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Laboratory for Astrophysics and Space Research

February 1, 1985

Dr. Basilis Xanthopoulos
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University of Crete, P.O. Box 470
711 10 IRAKLION, GREECE

Dear Basilis,

I have now received your various communications; but more of this later.

I am enclosing with this letter a completely revised and completed n-1 version of the paper; the notes relative to the last two sections; and the diagrams. (Not all of the diagrams will be included in the final paper.)

I have spent a great deal of time (far more than what I normally do) with the n-1 copy I am enclosing. I should particularly like you to check all the formulae for scientific accuracy. My plan is that on returning from India I will write the nth copy, taking into account whatever comments you may have on the present n-1 version, have the manuscript typed in final form, and send it to the Royal Society before the end of the month (February).

I cannot promise that when making the final copy of the paper I shall be able to accept all of your recommendations, even though I shall give them due weight. For example, I have in fact thought about many of the things you have written with respect to the earlier n-1 copy; and in most cases I have decided against them. To illustrate the nature of my reactions let me go through your various remarks in your letter of January 21 (a Xerox of which I am enclosing) concerning the n-1 copy you have read.

1. If "somebody" has never heard of the two previous papers, then it is high time that he did (i.e., if he cares).

2. I have deliberately postponed my comments on the physical grounds for choosing the equation of state $\epsilon = p$ to the very last section.

3. If one does not know that the signature I have always used (and used in <u>M.T.</u>) is (+ - - -) and is standard whenever one uses the Newman-Penrose formalism, then one had better learn (again, if he cares!).

5. I wish to retain equation (4) as written since it is exactly the terms which appear on the left-hand side of this equation that one eliminates from equation (3).

6,7,8, and 9. O.K.

10. I had already taken care of this in the parts underlined in red on pages 4 and 8. See also the note that I am now adding on page 10.

chandra is correct.

11. I cannot confirm the 3p.

12. O.K.

13. I cannot confirm your correction.

14. O.K.

15. Before section 4 I emphasize in each case whenever the assumption $\epsilon = p$ is involved.

16. I do not believe that there exists a single person, besides us, who has used the equations that have been used in the earlier papers. I am also sure that no one has checked the formulae in papers 1 or 2. And I am not really interested in making my papers readable for those who do not care.

17. O.K.

18. The revised manuscript takes care of these remarks.

19. Since $\mathcal{U}_0^2 - \mathcal{U}_3^2 = 1$ must be an integral of the equations of motion, I should not see any reason to exhibit an explicit verification. The mystery to which you refer is that they are explicitly in the gauge adopted.

20. O.K.

21. No, I do not think the denominator should be $(1-\mu^2)^{\frac{1}{4}}$.

22, 23. O.K.

24. The revised version takes care of this remark.

25. See the underlined word on page 27.

26. O.K.

Now with respect to the various letters and notes that you have sent. I am afraid that I have only glanced through the pages and not scrutinized any of it. My concern throughout has been to obtain a physically significant solution when χ, q_2, β, and μ_3 (not $\tilde{\mu}_3$), are exactly the same as for the vacuum. And I am satisfied that the resolution of the various difficulties that you raised (Cassandra-like!) have been satisfactorily resolved. In saying this, I am not suggesting that your various alternative considerations may not be relevant for further extensions and generalizations. I shall return to these other possibilities at a later time, but I should like to keep the present paper as it is. Please forgive the stoccato prose of this letter; but I am under great pressure.

Best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar

enclosures

p.s.: Bob Geroch has meantime read the manuscript. While he finds nothing mathematically inconsistent or physically without meaning, he confesses to some uneasiness. This uneasiness is expressed in the additional paragraph I have now clipped to page 36. I am also sending a copy of the manuscript to Roger Penrose.

Even though you seem psychologically averse to accepting my view that the assumption, for χ_2 and q_2, the solutions that are valid for the pure gravitational problem, must lead to a non-trivial solution of the present problem, I should be grateful if you can make a real effort to resolve Bob's uneasiness. He has promised to devote some time to this problem himself during my absence in India.

I hope that my 'castle' does not collapse to dust!

Iraklion, January 2, 1995.

Dear Chandra:

From what you told me in the phone two hours ago I think that I was able to repeat what you have done about the problem of colliding perfect fluids. I am very skeptical whether it will work and for this reason I am writing my thoughts without waiting for your letters.

Lets agree first about the boundary conditions:

The function f determines the change in the metric imposed by the perfect fluid, by introducing the term e^f. And we had decided, back in September, that for the spacetime to be flat in regions II and III we should have $f = \text{const}$ there. Hence, f should be a constant on both boundaries, u and v.

$f(u=0, v)$ generally would be a function of v on the $u=0$ boundary but we want it to be a constant. Hence we want

$f_{,v} = 0$ for $u = 0$.

The function f is related to the stream potential ϕ by

$$f_{,v} = \frac{2\phi_v^2}{v(1-u^2-v^2)}, \quad f_u = \frac{2\phi_u^2}{u(1-u^2-v^2)}.$$

For f to be well behaved near $v=0$ the first equation gives $\phi_v = 0$ for $v=0$ (to kill the v in the denominator). For $f_v = 0$ for $u=0$ the same equation demands $\phi_v = 0$ for $u=0$. Hence we have the boundary condition

1) $$\left[\begin{array}{llll} \phi_v = 0 & \text{for } u=0 & \text{and for } v=0. & \text{Similarly,} \\ \phi_u = 0 & \text{for } u=0 & \text{and for } v=0. & \end{array}\right.$$

Let me turn now to what I think you have done. You introduce $w = \alpha(u^2 - v^2)$ and $z = \alpha(1-u^2-v^2)$. In terms of u and v the two boundaries are

$$u = 0 \iff z - w = \alpha$$

$$v = 0 \iff z + w = \alpha.$$

You consider the solution

$$\phi = (\sinh w)\, z\, K_1(z).$$

Then, for instance,

$$\phi_u = 2\alpha u \left\{ \cosh w \; z K_1(z) - \sinh w \, (z K_1(z))' \right\}.$$

Since we want $\phi_u = 0$ for $v=0$, this should come from the vanishing of the curly bracket, i.e., we should have that

$$\cosh w \; z K_1(z) - \sinh w \, (z K_1(z))' = 0 \text{ for } z + w = \alpha$$

and I do not see how this is going to happen. It is not for some particular values of z and w but for a whole range of values of z. In fact it will demand that

$$\cosh(\alpha - z) \cdot z K_1(z) - \sinh(\alpha - z) \cdot (z K_1(z))' = 0$$

which is certainly not true. Probably ~~w~~ I am demanding too strong boundary conditions, what do you think?.

Of course ~~…~~ we may consider weaking the assumptions and do not demanding ~~only~~ that the metric is continuous across the $u=0$ and $v=0$ boundaries. In this case we have an even simpler solution for which the energy density is positive everywhere. It is

$$\phi = \alpha(u^2 - v^2) \quad , \quad f = -4\alpha^2 \log(1-u^2-v^2), \quad \alpha = \text{const.},$$

$$\varepsilon = \frac{4\alpha^2 uv}{(1-p^2\eta^2-q^2\mu^2)} \sqrt{(1-u^2)(1-v^2)}\,(1-u^2-v^2)^{4\alpha^2-\frac{3}{2}} .$$

According to the values of α we can have the energy density to diverge, vanish or be constant on the curvature singularity. But I am not certain on how to make the extension into regions II and III. The substitution $u \to uH(u)$ and $v \to vH(v)$ certainly does not work.

In the notes I am inclosing I stick to the boundary conditions (1) (previous page), work in the η, μ coordinates, write ϕ as a ~~power~~ series of Legendre functions and I try to determine the coefficients A_m, B_m, Γ_m and Δ_m. I obtain an infinite system of linear equations which involves as coefficients the numbers α_{mn}, β_{mn}, γ_{mn} (defined on top of page 258) which, however, I failed to determine (or find in some book, although I feel that they must have been evaluated). The final conditions, described on pages 272-275 look quite simple. I donot have a proof that the system has a solution (different from the trivial solution) but we always seam to have more unknowns than conditions. I have no control on the positivity of the energy density. Although it will be difficult to obtain f explicitly except if, for fixed m, α_{mn}, β_{mn} and γ_{mn} vanish after some values of n and higher.

I hope that I am wrong about the boundary conditions and your simple solution is the correct one.

I was in Crete all the time – but not very much at home at night – ~~expe~~ except from November 21 – December 2, when I was in Adana, Turkey, to attend an UNESCO meeting on Mathematical Physics. I talked alot of with Yavuz there. They (with Metin Gürses) consider collision of gravitational + scalar field waves, which are, in a sense equivalent to $p=\varepsilon$ perfect fluids. But they had – they told me – some difficulties in the $u=0$ and $v=0$ boundaries.

With best wishes, to your wife as well, for a happy new year.

Sincerely, Basilis.

Collision of two null Maxwell fields in region I

Iraklion, August 4, 1985.

Dear Chandra:

I do not think that the ambiguity in the evolution of the null dust of regions II and III into the sum of two pieces of null dust or ~~an~~ $\epsilon = p$ (massive) fluid has much to do with our inadequecy to distinguish "an $\epsilon = p$ from an $\epsilon \neq p$ dust because null dust is secretive". To substanciate my claim I propose the following example which shows that the same ambiguity arises even in the collision of gravitational and electromagnetic waves.

Consider the metric tensor that prevails in region II and which was studied in detail in our electromagnetic paper, § 8. Its ~~..~~ Ricci part is described by a single scalar field, $L+M$. When $L+M<0$, it is a pure radiating electromagnetic field when $L+M=0$ it is a vacuum solution and when $L+M>0$ it is a null dust solution. When it is an electromagnetic field it satisfies (equations 157–160 of § 8) $F_{mn}F^{mn} = 0$ and therefore $T_{ab} = F_{am}F_b{}^m$. Moreover, since the only non-vanishing components of the energy-momentum tensor are

$$T_{00} = T_{33} = T_{03} = -e^{2\psi-2\nu}\left[(A')^2 + (B')^2\right],$$

it is of the form

$$T_{00} = -E\, l_0 l_0, \quad T_{03} = -E\, l_0 l_3, \quad T_{33} = -E\, l_3 l_3$$

with $l_0 = l_3 = l$ or $T_{ab} = -E\, l_a l_b$. So, we can generally say that

$T_{ab} = E\, k_a k_b$ is null dust when $E > 0$ and null electromagnetic field when $E < 0$.

We can try to solve the Einstein equations in region I for an energy-momentum tensor

$$T_{ab} = -E\, l_a l_b - \bar{E}\, \bar{l}_a \bar{l}_b \, ;$$

it will be interpreated as region I been filled up with two null electromagnetic fields propagating in opposite directions. The solution will be exactly the same as in our last paper (on null dust). The only difference will be that $E \to -E$ and therefore the conditions of equation 62 should now read

$$\bar{F}_{,u} < 0 \quad \text{and} \quad F_{,v} < 0.$$

And after the extension into regions

I and III it will imply that $L+M<0$. Therefore, we also have a solution with Einstein-(null) Maxwell fields in region II and III colliding and producing, instead of the electromagnetic (non-null) field that we have obtained in our electromagnetic paper two Maxwell fields propagating unscattered in region I.

My conclusion: I do not think that the reason for the ambiguity should be searched for in the $\epsilon=p$ equation of state. I would rather look for in the method of solving the problem, the method of the extension, for instance. I do not know the reason but I am suggesting that we should not focus that much to the $\epsilon=p$ equation of state.

Best regards,

Yours Sincerely,

Basilis.

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Laboratory for Astrophysics and Space Research

September 4, 1985

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Dear Basilis,

I have read all your letters concerning the 'null dust' and its relation to the perfect fluid $\epsilon = p$ quite carefully. I do not have any objections to any of the specific things you have said; but I am afraid that I have been unable to communicate to you my own point of view on the subject.

Please consider the following not for arguing for or against any particular position, but rather as a clarification of what I understand and what I do not understand.

To first state matters on which I think all will agree:

An explicit expression for the energy momentum tensor T^{ij} together with the divergence condition ($T^{ij}{}_{;j}$) cannot specify a problem uniquely. By suppressing some information or ignoring some other, we can have different evolutions. <u>All</u> the examples you have given are in this category.

Your reduction of the energy momentum tensor for a Maxwell field to the form $T^{ij} = K k^i k^j$ (k null) and then arguing only in terms of T^{ij} ignores other information derived from Maxwell's field equations. I do not need to labor this point since, after all, the energy momentum tensor for a perfect fluid involves two functions, ϵ and p, unrelated, if you choose to ignore the equation of state which <u>physics</u> provides.

The facts on which I put the maximum emphasis are the following: When one has a solution of the field equations for $\epsilon = p$ in region I, the nature of the substance in regions II and III is uniquely determined; and the transition is made possible across a null boundary on which $\epsilon = p = 0$ and the four-velocity becomes null. The continuation of the solution into regions II and III as massless particles describing null geodesics, appears therefore entirely reasonable. I understand then how massless particles derive from an $\epsilon = p$ fluid in region I. The question I want to ask is, is there any way in which the massless particles in regions II and III can be characterized so that their transition into a perfect fluid will appear equally naturally? And the fact that we cannot is, in my judgement, the basic problem.

To state the matter somewhat differently, at a macroscopic level — a level to which you seem to restrict yourself — the problems you seem to have considered do not violate other things we know. But at a microscopic level, there is clearly a lack of understanding. The principal question is, what is the physics underlying a substance with the energy momentum tensor, $T^{ij} = Kk^i k^j$, which can transform into a perfect fluid with $\epsilon = p$ when $\epsilon = p = 0$. In other words, by what other information must we supplement the expression for T^{ij} in order that we can understand its transition to a perfect fluid? I do not know how to answer these questions.

One last point: I do not think that one can use the word 'null dust' to justify that one must necessarily associate the substance described by $T^{ij} = Kk^i k^j$ as pressureless. One does not solve problems by definitions. In fact, I think all the words we have used in this connection, namely, null dust, null fluid, or pure radiation, do not have precise meanings. It is not impossible that there is a single uniquely definable substance which can effect transitions to an '$\epsilon = p$'-fluid in both directions. Perhaps one ought to read the original paper of Zeldovich more carefully.

With best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar

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Laboratory for Astrophysics and Space Research

August 6, 1985

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Dear Basilis,

With regard to the coefficient 2 in equation (2), this is what follows from Paper I, equation (11), if you put $\epsilon = p$. On the other hand, if you put p = 0 in equation (10) you get

$$T^{ij} = \epsilon u^i u^j$$

The fact that letting $\boldsymbol{u}$ become a null vector depends on whether one first puts p = 0 and then lets the four-velocity become null, or one puts $\epsilon = p$ and then lets the four-velocity become null. This clearly illustrates the nature of the ambiguity. The apparent conflict between equation (137) of Paper I and what I have written as equation (2) in the present paper has its origin in this ambiguity. On the other hand, if one considers tracelessness as the essential criterion for a null dust, then what we have in regions II and III in Paper I is indeed an ($\epsilon = p$)-dust. I believe this is in agreement with what you first found already last fall. I do not want to go into all these in this paper; and so I should prefer to leave equation (2) as it is.

I have checked the formulae in this paper rather carefully with the nth version; and I hope it is O.K. As I told you, I shall be on a short vacation between August 16 to 26th inclusive, partly for the reason that I am extremely tired at present and partly for the reason that my doctor has so advised, rather strictly.

I hope that the proof of our first paper will be here before I leave on vacation.

With best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar

enclosure

p.s.: I have just received the acceptance of Paper II.

Iraklion, February 8, 1985.

Dear Chandra;

I will try to explain what I've understood the last few days about the colliding gravitational and hydrodynamic waves. I think, it is interesting.

We have agreed to take the following point of view: We do want flat spacetimes in regions II and III and we are willing to accept discontinuities in the metric (f) and the stream potential ϕ along the $u=0$ and $v=0$ boundaries. Then the only requirement for choosing the correct solution for the stream potential was that it leads to a positive definite energy density in region I. And the separable solution you discovered, which is expressible in terms of Bessel functions, does satisfy the positive definite energy condition.

What I have understood in the last few days is how to construct many more solutions which lead to a positive density. In principle one can have as many free parameters as we would like. In practice, of course, I can only work out explicitly only a few of them. The stream potential ϕ is always polynomial in u^2 and v^2. The corresponding f, which appears in the metric, has a logarithmic term $-4k^2 \log(1-u^2-v^2)$, with k constant, followed by extra polynomial terms.

The very simple solution

$$\phi_0 = k(u^2-v^2)$$
$$f_0 = -4k^2 \log(1-u^2-v^2)$$

plays the central role in what follows. From equation (65) of the draft it is clear that

$$\phi_{0,u}\,\phi_{0,v} = -4k^2uv\,,$$

$$\phi_{0,u}\,\phi_{0,v} \le 0 \quad , \text{ and therefore } \quad \varepsilon \ge 0.$$

This solution is obtained by changing the constant of equation (73) to $\frac{C}{\alpha}$, i.e. writing

$$\phi = \frac{C}{\alpha}\, z K_1(z) \sinh w$$

and taking the limit $\alpha \to 0$.

Let ϕ be any solution of the stream potential equation, which is polynomial in u^2 and v^2. Then $A = -\frac{\phi_u}{2u}$ and $B = \frac{\phi_v}{2v}$ will be also polynomials and therefore they will be bounded in region I, up to the singularity. ϕ generally will not lead to a positive energy but we can cure this thing by including a suitable multiple of the fundamental solution ϕ_0. I.e., for any ϕ we consider

$$\tilde{\phi} = \phi + k(u^2-v^2) \quad . \quad \text{Then}$$

$$\tilde{\phi}_u = 2u(k-A), \qquad \tilde{\phi}_v = -2v(k-B)$$

and $\tilde{\phi}_u \tilde{\phi}_v = -4uv(k-A)(k-B)$.

We want $\tilde{\phi}_u \tilde{\phi}_v \le 0 \iff$
$(k-A)(k-B) \ge 0.$
If $a = \max\{A, B\}$, $b = \min\{A, B\}$ in region I
by choosing $k \ge a$ or $k \le b$
we will certainly have that $\tilde{\phi}$ produces a positive energy.

There are infinite-many solutions for which the previous idea does applies. Recall that for any solution $Y_m(x)$ of the Legendre equation of order m
$$\phi(y, \mu) = (1-y^2)(1-\mu^2)\, \dot{Y}_m(y)\, \dot{Y}_m(\mu)$$
solves the stream potential equation. If we consider only Legendre polynomials we get
$$\phi_m(y, \mu) = (1-y^2)(1-\mu^2)\, \dot{P}_m(y)\, \dot{P}_m(\mu)$$

All these solutions ~~should~~ will be polynomial in u^2 and v^2. [I do not have a complete proof for this statement but it should be "obvious"; and I have worked out explicitly a lot of examples]. Hence any linear combination of these will also be polynomial in u^2 and v^2, the corresponding A and B will be polynomial, therefore bounded in region I, and we can always choose the constant k suitably – I mean large enough or small enough – so that
$$\tilde{\phi} = k(u^2 - v^2) + \phi(u, v)$$ leads to

positive energy.

I have worked out explicitly some examples.

(1) Let $\phi = a(1-u^2-v^2)^2$,
$A = 2a(1-u^2-v^2)$, $B = -2a(1-u^2-v^2)$.

when $a > 0$, $A_{max} = 2a$, $A_{min} = 0$
$B_{max} = 0$, $B_{min} = -2a$

when $a < 0$, $A_{max} = 0$, $A_{min} = +2a$
$B_{max} = -2a$, $B_{min} = 0$.

Hence, for $|k| \ge 2|a|$
$$\tilde{\phi} = a(1-u^2-v^2)^2 + k(u^2-v^2)$$
gives positive energy.

The corresponding f is
$$f = -4k^2 \log(1-u^2-v^2) - 8a^2(1-u^2-v^2)^2 - 16ak(u^2-v^2).$$

I have verified, with the use of the computer – an analytic not a numerical verification – that the above f satisfies the equation
$$(1-u^2-v^2) f_{uv} - 2\sqrt{uv} f_u f_v = 0.$$

(2) $\phi = b(u^2-v^2)(1-u^2-v^2)^2$. (corresponds to $m=2$).

$$A = -b(1-u^2-v^2)(1-3u^2+v^2)$$
$$B = -b(1-u^2-v^2)(1+u^2-3v^2).$$

Extrema for $u = v = 0$, $u = \sqrt{\tfrac{2}{3}}$, $v = 0$, $u = v = \frac{1}{\sqrt{2}}$.
They are $b/3$ and $-b$. Hence,
for $|k| \ge |b|$,

$$\tilde{\phi} = b(u^2-v^2)(1-u^2-v^2)^2 + k(u^2-v^2)$$

gives positive energy.

(3) Combination of the previous two.

$$\phi = k(u^2-v^2) + a(1-u^2-v^2)^2 + b(u^2-v^2)(1-u^2-v^2)^2$$

The extrema of the combined second and third terms are found, up to a scaling factor, on page 789. The corresponding metric function f is ~~at~~ the bottom of page 790. It has been checked that this expression satisfies the f-equation.

It seems to me that $\phi_0 = u^2-v^2$ is the most interesting of the different solutions, since it can "correct" so many other solutions. But the problem – by accepting discontinuous solutions – admits an infinite-many-free parameters family of solutions.

I guess you had a nice trip to India.
With best regards,

Yours sincerely,
Basilis.

I include the relevant personal notes, pages 781–791.

Iraklion, July 9, 1985

Dear Chandra:

A few words now about the $\epsilon = p +$ const problem. Pages 39 – 44 describe the solutions I've obtained. For each of them I have calculated the corresponding A and B and I have checked ab initio, the field equations, with the help of the computer. The different solutions and their results are given on pages 71 – 81. For all of them I have calculated the quantity $2ke^{2\Omega}$, where k is the constant of the equation of state. It better be true that $2ke^{2\Omega}$ does not change sign throughout region I. It seems that only the solutions "1" and "4" ~~which~~ will survive this requirement. (I am not certain about the solutions involving the arcsines).

For the solution # 1, $k > 0$ while for the solution # 4, $k < 0$. The next thing will be to find the separable solutions for Φ or $\log \chi$ with the correct behavior for the energy density. If both are well behaved globally we have, it seems to me, an interesting and unexpected new phenomenon: since

$\epsilon = p + k$ with $k < 0$, the speed of sound will be greater than the speed of light! But we are inside a fluid and my mind goes directly to Cherenkov radiation. But these are just speculations. I have not worked out anything more than I am sending you.

A caveat. We still do not have a solution with a free parameter which for some particular value of the parameter reduces to the standard solution for the $\epsilon = p$ equation of state.

Best regards,

Yours Sincerely,

Basilis.

Irak lion, February 14, 1985.

Dear Chandra:

It seems that I am in rather bad shape the last few days and I have trouble to check the behaviors of the Ricci and the Weyl scalars across the $u=0$ boundary, namely the equations of pages 33 and 34. Although I've tried, I will not be able to finish the calculations – and trust what I am doing! – before tomorrow morning, when the last mail of the week leaves. So, I have decided to write you about everything else and send you my results of these last computations on Monday.

I've liked the paper a lot of. And I was surprised to see how much you had done – and how interesting the situation is – about the behavior of the energy density on the curvature singularity $u^2+v^2=1$.

I suggest that for the moment you ignore my previous letter in which I explain how to use the solution $\phi=u^2-v^2$ to cure any other polynomial in u^2 and v^2 solution, and send the paper as it is. I plan to repeat the analysis of section 11 for these solutions as well and see what they give.

Some minor suggestions now about the paper:

1. Page 2, first line: I suggest to write "an explicit solution", since we now know that it is not unique. Also in the third line I would write

   and therefore the velocity of ---

   to make sure that the velocity of sound = the velocity of light is not an additional assumption but a consequence.
2. I was wrong, the third of equations (11) on page 5 is correct as it is, it does not require a three.
3. I insist that the third term of equation (16) should be $+e^{-\nu}h_{3,(0)}u_{(3)}u_{(0)}$. I include page 9 of your calculations where I indicate the error. A plus sign is also required for consistency with equation (37).
4. Page 9, Equations (26) and (27), I suggest that we include that $p^2+q^2=1$.
5. Page 12, second of equations (39), obviously a misprint. The left hand side should be $\nu_{,3}+\frac{\varepsilon_{,3}}{2\varepsilon}=--$
6. Page 13, top, Equation (42) is not the wave equation ($\phi_{uv}=0$). I suggest you remove the remark "a simple wave equation".
7. Page 13, Equation 45. There was never a disagreement about the present equation 45. My remark in

the previous set of comments was referring to ~~the~~ equation (45) of the previous draft.

8. Page 14, Equation 51, the R.H.S. should be
$$-\frac{e^{-2\beta}}{\Delta\delta}\left[\Delta\,\phi_{,0}^2-\delta\,\phi_{,3}^2\right].$$

9. Page 16, Equation 58. We can probably use $\cot(\zeta-\eta)$ and $\cot(\zeta+\eta)$ to save a line.

10. Page 20, After equation 76. I suggest to include "since $k_1(z)>k_0(z)$, ...". Without this remark, which is not well known, the expression (76) is not clearly positive definite.
I also suggest again to include the two properties $[z\,k_1(z)]^{\bullet}=-z\,k_0(z)$ and $\dot{k}_0(z)=-k_1(z)$ of the Bessel function. These are the only ones that we use in all our differentiations and integrations — the more general equation 92 is not needed. ~~Also~~ Also otherwise the reader will suspect that there is a ~~a~~ misprint in the second of equations (75), since k_0 suddenly appears.

11. Page 20, lines 7 and 8. The statement "it represents the most..." is too strong. The solutions $\phi_{m\eta}=(1-\eta^2)(1-\mu^2)\,\dot{P}_{m}(\eta)\,\dot{P}_{m}(\mu)$ are also separable and we can construct from them solutions with $\varepsilon\geq 0$. I suggest we say

"the most general separable solution in the r,s coordinates ---"

12. Page 22, Equation 82, the third term should be
$$\frac{2\,[\,2u\,(\phi_r-\phi_s)\,]^2}{u\,(1-u^2-v^2)}$$

13. Page 26. I would remove the parenthesis in the sentence after equation (99); the statement is crucial, it cannot be ignored.

14. Page 26, End. The results of the entire section 9 are general, for any f, and not only for the f given by equation (94) as stated in the last sentence.

15. Page 29. I suggest to remove the "(100)" from the title of Section 10.

16. Page 32, after line 5 (circumstances) I suggest to include "unless the metric (109) is flat. And from the analysis of section 8 of paper II one readily sees that this can happen only when $f(0,v)$ is constant".

17. Page 36, End of section 10. We can probably ~~we~~ mention that since regions II, III and IV are flat, the origin of the fluid in

region I is precisely these singularities across the null boundaries. Also we should probably mention that the Einstein-perfect fluid equations fail to be satisfied on these ~~to~~ singularities.

18. Page 37, about the remarks made after equation (117). The same as in remark 11 applies.

19. Page 38, the second line of equation (118). I get

$$f \to -4c^2\alpha^2 \left[(\gamma_E - \ln 2 + \ln\alpha + \tfrac{1}{2})\cosh^2 w - \tfrac{1}{2}\sinh^2 w + \right.$$
$$\left. + \ln(1-u^2-v^2)(\cosh^2 w) \right].$$

This also changes the expression (120) for A, it should be

$$A = \exp \left[\!\left[4c^2\alpha^2 \left[(\gamma_E - \ln 2 + \ln\alpha + \tfrac{1}{2})\cosh^2 w - \tfrac{1}{2}\sinh^2 w \right] \right]\!\right] =$$
$$= \exp \left[\!\left[4c^2\alpha^2 \left[(\gamma_E - \ln 2 + \ln\alpha)\cosh^2 w + \tfrac{1}{2} \right] \right]\!\right].$$

20. Page 38, Equation (122), the last term should be

$$\frac{A}{(1-u^2-v^2)^{\frac{3}{2}-K}}.$$

21. Page 39, Equation (125) should be

$$\Delta \sim \frac{1}{(2uv)^2}(1-u^2-v^2)^2$$

22. Page 40. I think that we have to distinguish some subcases.

(1) $c \leq c_0$. When $c < c_0$, ε diverges everywhere
when $c = c_0$, ε diverges for $v \neq 0, v \neq 1$
ε is finite for $v=0$ or $v=1$.

(2) $c_0 < c < c_1$. We should mention that ε is finite for $V = V_\pm$, the turning points.

(3) $c = c_1$. We should mention that at the center $u = v = \frac{1}{\sqrt{2}}$ ε is finite. Somebody may think that it becomes infinite.

(4) $c > c_1$ is O.K.

I think we should split case 1 into two and rename:

(1) $c < c_0$
(2) $c = c_0$
(3) $c_0 < c_0 < c_1$
(4) $c = c_1$
(5) $c > c_1$.

23. Page 41, top. We should mention "... Ψ_2 has the behaviour $(1-u^2-v^2)^{-3/2}$ for $q \neq 0$ and $(1-u^2-v^2)^{-3.5}$ for $q=0$ ...". Then it is indeed exactly the same behavior as in the vacuum. (I found it extremely interesting!)

24. Page 43, middle. Can the statement about the $\varepsilon = p$ equation of state be made a little bid stronger?

25. Page 44, beginning of Appendix. The same as remark #11. We should mention "in the r, s coordinates".

26. Page 45, end of the top paragraph, we need the values of Φ and its derivatives, Φ_x and Φ_y respectively. Although since PR // AS and PQ // SB, AS and SB are in fact characteristics of the equation (A3) and I think that we have a characteristic ~~initial~~ boundary value problem, not a Cauchy one.

27. Page 45, line six from the end. We can write "... boundary conditions along the characteristics $x = x_0$ and $y = y_0$ of equation (A3)"

I've learnt the last two weeks how to use the computer and perform symbolic manipulations. I was in fact able to repeat some of last summers calculations, in particular the determination of q_{2e} and the evaluation of the Weyl ~~scalars~~ and the Maxwell scalars for $q=0$. But I had to know the existence of the nice simple expressions – and their factorizations – in order to write them nicely.
For $q \neq 0$ we can certainly get pages and pages of formulae but I am quite skeptical as the whether there would be nice expressions and whether ~~[illegible]~~ I will be able to ~~[illegible]~~ discover them. But I would certainly give it a serious trial.
I will work, over the weekend, the singularity structure of the Weyl and Ricci scalars in the $u=0$ boundary.
Best regards to your wife.

Yours Sincerely,
Basilis.

August 15, 1985.

Dear Chandra:

This is my second letter. You should first read the long one.

Please note that in Penrose's "Respose to author's reply to referee's comments" twices (second line from the top and third line from the bottom of the first page) he uses the terminology $\underline{\epsilon = p}$ fluids; he never uses ~~$\epsilon = p$ dust~~. And he uses the same terminology in his letter. This is to convince you that there is no ambiguity in the notion of null dust.

Ano other thing that I do not like in the Introduction is the statement that "two things have been clarified to us by...". The ambiguity is not clear at all, it is still on the air.

I have prepared an alternative introduction to the paper which does not suffer from the "$\epsilon = p$ null dust" problem, states the history, gives the correct credit to the different people, states that the present paper really establishes the ambiguity, and makes clear that we are not sure

for the origin of the ambiguity.
The last possibility — the last question in the introduction — will please the people working in chaos, stochasticity, bifurcations, and all that.

~~Not~~ Nothing else from the remaining of the paper has to change.

Please, give some serious thought to what I am telling you! Or, submit the two introductions to one of your local referee's — like Bob Geroch — to see their reactions.

Best regards.

Yours Sincerely,

Basilis.

1

Iraklion, February 17, 1985.

Dear Chandra:

I have now checked all the expressions of pages 33 and 34 — basically equations (114) — which describe the nature and the magnitude of the singularities and the discontinuities along the $u=0$, $0<v<1$ boundary. There are two minor changes. The expression for $\tilde{\Psi}_2$ should be

$$\tilde{\Psi}_2 = e^{-f_v}\left[\frac{(p-iq)^2}{2(1-v^2)^{3/2}} H(u) - \frac{1}{12} f_v' \,\delta(u)\right]$$

and the last expression should read

$$\tilde{\Lambda} = \frac{1}{12} R_{12} = \frac{1}{24} e^{-f_v} f_v' \,\delta(u).$$

But these expressions are correct only because

$\lim_{u\to 0} f_{,u} = 0$ for our particular solution.

For any $f(u,v)$ extended via $f \to \bar{f} = f\, H(u) H(v)$, $\tilde{\Psi}_0$, $\tilde{\Psi}_2$ and $\tilde{\Lambda}$ pick up one extra term, they become

$$\tilde{\Psi}_0 = \frac{(p-iq)}{2(1-v^2)} e^{-f_v} (1-f_v)\,\delta(u) +$$

$$+ \frac{(p-iq)e^{-f_v}}{2(1-v^2)}\left[\frac{3(p-iq)^2 v}{\sqrt{1-v^2}} - \left(\frac{\partial f(u,v)}{\partial u}\right)_{u=0}\right],$$

$$\tilde{\Psi}_2 = \frac{1}{2} e^{-f_v} \frac{(p-iq)^2}{(1-v^2)^{3/2}} H(u) - \frac{e^{-f_v}}{12} \left\{ \left(\frac{\partial^2 f(u,v)}{\partial u \partial v} \right)_{u=c} H(u) + \frac{df_v}{dv} \delta(u) \right\},$$

$$\tilde{\Lambda} = \frac{e^{-f_v}}{24} \left[\left(\frac{\partial^2 f}{\partial u \partial v} \right)_{u=c} H(u) + \frac{df_v}{dv} \delta(u) \right].$$

Since we ~~now~~ know that there are other solutions of the problem for which the energy is again positive, and since in so many cases we keep the discussion general – for any ϕ, for any f – do you think that it will be helpful to include these expressions in the paper as well and then mention that for the solution that we ~~st~~ study one gets equations (114)?

I apologize for the delay. I am still hopeful that it will reach you on time and that, by that time, we will have discussed the matter over the phone.

With best regards,

Yours sincerely,

Basilis.

I include pages 1 + 2 of the relevant computations.

Iraklion, July 17, 1985.

Dear Chandra:

I am sending you my report on the solutions with $\epsilon = p + k$ equation of state. It took me some time to work them out explicitly and to check them abinitio, even with the help of the computer.

The strategy for obtaining solutions is now completely different – somehow the opposite – of what we were used to so far with the vacuum, the electromagnetic, and the $\epsilon = p$ cases. Now we first solve equations (9.7) and (9.8) for the gauge functions $A(\eta)$ and $B(\mu)$, this is the difficult step. Then it is straightforward to evaluate $2ke^{2\Omega}$ (equation 8.11). It better be true that in the region of interest the resulting expression does not change sign. Then we solve the linear equation (8.5) for $(\log \chi)$, it always admits separable solutions. And finally, we determine the stream potential ϕ by quadratures, via equations (9.1). The last two steps can of course be inverted: We can first solve equation (8.8) for ϕ and determine subsequently $\log \chi$ by quadratures. In both cases

$$2\epsilon - k = \epsilon + p = \frac{e^{-2\Omega}}{A^2B^2}\left(\delta \phi_{,\mu}^2 - \Delta \phi_{,\eta}^2\right) \quad \text{should } ~~\text{be}~~ \text{ remain}$$

greater or equal to zero in the region of interest.

The method for solving equations (9.7) and (9.8) is described in pages 37 – 45 and I think that I have all the solutions for that gauge. Then I have constructed different combinations of these solutions for A and B for each of them I have verified equations (9.7) and (9.8) ab initio, I have evaluated $2ke^{2\Omega}$ and I have studied its ~~to~~ behavior for small u, v to see whether it is extendable into regions II, III and IV.

Symmetrical in A and B, or in η and f, solutions are described in pages 71 – 85, and asymmetrical ones in pages 131 – 144. One crusial test for all of them was that $2ke^{2\Omega} \neq 0$, where the previously reported solutions had failed.

Unfortunately, I still do not have a solution – and I do not know how to find it – involving a free parameter which will reduce to the $\epsilon = p$ solutions for some particular value of the parameter. More over, all the solutions involving free parameters also involve transcendental functions, namely arcsin x. However, there are also a lot of simple solutions for A, B – remarkably simple I would say, for which the integration of equation for ϕ is very simple.

Extendability in the standard way into regions II, III and IV imposes severe restrictions on these solutions. For instance, none of the symmetrical solutions can be extended into regions II and III since for all of them $2ke^{2\Omega} \sim (\eta^2-\mu^2) \sim uv$ which vanishes after we perform the standard extension. This is mainly the reason that I had to consider the asymmetrical solutions. Most of these can be extended into regions II and III. But the simple ones will fail to get an extension into region IV!! But the ones involving free parameter - and arcsin η and arc sin μ - can be extended into all regions for suitable ranges of the parameters.

I wanted to impose the conditions that $2ke^{2\Omega}$ does not change sign and $\epsilon + p \geq 0$ throughout region I but I am not quite sure how large region I should be. Certainly, the singularity will not be at $u^2+v^2=1$. The singularity will be determined from the condition $2ke^{2\Omega} = 0$ - singularities can only occur where the determinant vanishes - and in the simple cases that I have worked out this condition leads to singularities for $u =$ const or $v =$ const, i.e., on null surfaces!

But this result has only been established so far only for solutions which fail to admit an extension into regions II, III and IV. ~~that~~ Until a few days ago I was disqualifying the solutions which were changing sign in the old region I – up to $u^2+v^2=1$ – but now I keep all of them because I do not know how large should be region I.

I am planning to work the Weyl scalars and check for sure that the singularity determined from $2\kappa e^{2\Omega}=0$ is a real curvature singularity.

That the singularity is not at $u^2+v^2=1$ is not very much surprising to me. After all, this was the condition that $\sin\alpha\sin\psi=0$ which was ~~f~~ mainly fixed by gauge. But if it really ~~turned~~ turns out to be on null surfaces – and not on spacelike ones – it will be a really new feature. Certainly the problem needs much more understanding and thinking.

What do you think? You never really expressed your feelings about the new approach and these solutions.

Hoping that I will soon know more to tell you,

Yours sincerely,

Basilis.

1.

Iraklion, January 19 1995.

Dear Chandra:

I am writing down the derivation of the solution for ϕ by working entirely with the non-self-adjoint equation and my thoughts about it.

Original equation

$$(1-u^2-v^2)\,\phi_{uv} + u\,\phi_v + v\,\phi_u = 0 \qquad (1)$$

Boundary conditions

$$\phi_u = \phi_v = 0 \quad \text{for both } u=v=0. \qquad (2)$$

Change (u,v) to $x = 1-2u^2$, $y = 1-2v^2$. (3)

Equation becomes

$$2(x+y)\,\phi_{xy} = \phi_x + \phi_y. \qquad (4)$$

Half (two) of the boundary conditions are automatically satisfied. The other two give

$$\phi_x = 0 \text{ for } y=1 \quad \text{and} \quad \phi_y = 0 \text{ for } x=1 \qquad (5)$$

which are equivalent to $\phi = $ const. on the boundary.

I change now in the notation of Copson

$$\phi \longleftrightarrow u \qquad (6).$$

Original equation

$$L(u) = u_{xy} - \frac{(u_x+u_y)}{2(x+y)} = F(x,y) = 0 \qquad (7).$$

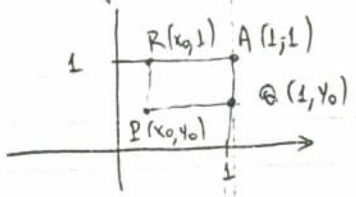
Self-adjoint equation

$$L^*(v) = v_{xy} + \frac{v_x+v_y}{2(x+y)} - \frac{v}{(x+y)^2} = 0. \qquad (8)$$

It satisfies $v L(u) - u L^*(v) = \frac{\partial H}{\partial x} + \frac{\partial K}{\partial y}$ (9)

$$\left.\begin{aligned} H &= -\left[u v_y + \frac{uv}{2(x+y)} + \Theta_y\right] \\ K &= v u_x - \frac{uv}{2(x+y)} + \Theta_x \end{aligned}\right\} \qquad (10)$$

Θ is an arbitrary function of x and y. I included it in the considerations in order to get the most general solution one can obtain by using Riemann's method. For $\Theta = 0$ the H and K are slightly different from what Copson's theory uses. This is the reason that the final general expression is slightly different from what Copson has on page 81 (last expression of the section). I derive now this expression for our problem.



$$0 = \iint F v \, dx\,dy = \iint v\, L(u)\, dx\,dy = \iint \left(u L^* v + \frac{\partial H}{\partial x} + \frac{\partial K}{\partial y}\right) dx\,dy = \iint \left(\frac{\partial H}{\partial x} + \frac{\partial K}{\partial y}\right) dx\,dy.$$

$$\iint \frac{\partial H}{\partial x} dx\,dy = \int_{y_0}^{1} dy \int_{x_0}^{1} \frac{\partial H}{\partial x} dx = \int_{y_0}^{1} dy \left[H(1,y) - H(0,y)\right] =$$

$$= \Theta(x_0,1) - \Theta(x_0,y_0) - \Theta(1,1) + \Theta(1,y_0) - \int_{y_0}^{1} \underbrace{\left[u v_y + \frac{uv}{2(x+y)}\right]}_{\text{for } x=1} dy$$

Similarly,

$$\iint \frac{\partial k}{\partial y}\, dx\, dy = \int_{x_0}^{1} dx \int_{y_0}^{1} dy\, \frac{\partial k}{\partial y} = \int_{x_0}^{1} k(x,1)\, dx - \int_{x_0}^{1} k(x,y_0)\, dx =$$

$$= u(x_0,y_0) - u(1,y_0)\, v(1,y_0;x_0,y_0) - \Theta(1,y_0) + \Theta(x_0,y_0) + \Theta(1,1) - \Theta(x_0,1) +$$

$$+ \int_{x_0}^{1} \underbrace{\left[v u_x - \frac{uv}{2(x+y)} \right]}_{\text{for } y=1} dx .$$

All together they give

$$u(x_0,y_0) = u(1,y_0)\, v(1,y_0;x_0,y_0) +$$

$$+ \int_{y_0}^{1} \underbrace{\left[u v_y + \frac{uv}{2(x+y)} \right]}_{x=1} dy + \int_{1}^{x_0} \underbrace{\left[v u_x - \frac{uv}{2(x+y)} \right]}_{y=1} dx . \qquad (11)$$

Note that all the terms involving the arbitrary function Θ do cancel out.

The expression (11) is not symmetrical in (x_0,y_0). To make it symmetrical we use that

$$\int_{y_0}^{1} \underbrace{\left[u v_y + \frac{uv}{2(x+y)} \right]}_{x=1} dy = \int_{y_0}^{1} \underbrace{\left[(uv)_y - u_y v + \frac{uv}{2(x+y)} \right]}_{x=1} dy =$$

$$= u(1,1)\, v(1,1;x_0,y_0) - u(1,y_0)\, v(1,y_0;x_0,y_0) + \qquad (12)$$

$$+ \int_{y_0}^{1} \underbrace{\left[-u_y v + \frac{uv}{2(x+y)} \right]}_{x=1.} dy$$

Then we get

$$u(x_0,y_0) = u(1,1)\, v(1,1;x_0,y_0) +$$

$$+ \int_{y_0}^{1} \underbrace{\left[-v u_y + \frac{uv}{2(x+y)} \right]}_{x=1} dy + \int_{x_0}^{1} \underbrace{\left[-v u_x + \frac{uv}{2(x+y)} \right]}_{y=1} dx . \qquad (13)$$

This expression is general: It refers to the particular equation (7) and the boundary of the figure of page 2 but for any boundary conditions.

The Riemann–Green function in

$$v(x,y;x_0,y_0) = \frac{(x_0+y_0)^{1/2}}{(x+y)^{1/2}}\, P_{1/2}\left(1 + 2\, \frac{(x-x_0)(y-y_0)}{(x+y)(x_0+y_0)} \right) \qquad (14).$$

The boundary conditions are only $u = \alpha = \text{const}$ on the boundaries $x=1$ and $y=1$, which seems very little for a unique solution. But $u=\alpha$ suffices to evaluate all the integrals of expression (13) and this is the reason I am very much worried that we should get the unique solution of the problem – after the failure of the inclusion of the function Θ to leave some arbitrariness.

By including $u=\alpha$ on the boundaries $\Rightarrow$ $u_y = 0$ for $x=1$ and $u_x = 0$ for $y=1$ we get

$$u(x_0,y_0) = \frac{\alpha(x_0+y_0)^{1/2}}{\sqrt{2}} P_{1/2}\left(\frac{1+x_0y_0}{x_0+y_0}\right) -$$

$$- \frac{\alpha(x_0+y_0)^{1/2}}{2}\left\{ \int_1^{x_0} (1+\xi)^{-3/2} P_{1/2}\left(1+2\frac{(\xi-x_0)(1-y_0)}{(1+\xi)(x_0+y_0)}\right) d\xi + \right.$$

$$\left. + \int_1^{y_0} (1+\xi)^{-3/2} P_{1/2}\left(1+2\frac{(\xi-y_0)(1-x_0)}{(1+\xi)(x_0+y_0)}\right) d\xi \right\} \quad (15)$$

or

$$\phi(x,y) = \frac{\alpha(x+y)^{1/2}}{\sqrt{2}} P_{1/2}\left(\frac{1+xy}{x+y}\right) -$$

$$- \frac{\alpha(x+y)^{1/2}}{2}\left\{ \int_1^{x} (1+\xi)^{-3/2} P_{1/2}\left(1+2\frac{(\xi-x)(1-y)}{(1+\xi)(x+y)}\right) d\xi + \right.$$

$$\left. + \int_1^{y} (1+\xi)^{-3/2} P_{1/2}\left(1+2\frac{(\xi-y)(1-x)}{(1+\xi)(x+y)}\right) d\xi \right\}. \quad (16)$$

The real question is: Is $\phi(x,y)$ different from the trivial solution

$$\phi(x,y) = \alpha$$

which also satisfies the same boundary conditions? I understand that we impose boundary conditions on characteristics – and seemingly very little, very weak boundary conditions – ~~to~~ to demand a unique solution. On the other hand, I do not seem why the Riemann method will miss the trivial solution and I do not seem any arbitrariness left in the application of the method to discover it.

If the solution (16) is not the trivial, we only have to check positivity of the energy, namely that $\phi_x \phi_y \le 0$ in region I. If it is trivial, we have discovered the identity

$$\int_1^x (1+\xi)^{-3/2} P_{1/2}\left(1+2\frac{(\xi-x)(1-y)}{(1+\xi)(x+y)}\right) d\xi +$$

$$+ \int_1^y (1+\xi)^{-3/2} P_{1/2}\left(1+2\frac{(\xi-y)(1-x)}{(1+\xi)(x+y)}\right) d\xi =$$

$$= -\frac{2}{(x+y)^{1/2}} + \sqrt{2}\, P_{1/2}\left(\frac{1+xy}{x+y}\right).$$

Both alternatives can be checked numerically. I will try to check them when our computer is up again (our computer actually is very reliable, but the air condition in the computer room is not, and we have to shut the computer down).

Even in the worst case – ~~they~~ they it is an identity – the choice of different constants will lead to a non-trivial solution. I was thinking of imposing the symmetrical choice

$\phi = u = \alpha$ for $x=1$, $y \neq 1$

$\phi = u = \beta$ for $y=1$, $x \neq 1$

$\phi = u = \frac{\alpha+\beta}{2}$ for $x=y=1$

but I do have some problem in writing down the solution explicitly. You told me last night over the phone that one cannot impose this boundary conditions for $\alpha \neq \beta$.

I donot like it, to be the Kassandra this period, Chandra, but I do worry a lot of that we have discovered something about mathematics, the identity.

I am certain that we will interchange views over the phone a few times before this letter reach you.

With best regards,

Yours sincerely,

Basilis.

1

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August 13, 1985

Dr. Basilis Xanthopoulos
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P.O. Box 470
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Dear Basilis,

I have your two letters of August 4. My answer refers to the one in which you express your doubts about the 'secretive' nature of the null dust in its relation to (ϵ = p)-fluid.

Without answering any of your questions directly, let me point out two aspects in which the problem considered with respect to the (ϵ = p)-fluid differs from the other problems of a similar kind.

probably no, I get one free extra parameters

(1) Both the Khan-Penrose and the Nutku-Halil solutions are 'time reversible' in the following sense: start with two impulsive waves moving in opposite directions; the result is the scattering and focussing described by the solution one finds in region I. At the same time, one can start with the solution in region I and extend it to regions II and III and IV and one obtains the time-reversed solution without ambiguity.

On the other hand, starting with the solution appropriate for ϵ = p in region I, you get uniquely null dust in regions II and III. The time-reversed of this problem does not yield a unique solution for region I.

(2) By extending (with time reversed) the solutions obtained in region I into regions II and III, you either get a flat space or a solution of Einstein's equations, when you are dealing with pure gravitation. In the case of a coupled gravitational and electromagnetic field, again the problem, as far as I can see, is time reversible in the sense I have described. On the other hand, you can suppress some known internal parameters and you can get ambiguities of the kind you point out. The essential difference with the case of null dust is, you do not know what the internal parameter is that is suppressed or not suppressed.

The conventional wisdom is that the specification of the energy momentum tensor does not specify the system uniquely. But the parameters and functions not included in the energy momentum tensor are known on physical grounds and governed by equations which supplement the Einstein equations. But what are these other functions or parameters which yield the same macroscopically described null dust?

I talked to Roger Penrose over the telephone the other day and he agrees with me to the extent that the problem presented by null dust and its relation to (ϵ = p)-fluid is far from clear. Bob Geroch and Lee Lindblom think there are no problems; and so do you. I may not understand the problem as well, but I am satisfied for the present that I am in good company!

Now to another matter which is of immediate urgency to me. Going through Chapter 1 of my book, there are unfortunately many misprints derived from boldface characters appearing as light face characters and conversely. In the enclosed Xerox copies of pages from my book I have indicated all such places which I have noticed. I should be grateful if you will scrutinize the entire Chapter 1 for such misprints and see if you can spot others which have escaped me.

I am leaving for a short vacation later this week, and I expect to be back by August 24 or 25.

Yours sincerely,

Chandra

S. Chandrasekhar

p.s.: I believe the energy condition requires $\epsilon \geqslant p$. Therefore ϵ - p can only be a positive constant: $(\epsilon - p) < 0$ is unphysical.

enclosures

Iraklion, January 20, 1985.

Dear Chandra:

After last Friday's phone call, I am thinking again about the drastic solution to our problem, on which you said that Bob Geroch has agreed. The drastic solution is to force the extension to be flat in regions II and III and "that's it", or, better, "this is the extension". But it seems that we do run again into difficulties, the equations refuse to be satisfied by force, and if my considerations are true, they also seem to be independent of the details of the particular solution ϕ.

Let me try to communicate my thoughts:

We have some solution $\phi(u,v)$, which corresponds to some $f(u,v)$ in region I. As "background" gravitational metric we keep the one you have obtained with Ferrari. The presence of the fluid introduces a factor e^{f} in the $e^{\nu+\mu_3}$ – or $du\,dv$ – part of the metric. The extension $u \to uH(u)$, $v \to vH(v)$ leads to

$$ds^2 = 4(du)(dv)\,e^{f(v)} - (1-v)^2(dx^1)^2 - (1+v)^2(dx^2)^2$$

which is flat only when $f(v) = \text{const}$.

The drastic solution will be the following extension:

" You perform $u \to uH(u)$, $v \to vH(v)$ in the (C+F) part of the metric, the gravitational background,

and $f(u,v) \to \tilde{f}(u,v) = f(u,v)\,H(u)\,H(v)$ (2)

in the "fluid part of the metric"."

With the extension (2) $\tilde{f}$ is forced to become constant – actually zero – in region II. I feel that we will have to keep extensions by Heaviside functions, since their derivatives are δ-functions and we do want impulsive waves. By using the "drastic" extension the metric is no longer continuous. Fortunately, the discontinuity is confined in a part whose first derivatives are only involved in the curvature. Hence the Weyl scalars will suffer at most δ-function singularities.

Let $\tilde{\phi}(u,v)$ be the stream potential corresponding to $\tilde{f}(u,v)$. The field equations should be satisfied everywhere, including the boundary. Part of the field (Einstein) equations is that

$$2\phi_u^2 = u(1-u^2-v^2)\,f_u \qquad (3)$$
$$2\phi_v^2 = v(1-u^2-v^2)\,f_v .$$

Hence, we should now have

$$2\tilde{\phi}_u^2 = u(1-u^2-v^2)\,\tilde{f}_u \qquad (4).$$
$$2\tilde{\phi}_v^2 = v(1-u^2-v^2)\,\tilde{f}_v$$

From eq.(2) we get

$$\tilde{f}_u = f_u\,H(u)\,H(v) + f\,\delta(u)\,H(v) \Rightarrow$$

$$u\tilde{f}_u = u f_u\,H(u)\,H(v) ,$$

since $u\delta(u) = 0$. Hence we get

$$2\tilde{\phi}_u^2 = u(1-\tilde{u}^2-\tilde{v}^2) f_u H(u) H(v) = 2\phi_u^2 H(u) H(v)$$ and

since $H(x) = H^2(x)$,

$$\tilde{\phi}_u = \phi_u H(u) H(v) \quad (5)$$

similarly

$$\tilde{\phi}_v = \phi_v H(u) H(v). \quad (6)$$

The integrability condition of (5) and (6) gives

$$\phi_u H(u) \delta(v) = \phi_v \delta(u) H(u) \quad (7).$$

Lets choose a small neighborhood of $u=0$, $0<v<1$. Eq (7) reads $\phi_v \delta(u) = 0$. We multiply by du and integrate. We find

~~[illegible]~~ $\phi_v(u=0, v) = 0$. Similarly in the other boundary we will have

$$\phi_u(u, v=0) = 0,$$

i.e. that ϕ should be constant on the two boundaries.

It seems to me that the Einstein equations want us to be conservative, not revolutionary! When ϕ is constant on the boundaries equations (3) imply that f should be constant as well, the metric is continuous and we do not need to perform the "drastic" extension, $u \to uH(u)$, $v \to vH(v)$ will suffice in this case.

The only way out of the dead lock I can think of is to ~~[illegible]~~ perform "drastic" extensions even in the "background" ~~[illegible]~~ purely gravi-

tational part of the metric. But it is now clear that we cannot perform any extension, since some of them ~~[illegible]~~ are not compatible with the Einstein equations.

I enclose pages 711 and 712 with the calculations I have just outlined and pages 671 – 675 with the solution of Copson's equation for any v and some more identities.

With best regards,

Yours Sincerely,

Basilis.

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AREA CODE 312-962-7839

Laboratory for Astrophysics and Space Research

September 20, 1985

Dr. Basilis Xanthopoulos
Department of Physics
University of Crete
P.O. Box 470
711 10 IRAKLION

Dear Basilis,

I am enclosing herewith my revision of the introductory section of the null-dust paper. You will notice that I have made use of your description 'phase transition.' I believe that the introduction adheres strictly to what can be said consistently with what we know.

After considerable hesitation we have given in to the temptation of spending two weeks in Crete, leaving on October 26th or 27th and staying for the two-week period ending on November 9th. The reason for the alternative dates for the date of our departure is that we have definite reservations for October 27th but we are 'wait-listed' for October 26th; but in either case we shall come via Copenhagen, Frankfort, and Heraklion by Lufthansa Fight 314 (from Frankfort) arriving at 4:50 p.m. I shall let you know the exact date of arrival October 27 or 28 as the case may be, at least a week before our departure.

I sent yesterday my formal request to the Administration for your appointment as a Visiting Scientist for the period January 1—August 31, 1986. While I am of course very glad that you will be spending six months at Chicago, I think it is only fair to warn you that our collaboration may not be as fruitful or as fortunate as during the past year: the problem of colliding waves, first coupled with the electromagnetic field and then with fluid motions with the equation of state $\epsilon = p$ turned out to be more fruitful than I had any reason to expect. And I do not know whether I am sufficiently ambitious at the present time to embark on anything new which may have a comparable outcome. As you know, already when I was writing the book I was contemplating giving up practicing science as I have been used to. Four years have elapsed since then; and I am very clearly at a watershed at the present time. I certainly would not recommend anyone depending on my future.

In saying all this I am not suggesting or recommending that you change your present plans for your sabbatical — at least for the earlier part of it. There are others in Chicago who would provide as stimulating an atmosphere for scientific research as any that you can find elsewhere in this country.

We will have more chance to talk about these matters when we come to Crete.

With best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar

enclosure

Iraklion, January 21, 1985.

Dear Chandra:

I am writing my comments on the draft on colliding gravitational and hydrodynamic waves.

1) Page 1. After the first sentence I would explain in words, in one sentence, that the colliding waves solutions correspond to the black hole solutions. Just to attract the interest of somebody who never heard of the previous two papers.

2) Page 2. At the end of section 1 I would mention — and give a reference — to $\varepsilon = p$ is the extrem relativistic limit. A lot of people think that $p = \frac{1}{3}\varepsilon$ is the extrem limit. In fact I myself do not know the argument, and I would like to learn it from the paper!

3) Page 2. Immediately after equation (2) I will mention that we are using the signature $(+---)$ since the expression (2) depends on the signature. In addition, I would mention that ε, p are energy density and pressure and that u^i is a unit timelike vector.

5) Page 2. In equation (4) one term in the L.H.S. chancels with the R.H.S., so, I would write it

$$\varepsilon_{,j}\, u^j + (\varepsilon + p)\, u^j_{\ ;j} = 0.$$

6) Page 3. Equation (6) should read

$$(u^j \sqrt{\varepsilon})_{;j} = \frac{1}{\sqrt{-g}} \left[u^j \sqrt{(-\varepsilon g)} \right]_{,j} = 0,$$

$\sqrt{-g}$ is

missing. I would also mention that g is the determinant of the metric.

7) Page 3, three lines after equation (6). I would write the Killing fields $\partial/\partial x^1$ and $\partial/\partial x^2$. dx^1, dx^2 are the corresponding one-forms.

8) Page 3, Equation (8). u^0 and u^3 should have their indices upstairs.

9) Page 4. Equation (9). I would put parenthesis, $w^{(0)}$, $w^{(1)}$, $w^{(2)}$, $w^{(3)}$.

10) Page 5. Starting in this page we have a lot of $u_{(0)}$ and $u_{(3)}$. And later on, for instance in Section 4a, we have the same quantities which are now denoted u_0 and u_3 without parenthesis, because of the change of notion. I propose to set from the very beginning

$u_{(0)} = A$, $u_{(3)} = B$ (or any other letters).

It avoids confusion and it is easier to print. I understand ~~any~~ much faster $A A_{,0}$ from $u_0 u_{0,0}$.

11) Page 5. Third line, a factor 3 is missing, it should read

$$T_{(2)(2)} + \tfrac{1}{2} T = T_{(1)(1)} + \tfrac{1}{2} T = \tfrac{1}{2}(\varepsilon - 3p) = \ldots$$

Also in the fourth line the last term should be

$$-\tfrac{1}{2} R_{(0)(3)} = -\tfrac{1}{2} G_{(0)(3)}$$

12. Page 5. In equation (12) I would write $\frac{1}{2} R = T = \varepsilon - 3p$, since both R and T are needed later.

13. Page (6), equation (16), the third term in the L.H.S. should be $+ e^{-\nu} \mu_{3,(0)} u_{(3)} u_{(0)}$.

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$$u^{(0)} = u_{(0)} = e^{\nu} u^{0} = \underline{\underline{e^{-\nu} u_0}}\,, \qquad u^{(3)} = -u_{(3)} = e^{\mu_3} u^{3} = \underline{\underline{-e^{-\mu_3} u_3}}\,,$$

i.e., I would add the two underlied terms.

15. In pages 2 - 6 there is a mix up of the general (any ε and p) and the particular ($\varepsilon = p$) case. I suggest that we omit equations (6), (8), (18) - so that the discussion remains uniformly general - and write there equations at the beginning of section 4.

16. Page 7. What about including in the paper, before equation 19, Equations ①, ②, ③, ④, ⑤ and ⑥ of pages 3 and 4 of your original notes? I've checked them, they are all correct. Then the paper becomes more readable.

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is exactly where I've punched a hole!

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$$\sqrt{\Delta}\, u_0 (u_0^2 - u_3^2)_{,0} + \sqrt{\delta}\, u_3 (u_0^2 - u_3^2)_{,3} = 0,$$

i.e., an identity. Hence, equations (36) and (37) are equivalent. The way it is written on top of page 12, leave some mystery to the reader.

20. Page 13, Equation (43), the last term should be $- \frac{e^{-\mu_3}}{\sqrt{\delta}} \phi_{,0}$.

21. Page 13, Equation (45), the last term in the denominator should be $(1-\mu^2)^{1/4}$.

22. Page 14, Equation (50) should read

$$f_{,0} = \frac{4}{\eta^2-\mu^2} \left\{ -2\mu\, \phi_{,0}\phi_{,3} + \frac{\eta}{\Delta}\left[\Delta(\phi_{,0})^2 + \delta(\phi_{,3})^2 \right] \right\}.$$

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$$\varepsilon = \cdots = -\frac{e^{-2\mu_3}}{\Delta\delta}\left[\cdots\right].$$

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26. Page 28, Equations (103), I get $\tilde{\Psi}_4 = e^{-f}(\Psi_4 + \lambda\, \Delta f)$.

I still do not know how to make the extension to region II compatibly with the Einstein equations.

Best regards.

Yours sincerely,

Dori[illegible]

Iraklion, January 21, 1985.

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nc

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I still do not know how to make the extension to region II compatibly with the Einstein equations.
Best regards.

Yours sincerely,
Basilis.

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P. O. Box 470

August 21, 1985.

Dear Chandra:

I am now replying the part of your letter of August 13, concerning the collision of null dust.

By "time reversible" solution you mean that the metric of region I determines uniquely those of regions II and III (always) and the metrics of regions II and III determine uniquely that of region I. Well, I think that even the pure gravitational solutions of Khan-Penrose and Nutku-Halil are not "time reversible".

~~I think that I can write an one-parameter family of Kahn-Penrose solutions in region I which all have flat regions II and III and therefore represent the same physical situation. The idea is the following: Consider surface orthogonal Killing fields, take the general form of the metric in region I, apply the~~

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~~Penrose extension and get the general form of the metric in region II. Demand that this metric is flat. You can solve the equations, you get an one parameter family worth of solutions. Then use these characteristic data on the null boundaries $u=0$ and $v=0$ and write (we know how to do it) the corresponding solution in region I. You get a free parameter over the Kahn-Penrose solution.~~

I think that you already have my calculations for this problem, we even discussed it for a while last March. I donot know how to interpreat physically this extra parameter but it seems to be there, to create an ambiguity even in pure gravity. And although I donot know how to work the equations, this free parameter should survive in the Nutku-Halil solution.

I am thinking seriously that we may have established some ambiguities and

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found some stochasticity in General Relativity. Then, my second choice will be that the ambiguity is due to the procedure.

Best regards.

Yours Sincerely,

Basilis.

I've thought that the solution in region I should be determined uniquely from the metric of regions II and III — the tails of the passage of the waves — and the profile of the δ-function singularity — the particular details of the impulsive wave. I now believe that there is no ambiguity, even for null dust: The two solutions (with ε=p or two pieces of null dust) agree in regions II and III but not in the profile of the δ-functions. Hence there is no ambiguity. I have to do some calculations but it seems right to me. But now the situation looks like a non-equilibrium

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critical phenomenon: A slight change in the form of the impulsive wave produces completely different results after the collision!
What do you think?

Basilis.

Iraklion, June 24, 1985.

Dear Chandra:

My report on "the collision of impulsive gravitational waves when coupled with null dust", pages 1-8 of your notes:

We have a lot of disagreements. And I think that I have solved the problem completely in the enclosed pages 1-7 of my notes.

I understand your notation with the ± sign's for the unbarred and barred quantities. But I do not get your equation for $k^{(b)}{}_{|(b)}$. You get the fourth, the underlied, equation of page 2; I get, when expanding the intrinsic derivatives, four additional terms, from the $\gamma_{(a)(b)(c)}$'s. I get the first and the second equation of page 1 of my notes. I agree with your expression II (page 2) for $k^{(b)}(\epsilon k^{(0)})_{|(b)}$. But then I do not understand your equations on page 3. Every equation of motion should involve both barred and unbarred quantities, as you have it with your last equation on page 1.

With the extra four terms in $k^{(b)}{}_{|(b)}$ I get much nicer equations for the null dust equations (1) of page 2 and equation (2') of page 3. Then I decided that it is much better to introduce only one potential ϕ, to solve equation (2.1); equation (3.2) then gives the hyperbolic equation (3.6) that the potential ϕ should satisfy. The ϕ-equation is wonderful in the $(\xi,3)$ coordinates where it just reads

$$\phi_{,\xi 3} = 0.$$

The Einstein equation reduce to equations (6.11) and (6.12) for the f of $r + \frac{h}{3} = (r + \frac{h}{3})_{vac} + f$. In the beginning I did not like that they are not symmetrical in δ and Δ. But they are again wonderful in the $(\xi,3)$ coordinates, equations (7.15) which, by virtue of

$\phi = \phi_1(\xi) + \phi_2(3)$ being the general solution for ϕ, are just quadratures. Since you can certainly set ϕ_1 or $\phi_2 = 0$ and leave the other unaffected it seems to me that the two parts of null dust are not coupled at all, even through the Einstein equations.

I hope that I have not made some conceptual mistake and the solution of the problem is so simple.

Best regards,

Yours Sincerely,

Basilis.

Iraklion, January 25, 1985.

Dear Chandra:
I went through pages 18 - 24 of the draft, describing the ~~sol~~ separable solution for ϕ, and I've noticed some minor errors. Here are my suggestions:

1) Page 18, the second of equations (71) should be

$$d^2g/ds^2 - \frac{1}{s}\frac{dg}{ds} - \alpha^2 g = 0.$$

2) Page 19. Just before equations (75) I suggest that we write: "Since the K functions satisfy the identities

$$[z K_1(z)]' = -z K_0(z) \quad \text{and} \quad K_0'(z) = -K_1(z)$$

we obtain". These are the only two identities we need for all the subsequent differentiations and integrations and I suggest that we include them. In fact, we never need the more general identity (92).

3) Page 19. Equation (76) should read

$$\phi_u \phi_v = -4uv\,[(\phi_r)^2 - (\phi_s)^2].$$

4) Page 20 Immediately after equation (77). It is not clear why the r.h.s. of equation (77) is positive definite. Can we explain a little more?

5) Page 21, Equation (80) should read

$$\epsilon U_3^2 = -\frac{1}{4}R_{33} = c^2\alpha^4 e^{-2k_3}\Big\{[u\sqrt{1-u^2} - v\sqrt{1-v^2}]\,K_1 \cosh w + [u\sqrt{1-u^2} + v\sqrt{1-v^2}]\,K_0 \sinh w\Big\}^2,$$

please correct the terms in red.

6) Page 22, Equation (82) should read

$$f_{,u} = 2u(f_{,r} - f_{,s}) = \frac{2\,[2u(\phi_r - \phi_s)]^2}{u(1-u^2-v^2)}$$

7) Page 22. Equation (83) should read

$$f_{,r} - f_{,s} = \frac{4}{s}(\phi_r - \phi_s)^2$$

and equation (84) should read

$$f_{,r} + f_{,s} = -\frac{4}{s}(\phi_r + \phi_s)^2$$

8) Page 23, Equation (88) should read

$$f = 2c^2\alpha^2 z K_1 K_0 \cosh 2w + F(s)$$

please, eliminate the extra "4" in front.

Best regards.

Yours sincerely

Basilis

July 25, 1985.

Dear Chandra:

I now have a solution for the $\epsilon = p +$ const. problem which admits regular extensions into regions II, III and IV. It corresponds to

$$A = \frac{1}{1 + \eta\sqrt{1-\alpha^2} + \alpha\sqrt{1-\eta^2}}, \quad B = \frac{1}{1 + \mu\sqrt{1-\beta^2} + \beta\sqrt{1-\mu^2}},$$

α, β any parameters.

Pages 321 – 336 describe the details of the solution. In regions II and III the singularities look like to what we were ~~accustomed~~ used to, singularities for some $v = v_0$ or $u = u_0$. But in region I the singularities are either on some $\mu =$ const (timelike) or $u =$ const or $v =$ const (null surfaces). I still have to work the Weyl scalars to prove that there are real curvature singularities. But there is no singularity in a spacelike surface in region I.

Also it seems to me that for suitable values of the parameters α and β I can make $k > 0$ or $k < 0$ a fact which physically is rather surprising. And always there is the drawback I've mentioned in a previous letter: The solution is not continuously connected by ~~the~~ some parameters to the known solutions for vacuum ~~or~~ or $\epsilon = p$.

Even the fluid part of the solution is nice. The equation for the stream potential ϕ can be integrated (pages 1 – 12) admitting the nice solutions of page 9 (for any value of the parameters). But I still have to check that these solutions lead to $\epsilon \pm p \geq 0$ in the allowed region.

Except from a reprint of your lecture in the Indian Academy of Sciences, I did not receive anything from you in the last three + weeks. And I was expecting, day by day, the draft of the collision of two null dusts. Are they lost in the mail or the story has changed? What is the situation with the other papers?

I hope everything is O.K. with you.

Best regards, to your wife as well,

Yours Sincerely,

Basilis

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*Laboratory for Astrophysics
and Space Research*

July 26, 1985

Dr. Basilis Xanthopoulos
Department of Physics
University of Crete
P.O. Box 470
711 10 IRAKLION

Dear Basilis,

I am sorry that since our brief conversation over the telephone on July 8 (soon after our return from the Canary Islands) I have not been in communication with you even though I have received several weighty (!) packages from you including the revised version of what was once "paper III" (my version of which you described as 'weak'); the manuscript of the paper based on the simplest solution of the Ernst equation for $E^{\dagger}$ (in the notation of our Einstein-Maxwell paper, §4); and the extensive calculations you have done on the problem $p = \epsilon$ + constant. The fact is, I have been extremely busy and deeply concerned with the problem of the null dust which has now resulted in the nth version of a paper that I am enclosing. I shall deal with the circumstances of this last paper first, and return briefly to the various other papers later.

Let me first explain the origin of the present paper. As I have told you on several occasions the underlying cause for the transformation of null dust into an ($\epsilon = p$)-fluid has been discussed at great length, particularly by Geroch and Lindblom (but almost entirely in my absence); but they have kept me informed of the results of their discussions. I am enclosing my account of the principal results of their discussions (document A) which I originally intended as a part of the present paper -- an intention that I have now discarded. A further development originated in a referee's comment and in my response to it (copies of which I am enclosing -- documents B.) The referee, as I suspected, was Roger Penrose, whose subsequent letter coming into the open is also enclosed (document C). All these provide the background for the paper I have just written; and I gave priority to writing it over all other matters.

I think the final version of the paper, together with the various enclosures, should make your reading of it and checking the details very easy. (May I say parenthetically that after your telephone call telling me that my original equation of continuity for null dust had omitted some terms, I was able to correct the derivation the same afternoon; and my efforts to reach you before we went to the Canary Islands the following day did not succeed.)

I should be grateful if you will check all the formulae very carefully, since I have not taken as much care over them as I normally do; also, that you call me as soon as you have checked the paper for whatever corrections and comments you may have. (Since it has happened that, whenever you call me from your office, the connection was broken in the middle, would you please tell me first the number from where you are calling so that I could call you back.)

There are a very large number of questions that require clarification in the context of our two papers (the one in press and the present one). But I should like to postpone these matters until such time as we are able to meet either in Crete or in Chicago. [Incidentally, what is the status of your sabbatical? When is it likely to start and what is the length of the period you might stay in Chicago? As I have told you, I can promise support to the extent of $10,000. I may be able to increase the stipend a little if the period of your stay in Chicago exceeds six months; but I cannot promise.]

Now to your various other packages. First, I have not spent any substantial time on them; and I do not know how much time I will be able to spend during the next two months. The fact is, my Mathematical Theory of Black Holes will go for its third printing in September; and I want to go over the entire book scrutinizing it for minor misprints. I must give the highest priority to this awful job; but I have no choice! For these reasons my following remarks are first impressions; and hopefully I shall write to you in greater detail within the next week or ten days.

With regard to "paper III", I hope you will not mind my saying that it is very much a "portmanteau", in that many unrelated matters are included though each matter has some relevance (like objects in a portmanteau!). And, changing metaphors, I feel that the focus of the paper is blurred by mixing it up with the coupling of the gravitational field with (ϵ = p)-fluid. I feel this more strongly now, since matters concerning null dust and an (ϵ = p)-fluid are special and must be treated in their context. The appendices relate of course to the more general problem of the identities among special functions that one can obtain by using Riemann's integral formulation of the boundary value problems derived from hyperbolic equations in the cases where the Riemann's function is known. My old paper (of which you have a copy) gives some examples. And again the inclusion of this material distracts from the main purpose of the paper. My earlier version, though weak in your opinion, had at least a well-defined focus. If I were to write the paper (and I am not sure that I want to) I should reorganize the material differently.

With regard to your solution for the collision of gravitational waves based on the simplest solution for the Ernst equation for $E^{\dagger}$, I think the introduction is far too long. I really do not see why a fourth or fifth paper in the present series should trace the history back to Adam and Eve. Again, the focus of the paper is lost. I should drastically cut the first 4 pages of your introduction to no more than a page; and perhaps I would also eliminate much of what you say in pages 7 to 10. And again I must defer my final judgement to a later letter after I have read the paper more carefully.

Finally, I am most impressed by the amount of calculations you have done on the ($p = \epsilon$ + constant)-problem. But I have not been able to form any coherent picture. And as I told you, with the limitations on my time I do not know when I shall get to this work.

I hope to receive your call concerning the enclosed paper before August 12 since I should like to have the paper typed and sent to the Royal Society before my wife and I go on a short vacation in northern Wisconsin for the period August 16-26.

With best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar