Dear Mr. Crothers,

[The following opening was written some time ago but there has been a long gap until I had enough time to complete this email] I at last have a little time in which to write an answer to your long email setting out your arguments and views. I have to be honest and say my heart sank as I read it. It contains the sort of errors which often take even more paper to correct than they take to express, and from which, in my experience, it is almost impossible to move people once they have developed the false belief in their correctness (maybe there's too much psychological value in believing one has outsmarted Einstein). However, towards the end I think some genuine points come to light and I want to address them first, returning to the technical issues later in the hope you are not yet doomed to become a crank.

(In fact, one question that does interest me is how you arrived at the arguments you put - they appear to have a lot in common with the mistakes made by Abrams and by Liebscher and Antoci. Were you in fact following them, or had you arrived at the same errors for yourself?)

For avoidance of doubt, I should make clear at the beginning that I shall follow the convention of attaching Schwarzschild's name, in his honour, to concepts and structures related to his vacuum solution even if they were not discovered by Schwarzschild himself. The historic question of who did what is of no importance to the correctness of modern interpretations, but I fully agree that not all these ideas actually come from Schwarzschild himself and that Hilbert's treatment is inadequate. Also, unless otherwise stated I will use the normal r coordinate for the Schwarzschild metric (which Schwarzschild himself denoted R in his equation (14)), though when I refer to your r_o I do not assume the r in it has that meaning.

Although my view is that you (and others who have followed similar tracks) are overly concerned with mistakes, lacunae, and other things to criticise in the historical development, whereas the present view is not, repeat not, dependent on the exact wordings of the papers of Schwarzschild, Hilbert et al, one historical point about (what I understand to be) your own stance may be worth making. Schwarzschild's form using his r was derived entirely because at that time Einstein had adjoined to what we now take as the basis of General Relativity (GR) the coordinate condition which is Schwarzschild's

(5). This requirement, later abandoned as unnecessary, is partly responsible for the fuss you and others make about what happens as Schwarzschild's own r approaches zero. If he had taken his R as the best coordinate, which he could have done had he jettisoned (5), there would still have been problems about the treatment of $R = \alpha$ but the specific line you are taken might never have been developed. So your position grows from Einstein's mistake, in my view.

The point towards the end that struck me was that you formulate an objection to the usual interpretation, if I may paraphrase, as based on the lack, in the Kruskal-Szekeres form, of an invariantly-defined radial distance from a 'point mass' (*). The requirement is stated clearly as a boundary condition after your 10 axioms. The lack is clearly true, but rejection on this ground is an opinion: it is one to which you are perfectly entitled, but to my mind if you reject the usual interpretation because it does not provide such a radius, then you reject general relativity. It is not viable in my view, for reasons given before and amplified below, to try to preserve GR at the cost of identifying the sphere $r = 2m$ as a point. Point masses are singular in Newtonian theory, and all laboratory objects, including elementary particles, on close enough inspection behave as extended distributions. I therefore find it odd that you seem to hang your objection on a requirement to model point masses. It is reasonable to say that a key step in the evolution of our understanding of the spherically symmetric black hole was the recognition that GR has no point mass solution of the type found in Newtonian theory.

That was my response to the long email but I then became confused about your actual view since in a later email you say that all point-mass and point-charge solutions describe a fictitious object.

There is a genuine issue, well-known in the subject, of the inconsistency between the idea of a test mass (whose gravitational field is neglected) and the result of working out the field for a spherical object, as this does not give a point-mass. If you had that in mind I can only say (a) yes and (b) this is very old news: it was for instance the driver for Einstein's work with Infeld and Hoffman, and is close to being resolved by the efforts of several groups (Schaeffer, Poisson and Will and their coworkers have each worked on this in recent years).

I now proceed to the comments prefaced by 'I would now like to make a few general remarks'.

Of course you are right that the Newtonian 'black hole' ideas give nothing more than an order of magnitude calculation. Physics often develops by such intuitions. However, I don't understand your criticism. If it is true that light has a definite speed (or a maximum speed) then it will be impossible for observers more than a certain distance from a sufficiently massive object to receive light from that object, in Newtonian theory. Do you dispute that?

There is a big difference between 'we do not know a two-body exact solution' (except in very artificial cases in axisymmetric systems) and the claim that (not closed form) solutions of that character do not exist. In fact initial value problem arguments give pretty good results on the existence of such solutions, and their nature can now be investigated by numerical methods (the methods used until recent years were not adequate). It is equally true that there are solutions in Newtonian theory which are known only numerically and will never be given in an exact form: almost any realistic stellar model is in this category. So I really cannot see this as a serious objection: physical reality is much too complicated for us to hope to be able to give a closed-form solution for the universe (my other interest, in computer algebra, means I can give quite precise definitions and arguments but it's surely not necessary here).

Your comments about open discussion and the 'cult-like' following of Einstein really do border on crank territory (see e.g. the points in John Baez's amusing 'crackpot index'). Nobody would disagree about your general comments on the need for free discussion and most agree that known theories are not absolute. But the reason the sort of detailed arguments you make are not accepted by reputable journals is that they are bad arguments, not any adherence to orthodoxy. In fact almost nobody believes GR is absolutely correct, i.e. what some people call a 'basic theory' (I have never heard anyone take that view). They believe it is, like Newton's theory, a very good approximation to reality in a certain domain of applicability (and in fact the next approximation after Newton's theory, which has been shown not to be accurate enough for some solar system measurements). They also believe the same of all other widely-used present-day theories. Experimentalists do high-precision tests partly in order to check the predictions in great detail, and I'm sure they hope to be the ones who find the discrepancy that will only be explained by the next, and better, theory, maybe a true quantum gravity theory, for instance.

However, most attacks on (Special and) General Relativity are not at that level. It helps to see physical theories as composed of a mathematical model and a set of rules for interpreting that model to make predictions about experimental results. Some attacks claim to show that the mathematical structure of SR and GR is inconsistent (which to most of us seems ludicrous since it is just standard Riemannian geometry with some additional well-posed differential equations), and others that the interpretative rules are inconsistent, which again to most of us appears inevitably wrong. Of course in neither case can one give a definite proof, but in the first case, where a proof would get into the territory of Godel's famous results, lots of mathematics would fall if this does, and in the second, experience and a deep enough understanding gives one pretty strong faith. So most people in the field believe attacks at those levels will inevitably be wrong, and usually it's not hard to see where the errors occur (though sometimes people come up with new and subtle variants). Again bitter experience shows that trying to get the errors' authors to recognise their mistakes is usually a lost cause, and so journals and scientists simply give them the brush-off to avoid wasting their time, which is what I may do to you if you persist in some of your arguments - I really have better things to do beyond a certain point.

To conclude that part (note my answer is longer than your statement, which is usual in this kind of correspondence and one reason serious scientists do not have time to engage in it usually) I should also comment on the issue of alternative theories of gravitation. Most of these cover the same domain as GR, and can be tested by the same means. Almost all such have been found wanting (see Will's well known book, plus the papers of Coley and others) i.e. disagree with experiment. Those that do not, avoid doing so, usually, by being indistinguishable from GR in their predictions (to the possible measurement limits). I do not think constructing new theories at this level is worthwhile (there are already at least 150 in the literature): until or unless an experimental discrepancy is verified on the scales GR works on (and the leading candidate is the Pioneer 10 anomaly) a new theory needs to have a wider domain of applicability and agree with GR for solar system tests.

As for your treatment in your PhD, I have only the evidence of the arguments in your long email as far as the quality and content of your work is concerned. If they are typical, then your supervisor was right that this would not lead to a PhD - not because it's unorthodox, but because it's wrong in some parts and not new or physically interesting in the others. His and the university's subsequent handling of the issue, if it is as you report, sounds unprofessional and just like avoiding confrontation. We would record the evaluation, and there would be an assessment by two other staff members, which if negative would be followed by a formal process of deregistration, with an appeal mechanism and so on. But the conclusion might well be the same.

Now let me revert to the start of your arguments.

Your opening remark (a) about the regions $r > 2m$ and $r < 2m$ talks about validity. But you do not define valid. (Your use of this word has the unfortunate consequence that to avoid confusion I have to avoid use of 'valid' at some points below.) Until you do I cannot argue with this or subsequent similar remarks very usefully. In the end, as I try to explain below, it seems to me all your arguments are either wrong or reduce to saying that the K-S form and interpretation are invalid because they are invalid i.e. to an opinion rather than a logical argument.

What I would treat as 'valid' within GR is any manifold obeying Einstein's field equations (EFE) with a matter content subject to reasonable restrictions (e.g. speed of sound less than or equal to that of light, positive energy density): I would then say that many of those are not appropriate to the actual universe, of course. An example I would say that about is the general Taub-NUT solution, as a global model. I need some better criterion than you have given if I am to be persuaded to change this view of what is valid.

Your remark (b), however, is wrong in its remark on measurability, within GR. In GR one can measure the invariants of the Riemann tensor and its derivatives, and in principle do so locally (not pointwise, though), and this fixes r. I return to this below. I seem to recall there was also a paper by Duff in the 70s which discussed the measurability of r, but I have not had time to track it down and check what it says. [You repeat point (b) later in your email but I will not repeat my reply each time.] The comment that r is not a radius, in the sense of a proper invariantly-defined spacelike distance from a central point, is correct of course: this has already been discussed at * above. r is invariantly-defined and measurable, but not as such a spacelike distance.

Next you develop an argument from a spherical polar form of flat space. You say that you place a mass at a point r_o, and then write down a spherically symmetric metric, your (5), based on the original origin. This cannot be correct. A point mass at r_o destroys the spherical symmetry about the original origin (though there will still be symmetry about a new origin at r_o, but this is not what you appear to be doing - if you were, the mass would be at $r = 0$ in your spherically symmetric form). We can go further if we instead take a spherical shell on the sphere $r = r_o$ and I shall assume that from now on (as otherwise everything that follows simply falls completely).

Then you obtain (various forms of) the Schwarzschild solution. What is then puzzling is talking about 'the correct form of $C(r)$'. It is fundamental to differential geometry that there is no such thing - any form is 'correct' (providing it is suitably differentiable, and 1-1). All the different forms are just different coordinate systems on the same manifold (or some extension or restriction of it).

You then derive a radial distance from r_o to r in (16). However, I cannot see why you have neglected the constant of integration in (13) or equivalently failed to specify a lower limit of integration in (12). You may take it at r_o: this adds terms to the right side of the second line in (13), which are the negatives of the ones given but with r_o substituted for r. Then your condition on R_p as $r \to r_o$ is certainly true. But assuming that condition and that the terms involving the lower limit of integration vanish, as you have, gives a restriction which emerges as (15), i.e. that r_o is chosen to be at the point where that expression you took to be zero vanishes. The r_o is no longer arbitrary. You have added a hidden assumption. There is no a priori reason why r_o and α should be in any particular relation. [Note: I have not tried to verify that (15) is the unique solution to (14).]

In fact, that r_o is arbitrary is what one would expect. It is a basic result of Newtonian theory that the field exterior to a spherically symmetric object is exactly the same as that of a point mass at the centre, regardless of the actual distribution of mass with radius, or the location of the outer boundary of the matter-filled region. Hence the exterior gravitational field of a spherical body cannot tell you the radius of the body. The only things the Newtonian field exterior to a spherically symmetric body fixes are the mass m and the location of $r = 0$. In order for GR to give Newtonian theory as a limit, the same must be true in GR (the radius $r = 2m$ is of course fixed by m: if using general units rather than the geometrized units normally used in GR, one would have to add factors of the constant velocity of light c and the Newtonian gravitational constant G of course). Hence the exterior field cannot possibly fix an arbitrarily chosen r_o and such a quantity need not be related to the mass. I am therefore confident that (15) is wrong unless you have chosen a relation between r_o and m a priori.

Next you impose (17) and the preceding equation, which from my point of view are coordinate conditions. However, your claim about the possible forms of C does not follow from them: there are an uncountable infinity of other choices, as is inevitable when all that has been imposed is values at two boundaries and monotonicity (and some appropriate differentiability, say C^5). As a specific uncountable set, consider the three-parameter set of choices obtained by adding a small multiple ϵ of a 'bump function' (if you don't know what that is, you need to learn some basic differential geometry: look in texts for the discussion of 'partitions of unity') supported on the range $[r_1, r_2]$ where $r_o < r_1 < r_2 < \infty$. So the following discussion of possible forms of $C(r)$ is no more than a consideration of special choices of coordinate - and by definition in GR is of no physical consequence.

You call the various forms 'solutions'. Of course they do give, in the differential equations sense, different solutions of the Einstein equations, but in GR we always consider only the equivalence classes of such solutions under coordinate transformations, and call the class, all of whose members represent the same gravitational field, or some region thereof, a solution of the EFE. Adopting a different convention of your own can only be confusing.

The argument following (20) does not show that $r = r_o$ is a quasi-regular singularity. You need to understand better the definition of singularity (read Geroch's famous paper for example) and then the definition of quasi-regular (see the Tipler Clarke and Ellis article in the Held volumes of 1980 for instance). This is similar to the error Antoci and Liebscher make. It is perfectly correct that one can define an invariant which blows up at $r = 2m$. There is no need to go to complicated ways, like yours, to define one - just take, for example, the inverse of the invariant Aman et al found which vanishes there. But that does not make $r = 2m$ singular.

Going quickly through the next bits: The paragraph following (21) re Kruskal-Szekeres is thus based on two errors. Re the next bit: your curvature radius (the usual r) is indeed well-defined but R_p depends on the choice of origin: you have taken the origin at $r = 2m$ so your claim is in effect that r and m are invariantly-defined. I completely agree with that. But taking your r_o to be at $r = 2m$ is, as mentioned above, an extra assumption which removes the arbitrariness of r_o. I cannot see why writing down a form for the rotating or charged cases leads to a claim that there is no black hole. I could not find where r_c is defined but this part seems to have no outcome other than a further restriction on the coordinates considered. Incidentally, I would guess the reason Brillouin's form is not used is simply that there are no known physical problems for which it is more convenient than other coordinate choices.

Now to come to your comments on my email. The first part again talks about validity (undefined) and repeats your (b) (partly wrong). It is correct that singularities can occur where (the limit of) the curvature is finite, or even zero, but these are exactly the extendible ones or the 'whimpers': at the latter higher order invariants of the Riemann tensor blow up. At the time Kruskal, Szekeres, and Synge wrote, these issues were not fully understood (the definitions were developed for the singularity theorems in the 1960s and continued to evolve in the 70s, so Tipler et al is the place to start).

So your 'unproven assumptions' reduce, as I see it, to your view that the interior regions are 'not valid' and your requirement that there be a spacelike radial distance from a central point: i.e. it looks as if you are saying the black hole ideas are wrong becuse you do not like them. Just to respond to your specific points, the behaviour of Doughty's acceleration has to do with the track he chose to look at - it's not hard to define a track in flat space which develops infinite acceleration. There is no reason to think any physical body follows such a track. And I can see no reason why 'spacelike loci' cannot describe a physical field: in fact I am pretty sure any field whatsoever could be described by (non-orthogonal) coordinates all of whose constant surfaces are timelike or all spacelike. So I don't see any reason here to reject K-S. And saying the inner region is not a region is even less of an objection: what can you mean?

You say that K-S is not a solution for a point mass - and I think everyone agrees there is no point mass in the sense people set out to find. GR just does not have such a solution. K-S is the universal covering analytic solution to the problem of a spherically symmetric vacuum field. The main argument for believing this solution is relevant to physical objects is the study of the evolution of collapsing objects (starting from the exact solutions of Oppenheimer and Snyder, etc): one thing you will have to do if you insist $r = 2m$ is a point is reconcile that view with those solutions.

Your assumptions 1-10 are not the usual ones and not suitable in general (e.g. your 1 would make it impossible to model a spherical star joined to a vacuum exterior) but I guess you mean them as assumptions from which to derive Schwarzschild's field. I have already commented your following point.

You argue that $r = r_o$ is invariantly defined. Assuming we set aside your hidden assumptions which led to r_o being at $r = 2m$, your argument is an error based on a misunderstanding about what an invariant definition of such a constant would require. You correctly state that all the invariants have well-defined values at $r = r_o$, which are dependent on r_o. But this is not the point. What is required for r_o to be invariantly defined is that from measured invariants at any r one can obtain r_o. At a point on the sphere $r = r_1$ all invariants of the Riemann tensor and its derivatives depend only on m and r_1. By eliminating between invariants one can find each of those quantities separately: hence as I have asserted above, r and m are invariantly defined, and measurable in the field, and are the only independent such quantities. But m is the same at all r, whereas of course r depends on position. Hence the only independent **constant** invariantly defined by the Schwarzschild solution is m. The solution also defines where the spheres of symmetry are, and hence from their areas, in a sense, where $r = 0$. They do not define an arbitrary r_o (again, setting aside your hidden additional assumption). I of course agree that $r = 2m$ is invariantly defined.

Finally to come back on the question of treating $r = 2m$ as a point, I do not assume r is a radial coordinate, at least not in the sense you seem to mean - it is quite clearly an area coordinate. My contention is simply that points do not have area. Another way to phrase the point is to consider two particles freely falling radially inwards on identical paths except that they are at different angular coordinate values. The distance between them remains finite as they reach $r = 2m$ but in your interpretation they are supposed to reach the same point.

Your argument about the gravitational tensor is very vague and offers no mechanism. I don't know Levi-Civita's argument, but I really think you have to get away from worrying about the historical development (unless you want to be a historian of science) and worry about the theory as it is now. As far as the oddity of Hilbert's arrow of time is concerned, Hilbert simply made a wrong argument (the coordinate system cannot be used at $r = 2m$ as it fails to give a 1-1 map of the manifold to 4-dimensional space there and the regions cannot be joined in the way he had them). Even the greatest make mistakes, and in his case it had one correct outcome in leading people to think that $r = 2m$ was not singular, albeit on wrong grounds.

Just to make the point very explicit, consider the metric

$$ds^2 = dt^2 - \left(\frac{r^4}{(r^3 + a^3)^{4/3}} dr^2 + (r^3 + a^3)^{2/3}(d\theta^2 + \sin^2\theta d\varphi^2)\right).$$

This is nothing more than flat space (your (1)), transformed to coordinates like Schwarzschild's own r. In this metric, is $r = 0$, for fixed t, a point or a sphere? If a sphere, why is the same not true for $r = 0$ in Schwarzschild's form? The response "because the g_{rr} in Schwarzschild's form becomes infinite as $r \to 0$" can be answered by recasting Schwarzschild's using an Eddington-Finkelstein-like transformation to r and

$$u = t + 2m \ln(R - 2m),$$

where R is Schwarzschild's original R, and an analogous transfomation $u = t - R$ for my metric above, and the resulting metrics have no components tending to infinity as $r \to 0$. (I'm assuming you agree that no physical consequence can arise from a change of coordinates in the region $r > 2m$. If that's not the case we have no basis for dialogue, since 'general covariance' is a fundamental principle in GR.)

So I maintain you have in no way reduced the force of the points that
(a) the K-S solution is a solution of the EFE including a region isomorphic to Schwarzschild's original solution, and is a maximal analytic solution, and therefore provides the correct understanding of the global solution of which Schwarzschild's is a part
(b) it is wrong to treat $r = 2m$ as a point, i.e. to identify the points in the K-S form at which $r = 2m$ and Schwarzschild's t takes any value: these form one sphere, not a one-parameter family of spheres at different t - that's the origin of Hilbert's mistake.
(c) this is supported by the development of collapsing solutions which lead to a black hole.

Best wishes
Malcolm MacCallum