I should first make it clear that the GRG Society itself does not engage in issues such as you raise, unless, of course, backed by a resolution of the General Assembly. There are many academic controversies and we would not serve our members properly if the officers started to take sides in them as representatives of the Society.

However, it happens that I can respond, in a purely personal capacity, on the papers you cite and those related to them, because I have read some of them and even some of the commentary on them by referees etc.

It should first be made clear that what was said by Schwarzschild or by Hilbert is only of historical interest in itself. It is not, contrary to what some people seem to believe, wholly responsible for the present understanding reflected in texts and papers. (This kind of misreading of where the present view has its basis is even more common among people who reject the whole theory of relativity, who often base their arguments on specific phrases in Einstein's papers, as if those sentenences were what a current treatment depended on.) In the case of the global picture of the Schwarzschild black hole (I comment below on the name) the proper treatment only emerged in the 50s and 60s and was perhaps set out pedagogically in the 1963 Les Houches proceedings (and of course in subsequent good textbooks).

Secondly, however, there is a strand of building from (say) the mistake Hilbert made to an indirect argument, on rather poor foundations, that our present concepts of black hole and interpretation of the Schwarzschild metric are wrong.

Both these aspects are supported by a mixture of correct arguments, and arguments which are in part technically correct but joined with parts which show serious technical inadequacies. For instance, some of the remarks of Antoci and Liebscher, and earlier those of Abrams, show a lack of understanding of some basics of differential geometry, and the claims of serious relativists are misrepresented.

You suggest that people don't reply because they prefer to brand you a crank, possibly for some sinister motive connected with funding, and that this is arrogant and unscientific. I'm afraid it's almost certainly simpler than that. Most of the points made, in various disguises, have been argued before and anyone who understands the subject properly knows what the correct geometric description is, and should readily be able to correct the wrong parts of the papers of Antoci, Abrams etc. However, experience shows that no matter how patiently and carefully one explains the correct description and the geometric understanding we now have, the recipients often fail to grasp the technical points being made, and can never be convinced, and so it is a waste of time and effort to try. It is that failure to grasp the corrections which is truly unscientific - perhaps the psychological hold of the belief that one has seen an insight the mainstream has overlooked is too strong to overcome by argument alone.

I don't know you, so I've assumed you are a reasonable and competent person and tried to explain, but I am only going to do it once. If you come back repeating the incorrect arguments, I shall behave like your other correspondents and become silent - that's not arrogance, it's self-defence against wasting my time. On the other hand, if you are trying, from my email or from textbooks, to reach a proper understanding and need additional help to do so, I shall be happy to assist.

I shall refer to equations in physics/0310104, and I shall also refer to the book on Exact solutions of Einstein's field equations by Stephani, Kramer, myself, Hoenselaers and Herlt (SKMHH).

In 0310104 it is claimed that relativists generally believe in equations (1) and (2). This is not true. A relativist making a careful statement would say that (1) is correct either in 2m < r < \infty or 0 < r < 2m. The two regions where r>2m and r<2m were carefully characterized, generalized to R and T regions, in the Novikov paper which appeared as a Golden Oldie in the GRG journal (translated by me and Beisekeev). A decent modern text on relativity would get this right
and indeed discusses it carefully.

Modern relativists would therefore not accept the way Hilbert discusses the matter (as translated in 0310104) as technically correct, because it treats the whole range 0 < r < \infty as a single coordinate patch. This fact may be of some historical interest. It has nothing to do with a correct modern understanding of the Schwarzschild metric or its analytic extension, singularities, and so on. (In mitigation of Antoci's claims, one confusing factor may be that physicists and mathematicians often say things in a technically incorrect but intuitively helpful way. This only causes confusion if the audience is not clear about what is being done. If he had heard somebody talk in this loose way about r=2m he may have formed a wrong impression about what relativists really claim. However, as I point out below, if one regards r as a function on the Kruskal-Szekeres manifold, it is everywhere well-defined.)

This leads to various subsequent misleading claims. For example, Antoci states that Schwarzschild never wrote a solution given by (1) and (2). Quite true. But the form (1) of the solution, without (2), does appear in Schwarzschild's original paper (with slightly different variable names): it is equation (14) there. It was rejected only because at that time Einstein's theory was stated with an unnecessary coordinate condition which (1) does not fulfil.

One has to appreciate that a solution in general relativity is defined by the geometry it describes or by its invariant properties, not by the specific coordinates used. However, in this case, it seems to me perfectly reasonable not only to use the terms 'Schwarzschild metric' or 'Schwarzschild solution', but also 'Schwarzschild coordinates'. One could certainly maintain that only the outer region r>2m should be given Schwarzschild's name, and that therefore the terms 'Schwarzschild horizon' and 'Schwarzschild black hole' are misnomers, but this seems to me ludicrously pedantic. If that's the issue, I suggest you relax and reflect on the metatheorem of mathematics which claims that all named theorems have the wrong name.

To come to Hilbert's paper, it is indeed in error. The proposed coordinate chart ((1) and (2)) is not a correct chart on the region intended to be covered. One can see this as follows.

The Kruskal-Szekeres form (SKMHH 15.24) is undeniably a solution of the Einstein vacuum field equations which is regular everywhere except where r tends to 0 (r being defined by uv = -(r/2m -1)exp(r/2m)). r is a perfectly good scalar defined everywhere on the manifold (which, by the way, does not include its boundary r=0: I use the term manifold strictly, not to be confused with manifolds with boundaries). Either of th manifold's two regions r > 2m is isometric with the original Schwarzschild form in its chart, and one of its two regions r < 2m is similarly isometric with the T-region r < 2m of Hilbert's form. I can talk about r=2m now, sensibly, in the K-S manifold, but it is not part of the manifold covered by the Schwarzschild chart or the chart giving the similar T-region. (By the way, Antoci and Liebscher make a lot of play of the issue of 1-1 maps or their lack: but it is perfectly OK for a metric expressed in particular coordinates to be valid only on one chart neighbourhood of a larger manifold, and for the isometry of that metric to be with only part of the complete manifold. This in no way diminishes the correctness of either form. Indeed, even for the r>2m region in the usual form (1), one should take at least two charts, strictly speaking, since the identifications of angular coordinates such as \phi=0 and \phi=2\pi makes the coordinates no longer 1-1 and hence inadmissible. Would the same authors then make a fuss about the fact that one of the two or more strictly correct metrics on chart neighbourhoods was not isometric to the whole region? Of course not!)

However, the coordinate system of either of the Hilbert forms breaks down at the surface r=2m, where from the Kruskal form it is obvious that all values of t, in the limit r tends to 2m, relate to the same two-dimensional sphere, so that the r=2m points at the boundary of those coordinate systems are a 2-, not 3-, dimensional space. Hence the Hilbert coordinates are not proper coordinates across r=2m (though fine if one sticks to one side or the other), since they are not a 1-1 map between the manifold and R^4. Unsurprisingly, strange things happen like problems with the arrow of time, as Antoci pointed out.

So, if all you want to tell me is that Hilbert made a mistake and Schwarzschild did not make the same mistake, I agree. But if, as Antoci does, you want to go on and argue that the Schwarzschild form is meaningfully singular at r=2m, implying we should not use the Kruskal form, and so on, I completely disagree.

Let me just pick up some of the points and leave you to study good literature enough to deal with the rest. Antoci talks about the choice of constants in Schwarzschild's form. This is totally irrelevant. I could rewrite the metric of flat Euclidean space in a way that adds extra constants in a manner similar to Schwarzschild's and then ask about the need for 'an additional postulate'. An infinity of coordinate transforms are possible, with extra constants in them - no such constant has any invariant meaning unless it appears in an invariant derived from the solution, and Schwarschild's extra one does not. If one does not understand that, then one does not understand coordinate invariant theories. So discussing this as if it mattered is a mistake.

Antoci mentions that one can find an invariant which blows up at r=2m, and constructs one from the timelike Killing orbits. Correct. But this does not prove there is a singularity in the usual sense. Antoci does not discuss the definition of singularity, but seems to wrongly think it just means that some invariant blows up. Such an invariant can trivially be constructed if one has one that passes through zero, by taking a reciprocal, and that is in effect what is happening here. Antoci need not have done any new work. An invariant that passes through zero exactly at r=2m was given by Karlhede, Lindstrom and Aman in GRG 14, 569 (82); taking its reciprocal gives one that blows up. To define a singularity, one has to follow the arguments presented for example by Geroch, Ann. Phys. 48, 526 (1968). Then one finds the Schwarzschild horizon is not singular.

Abrams has argued that we should stick to Schwarzschild's original radial coordinate and regard r=2m as a point. Its strange features (that spheres around it have limiting area 16\pi m^2, radial geodesics approaching it stay a finite distance apart etc) apparently cause him no problems. However, it's quite a revealing exercise to cut a sphere out of flat space, lay down coordinates on the rest with a rdial coordinate vanishing on the sphere, and treat that origin as a point - one gets the same pathologies. In other words, Schwarzschild's original coordinates implicitly take a whole sphere and topologically identify it to a point and this is reproducible with flat Euclidean space. Abrams seems not to accept that most people, if given the flat space in the funny form, would happily reverse the identification and restore the sphere, just as we now do in understanding black holes.

I could go on for some time, but there's perhaps one other point worth stressing. People who dislike the correct interpretation of the Schwarzschild solution (as extended into the Kruskal picture) tend to object to singular coordinate transformations. But (a) the transformations needed only become singular at points not in the coordinate charts (but on their boundaries - and coordinates are by definition given only in open regions in usual manifolds) and (b) the singular transformations are needed between polar and cartesian coordinates in flat space on the polar axis, which of course can be chosen to run through any given point - so everyone who thinks needing a singular coordinate change is a sign of a singularity presumably believes that every point in flat space is singular.

Best wishes
 Malcolm MacCallum

I was hoping to shortly have some time to respond again to you. I have to say that the bulk of your arguments are in my view misguided and in several places technically incorrect. You do not mention the name of the journal or physicists you have been in touch with but I doubt very much that they are really taken seriously by the main stream. You may be in grave danger of getting yourself into a ghetto with people who really are regarded as being of no scientific merit, even if they are not actually cranks. If you want to be taken seriously you need to convince the people who read and write for Physical review, Classical and Quantum Gravity and so on. (No journal is immune from publishing bad papers, but those are the ones which are considered usually reliable.)

Best wishes
 Malcolm MacCallum

You are perfectly correct that my most recent emails have not given the scientific arguments. As I warned you, my time is extremely limited and I have so far only had time to get perhaps 1/3rd of the way towards writing out the points I wanted to make in response to your long email.

While I cannot judge your arguments on the other matters yu raise, since I have not seen your papers, they do sound to me horribly as if you are moving into crank territory in claiming things like

"I have completely disposed of the black hole in all its varieties."
"There is no theoretical substantiation of the Big Bang hypothesis whatsoever."
"I have proved that the cosmological constant must be precisely zero."
"I have proved that ALL solutions purporting an expansion of the Universe are also complete and utter gibberish."

When I have time I will take a look at the website you mention, but I can say immediately that it does not relate to any journal of standing in the field that I am aware of (because I know their websites), and your work is therefore very unlikely to have any influence on gravity research. I have never heard of Prof. Smarandache: he may or may not be an excellent mathematician but that does not necessarily mean he has the background and experience to judge papers in this area of mathematical physics.

Best wishes
 Malcolm MacCallum

Since you seem to be unable or unwilling to engage with the technical arguments and have resorted to vulgar abuse, I do not intend
to continue this discussion further
 Malcolm MacCallum

You may circulate what you wish provided it is in no way extracted, i.e. that the full correspondence is used.