Correspondence between J. Szeftel and M. Dafermos-G. Holzegel concerning [4]

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We have received a number of questions[1] from Mihalis and Gustav concerning our book [4]. Though we have replied to them in full, and offered to continue to have discussions between Jérémie and either Gustav or Igor, we are surprised to hear, from multiple sources, that they continue to claim that there are errors in our book. After a final unsuccessful attempt, October 2022, to engage Gustav into a zoom conversation about these claims we have decided to make public the correspondence between Jérémie and Mihalis-Gustav in connection to these questions.

The most contentious question, number one in the original set of nine questions (see section 1.1), led a large number of e-mails between Jérémie and Mihalis which ended with a concession by Mihalis that our original argument was correct as stated[2], even though it may require an additional explanation (see the first item of section 3).

The remaining questions, show concern for the way we construct global smooth frames matching the two spacetime regions ${}^{(ext)}\mathcal{M}$ and ${}^{(int)}\mathcal{M}$. Though they appear to us less important, we nevertheless answered them in full in the first reply of Jérémie, see section 1.2. Further, more detailed, explanations are given in section 3.

[1]We received these questions after we have repeatedly asked the authors of [9] to correct, what we think are serious omissions and misrepresentations of our own work [4], in the introduction to [9]. Our request for changes was discussed with I. Rodnianski in November 2021. So far, unfortunately, no changes have been made on their arXiv version. For full transparency we provide our correction proposal in section 6.

[2]Regarding this question, Mihalis and Gustav originally said that "one would have to redesign the basic constructions" (see item 1 of section 1.1) and "This issue lies at the very foundation of the architecture of your paper" (see item 1 of section 2.1) while Mihalis in the end concluded "let me state unequivocally, for whatever its worth, that I personally now find the foundational architecture of your paper, as such, not only to be in principle unproblematic, but in fact to be very interesting, highly original and in particular quite different from that of [CK] or other works I know" (see section 4.15).

Moreover, Mihalis and Gustav keep insisting that our work is heavily dependent on polarization, a statement with which we respectfully and strongly disagree as we argue in section 3 (see first paragraph of item 4).

Following the heated exchange of e-mails between Mihalis and Jérémie, see section 4, Jérémie proposed to continue the discussions with either Igor or Gustav with the hope of avoiding further misunderstandings. His offer remained unanswered, which led us to believe that the issue was settled.

The first set of questions of Mihalis-Gustav is in section 1.1, and the reply by Jérémie is in section 1.2. The second message by Mihalis-Gustav is in section 2.1, and the second reply by Jérémie is in section 2.2. In section 3, we have included further explanations. Section 4 contains the full exchange of e-mails between Jérémie and Mihalis with regard to the first question. As a conclusion to the exchange, section 5 contains a short Errata and further clarifications for the interested reader. Section 6 contains our correction proposal mentioned in footnote 1. Finally, in sections 1.2, 2.2, 4 and 6, we have added in blue additional comments which may help the reader to follow the exchange.

Contents

1 First set of questions and answers

This section includes the original questions sent by Mihalis-Gustav (November 16, 2021) in section 1.1, and the replies by Jérémie in section 1.2.

1.1 First message by Mihalis-Gustav

1. The norms defined in Section 3.2.in general seem not to depend continuously on the bootstrap time u_*, in view of the fact that the timelike hypersurface $\mathcal{T}$ is defined in a way which is manifestly discontinuous in u_* in general. Thus, it would appear, simply as a matter of principle, that one cannot base a continuity argument (as sketched in Section 3.6.2) on these norms and one would have to redesign the basic constructions.

2. In fact the definition of the timelike hypersurface $\mathcal{T}$ seems circular. The region $\cup_{r_0 \in I_{m_0,\delta_{\mathcal{H}}}}\{r = r_0\}$ is not defined. In Definition 3.2 and Figure 3.2 it would seem that the region ${}^{(ext)}\mathcal{M}$ terminates at $\mathcal{T}$. Presumable the region $\check{R}$ means ${}^{(ext)}\check{R}$? This would the have to mean that $\check{R}$ defined with respect to the extended exterior frame of Section 3.5.1? If so $\cup_{r_0 \in I_{m_0,\delta_{\mathcal{H}}}}\{r = r_0\}$ should then be v-shaped and clearly already depends on the definition of $\mathcal{T}$. Any other interpretation of (3.8.8) seems to lead to even more issues.

3. Even ignoring the fact that $r_{\mathcal{T}}$ is possibly ill defined, in Proposition 3.23 it seems that the global frame can be defined so that one of (d) (i) and (d) (ii) hold, but one does not get to choose which (e.g. if $r_{\mathcal{T}} = 2m_0(1 + \frac{3}{2}\delta_{\mathcal{H}})$ then it does not seem possible to ensure that (d) (ii) holds). In particular, Remark 4.13 seems to be incorrect. One perhaps would be allowed to choose which one if $r_{\mathcal{T}}$ is allowed to lie outside the interval $2m_0(1 + \frac{1}{2}\delta_{\mathcal{H}}), 2m_0(1 + \frac{3}{2}\delta_{\mathcal{H}})]$. Regardless, only one choice of (d) (i) and (d) (ii) can be made. In Section 6.2.1 it seems that (d) (ii) is chosen. Is a different choice being made in (7.1.26)?

4. In view of the formalism discussed in Section 2.1, which seems to be invoked throughout to write down the equations, it seems necessary to know the existence of integral spheres of the various non-geodesic frames defined, in particular of the global null frame defined by Proposition 3.23 (see Definition 4.12). Even the local integrability of these spheres is not actually discussed anywhere (note that this would depend on both axisymmetry and polarisation, and thus it would appear that were one to relax simply the latter, one already would have to deal with non-integrable frames under

your approach). The global integrability, which seems to be implicitly invoked later (see 8. and 9. below) would require more information. In particular, the global spheres ${}^{(glo)}e_3$, ${}^{(glo)}e_4$ normal to the second global frame appear to necessarily exit the bootstrap region to the future.

5. In fact, as defined, the extended frames defined in Section 3.5.1 do not seem to be smooth in general. In particular, the global frame of Proposition 3.23 appears to be merely continuous.

6. The tensorial nature of the relations appearing in Proposition 2.90 is never discussed, thus it is not at all clear how to interpret the right hand side in the reduced picture corresponding to the primed frame. Can the individual terms appearing on the right hand side be individually re-interpreted as elements of $\mathfrak{s}_k$ corresponding to the primed frame of the left hand side? If not, how are they dealt with individually with respect to the formalism of the primed frame which seems to always involve such tensors. How does one commute these terms by the operators corresponding to the primed frame? (As an example, consider the last equation at the bottom of page 304 in the published version. As far as we can see, the operation of the operator $\not{d}'_1$ on $\underline{\beta}$ is undefined and the remaining terms on the right hand side are of unknown tensorial type.) It would seem that one needs to develop a geometric formalism to house such terms before one could possibly try to estimate them. This issue is relevant everywhere in the paper where change of frames are invoked.

7. Looking randomly at some equations, it would appear for instance that the first equation in Section 8.7 does not seem to be correct. Since the terms on the left involve the frames ${}^{(int)}e_\theta$, ${}^{(int)}e_4$, ${}^{(int)}e_4$ and the terms on the right involve the frame, ${}^{(glo)}e_\theta$, ${}^{(glo)}e_3$, ${}^{(glo)}e_4$ there should be terms involving the change of frame coefficients $(f, \underline{f}, \lambda)$ on the right hand side? It seems that there should be a linear term on the right hand side involving $J+1$ derivatives of $(f, \underline{f}, \lambda)$.

8. What frames are being used in Chapter 10? One finds no explicit comment until the paragraph at the beginning of Section 10.5, which suggests that all along until that point it has been the second global frame of Proposition 3.26. To use the formalism of Section 2.1, it would appear that this would mean that the commutation operators have to be defined with respect to this frame, the spheres of integration S(never explicitly identified, but appearing, say in Lemma 10.18) should be the integral spheres of this frame, and one must distinguish between the r defined by this frame (whose definition again would require entire spheres) and the pasted r you appear to want to use. (As far as we can see, these spheres S appropriate to this frame are never discussed or controlled and may actually leave the bootstrap

domain.) Similarly, when integrating in the domain, e_4 appears to be identified with the normal of the outgoing null hypersurfaces (see for instance Section 10.1.13.2), whereas, if indeed it has been all along the e_4 of the second global frame, then as far as we can see it does not coincide with this normal and is not tangential to the hypersurface. Thus it appears that the tensorial commutated divergence identities for the quantity you denote as $\mathfrak{q}$ have not been integrated correctly here and this puts into question the entire chapter.

9. There appear to be similar issues to the above concerning say Section 6.2.2. The preamble of the section suggests that calculations should be interpreted in the second global frame. Thus differential operators $\not{d}$ of Proposition 6.14 and the e_θ appearing in Remark 6.15 should be that of the second global frame which does not appear to be tangential to Σ_*. Thus it would appear that this is not in fact a parabolic equation on Σ_*, the spheres S implicit in say Lemma 6.19 (used in the proof of Lemma 6.16 on page 294) are not tangent to Σ_* and thus the results of the entire section are put into question. Finally, the relation between $\underline{\alpha}$ of the second global frame and ${}^{(ext)}\underline{\alpha}$ of the exterior frame in the middle of page 282 is replaced by ${}^{(ext)}\underline{\alpha}'$.

1.2 Jérémie's reply to the first message

1. The timelike hypersurface $\mathcal{T}$ is indeed defined in a way which is manifestly discontinuous in u_*. As a result, the norms defined in section 3.2 are too. On the other hand, the only thing that matters is that the spacetime is indeed extended, and that the norms are still controlled by a universal constant times ϵ_0. In particular, note that the extension criterion, namely that $u'_* > u_*$, see the statement of Theorem M7 in section 3.6.2, is defined in terms of the value of u_* which is itself continuous.

2. $\check{R}$ indeed means ${}^{(ext)}\check{R}$. The point is that one can always extend ${}^{(ext)}\mathcal{M}$ up to $r = 2m_0(1+\frac{\delta_{\mathcal{H}}}{2})$. Then, $\{r = r_0\}$ should be understood as a hypersurface in ${}^{(ext)}\mathcal{M}$, $\cup_{r_0\in I_{m_0,\delta_{\mathcal{H}}}}\{r = r_0\}$ as a spacetime region of ${}^{(ext)}\mathcal{M}$, and $\{r = r_{\mathcal{T}}\}$ as the hypersurface in ${}^{(ext)}\mathcal{M}$ achieving the minimum. Once $r_{\mathcal{T}}$ is chosen, one restricts ${}^{(ext)}\mathcal{M}$ to $r \geq r_{\mathcal{T}}$.

   The question refers to section 3.8.9 with $I_{m_0,\delta_{\mathcal{H}}} := \left[2m_0\left(1+\frac{\delta_{\mathcal{H}}}{2}\right), 2m_0\left(1+\frac{3\delta_{\mathcal{H}}}{2}\right)\right]$.

3. You correctly assume that the choice (d) (ii) is made in section 6.2.1, and that the choice (d) (i) is made in (7.1.26). Thus, both choices should be allowed. Also, as written, one can indeed not necessarily satisfy d) i) or d) ii) if $r_{\mathcal{T}}$ is close to the boundaries of the interval $I_{m_0,\delta_{\mathcal{H}}}$. To fix this typo, it suffices to slightly enlarge the

matching region to strictly include the range of $r_{\mathcal{T}}$ while being outside the black hole. For example, one could replace Definition 3.22 by

$$\begin{aligned} Match \;\; := & \;\; \left({}^{(ext)}\mathcal{M} \cap \left\{ {}^{(int)}r \leq 2m_0\left(1+\frac{7}{4}\delta_{\mathcal{H}}\right)\right\}\right) \\ & \cup \left({}^{(int)}\mathcal{M} \cap \left\{ {}^{(ext)}r \leq 2m_0\left(1+\frac{1}{4}\delta_{\mathcal{H}}\right)\right\}\right). \end{aligned}$$

4. It is correct that all frames, and in particular the global frames, do fit in the formalism of section 2.1 for free, thanks to the assumption of axial polarization. And you correctly point out that relaxing these assumptions leads to consider non integrable frames. Such a formalism has in fact been introduced in [3] and is heavily used in [5] [6] [7]. Also, concerning the second global frame, it is correct that the spheres exit the bootstrap region. This, as can be easily seen, does not create extra problems.

5. The procedure in section 3.5.1 is in fact slightly off. This can be easily fixed by defining two regular frames where one covers ${}^{(int)}\mathcal{M}$ and most of ${}^{(ext)}\mathcal{M}$, and the other one covers ${}^{(ext)}\mathcal{M}$ and most of ${}^{(int)}\mathcal{M}$.

6. Yes, the individual terms appearing on the various RHS in Proposition 2.90 can be reinterpreted as elements of $\mathfrak{s}_k$. You can compare with the same computations done in full generality in Proposition 3.3 of [5]. Also, when applying the operator $\not{d}_1'$ to $\underline{\beta}$, there is a slight abuse of notation where $\underline{\beta}$ should in fact be replaced by $\underline{\beta}^\dagger$ with $\underline{\beta}^\dagger_{e'_a} = \underline{\beta}_{e_a}$.

7. In that equation, there is only one frame, and no change of frame is needed. For all curvature estimates in the proof of Theorem M8, the curvature components are expressed relative to the same global frame.

8. In Chapter 10, one simply needs a frame satisfying the assumptions Mor1-Mor3 (page 602), RP0-RP4 (page 657), RP5-RP6 (page 675) and the ones in section 10.4.1, so that the global frame of Proposition 3.26 works (in fact, the frame of Proposition 3.23 works as well and is better suited to deal with the Morawetz estimate for the wave equation). It is indeed implicitly assumed that the spheres are adapted to the frame and that r is the corresponding area radius, though this is certainly not necessary. Also, while the global frame and the frames defining the boundaries are not the same, they are close so that the difference generates small errors in the boundary terms that are easily absorbed.

9. Indeed, the second global frame cannot be used directly for the parabolic equation. This can be easily fixed by a change of frame to the frame tangent to Σ_*, with the extra terms being suitable errors thrown on the RHS.

2 Second exchange

This section includes the second exchange with a message of Mihalis-Gustav (December 20, 2021) in section 2.1 and the reply by Jérémie in section 2.2.

2.1 Second message by Mihalis-Gustav

1. We're not sure how to interpret your answer. There is no question that u_* is continuous as a function of u_*, since the identity function is continuous. The continuity necessary to infer the statement claimed at the very top of page 116, however, is the continuity of the norms as a function of u_*, and these have a built-in discontinuity, inherited from your construction (which you appear to have indeed now accepted). (Let us also point out that it is not only in the closedness but also in the openness that this issue appears. You wish to extend the spacetime and infer that there exists a u'_* in the set $\mathcal{U}$ for a $u'_* > u_*$. Even disregarding the discontinuity of the norm, in view of the jump of the quantity you denote as $r_{\mathcal{T}}$, the final ingoing null hypersurface may itself immediately jump to the future by a finite amount (independent of the smallness of initial data) and thus not even lie in the extended region which you want to infer from Theorem M7.) Thus, we repeat that, as a matter of principle, it does not appear possible to correct the proof of the Main Theorem along the lines currently sketched in Section 3.6 without changing its basic design so as not to be based on the discontinuous norms of Section 3.2. This issue lies at the very foundation of the architecture of your paper.

2. It would appear to us that alternative interpretations like the one you are now suggesting have their own problems. With your suggested re-interpretation, the right hand side of (8.3.2) concerns a frame which has not been directly estimated and a region which is not completely included in the exterior region defining the norms. Thus, for instance, with this interpretation, it would appear that the second inequality of page 399 could not possibly be correct.

3. In view of your correction, following our suggestion, of Definition 3.22, are we now to understand that you want to use two distinct "second global frames"? In any case,

which of the choices of global frame of Proposition 3.26 is being made to estimate $\mathfrak{q}$? If both are made, i.e. if the claim is that one has estimates for $\mathfrak{q}$ in each of the two frames of Proposition 3.26 then, in order to control the error terms which arise, one has to control all of the Ricci coefficients and curvature components of both of the these two double null frames. Alternatively, one could try to argue that the estimates for $\mathfrak{q}$ in one of these two frames implies the estimates for q in the other. Either way, it seems necessary, at some stage, to control the f, $\underline{f}$ and λ, which relate these two global double null frames. This does not seem to be discussed anywhere.

4. First of all, we note that you appear to accept that the polarisation assumption is being used in an essential way (on top of the axisymmetry) in the context of your argument. It is a pity that you never mentioned the connection of local integrability to your symmetry assumptions, indeed, that there is absolutely no remark on it anywhere. The omissions of any mention of global integrability is more serious, however, because it is not just an issue of omitted justification but it would seem to us that some parts of what is written are not in fact correct, with or without additional justification (see for instance the specific comments in point 8 regarding spheres leaving the domain to the future; such spheres cannot be estimated as a matter of principle). Thus, we really do not know how to interpret your blanket assertion "...it is correct that the spheres exit the bootstrap region. This, as can be easily seen, does not create extra problems." It manifestly does create extra problems, the question is how extensive must the corrections be to produce a complete correct argument. We could not possibly understand that until specific corrections to incorrect integrations are written down in detail wherever such integrations occur, so one can evaluate the necessary changes and modifications and track how they propagate to the rest of the argument. See in particular our remarks in point 8.

5. You accept that the definition of the global frame was not correct and must be modified. This affects all other frames derived from the global frame, and thus, potentially all estimates in the paper. Your suggested modification is, to say the least, vague. Thus, it would appear impossible to assess the impact of these changes unless they are made specific and then implemented. We note the following worrying fact: Despite the original definition of your frame not being differentiable, your main theorem asserted estimates at a rather high level of regularity. Thus, somewhere there must be (at least) two mistakes which cancel. Indeed, it is rather curious that the incorrect, non-differentiable construction of Section 3.5.1 never appeared as an obstacle in estimating higher order derivatives of quantities expressed in this frame, and this error raises the prospect that similar errors occur elsewhere. For some peace of mind, it would be nice to identify where exactly in the current version your non-differentiable frames were incorrectly quantitatively estimated, so as to be sure

that this mistake was not introduced in one of the dropped change of frame terms which appear ubiquitous in the paper (cf. points 2, 3, 8, 9, etc.).

6. We hope that you sympathise that one cannot expect the reader to check any of your change of gauge relations without a proper formalism where a specific prescription is given to write identities in terms of tensorially meaningful quantities in the notation of Chapter 2. We believe this to be a fundamental point that should not have to depend on subsequent papers which moreover lie outside the domain of polarised axisymmetry which is the setting of your paper. Even if one wants to rely on subsequent work for the completeness of this paper, as it appears you now do, then one must still translate this into something which can be understood in terms of the reduced equations of Chapter 2, for the benefit of the reader who wants to understand your paper as a self-contained document. As written currently, it is simply impossible to check a single identity when these involve change of frame and dropped terms. Given that we have found mistakes from a very cursory glance at those few things which we could check explicitly, we consider this to be an important point.

7. We do not understand your answer. Are the objects on the left hand side of the first equation of Section 8.7 not those defined in Section 3.2.1.1 and Section 3.2.2.1, involving the (non-global) interior and exterior frames? If all curvature estimates in the proof of Theorem M8 involve a global frame, how does one arrive at the statement of Theorem M8, which involves the (non-global) interior and exterior frames? Moreover, how does one use identity (8.3.2), which you say involves only ${}^{(ext)}\check{R}$ (not $\check{R}$ defined with respect to a global frame)? There should presumably be (linear) terms arising from the change of gauge, which do not seem to be discussed anywhere. (We note that in referring to the first equation of Section 8.7, we have simply picked a random example of an equation. We cannot tell exactly how ubiquitous such term dropping is, but on the basis of what we have seen, it appears to be widespread.)

8. Again, it is hard to interpret what you write in your response. We infer that you indeed accept that what is written in your paper is not correct, but a more explicit statement would have been welcome. We again repeat that this Chapter includes integrations over spheres which leave the bootstrap domain toward the future (as you have now accepted), and thus are not even included a priori in the spacetime. As a matter of principle, no estimate could possibly be deduced on such a sphere, and thus, the estimates themselves which are written are not correct, as far as we can see. (We note that even for those spheres which are fortunate enough to remain in the bootstrap region, their global integrability, global closeness, etc., would need to be deduced, r-weights tracked, etc. etc. It is worrying that there is no mention

of these issues at all.) In any case, we do not wish to speculate on what you intended. We can only judge what is actually written in the paper, not on potential corrections which you have in mind. We worry that an attempt to correct one issue often gives rise to others when an implementation is actually made. Thus, in our view, whether this Chapter can in principle be altered so as to be made correct (at least with respect to the issues pointed out here) would only really become clear if it were to be rewritten extensively. Note that, if additional changes of frame are introduced, it would not be possible to check them if these are not presented as tensorial relations between well defined quantities (cf. point 5).) It would be impossible for us to check other things in the Chapter until these issues are sorted.

9. You appear to accept that what is written in your paper for the proof of Theorem M3 is indeed incorrect. However, the very brief indication of a possible correction is vague and can only be assessed if these changes were actually implemented precisely. (If the correction is to involve yet another appeal to change of frame, introducing additional terms, then note that in our view these would have to be written with respect to a tensorially appropriate formalism in order for them to be in principle checkable (cf. point 5)

2.2 Jérémie's reply to the second message

Dear Gustav and Mihalis,

Thank you for your new set of questions and your sustained interest in our work. Glancing at them, I realize that you have misunderstood my original answers, but I am sure this can be easily fixed. Let me make another try, this time putting more emphasis on the general concepts that you might have overlooked:

- *Stability of Minkowski.* To help you understand my answer to your first question, I suggest that you read the corresponding part in Stability of Minkowski in the original version. Indeed, a Lebesgue point argument is also used there for the interior gauge in order to pick up the axis. I am sure that once you fully understand this aspect in the case of Stability of Minkowski, my answer will make perfect sense to you.
- *Local existence.* Your issues in your second question seem to be related to local existence. Indeed, given that we control frames on ${}^{(ext)}\mathcal{M}$, going from $r \geq r_{\mathcal{T}}$ to $r \geq 2m_0\left(1+\frac{\delta_{\mathcal{H}}}{2}\right)$ is just a local existence argument (for the geodesic foliation!) (in fact on a tiny region). I apologize for thinking that such routine arguments are superfluous in a book targeted towards advanced readers; your repeated questions

on this matter suggest that we should add a footnote next time we appeal to a standard local existence result in such situations.

- *Hyperbolic estimates.* This concerns your questions 4 and 8 which seem both to be concerned with spheres leaving the domain. It is important to realize that hyperbolic estimates depend very little on the frame you use, and certainly not on its integrability (even less on whether spheres leave the domain, given that frame integrability is absolutely not needed). Indeed, the only thing you need is:

  – good signs (or vanishing) for the spacetime integrals containing the deformation tensor of vectorfields constructed from the frame,

  – good signs for boundary integrals containing the scalar product of the frame with the normal to the boundary.

  Thus, nothing is required on spheres, which is an important aspect of hyperbolic estimates. Again, I apologize for thinking that these basic facts are not needed in a book targeted towards advanced readers; your insisting questions on this matter suggest that we should add a remark whenever we use hyperbolic estimates.

- *Global frames and curvature estimates.* Most of your remaining questions concern global frames in connection with curvature estimates. Please note the following:

  – It is clear from Proposition 3.23 d) i) and ii) that there are two global frames. Also, since these frames satisfy exactly the same control, it is immediate that any estimate performed with one frame can be performed with the other. In particular, it is unnecessary to control the change of frame between these two global frames (although that would certainly not be an issue anyway).

  – Given Proposition 3.23 d), the control of curvature in the global frames immediately yields the control of curvature in the frames of ${}^{(int)}\mathcal{M}$ and ${}^{(ext)}\mathcal{M}$.

  – All global frames are smooth (see my orignal answer 5).

  Given your repeated questions on this topic, we will make sure to mention the above points in future works in a footnote, of maybe even a remark.

Here are some additional remarks that you might also find helpful:

- *Concerning our subsequent works in connection to your question 6.* I just mentioned these references hoping that you would find the comparison helpful. But the formalism of Chapter 2, including the change of frame identities, is of course self-contained. Although this is in fact automatically inherited from the $3+1$ picture, one can certainly check that every identity comes from well defined tensorial quantities.

- *Concerning the role of polarized symmetry in your question 4.* While the formalism of section 2.1 is for integrable frames, as pointed out above, the integrability of the global frames is nevertheless totally irrelevant whenever they are used in the book. In fact, in that book, apart from ensuring that the final state is Schwarzschild, polarization is used in an essential way only to construct the GCM spheres (a fact that has been relaxed in our GCM papers for general perturbations of Kerr).

- *Concerning your worry on what you describe as serious potential mistakes.* As pointed out in my original answer, any correction, if needed, would require at most a one line correction or an additional clarification for your questions 3, 5, and 9, and likely none for the others. We will be happy to correct these typos in the next version of our book and warmly thank you for pointing them out to us.

Finally, I think our exchanges would be even more productive if you could not only glance at our book, but also read it in more depth. While it might look at first time consuming, I think it would in the end be worth it as you would likely be able to understand my original answers without even having to ask for more explanations. A graduate student would probably need a week to figure this out, but given your expertise, I expect that you likely need much less.

3 Further explanations

Here are some further explanations regarding the 9 questions raised by Mihalis-Gustav:

1. *Regarding question 1*: Note that this question is settled in section 4.15[3]. In the Lebesgue point argument (8.3.2), the value $r_{\mathcal{T}}$ is allowed to vary in the region $I_{m_0,\delta_{\mathcal{H}}}$ in (8.3.1) which is of size $\delta_{\mathcal{H}}$. To make sure that the extended spacetime of Theorem M7 is a true extension, it suffices to notice that once an ${}^{(int)}\mathcal{M}$ region initialized at $r = r_{\mathcal{T}}$ with some $r_{\mathcal{T}}$ in $I_{m_0,\delta_{\mathcal{H}}}$ is covered, then, the standard local existence argument used at the beginning of Theorem M7 strictly includes a region much larger than the size $O(\delta_{\mathcal{H}})$ needed to cover the corresponding ${}^{(int)}\mathcal{M}$ regions for any value of $r_{\mathcal{T}}$ in $I_{m_0,\delta_{\mathcal{H}}}$.

[3]Where Mihalis states "let me state unequivocally, for whatever its worth, that I personally now find the foundational architecture of your paper, as such, not only to be in principle unproblematic, but in fact to be very interesting, highly original and in particular quite different from that of [CK] or other works I know" (see section 4.15).

2. *Regarding question 2*: The issue raised above concerns the meaning of the right hand side of the equation

$$\int_{\{r=r_{m_0,\delta_0}\}}|\mathfrak{d}^{\leq k_{large}}\check{R}|^2 \quad = \quad \inf_{r_0\in I_{m_0,\delta_{\mathcal{H}}}}\int_{\{r=r_0\}}|\mathfrak{d}^{\leq k_{large}}\check{R}|^2,$$

where $\check{R} = ^{(ext)}\check{R}$. Indeed $^{(ext)}\mathcal{M}$ is only defined for $r \geq r_{\mathcal{T}}$ with $r_{\mathcal{T}}$ fixed in the interval $I_{m_0,\delta_{\mathcal{H}}}$. To make sense of the definition, it suffices to extend, using a trivial local existence argument, the geodesic foliation of $^{(ext)}\mathcal{M}$, slightly into $^{(int)}\mathcal{M}$, to cover the region $r \geq 2m_0\left(1+\frac{\delta_{\mathcal{H}}}{2}\right)$. This kind of extension is done repeatedly in the book (see for example section 3.5.1). In particular the inequality, on page 399,

$$\left(\int_{\mathcal{T}}|\mathfrak{d}^{k_{large}}(^{(ext)}\check{R})|^2\right)^{\frac{1}{2}} \lesssim \left(\int_{^{(ext)}\mathcal{M}\left(r\in I_{m_0,\delta_{\mathcal{H}}}\right)}|\mathfrak{d}^{\leq k_{large}}\check{R}|^2\right)^{\frac{1}{2}} \lesssim {}^{(ext)}\mathfrak{R}_{k_{large}}[\check{R}]$$

is perfectly justified.

3. *Regarding question 3*: Note that Proposition 3.26 does not just provide the construction of one global null frame, but in fact of a family of global frames. In particular, Theorem M1 provides decay estimates for $\mathfrak{q}$ in any global null frame constructed from Proposition 3.26, and in particular in a global frame satisfying either the properties d) i. or d) ii. of Proposition 3.26. This remark is in fact used often whenever a global null frame is involved. Finally, even though this is not needed, one can, of course, control the change of frame coefficients $(f, \underline{f}, \lambda)$ relating such two global null frames as can be seen immediately from the proof of Proposition 3.26 in section 4.6.1.

4. *Regarding question 4*: Though we perform the symmetry reduction form the very beginning in Chapter 2, which in retrospect is unnecessary, all the arguments could have been done with respect to the standard spacetime formalism used in stability of Minkowski. The only place where the polarization is used in an "essential way" is in the construction of GCM surfaces (in Chapter 9) where polarization simplifies the construction (the polarization was later completely removed in [5]). Apart from that, the only reason polarization is used is to ensure, as in [9], that the spacetime has to converge to a Schwarzschild solution. In particular, except for the construction of GCM surfaces in Chapter 9, all estimates are done exactly as in the spacetime formalism.

   Concerning the misconception about non integrability of spheres, note that the global null frame used in chapter 10 is consistent with the first global frame of Proposition 3.23: this frame is non integrable only in the matching region (where the decay is uniform in all directions). The only place in the proof of the Morawetz estimate which invokes the spheres $S = S(\tau, r)$ is the Poincaré inequality of Lemma 10.18, which is only needed between $r = 3m$ and $r = 4m$ where the frame is integrable, see also further discussions in section 2. Additionally, note that even if the spheres were to slightly exit the bootstrap region this would still not be a problem, as one could then simply extend the spacetime by a trivial local existence result which would at most double the constant in the bootstrap assumptions.

5. *Regarding question 5*: As written, the global frames as defined in Proposition 3.23 and 3.26 are indeed non smooth at the upper end of $\mathcal{T} = {}^{(int)}\mathcal{M} \cap {}^{(ext)}\mathcal{M}$. The point is that the frame of ${}^{(ext)}\mathcal{M}$, when extended inside ${}^{(int)}\mathcal{M}$, only covers the region ${}^{(int)}\mathcal{M} \setminus \mathcal{R}_0$ where $\mathcal{R}_0$ is a small causal local existence type region included in ${}^{(int)}\mathcal{M}(\underline{u} \geq u_* - 1)$. The trivial fix is to do the matching away from that region so that the resulting global frames in Proposition 3.23 and 3.26 are smooth but only cover $\mathcal{M} \setminus \mathcal{R}_0$. Whenever these global frames are used (i.e. in Chapter 5 and Chapter 10), they thus allow to derive estimates only on $\mathcal{M} \setminus \mathcal{R}_0$ (exactly as written) and the remaining estimates on the small causal region $\mathcal{R}_0$ of ${}^{(int)}\mathcal{M}$ follow immediately by a trivial local existence argument (in the frame of ${}^{(int)}\mathcal{M}$).

6. *Regarding question 6*: As we have explained in item 4 above, the entire paper could have been written without the polarization reduction. In our reply we referred to Proposition 3.3 of [5] as a possible way to check the calculations of Proposition 2.90 and not as a justification of the result itself. Our proof, in the reduced setting is explained in detail in the appendices A6–A7. Finally, note the slight abuse of notation where we identity $\underline{\beta}^\dagger_{e'_a} = \underline{\beta}_{e_a}$ as well as a similar identification for all the other quantities, as explained in our response.

7. *Regarding question 7*: This is the same question as question 3. To repeat, first, notice that the global null frames in Proposition 8.4 only differ in the matching region. Then, notice that Proposition 8.9 provides decay estimates for the curvature components in any global null frame constructed from Proposition 8.4. Then, the objects on the left hand side of the first equation of Section 8.7 are indeed those defined in Section 3.2.1.1 and Section 3.2.2.1, simply because one can choose the global frames of Proposition 8.4 to either coincide in ${}^{(int)}\mathcal{M}$ with the frame of ${}^{(int)}\mathcal{M}$ or in ${}^{(ext)}\mathcal{M}$ with the frame of ${}^{(ext)}\mathcal{M}$ in view of property d) of Proposition 8.4. In particular, there is definitely no need to go from one frame to the other in the first equation of Section 8.7.

8. *Regarding question 8*: This is connected to question 4 about non integrability of spheres. Note that the derivation of the wave equation for $\mathfrak{q}$ done in Theorem 2.108 is done in the global from of Proposition 3.26. On the other hand, the estimates in Chapter 10 do not need to be done in that global, and can be done in any frame satisfying the assumptions Mor1-Mor3 (page 602), RP0-RP4 (page 657), RP5-RP6

(page 675) and the ones in section 10.4.1, which are consistent both with the global frame of Proposition 3.23 and of Proposition 3.26. At the beginning of Section 10.5, it is carelessly stated that we use the global frame of Proposition 3.26, when in reality the global frame of Proposition 3.23 perfectly appropriate and easier to deal with. Indeed, the global frame of Proposition 3.23 is non integrable only in the matching region (where the decay is uniform in all directions). The only place in the proof of the Morawetz estimate which invokes the spheres $S = S(\tau, r)$ is the Poincaré inequality of Lemma 10.18, which is only needed between $r = 3m$ and $r = 4m$ where the frame is integrable.

In any case, even if we were to use the global frame of Proposition 3.26, for which the spheres exit slightly the bootstrap region, there are two obvious ways to do it:

- One could simply extend the spacetime by a trivial local existence result, so that the spheres are included in the extended spacetime, and such that the bootstrap assumptions still hold true.
- A second way is to work with a frame not adapted to the spheres $S(r, \tau)$ in which case the Poincaré of Lemma 10.18 must be slightly modified as follows:

$$(2 - O(\epsilon))r^{-2}\int_S |\Psi|^2 \leq \int_S |\nabla\!\!\!/\Psi|^2 - O(r^{-2}\epsilon)\left(\int_S |\nabla_3\Psi|^2 + \int_S |\nabla_4\Psi|^2\right). \quad (3.1)$$

  Note that the additional error terms on the right hand side are compatible with all the other error terms generated in our proof of the Morawetz estimate. Estimates of the type (3.1) are in fact constantly used in [7] and [8] where lack of integrability of our frames is at the heart of the matter.

9. *Regarding question 9*: The change of frame from the global frame of Proposition 3.26 to the frame tangent to Σ_* involves the change of frame coefficient f satisfying (3.4.11). To go from the equation in Proposition 6.14 to one with derivatives tangent to Σ_* generates, in addition to the RHS F_k, terms of the form $r\mathfrak{d}^{\leq 3}(f \cdot \Gamma_b)$. One can immediately check from (3.4.11) and the estimates for Γ_b that these quadratic error terms satisfy the same estimates as the one for F_k in Proposition 6.14.

4 Further exchanges concerning the first question

4.1 Email from Mihalis January 7

Dear Jérémie,

Thanks for your message. We are a bit surprised, however, by your response. You have entirely avoided addressing any of our specific points.

The issue is not one of general concepts. We are of course very familiar with the general concepts which you outline. Our issues are with specific lines in the paper which appear to us to be incorrect, and for which it would appear that any possible correction would necessarily require further changes which propagate to other parts of the paper.

So as to focus on mathematical content, perhaps it would be better to take our questions one by one. So let us try to first resolve (the first part of) our first question.

To repeat what we wrote, it appears to us, in particular, that the deduction

"Then, by continuity of the flow, $U_* \in \mathcal{U}$."

(at the top of page 116) is incorrect, as this deduction clearly would seem to have to invoke the continuity of the norms $\mathfrak{N}$ (since the set $\mathcal{U}$ is itself defined by an inequality to be satisfied by $\mathfrak{N}$; see Definitions 3.28 and 3.29), and we agree now that the norms $\mathfrak{N}$ are not in fact continuous as a function of u_*.

In your latest response, you suggest that you believe your argument to be correct as written!

We would appreciate it if you could then explain exactly how you obtain the deduction $U_* \in \mathcal{U}$.

(We can discuss the issues concerning the openness part once this is cleared up and then move on to our other questions 2.-9.)

Best wishes,

Mihalis and Gustav

PS. We are very familiar with the corresponding argument in the proof of the stability of Minkowski space [CK], which does not suffer from the problem that your argument appears to suffer from. Indeed, in [CK], it would appear to us that the continuity argument has been specifically designed so as not to refer to norms connected to the interior optical function, which is non-unique and thus discontinuous. This is possible because the t-foliation and exterior u foliation are canonical, while for the interior, one may rely on norms connected to the electromagnetic decomposition (which depends only on t). It is only such quantities that appear in the relations defining the set $\mathcal{S}$ of Section 10.2 of [CK]. In fact, it is clear that a lot of thought has gone into the architecture of the proof of the main theorem of [CK] as described in Section 10.2, precisely because of this issue. We believe that it is not possible to correct the proof of the main theorem of your paper without rewriting extensively, introducing additional background structure which does not suffer from this discontinuity, and then showing that your discontinuous structure can be suitably bounded from the continuous structure. Any statement required should be stated precisely so the reader can check that the proof of the main theorem is indeed reduced to well defined substatements each of which could in principle be checked.

4.2 Jérémie, January 8 at 11:09 p.m.

Dear Mihalis and Gustav,

Thanks for your message. Indeed, the misunderstanding was not only about the stability of Minkowski, but in fact about something much more basic!

Let me explain why $U_* \in \mathcal{U}$. First, consider a sequence of spacetimes $\aleph(u_*^n)$ as in Definition 3.28 with $u_*^n \to U_*$ as $n \to +\infty$. Then the corresponding sequence $r_{\mathcal{T}}^n$, that seems to concern you so much, lies in the closed and bounded interval on top of page 93. At this point it is useful to recall that a closed and bounded interval in $\mathbb{R}$ is in fact compact which allows you to extract a converging subsequence $r_{\mathcal{T}}^{n_k}$. One then obtains immediately that $U_* \in \mathcal{U}$. I hope my answer is specific enough.

Concerning the proof of the stability of Minkowski, one of the authors tells me that continuity of the norms in the bootstrap assumptions plays no role, is never mentioned in the proof, and does in fact not hold. Indeed, the auxiliary bootstrap assumption on top of page 301 is part of the definition of $\mathcal{S}$ and depends manifestly on the interior optical function so that it is not continuous.

Finally, I want to thank you for your friendly concern that something may be missing in our proof, and hope that this and my previous e-mails will help to set your mind at ease. Meanwhile, allow me to repeat that while I am always happy to answer your questions, I would appreciate if you could respond in kind regarding the discussion I had with Igor two months ago. In case that, after such a long time, you may have forgotten the details of that discussion, I attach a file summarizing it.

Best wishes, Jérémie

4.3 Mihalis January 8 at 11:50 p.m.

Dear Jérémie,

There is no question that (some subsequence of) $r_{\mathcal{T}}^{n_K}$ converges to some r-value. Because of lack of continuity of the definition of $r_{\mathcal{T}}$, however, it does not however necessarily converge to the specific value $r_{\mathcal{T}}^{U}$. Thus, it would appear that the corresponding norms $\mathfrak{N}(U_*)$ may be strictly bigger than ϵ, and as a result, U_* may not lie in the set $\mathcal{U}$ (membership in which depends on the value of $\mathfrak{N}$). This is our concern.

(Regarding the proof of stability of Minkowski space, I suspect that the auxiliary assumption is in fact, like the other assumptions BA_0 to BA_2, not meant to refer to the interior foliation, and that this was simply a typo. (This is strongly suggested by the fact that the interior foliation is only introduced in Step 2 of page 301, after the statement "Clearly $S \in \mathcal{S}$" which, it would appear to me, is precisely an appeal to continuity.) If one looks at how the auxiliary assumption is used in the proof, this indeed seems to only concern the exterior region, and thus I suspect the supremum there is meant to be taken only on the exterior region. But in any case, the proof of [CK] is not really my concern here.)

Best wishes, Mihalis

P.S. Your attachment file was corrupted and I could not open it.

4.4 Jérémie January 9 at 11:53 a.m.

Dear Mihalis,

$r_{\mathcal{T}}$ is any number in the interval on top of page 93 so that my explanation is perfectly correct. I hope you agree so that we can move on with your other misconceptions.

While I am not concerned about the proof of CK either, I very much doubt that the auxiliary bootstrap assumption on top of page 301 is a typo. You are welcome to check!

Concerning the attachment, it is likely that Gustav has been able to open it.

Best wishes, Jérémie

4.5 Mihalis January 9 at 12:07 p.m.

In the message below, the message of Mihalis is in red and uses the text of the previous message by Jérémie which is in black.

Dear Jérémie,

$r_{\mathcal{T}}$ is any number in the interval on top of page 93 so that my explanation is perfectly correct. I hope you agree so that we can move on with your other misconceptions.

I understand from your footnote 3 that a specific choice must be (and has been) made. It is this choice that is discontinuous and gives rise to the problem which I described.

While I am not concerned about the proof of CK either, I very much doubt that the auxiliary bootstrap assumption on top of page 301 is a typo. You are welcome to check!

I have in the meantime, and I am convinced that the auxilliary bootstrap assumption is meant to refer only to the exterior foliation, just like the other bootstrap assumptions manifestly do. (It only appears to be used in Chapter 14 and I think this is also clear from the context, since the interior optical function has not yet been defined in the logic of the proof, as I wrote in my last email.) Also, concerning [CK], do you not agree that the phrase "Clearly, $t* \in \mathcal{S}$" in Step 2 is an appeal to continuity, as is the argument in Step 4? Do you actually think that continuity is not used?

Concerning the attachment, it is likely that Gustav has been able to open it.

I will check with him.

Best wishes, Mihalis

4.6 Jérémie January 9 at 12:28pm

Dear Mihalis,

A member of $\aleph(u_*)$ is one for which there is a $r_{\mathcal{T}}$ in the interval on top of page 93 without any specific choice. Then, if you want to extend the space-time for some $u'_* > u_*$, this is where you make a specific choice in that interval. So the argument is correct as written. A look at Remark 3.30 would certainly help you, as would a less superficial reading.

Concerning CK: we also have clearly $U_* \in \mathcal{U}$ despite discontinuity in the norms, and the same applies to CK. Given the level of your misconceptions with our book, I suspect you also might have some with CK.

Best wishes, Jérémie

4.7 Mihalis, January 9 at 12:31 p.m.

Dear Jérémie,

If you want to redefine the set like that, then it would appear that for the particular value of $r_{\mathcal{T}}$ that you end up choosing, the inequality of the bootstrap (which you certainly use everwhere) does not necessarily hold.

Best wishes, Mihalis

4.8 Jérémie, January 9 at 12:48 p.m.

Dear Mihalis,

I don't redefine, it is defined like that.

Concerning the second part of your sentence, I reiterate my advice to read the book less superficially. In particular, why don't you look at the place where the choice is made and check for yourself? Alternatively, you can ask one of your graduate students to help you. In fact, I suspect, as I mentioned in a previous e-mail, that you are stuck at the concept of local existence, and I'm sure any of your graduate students could easily help you with that.

Best wishes, Jérémie

4.9 Mihalis, January 9 at 12:53 p.m.

Dear Jérémie,

It looks to me from Remark 8.2 that you are actually defining it as "for every" and not "for some". Do you agree?

Best wishes, Mihalis

4.10 Jérémie, January 9 at 10:04 p.m.

Dear Mihalis,

This is already the seventh e-mail I receive from you, including five versions of the same question. Given the naïve nature of the issues you keep raising and the puzzling fact that they come from an expert at a top institution, it is hard not to suspect that you are in fact making fun of me.

I suggest that from now on all issues concerning our papers should be handled by Igor. He could filter out all questions you and the other members of your team have and send me only those he considers reasonable. This will save me a lot of time.

Let me now answer your latest misconception about our work:

Once you have established in Theorem M0-M7 that for a given $r_{\mathcal{T}}$ your bootstrap assumptions on decay have been improved from ϵ to $O(\epsilon_0)$, then, it is a triviality to obtain that for any $r_{\mathcal{T}}$ in the interval on top of page 93 one has an $O(\epsilon_0)$ improvement. I leave it to you as an exercice. If you can not do it yourself, please proceed as suggested above. I am quite certain that Igor will be able to explain it to you directly rather that sending me the sixth version of the same question.

Best wishes, Jérémie

4.11 Mihalis, January 10 at 11:53 p.m.

Dear Jérémie,

First of all, I have to say that I am a little bit surprised by the tone of your last few messages. But I really don't take any of that personally, and I am more than happy to leave those comments aside and move on from them.

As you recall, originally Gustav and I offered to talk to you over the phone. I think this would have resulted in both a more efficient and warmer, more personal form of communication between the three of us. If we had been able to communicate to each other in real time, I think that we would have all figured out earlier, say with regard to point 1., that we were interpreting a definition in a different way than was evidently intended.

I'm sorry that some of our questions on this particular point evidently sounded so silly to you, but please understand that your answers seemed to us quite incomprehensible given how we were interpreting what was written in the paper (and I am happy to explain to you why, for us at least, at various very crucial points of your paper, what is written is quite ambiguous). Moreover, you appeared to accept what I had said originally, namely that the norms of the bootstrap assumption do not depend continuously on U_*, whereas, in the sense that I mean it, if I now understand correctly, they do depend continuously. Please understand that your original answer to our question only confirmed to me our (false as it turns out) interpretation of your use of the notation $r_{\mathcal{T}}$. Finally, referring to stability of Minkowski space [CK] caused even more confusion to us, because, again if I understand correctly, the logic of the proof of the main theorem in both cases is fundamentally different (though in both cases, very much a continuity argument!).

All I am saying is that a little bit of patience is not a bad thing. If there is a point of misunderstanding, or, if you prefer, misconception, it is more likely that it is caused by something very specific in the text than because we do not understand stability of Minkowski space, the concept of local existence, hyperbolic estimates, or I don't know what. Perhaps all our comments are indeed "misconceptions". But perhaps they are not. Engaging thus with the precise text of our questions is the quickest route for clarification, and if it sometimes requires a quick round of brief clarifications over e-mail, from my perspective, so be it!

We originally looked at your paper precisely because you asked us to add more specific comments in [DHRT] referring to it. As Igor may have told you, we are more than happy to indeed include some such comments, and to modify the precise wording of some references. In looking at your paper, it was only natural for us to also look more carefully at certain aspects which are of particular interest to us. I would hope that you would welcome the opportunity for knowledgable readers on the subject of black hole stability to ask questions and make comments. I hope you give us the benefit of the doubt that there is content behind our questions and not try to simply brush them off. We are certain that with all of our comments, a common understanding of their status can be reached.

As I wrote above, I really don't take personally the unfortunate things you wrote (directed at me) in your last few messages and am more than happy to leave them aside. I am saddened, however, by one thing in particular: That you suggest that answering my messages is somehow unworthy of your time. I do hope you change your mind on that. As you might imagine, I am also very taxed for time, but, as a matter of principle, I cannot imagine writing that to anyone, let alone a scientific colleague. From my own perspective, however, let me reassure you that I am always happy to discuss with you over email, skype of phone about anything.

Best wishes,

Mihalis

4.12 Jérémie, January 10 at 11:56 p.m.

Dear Mihalis,

First of all I have to say that I was quite a lot more than "a bit surprised" by the tone of your first two sets of questions which included sentences such as:

- it would appear, simply as a matter of principle, that one cannot base a continuity argument on these norms and one would have to redesign the basic constructions.
- Any other interpretation seems to lead to even more issues.
- This puts into question the entire chapter.
- ... thus the results of the entire section are put into question.
- Thus, we repeat that, as a matter of principle, it does not appear possible to correct the proof of the Main Theorem.
- This issue lies at the very foundation of the architecture of your paper.
- It would appear to us that alternative interpretations like the one you are now suggesting have their own problems.

And this is just a small sample of your overall very unpleasant messages. You would pardon me if I interpreted these questions as aggressive, arrogant and in bad faith written with some kind of delight in destroying our work. There was nothing collegial or professional in those statements, nothing that betrayed a genuine interest in our work. In my case it is indeed " a matter of principle" that "I cannot imagine writing like that to anyone, let alone a scientific colleague".

The disrespectful way I wrote back to you in annoyance is something that I never did to anybody else, and I sincerely regret that I had to do it. But you could have asked the same questions without the presumption that we are idiots who don't even understand how to make a proper continuity argument. Not to speak of all your other questions which were equally insulting.

I also don't take any of that personally, and I am more than happy to leave those comments aside and move on from them. But it can only work if you completely change your attitude towards our work. To avoid further misunderstandings, I maintain that it is preferable from now on that Igor sends me your questions. I am sure he will be interested in the discussion anyway and that this will reduce the number of miscommunications drastically.

Best wishes, Jérémie

4.13 Mihalis, January 11 at 4 a.m.

Dear Jérémie,

There is nothing aggressive, insulting, unpleasant or in bad faith about any of these statements. They are not written with any kind of "delight". None of these sentences would be out of place in a referee report. On the contrary, they reflect precisely the formal language of professional discourse. Please try to reread for yourself any of these sentences with an open mind, in parallel with what you wrote in response, to see the difference. (I would rather not even repeat here as examples some of your own sentences referring to me here.)

We studied your work after your own requests for us to make comparisons. We saw a number of issues, some of which appear, on the surface at least, to be reasonably serious. We explain these issues, always careful to stress that this is how they appear to us. Maybe they all can indeed be relatively easily repaired, or are even due to "our misconceptions". By lashing out at questions, or brushing them off dismissively, you give to us a different impression, that you are annoyed by the idea itself of scrutiny of your work.

I am happy that you yourself admit that your messages were disrespectful. You write that you regret that "you had to do it". In my view, however, no one "has to be" consciously disrespectful. I too have been annoyed many times in responding to "referee report" type questions. Responding by being personally disrespectful, in my opinion, is never justified by annoyance.

Anyway, as I said, I don't take seriously your personal comments about me and I am happy to put them behind us. But please don't try to create a false equivalence between our sentences and yours.

Best wishes, Mihalis

4.14 Jérémie, January 11 at 4:39 p.m.

Dear Mihalis,

I believe very few referee reports are as nasty and dismissive as your comments addressed to me. Moreover you were not supposed to referee our book, which was already refereed and published, but to attempt to understand the way in which it compares to DHRT.

I am happy to see that you now acknowledge some issues " could be on the surface only", and " maybe can indeed be easily repaired" as this statement is nowhere to be found in your previous communications to me. Having read your questions, I can assure you that none are issues at all, while a few are typos, as was already pretty clear from my first answers. Given the speed at which you sent me new questions, I doubt that you seriously read them though (your six last e-mails came back very fast to me, some coming even few minutes after you had received mine).

The insinuation that I am somehow " annoyed by the idea itself of scrutiny of our work" is quite astonishing. I already answered seven e-mails, which is not too bad for someone trying to avoid scrutiny. And by the way, " scrutiny" betrays your true intensions, despite the fake pretense of a genuine interest in our work. I personally do not recall asking you to " scrutinize" our work, but to properly quote it. In any case, after seven exchanges of e-mails, it appears that the issue in your first question that you dubbed as:

" it would appear, simply as a matter of principle, that one cannot base a continuity argument (as sketched in Section 3.6.2) on these norms and one would have to redesign the basic constructions"

" Thus, we repeat that, as a matter of principle, it does not appear possible to correct the proof of the Main Theorem along the lines currently sketched in Section 3.6 without changing its basic design so as not to be based on the discontinuous norms of Section 3.2. This issue lies at the very foundation of the architecture of your paper "

was in the end unsurprisingly not an issue but in fact correct as written. Given that you have 8 more questions, at your pace, the remaining questions may well require 50 more e-mails and I certainly don't have the stamina to go through that. This is why I suggested to go through Igor. This would control both the pace and your tone, and give you more time to think about my answers. Another possibility is that I discuss directly with Gustav, for example by zoom, as I suggested from the very beginning. I understand he glanced through our book with you. I would be happy to elaborate my answers to him. That would spare Igor, if he prefers not to get involved, and would be another opportunity for you to check whether I am " annoyed by scrutiny" . In that case, I'll let

Gustav contact me directly to set up a zoom meeting.

Finally, I am absolutely not comparing my sentences to yours. Yours were far worse.

Best wishes, Jérémie

4.15 Mihalis, January 12

Dear Jérémie,

Thanks for your message. I appreciate that you are expressing how you are genuinely feeling, but I again find what you write to be unbelievably unfair. Let me not say anything more about this as I would really like to put this unpleasantness all behind us.

I very much hope that we can eventually come to a common understanding of the status of all our questions, through any suitable format, because, as we said, some of our questions do appear to us to concern serious issues whose resolution appears unclear. Again, it goes without saying that these issues reflect our understanding, and our understanding may always be incorrect. And again, it goes without saying that pointing out that a certain mathematical statement appears to be incorrect is not equivalent to a personal attack on the authors or a statement about their competence. Gustav and I have the highest possible scientific admiration for you and we are all connected to one another by very strong scientific bonds. Regarding the format for continuing, if you prefer not to receive any emails from me in the future, Gustav could of course take over handling of this email correspondence. At this stage, however, at the beginning at least, we both think it would be better for this to continue over email, if it is not to be all three of us.

Finally, so that the progress that we have in fact already made concerning point 1. of our remarks is not lost, in the rest of this email, let me just summarise the current status of the issue from our perspective.

Our original questions and comments concerning point 1. were based on an interpretation of what you write in the paper which, as we have established now through our exchange of emails, was not what was intended. With your clarification, let me state unequivocally, for whatever it's worth, that I personally now find the foundational architecture of your paper, as such, not only to be in principle unproblematic, but in fact to be very interesting, highly original and in particular quite different from that of [CK] or other works I know. Still, let me emphasise that our previous interpretation appears to us to also be entirely consistent with what is actually written, and was strongly suggested to us by the precise formulation and position of several remarks in the paper, as well as by the notation, and would have indeed suffered from the problem we described. This is why it was only once I started asking you specifically whether you agreed with our interpretation of the meaning of certain remarks in the paper that the ambiguity was broken and it became clear where the misunderstanding lay. (And let me emphasise that if you go back to our exchange, you will see that, not only was I not ignoring your answers, on the contrary, I was precisely engaging with them, and it was this engagement that led to things becoming clear. Thus, I don't see why you refer to this exchange so negatively; contentwise, it has been perhaps the most useful one so far.)

Now, from our part, concerning point 1., there is something that appears to us to may require an additional argument. If the argument at the very end of Theorem M7 is meant to refer to a new $r_{\mathcal{T}}$ which is not the same as the $r_{\mathcal{T}}$ implicit in the assumption of the theorem, then we do not completely understand the justification given. We would have expected to see reference to estimates of the transition functions between the exterior and interior frames since it would appear to us that estimates on the exterior region as defined with the new $r_{\mathcal{T}}$ may have to be deduced from the interior region as defined from the old one. It appears to us that all estimates referred to at that point are from exterior frame to exterior or from interior frame to interior. Finally, the new region could in principle protrude to the future out of $\mathcal{A}$ by an amount of order not controllable by the smallness of the extension parameter, precisely because the $r_{\mathcal{T}}$ values are different. Thus, it would appear to us that this would need an additional evolutionary estimate through the full length of $\mathcal{A}$, which would in particular lose some derivatives (if one is only using now L^∞ estimates) beyond those stated currently.

That closes the current summary of our understanding regarding issue 1. Let me conclude this message by reaffirming the sentiments of my first paragraph that it is important for tension to ease, not be exacerbated, so that these issues can be put behind us, and I am prepared to do anything possible on my part to help with that.

Best wishes, Mihalis

4.16 Jérémie, January 13

Dear Mihalis,

Thanks for your message. Let me first comment on your mention of an additional argument required regarding question 1. It indeed proceeds by comparing the inner and outer region in the spacetime region $r \leq 2m_0(1 + \frac{3}{2}\delta)$, and then invoking local existence for the possibly remaining uncovered region which is of width $O(\epsilon \underline{u}^{-1-\delta})$ in r. And yes, that would indeed loose a fixed amount of derivatives which is irrelevant as we started

Theorems M1-M7 with a lot more derivatives compared to k_{small} than needed, and we could of course start with even more if necessary. At this stage of the paper, the change of frames for the comparison have been done many times already in that chapter and previous chapters and is simpler here as it is only needed near the horizon, while the local existence argument is trivial due to the $O(\epsilon \underline{u}^{-1-\delta})$ width in r, so we skipped it, but it could certainly have deserved a footnote along these lines.

This brings me to the fact that in the process of asking further questions, I would appreciate if small issues as above are treated as such. In a 850 pages book providing a lot of details on involved estimates, it is understood that minor arguments are not always explicitly written. I got the impression from the two sets of remarks you sent me that you made a big deal of tiny things. As of now, we can certainly conclude question 1 was correct, and that for comprehensiveness, one could have added a footnote as discussed above.

Going forward, given that you want to continue by e-mail, I am ok to have from now on e-mail exchanges between Gustave and me. As we also have a number of questions on DHRT that appear substantial to us, this will be the opportunity to send them to him. I suggest though that we postpone the discussion until after you have let us know what modifications you are prepared to make in connection to the discussion I had with Igor. Given that this discussion took place more that two months ago, it seems reasonable to settle that issue first.

Best wishes, Jérémie

4.17 Mihalis, January 16

Dear Jérémie,

Thanks for your message.

As I mentioned earlier, we are happy to add some references and modify some sentences in our paper [DHRT]. If you are eager to see these quickly, I expect that we will be able to send you something in about a week's time.

Needless to say, if you have any other comment or question about [DHRT], feel free to email me or any of the other authors at any time! We are all always very happy to try to answer any question, major or minor, that our readers may have.

Finally, I am happy that concerning point 1., we now seem to agree as to what is missing from the proof. I don't doubt that the missing pieces can be provided with arguments similar (though as far as I can see not quite identical) to what's done in other places in the paper, and indeed, the additional loss of derivatives resulting from applying a further existence result would only require a change in the precise numerology currently stated. Moreover, we certainly agree that this further existence result is not difficult, but, besides the issue of change in numerology, we would have thought it important to bring it to the attention of the reader how a detailed proof would appear to have to appeal to some of the specifics of the bounds on the transition functions and on the geometry of $\mathcal{A}$. Obviously, the precise way you would deal with this were you to ever revise your paper is entirely up to you. I do want to emphasise again that it was precisely the fact that the expected necessary arguments related to the changing of $r_{\mathcal{T}}$ did not appear anywhere, even in sketch form, which made the logic of the proof appear to be so ambiguous.

Best wishes, Mihalis

5 Errata and clarifications to [4]

We thank M. Dafermos and G. Holzegel for bringing to our attention a few small errors in our book [4], as well as some places that require clarifications. The small corrections are discussed in section 1 and the clarifications are given in section 2.

5.1 Small corrections

1. As written, in Proposition 3.23, one can not necessarily satisfy d) i) or d) ii) if $r_{\mathcal{T}}$ is close to the boundaries of the interval $I_{m_0,\delta_{\mathcal{H}}}$. To fix this typo, it suffices to slightly enlarge the matching region to strictly include the range of $r_{\mathcal{T}}$ while being outside the black hole. For example, one could replace Definition 3.22 by

$$\begin{aligned} Match \ &:= \ \left({}^{(ext)}\mathcal{M}\cap\left\{{}^{(int)}r\leq 2m_0\left(1+\frac{7}{4}\delta_{\mathcal{H}}\right)\right\}\right)\\ &\quad\cup\left({}^{(int)}\mathcal{M}\cap\left\{{}^{(ext)}r\leq 2m_0\left(1+\frac{1}{4}\delta_{\mathcal{H}}\right)\right\}\right). \end{aligned}$$

2. As written, the global frames as defined in Proposition 3.23 and 3.26 are non smooth at the upper end of $\mathcal{T}={}^{(int)}\mathcal{M}\cap{}^{(ext)}\mathcal{M}$. The point is that the frame of ${}^{(ext)}\mathcal{M}$, when extended inside ${}^{(int)}\mathcal{M}$, only covers the region ${}^{(int)}\mathcal{M}\setminus\mathcal{R}_0$ where $\mathcal{R}_0$ is a small causal local existence type region included in ${}^{(int)}\mathcal{M}(\underline{u}\geq u_*-1)$. The trivial fix is to do the matching away from that region so that the resulting global frames

in Proposition 3.23 and 3.26 are smooth but only cover $\mathcal{M}\setminus\mathcal{R}_0$. Whenever these global frames are used (i.e. in Chapter 5 and Chapter 10), they thus allow to derive estimates only on $\mathcal{M}\setminus\mathcal{R}_0$ (exactly as written) and the remaining estimates on the small causal region $\mathcal{R}_0$ of ${}^{(int)}\mathcal{M}$ follow immediately by a trivial local existence argument (in the frame of ${}^{(int)}\mathcal{M}$).

3. Note that the global frame of Proposition 3.26 in not adapted to Σ_* which renders the proof of Proposition 6.14 slightly off. Indeed, the second global frame cannot be used directly for the parabolic equation stated in that proposition. This can be easily fixed by a change of frame to the frame tangent to Σ_*, with the extra terms being suitable errors thrown on the RHS. More precisely, the change of frame from the global frame of Proposition 3.26 to the frame tangent to Σ_* involves the change of frame coefficient f satisfying (3.4.11). To go from the equation in Proposition 6.14 to one with derivatives tangent to Σ_* generates, in addition to the RHS F_k, terms of the form $r\mathfrak{d}^{\leq 3}(f\cdot\Gamma_b)$. One can immediately check from (3.4.11) and the estimates for Γ_b that these quadratic error terms satisfy the same estimates as the one for F_k in Proposition 6.14.

5.2 Clarifications

1. In the Lebesgue point argument (8.3.2), the value $r_{\mathcal{T}}$ is allowed to vary in the region $I_{m_0,\delta_{\mathcal{H}}}$ in (8.3.1) which is of size $\delta_{\mathcal{H}}$. To make sure that the extended spacetime of Theorem M7 is a true extension, it suffices to notice that once an ${}^{(int)}\mathcal{M}$ region initialized at $r=r_{\mathcal{T}}$ with some $r_{\mathcal{T}}$ in $I_{m_0,\delta_{\mathcal{H}}}$ is covered, then, the standard local existence argument used at the beginning of Theorem M7 strictly includes a region much larger than the size $O(\delta_{\mathcal{H}})$ needed to cover the corresponding ${}^{(int)}\mathcal{M}$ regions for any value of $r_{\mathcal{T}}$ in $I_{m_0,\delta_{\mathcal{H}}}$.

2. In (8.3.2), i.e.

$$\int_{\{r=r_{m_0,\delta_0}\}}|\mathfrak{d}^{\leq k_{large}}\check{R}|^2 \quad = \quad \inf_{r_0\in I_{m_0,\delta_{\mathcal{H}}}}\int_{\{r=r_0\}}|\mathfrak{d}^{\leq k_{large}}\check{R}|^2,$$

the right hand side of the equation involves $\check{R}={}^{(ext)}\check{R}$, while ${}^{(ext)}\mathcal{M}$ is only defined for $r\geq r_{\mathcal{T}}$ with $r_{\mathcal{T}}$ fixed in the interval $I_{m_0,\delta_{\mathcal{H}}}$. To make sense of the definition, it suffices to extend, using a trivial local existence argument, the geodesic foliation of ${}^{(ext)}\mathcal{M}$, slightly into ${}^{(int)}\mathcal{M}$, to cover the region $r\geq 2m_0\left(1+\frac{\delta_{\mathcal{H}}}{2}\right)$. This kind of extension is done repeatedly in the book (see for example section 3.5.1).

3. Note that Proposition 3.26 does not just provide the construction of one global null frame, but in fact of a family of global frames. In particular, Theorem M1 provides

decay estimates for $\mathfrak{q}$ in any global null frame constructed from Proposition 3.26, and in particular in a global frame satisfying either the properties d) i. or d) ii. of Proposition 3.26. This remark is in fact used often whenever a global null frame is involved.

4. Though we perform the symmetry reduction form the very beginning in Chapter 2, which in retrospect is unnecessary, all the arguments could have been done with respect to the standard spacetime formalism used in stability of Minkowski. The only place where the polarization is used in a more substantial way is in the construction of GCM surfaces (in Chapter 9) where polarization simplifies the construction (the polarization was later completely removed in [5]). Apart from that, the only reason polarization is used is to ensure that the spacetime has to converge to a Schwarzschild solution. In particular, except for the construction of GCM surfaces in Chapter 9, all estimates are done exactly as in the spacetime formalism.

5. At the beginning of Section 10.5, it is carelessly stated that we use, in Chapter 10, the global frame of Proposition 3.26, when in reality the global frame of Proposition 3.23 also satisfies the assumptions of Chapter 10 (see pages 602, 657 and 675) and is easier to deal with. Indeed, the global frame of Proposition 3.23 is non integrable only in the matching region near $\mathcal{T}$ (where the decay is uniform in all directions). The only place in the proof of the Morawetz estimate which invokes the spheres $S = S(\tau, r)$ is the Poincaré inequality of Lemma 10.18, which is only needed between $r = 3m$ and $r = 4m$ where the frame is integrable.

6 Corrections Proposal (November 8 2021)

This was our proposal for changes to the introduction of [9] discussed with I. Rodnianski in November 2021.

The following aspects of the introduction to [9] are problematic to us and we hope that they could be corrected:

1. **Choice of S_*.** With regard to the sphere S_{u_f,v_∞} anchoring the $\mathcal{I}_+$ gauge in [9], the following similarities with [4] and [6] should be emphasized:

   (a) Your conditions on S_* are the same as those for the GCM spheres in [4] and [6].

   (b) Unlike [2], the construction of your gauge starts from S_*, exactly as in [4].

(c) The co-dimension 2 sphere S_* is in the future boundary of the bootstrap spacetime, and not connected to the initial data layer through transport equations. Such a construction has first appeared in [4], and is conceptually different[4] from that of [1] (or [2]).

2. **Definition of angular momentum.** The fact that the angular momentum is defined on S_* from the $\ell = 1$ modes of curl β as in [6], and unlike [2].

3. **Current quotations of [4] in [9]:**

   (a) Quotation on page 3: *"This is the statement of Corollary III.3.1. Thus in particular, our submanifold contains also the polarised axisymmetric data considered recently in [KS18], which itself is an infinite codimension subfamily of general axisymmetric data."*

   This statement is problematic in various ways. To start with, you need a lot more decay on the initial data[5]. You claim that these initial conditions can be relaxed in the axially symmetric case, but this is not actually done in your paper, and no precise statement is provided to back it up. But even if it was true, it does not mean it is the right way to present it. Announcing on page 3 that [4] is a corollary of a corollary of your result seems unnecessary, and maybe even mean spirited[6], especially since this is repeated a second time on page 16 after the corollary. For the sake of fairness we hope that you may either remove that statement, or provide substantive evidence for the claim.

   (b) Quotation on page 16: *"Like the present work, the starting point of [KS18] is the linear theory [DHR] discussed in Section II, though [KS18] opts for a different gauge which is still however governed by transport equations of geometric quantities associated to foliations."*

   As acknowledged in [4], and repeated in [7] as well as in all our talks on the subject, the treatment of the Teukolsky equation in [2], based on a physical space version of the Chandrasekhar transformation, played an important role in the "gestation of our own ideas". But the way the statement is phrased in the quote above may suggest that the whole paper [4] is based on [2], which is of course false. At the very least, you should be more specific and point out that [4] uses the treatment of the Teukolsky equation in [2]. If you think that other parts of [4] were influenced by [2], you should make precise statements, and avoid what may look like an insinuation that our work is derivative.

[4] It is also the only condition that we know of which might be called "teleological", precisely because it is not connected to the initial data layer through transport equations.

[5] The fact that [4] requires less decay than [9] is nowhere to be found in the introduction of [9]. This is important in view of Christodoulou's well known result on the lack of smoothness of scri in [Chr02].

[6] The same statement is an obnoxious (to us) presence in all talks by Mihalis on the subject.

(c) Quotation on page 16: *"Several of the non-linear difficulties discussed above already arise in [4] in simplified form and are addressed in that work in a different framework."*

Apart from the simplification in the construction of GCM spheres (induced by polarization), dispensed with in our GCM papers, and the actual selection of the co-dimension 3-set in your work, the difficulties in [4] are no less severe than in [9]. Once the GCM sphere S_* is constructed[7], polarization plays a minimal role in [4]. Could you please be more specific about the way in which these "difficulties are simplified"?

4. **Other quotations:**

(a) The following quotations:

- the last sentence of the abstract: *"In view of the recent [DHR19, TdCSR20], our approach can be applied to the full non-linear asymptotic stability of the subextremal Kerr family"*;
- the following sentence on page 3: *"With these more recent developments, the whole approach of this work can in principle be generalised to Kerr, in fact in the full subextremal range of parameters"*;
- and the following sentence on page 19: *"In view of our discussion and the recent [DHR19, TdCSR20], the path is now open to obtaining Conjecture IV.1 following the approach of the present work, although, at a technical level, the Kerr case introduces several new complications related to the necessity of applying frequency localisation to deal with the issues related to trapping."*

Your paper provides no substance to that repeated claim, which may thus be interpreted as a claim of priority for any future advance in the field. We hope that you either remove the claim or provide solid evidence to it. At this stage, apart from our own works [5] [6], which are completely ignored in [9], and [7], we are not aware of any work, that gives even a glimpse on how to construct the correct gauges in perturbations of Kerr.

(b) The following sentence on page 12: *"With these results, the full linear stability of Kerr in analogy to [DHR] can in principle be obtained, in fact, for the entire subextremal range $|a| < M$."*

The only evidence you give for that claim is the recent work by Yacov and Rita on the control of Teukolsky in Kerr. There are of course plenty of other issues to solve, including the issue of gauges, before being able to make such a claim.

[7] Which you keep saying that it is not a big deal anyway. If that is indeed the case, why ignore the GCM papers [5] [6], which are done in full generality in perturbations of Kerr?

> We hope you may also reconsider the statement in light of the fact that our GCM papers [5] [6] are not restricted to small angular momentum so that, in a sense they solve the gauge problem in the full subextremal case.

We are also seriously concerned about repeated statements such as *"[4] is based on [2]"*, or *"[4] is a corollary of a corollary of [9]"*, or that *"[9] provides a roadmap for Kerr"*. These have appeared not only in print in [9], but are repeated in all talks on the subject by Mihalis, as well as talks and papers of his students. The failures to properly quote our papers, as well as all these unsubstantiated claims, have done great damage to the visibility and reputation our own works, damage which we hope can be repaired, at least in part, by taking the actions which we recommend above.

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