (a ring placed off the equatorial plane would require some kind of supporting struts in order to remain stationary, otherwise it would be pulled towards this plane)." The explanation in brackets seems reasonable, however, as the difficulties to establish the simple non-existence of an equilibrium of two static compact sources shows, this is not necessarily guaranteed. Are there already arguments beyond the physical intuition known? They should either be cited or the explanation should be identified as physical intuition.
- P. 25, last §: "The Kerr space-time can be generated by thin discs — an infinite differentially (counter-)rotating one Bičák and Ledvinka [1993] or a rigidly rotating finite one Neugebauer and Meinel [2003], but in contrast to the Schwarzschild black-hole no real interior solution has been found so far." I assume that with generated it is meant that there is a source such that the vacuum region is isometric to the Kerr spacetime. In that case, the statement is wrong. The Neugebauer-Meinel disk does not generate Kerr, which can and was easily checked by calculating the multipole moments of that spacetime (Kleinwächter et al. Physics Letters A **200** 82 (1995)). The only case, where this is possible seems to be a quasi-stationary limit to the extreme Kerr black hole (as for perfect fluid configurations in general). Even more general: the so-called quadrupole conjecture, see, e.g., (arXiv:0902.1859 [gr-qc]) and references therein, states that under certain conditions a one component perfect fluid can never be the source of an under-extreme Kerr black hole. This should definitely be mentioned here. This would also clarify what "real" means. In other words: In which sense is the Bicak-Ledvinka solution not "real"?

Additionally, without further explanation nor referencing some terminology remains unclear:

- p. 14, 3 §: "the two mostly used 'extreme' cases being the Weyl–Lewis–Papapetrou (WLP) form .... And the Carter-Thorne-Bardeen (CTB) form". It is neither explained nor referenced why these two forms of the metric are "extreme". In fact, I did not find an answer to the question up to now. Here, the quotation marks seem to refer to a vague concept.
- p. 30, 1 §: The terms "self-gravity" and "back-reaction" are again used in quotation marks, where it is not clear what these signify. Does it refer to conceptual problems with these widely used terms. If so, an explanation or at least a reference would be in order.

It adds to the confusion that the quotation marks are seemingly used in two different meanings, where the two examples given above belong probably to the two different categories. At some instances, they signify a definition, i.e., a well-known and well-defined terminology (e.g.: p.9, 4 §; p.10, 3 §, ["Weyl", "orthogonal transititvity"]) and in other occasions they seem to signify a terminology, which is deliberately left vague (e.g.: p. 9, 2 §, 3 § ["permitted", "canonically"],...), which is of course justified in many instances but should be made clear.

Independently, there are a few points regarding the content of the thesis, which I list subsequently:

- After Eq. 1.60: The parameter a in the Kerr metric is not the angular momentum. It is rescaled by the mass and could be called "specific angular momentum".
- The Tables 2.1 and 2.2 with which the convergence is shown by example are not as telling as a plot of the relative error with the appropriate scaling would be. A short test plot of one of the lines in Table 2.1 showed that the relative error exhibits indeed a nice exponential decay. Such a plot would have been more valuable in my opinion or would have supplemented the tables considerably. Also I did not see a reference to which norm is used to evaluate convergence although I assume that pointwise convergence is meant.
- It is not described properly what is depicted in Fig. 2.2. I assume these are the equipotential surfaces of the metric functions. In that case the scale should be given as well. Even in the case, where it is provided (Fig. 1.3), it is not indicated from where to where it is taken (e.g. from the inside to outside).
- Regarding the Figures, e.g., Fig 3.2: Is the depicted behavior generic or due to the particular values. Choosing these values in a more arbitrary way would be convincing in this respect.
- p. 35, 1 §: How is the horizon defined geometrically in the perturbed spacetime? Only with this information, one can judge if the assumptions regarding the coordinates are sensible.
- After Eq. 2.70: What is meant by the "angular momentum of the black hole"? Is it, as I believe from later parts of the thesis the Komar integral evaluated quasi-locally?
- Eq. 2.41: What is meant physically by this condition and is it without loss of (physical) generality? This condition should be explained in more detail.
- Some parts of the thesis are not well motivated. For example, I still do not know why the Chazy-Curzon solution was discussed. The purpose of Chapter 3, I understood only when reaching the footnote 10 on page 66 and after a study of the original paper. A short introduction, what will be presented and why would have been very helpful. This would have also explained what is new compared to other works.
- In Section 2.5.3: In which part of the parameter space are the energy conditions satisfied? There are hints in the text (sub-luminal orbital velocity) but no explicit discussion. Since these are fundamental physical properties of a source, they should be discussed here at least shortly. At least, it should be made clear that there is a region in the parameter space, where some of them can be satisfied.

Moreover, the style of the thesis could be improved considerably.

- In the figures presented, e.g., Fig. 1.1, 1.2., 1.3 the scales are rather small and they are also not correctly cropped. Besides these style questions it would have been better to use rescaled quantities like r/M.
- There are plenty of instances, where the grammar is incorrect. To name just a few:
    - p. 11, 2 §: "Leaving aside numerical approaches, there is still another option how to tackle Einstein (or in fact any difficult) equations: perturbation techniques. These are not appropriate for *any* (sic! even with the emphasis) purposes, but can at least

provide solutions which are "close" to some known exact solutions.", which is particularly nice since it puts half of the thesis in question.
  - p. 9, last §: "in a suitable coordinates"
  - p. 15, 1 §: "Following astrophysical motivation, we rather adhere to the second limit possibility and interpret the source as an (sic!) orbiting particles or fluid."
- Typos in Equations. Again only a few examples are given:
  - Eq. 2.34: "frac14" instead of 1/4
  - Eq. 2.39: Equality sign missing
  - Eq. 2.69: Even with good will, I was not able to read this equation.
  - Eq. 2.81: One of the brackets does not open.
- Typos in text: In fact, while reading the thesis, I decided to collect 5 examples only when I reached Eq. 3.52. These examples are distributed over half a page and I give them here to give an impression of the amount of typos in that chapter, where a particular high number can be found. The amount of instance is much higher than I would expect in average in a PhD thesis.
    - "...denotes NUT parameter of solution with $z > 0$ and $z < 0$ (sic!) So something like NUT parameter is present."
    - Punctuation after Eq. 3.54
    - Punctuation after Eq. 3.55
    - Three occasions at once after Eq. 3.55: "Careful (sic!) in the vicinity of axis has to be used as λ can vanish there and limit approach must be employed. Due to the the (sic!) technical aspects of the discussion and appearance (sic!) physically severe problems in the equatorial plane it is present here.
- Independently of the above:
  - P. 11, 3 §: Unbound box

In summary, the thesis shows clearly the great mathematical skills of Mr. Pavel Čížek and results which promise many applications in the future. With such results, the thesis could be also highly recommendable if it were not for the quality of the presentation. Since a PhD thesis is not just about the obtained results but also about their scientific presentation as well as connections to previous results, my conclusions about the presented work are ambivalent. Nonetheless, after long considerations and with some hesitation, I find the quality of the scientific results outweighs the shortcomings in the presentation and I recommend the thesis to be accepted.

Although I cannot attend the defense in person, I would like to use this opportunity to ask some questions as well. I would appreciate it if the answers are send to me:

1) Which energy conditions are satisfied by the solutions (one and two stream) given in Section 2.5.3?
2) Do there exist general statements about the energy conditions in the two interpretations, such like if the weak energy condition is satisfied in the one interpretation it is also satisfied in the other interpretation?
3) How does the perturbation approach relate to the Post-Newtonian works in the Jena group, e.g., (arXiv:gr-qc/0309017), where similar expansions and systems of orthogonal functions appear?
4) Recently, quite some work was put in the description of distorted black holes, see, e.g., (arXiv:1503.07365 [gr-qc]), where sources are placed at infinity to cause the distortion.

What are the technical differences to their perturbation approach? In principle, it should be possible to use the obtained disk solution to calculate the Love numbers of black holes. This would be interesting, since the aforementioned work places the sources of the distortion effectively at infinity, which yields some conceptual and technical problems. If a black hole-disc system is available, this should be possible explicitly.

With kind regards,

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