From the office of
S. CHANDRASEKHAR

Banilis:

I should be grateful if you check the formulae in §106+107 with special care!

I am now starting on Chapter XI – at long last!

Best wishes & thanks
Chandra /

I sent some revises of earlier pages last week.

Thessaloniki, November 5, 1981.

Dear Chandra:

A have a few remarks on the n^{th} draft (the typewritten) of chapter X.

1. Page 1, line 5, in the parenthesis, you want to say spin (one and two)
2. Page 1, last lines at the bottom. You give the impression that the vector joining the origin with any point x^i in Minkowski space is a null vector, which is not correct. I think that you have to restrict your self only on the points which lie on the null cone of the origin. The same remark applies to page 2, the first line after eq. (4).
3. Page 3, in the last two relations of eq. (8) we need

$$\beta^0{}_1 - i\,\beta^0{}_2 = \alpha^0{}_0\,\bar\alpha^{0'}{}_{1'} + \alpha^1{}_0\,\bar\alpha^{1'}{}_{1'}$$
$$\beta^0{}_1 + i\,\beta^0{}_2 = \alpha^0{}_1\,\bar\alpha^{0'}{}_{0'} + \alpha^1{}_1\,\bar\alpha^{1'}{}_{0'}.$$

Otherwise you obtain $x^i{}_* = \beta^0{}_0 x^0 + \beta^0{}_1 x^1 - \beta^0{}_2 x^2 + \beta^0{}_3 x^3$.
Note that eqns (10) are not affected by this change.

4. Page 4, two lines above eq. (11). You say "it can be verified that ....". I think that you have already verified it.
5. Page 5, between eqns (14) and (15). Since you use the word "metric" it would be nice to clarify by saying that it is skew-symmetric, and invertible.
6. Page 8, Eq. (30). I would have used

$$X^i \longleftrightarrow \begin{vmatrix} X^{00'} & X^{01'} \\ X^{10'} & X^{11'} \end{vmatrix},$$ not $\mathfrak{z}^{00'}$.... Similarly

in the two middle lines of eq. (31).

7. Page 9, in the line after eq. (35) you could probably write "are <u>constant</u> Hermitian matrices". It would late help to understand why their derivatives vanish.

8. Page 9, the last matrices of eqns. (39) and (40) should be, I think, $\left|\begin{smallmatrix} 1 & 0 \\ 0 & -1 \end{smallmatrix}\right|$.

9. Page 16, In the remarks between eqns. (79) and (80). It seems to me that "the first of the two results needs the Leibnitz rule".

10. Page 36, in the third expression of eq. (156) you need P^1, "1" should be a superscript.

11. Page 39. I do not understand the motivating remark between equations (177) and (178).

12. Page 40, line 4 from the top you can write "to the <u>massless</u> netrinos". For some of the particle physicists, netrinos have masses.

These are my remarks up to page X-42. I have not received yet anything on the Klein-Paradox.

Let me also write everything that I know of about the Lagrangian of the stationary-axisymmetric Einstein-Maxwell Equations. The equations are

$$
\begin{aligned}
(\mathrm{Re}\,\mathcal{E} + \Phi\Phi^*)\,\nabla^2\mathcal{E} &= (\nabla\mathcal{E})(\nabla\mathcal{E} + 2\Phi^*\nabla\Phi) \\
(\mathrm{Re}\,\mathcal{E} + \Phi\Phi^*)\,\nabla^2\Phi &= (\nabla\Phi)(\nabla\mathcal{E} + 2\Phi^*\nabla\Phi)
\end{aligned} \qquad (1).
$$

We set $\Phi = x+iy$, $\mathcal{E} = f + i\Psi - (x^2+y^2)$, $\mathrm{Re}\,\mathcal{E} + \Phi\Phi^* = f$

The equations can be obtained from the Lagrangian

$$\mathcal{L} = \frac{(\nabla f)^2 + (\nabla \psi)^2}{f^2} - \frac{4}{f}\left[(\nabla x)^2 + (\nabla y)^2\right] + \frac{4}{f^2}\left[x \nabla y - y \nabla x\right] \cdot \left[\nabla \psi + x \nabla y - y \nabla x\right] =$$

$$= \frac{1}{f^2}\left[(\nabla f)^2 + (\nabla \psi + 2x \nabla y - 2y \nabla x)^2\right] - \frac{4}{f}\left[(\nabla x)^2 + (\nabla y)^2\right].$$

$\mathcal{L}_E = \frac{1}{f^2}\left[(\nabla f)^2 + (\nabla \psi)^2\right]$ is the Lagrangian for the stationary axisymmetric Einstein vacuum equations.

Unfortunately, I do not have any idea of how to motivate and obtain the entire Lagrangian from the simple Lagrangian $\mathcal{L}_E$.

I expect you had a nice time in Russia.

Regards to the Chicago people.

Sincerely yours,

Basilis.

P.S. One more remark about Chapter X. First you define spinors. Probably just before you start your discussion of differentiation, you could define — one sentence — spinor fields.

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Laboratory for Astrophysics and Space Research

May 6, 1981

Dr. Basilis Xanthopoulos
Department of Astronomy
University of Thessaloniki
Thessaloniki
GREECE

Dear Basilis,

Thank you for your letter of April 25 which arrived yesterday. I shall look up the reference which you have given.

Again it has taken a little longer than I thought to write the last section on The Naked Singularity. I have it now pretty well organized and I should be sending it to you today were it not for the innumerable interruptions I have been plagued with during the past few days (even as I have been during the past month). But looking ahead (in fact, to Chapter 10) I should like to ask your help in one particular matter in which you are an expert. It concerns the generation of stationary axisymmetric solutions of the Einstein-Maxwell equation from a knowledge of a solution of the Einstein equation. It seems to me that a simple algorism should be possible using the equations of Chapter II. If such an algorism exists, I should like to give an account of it in the first section of Chapter 10 to be followed by a derivation of the Kerr-Newman solution from the Kerr solution. Incidentally, my present intentions with respect to Chapter 10 are to include accounts of: 1) the Kerr-Newman solution, 2) the Hartle-Hawking solution of many black holes with equal masses and charges, and 3) the Hartle-Geroch solution. In none of these topics will I attempt---indeed, I cannot---any exhaustive account. Purposefully, they will be perfunctory. Once Chapter 10 is written I shall have only elegies to write!

With best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar

mt

enclosure: pps. 104-117

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April 8, 1981

Dr. Basilis Xanthopoulos
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Dear Basilis,

Thank you for your corrections for the nth copy of Chapter 6.

The discussions of the Kerr space-time on pages 72-76 of Chapter 6 is unsatisfactory, because du and dv, as defined in equation (238), are not 1-forms, unless we restrict ourselves to the 3-subspace in which θ is constant; the integrated forms of u and v given in equation (241) are valid only in this subspace. While with these restrictions the discussion is formally correct, it is clearly unsatisfactory. Abhay, who was here last week, agreed with you that with some explicit qualifications, the discussion as written could stand; but Bob Geroch is clearly not satisfied although he will not say so.

As you will notice, in the enclosed xerox of my notes for Chapter 7, I have found necessary and sufficient conditions for a general type-D metric to allow a Killing tensor. I have not verified if these conditions are manifestly satisfied in the cases for which one knows that a Killing tensor exists.

I am afraid that collecting my notes for Chapter 7 has taken an inordinate time; but so much had to be done ab initio. I still have to write about the Wald inequality for the Penrose process; and also something about naked singularities. I am still hopeful that I can begin my $(n-1)$-draft for Chapter 7 next week.

Congratulations on your invitation to spend the summer at the Max Planck Institute. With best wishes,

Sincerely yours,

Chandra

S. Chandrasekhar

p.s.: In spite of my resolutions, I've agreed to go to Poland (if the state of Poland will allow such a trip) for a week in September; and again to a conference in Armenia late in October. I am, of course, hoping that my book will have been sent to the printer's before we go to the U.S.S.R.

mt

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Laboratory for Astrophysics and Space Research

February 9, 1981

Dr. Basilis Xanthopoulos
University of Thessaloniki
Department of Astronomy
Thessaloniki, Greece

Dear Basilis:

Herewith the nth copy of Chapter V. You will note that section 49 has been left incomplete: my account in the (n-1) copy is, I am afraid, in error. I hope to be able to straighten it out before I come.

One of the things I really want to get done, while in Greece, is to be able to integrate the equations governing parallel propagation of a tetrad frame along null geodesics in type-D metrics. Penrose accomplishes this by using spinors. I should like to do it without spinors. I am sure it can be done in a pedestrian way: I do not want to give an account of spinors just for this single purpose.

On Wednesday I hope to send you the summary of my formulae for Chapter VI. And as I said I shall bring along with me the (n-1) copy of this chapter.

Looking forward to seeing you. With best wishes,

Sincerely,

Chandra

S. Chandrasekhar

SC:gw

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Laboratory for Astrophysics and Space Research

September 11, 1981

Dr. Basilis Xanthopoulos
University of Thessaloniki
Department of Astronomy
Thessaloniki
GREECE

Dear Basilis,

In response to your letter of September 3, I have today written to the two selection committees in Athens.

In many ways I am sorry that you will not come to Poland. I should have particularly liked to find out your reactions to Chapter 9; and also to discuss the material I plan to include in Chapter 10. I decided that I must include a brief introduction to spinor analysis before I went on to the separation of Dirac's equation.

I hope you understood my remarks about generalizing Ernst's variational principle for his equation. It seems to me that the additional term one must include in his Lagrangian to obtain the Einstein-Maxwell version of Ernst's equation must be quite simple and probably have an obvious physical meaning.

I plan to write the (n-1) copy of Chapter 10 while in Poland; and hopefully I will have the nth copy ready soon after we return and before we go to Russia.

Best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar

Thessaloniki, September 16, 1981.

Dear Chandra:

I read chapter IX and I liked it. I found it very clear and with sufficient details, many more in fact than I thought you will manage to give. I think that at every step the reader knows what he is substituting, which equations in using and what it gets. I do not think that you had more obligations for derivations.

I think that the following two things are missing from chapter IX.

(i) A discussion of what is known and what is not known about the completeness of the normal modes used, and what are the corresponding conclusions about the stability.

(ii). You mention that certain quantities are gauge dependent and you set them equal to zero. I do not think that it is true that we can make to vanish any gauge dependent quantity. This depends on the equations this quantity satisfies and we usually have to show that if we make it zero for $t = t_0$, say, then the equations preserve the vanishing of this quantity. Since the entire theory depends crucially on the gauge used (at least the $\psi_1 = \psi_3 = 0$ of the gauge) some discussion could have been including.

Some other minor remarks now:

1. First sentece of § 79. The equations are not linear, the involve, for instance, $\kappa \Psi_1$. You only

want to say that terms like $k \Psi_1$ will be of the second order and hence they will not contribute in the linearized equations.

2. Page 3, eq. (5), a bracket is missing in the exponent.
3. Page 7, The reference before eq. (24) should be to equation (20).
4. § 80, a. I am suggesting one sentence stating that the phantom gauge will not be used in the solution of the problem.
5. Page 13, line before eq. (47) you could write modulo $\Delta \mathcal{D}^\dagger_{-1} \mathcal{D}_0 + 6 i \sigma r - \lambda \equiv 0$
6. Page 22, Eq. (81) in the second expression you have one extra $\mathcal{D}$.
7. Page 27. Since you have introduced the "final" variables in eq. (95), I am recommending of using U and V instead of $A^1_1 + A^2_2$ and $A^3_3 + A^4_4$ in equations (97), and (98).
8. Page 47, line 14. I think that "the problem of specifying the relative normalization of the functions R_{+2} and R_{-2} <u>is the same</u> to the problem of specifying C_1 and C_2 of $C = C_1 + iC_2$ not merely <u>"closely related"</u>
9. Page 91, line 4 from the end, $Y = (-iP_2 + iP_{-2})$
10. Page 112. At the end of § 95 I am suggesting to mention explicitly the Newman-Penrose equations which were never used in your analysis. Somebody might decide to study their consequences too. For instance do they imply more identities among the Teukolsky functions?

11. Page 147. Equations 505, 507 and 508 imply that $\frac{d^2E}{dt\,d\Omega} = dM$, which does not seem right.

What is happening?

12. Page 150, Eqns (523) and (524), I think that they need an extra factor of "2". I've obtained

$$D\tilde{p}^{(2)} = 2|\sigma^{(1)}|^2 + 2\epsilon_0\tilde{p}^{(2)}$$

$$\tilde{p}^{(2)} = -2\int_v^\infty \ldots$$

And again, I don't quite understand the arguments which lead from eq. (527) to eq. (528), the change of the order of integrations and of the corresponding limits of integration.

Also we will discuss these matters in the phone, I will send this report to Chicago, you will find it after your return from Poland.

Sincerely,

Basilis.

Eq. 411 is wrong. Correct in the notes.

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Laboratory for Astrophysics and Space Research

December 21, 1981

Professor Basilis Xanthopoulos
Department of Astronomy
University of Thessaloniki
Thessaloniki
GREECE

Dear Basilis,

I am sorry to report that as a result of the seminar I gave on Friday on Toroidal black-holes, I have come to the conclusion that our enthusiasm for what we have done was exaggerated if not misplaced. As I wrote once before, "This is how the world ends: not with a bang but with a whimper."

It appears that static toroidal black-holes, as well as stationary black-holes (toroidal or otherwise) which are not asymptotically flat, require external distributions of matter with negative energy. Even though Bob Geroch tried to retract, I am convinced that his initial uninhibited remark "I do not think that toroidal black-holes are of much interest" is essentially justified. However, Jim Hartle seems to think otherwise. In any event, I believe that we should not hurry to come to a decision on the significance of the solutions we found. ?

On the static distorted black-hole, with spherical topology, the principal interest appears to be in satisfying the condition of asymptotic flatness along the axis of symmetry (see pps. 11 and 12 of my notes). Incidentally, with regard to these notes, I should be extremely grateful if you can check the final formulae on top of page 11.*

On the matter of Wald's method, we could not make very much sense since what he calls the "Einstein operator" does not really refer to the system of equations we were concerned with; it is an operator for which he does not even have to write an expression: it is from heaven! I do not think that I will explain further in this matter, since I have abandoned any thought of including it in the book.

I am starting on the n-1 of Chapter 11; and I do want to keep to the ultimate deadline of the Publishers, namely January 10.

I hope you had a happy time with your family in Rhodes. With best wishes,

Yours sincerely,

Chandra

S. Chandrasekhar

No

*When one has a system of equations like these for which the integrability condition is guaranteed, should one be able to write the solution in explicit form?

Thessaloniki, December 22, 1981.

Dear Chandra:

I would like to report a few more things which I think I understand about the toroidal black holes. Unfortunately, the conclusions are negative. I refer in the letter I send you a few days ago, pages 71 - 77.

We assume that the coordinate chart we are using covers the horizon at $n = n_0$. Therefore the coordinate chart should not break down for $n = n_0$ and the determinant of the four dimensional metric, denoted as g_4, should not vanish for $n = n_0$. This determinant is $g_4 = -e^{2p} \cdot e^{2\mu_2 + 2\mu_3}$ and from equations (2) and (13) it is easy to see that it has the behavior

$$g_4 = (s - s_0)^{2a+3} \times \text{function of } \theta, \tag{1}$$

where we are looking for a solution of the form

$$\psi - \gamma = k \log(s - s_0) + f(\theta) \tag{2}$$

for which

$$\gamma = a \log(s - s_0) + h(\theta). \tag{3}$$

From eq. (1) we conclude that $a = -3/2$. Then we also demand that the horizon is a regular surface and you conclude, as in the previous letter that

$$2a + 2k + 5 = 0. \tag{4}$$

Therefore $k = -1$, $a = -3/2$ and we are forced to choose the solution $k = -1$ which I worked out in the previous letter for simplicity. If we choose the solution with the plus sign in eq. 18 of the previous letter we obtain the metric

$$ds^2 = \frac{1}{2}\tan^2\frac{\vartheta}{4}(d\varphi)^2 + 2\cos^4\left\{\frac{\vartheta}{4}\right\}(d\vartheta)^2 \qquad (5)$$

$$0 \leq \varphi < 2\pi, \qquad 0 < \vartheta < 2\pi$$

on the horizon. The question is whether the metric (5) can be extended for $\vartheta = 0$, $\vartheta = 2\pi$ so that it represents the metric on a torus. Unfortunately, I think that it does not. Here is the argument:

I have evaluated its scalar curvature (of the two dimensional metric (5)). It is

$$R = -\frac{1}{4\cos^6 \vartheta/4} \qquad (6).$$

The scalar curvature is $-1/4$ for $\vartheta = 0$ and infinite for $\vartheta = 2\pi$. I think that this conclusion says that we cannot identify the points (rather circles) $\vartheta = 0$ and $\vartheta = 2\pi$ and construct a torus.

In addition, there is the Gauss-Bonnet theorem which ~~says~~ demands that

$$\iint\left(\frac{1}{2}R\right)\sqrt{g}\, d\vartheta\, d\varphi = 0 \qquad (7)$$

for a torus, because the Euler number of the torus is zero. $\frac{1}{2}R$ is the Gaussian curvature and $\sqrt{g}\, d\vartheta\, d\varphi$ is the volume element of the 2-dimensional metric (5). Again, for the expression (6): the integral (7) blows up.

I don't think that the blowing up of the scalar curvature (6) is due to the fact that the ~~vol~~ volume element of (5) vanishes for $\vartheta = 2\pi$. I have checked the same ideas for the Kerr metric, where the volume element also vanishes for $\rho = 0$, and the integral (7) gives the correct answer demanded by the Gauss Bonnet theorem. The calculations for the horizon of the Kerr metric are included as pages 11 and 12.

One more thing. The solution (2.9) of the Geroch-Hartle paper, which serves as the basic metric for the construction of the static toroidal black holes corresponds in our language to the choice

$$\psi - \nu = \log 8m^2 - \log(s - s_0) - \log(\sin \vartheta/2) \qquad (8)$$

For this solution

$$\mu_2 + \mu_3 = \log\left[(s - s_0) \operatorname{ch} \eta\right], \qquad (9)$$

and the metric becomes

$$ds^2 = -\frac{(s-s_0)^2 \sin^2 \vartheta/2}{16 m^2}(dt)^2 + 4m^2 (d\varphi)^2 + 2\operatorname{ch}^2\eta\,(d\eta)^2 + \qquad (10)$$
$$+ \frac{1}{2}(s - s_0)^2 (d\vartheta)^2$$

The coordinate chart breaks for $s = s_0$. Do all these mean that our coordinate system is not better than the Weyl coordinates, ~~or that~~ in the sense that the horizon does not appear as a regular surface with the coordinate chart valid from both sides of the horizon?

I will be thinking about this problem in Rhodes and I will call you if I have some progress. I will return to Thessaloniki on Monday, January 4.

Sincerely,

Basilis.

P.S. In the two equations between equations (1) and (2) of page 7L of the previous letter the curly bracket should be

$$\left\{ \frac{(\mathrm{sh}\,\eta - \mathrm{sh}\,\eta_0)^2}{4\,\mathrm{ch}^2\eta} (\psi - \nu)_{,\eta}^{2} - (\psi - \nu)_{,\vartheta}^{2} \right\}.$$

However, this mistake is not propagated in the remaining computations.

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*Laboratory for Astrophysics
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January 26, 1981

Dr. Basilis Xanthopoulos
Department of Astronomy
University of Thessaloniki
Thessaloniki, Greece

Dear Basilis: [illegible], 4:00', 4:30'.

I have now confirmed our reservations to Thessaloniki: we shall be arriving on Sunday, February 22 at 5:20 PM by Lufthansa flight 312 from Frankfurt. If it is convenient for George Contopoulos, we should plan to leave Thessaloniki on Saturday, February 28 for Athens; and our return trip is scheduled for March 1 leaving Athens by British Airways flight 561. 9:30 a.m.

I hope you can make reservations at some hotel for Lalitha and me for the period February 22 - February 27 inclusive. We should like to have a room with a private bath.

I expect your corrections for the (n-1) copy of Chapter V sometime this week; and if I do I shall write the nth copy next week and mail it to you Express before Monday, February 2. I shall bring along with me the (n-1) copy of Chapter VI.

Looking forward to seeing you.

Yours sincerely,

Chandra

S. Chandrasekhar

SC:gw

March 26, 1981.

Dear Chandra:

Here are a few minor remarks on the $n^{\underline{th}}$ copy of chapter VI.

1. At the end of page 2 it would help the reader to mention the specialization he has to make in equations II-75, for instance that $f_1 = f_{,1}$, that most of Q_{AB} vanish e.t.c.
2. Your presentation of the methods for generating more solutions has the disadvantage that they are mentioned in three different places, but I cannot suggest any easy cure.
3. Page 11, end. You mention that the choice of gauge "implies no essential loss of generality". I think that there is no loss of generality at all and that any choice of gauge should not imply any loss of generality.

4. Page 22. I am suggesting of saying first that from the behavior of w and equations (75) and (85) we obtain the behavior (86) and then that this behavior is consistent with the equation (81) The field equations should come at the very end and consistency is that the Kerr solution exists. Also, before eq. (83) you could refer to equations (61).

5. Page 28, Eq. (119), at the very end, I think you need $\sqrt{8}$.
6. Page 34. You have also to change $V_{,3,3}$ in line 7 into $V_{,\theta,\theta}$.
7. Page 39, Eq. (148). It would be more symmetrical if you change the second term of the first line into $+\frac{X_2}{X_1}(Y_1 - Y_2)F(X_2, Y_2)$.

8. Page 40, Eq. 149, in the first terms of the third and fourth lines you need $(Y_2 - Y_1)^2$.
9. Page 45, Eq. 155. I have the same trouble I had a month ago. I cannot see why the terms in $(X_{1,n})^2$ and $(X_{2,n})^2$ in lines 3–6 cancel.
10. Page 58, Eq. 183, The third term should be $-C^{m}_{ij}T_m{}^k$.
11. Page 65, Eq. 218, the first term needs a square: $\frac{\Delta}{\rho^2}(du - a\,d\tilde{\varphi}\sin^2\theta)^2$.

12. Page 70, After eq. 235. It seems to me that the singularity is a ring in a surface $t=$ const and a 2-dimensional cylindrical surface in the spacetime.

I cannot see what is wrong in the treatment of the nature of the spacetime. It would help me if you could put your suspicions in writing and mention the exact remark in the paper of Boyer and Lindquist you mention in the phone.

I agree with you that one will not be able to prove the existence of the Killing tensor quite generally, for any type D metric; in some point you have to use that you consider the Kerr metric. For instance, Paul Sommers writes (J. Math. Phys. 14, 787, 1973), at the very end of the paper) that any type D metric admits a conformal Killing tensor and that most of them admit a Killing tensor. Therefore, my idea of expressing the coordinate r in terms of Newman-Penrose quantities and then using the N-P equations for type D metrics will not work.

My plans are to go to Max-Planck-Institut, München, from June 1 to middle September. August 11-20 I plan to go to Berlin for a conference on mathematical Physics. I am not going to Erice, Italy.

Regards to your wife.

Sincerely,

Basilis.

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Laboratory for Astrophysics
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January 28, 1981

Dr. Basilis Xanthopoulos
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Thessaloniki, Greece

Dear Basilis:

Your letter with your detailed remarks on the (n-1) draft of Chapter V arrived yesterday. I am once again grateful for the time and effort you are spending in helping me in this way. As I said over the telephone, I shall try to send by Express mail the nth copy of Chapter V not later than February 9 so that you may have a chance to look through it before our arrival.

As for the title of my Colloquium talk, I hope that 'The Black Holes: The Why and the Wherefore' will not be too popular.

Looking forward to seeing you soon.

Yours sincerely,

Chandra

S. Chandrasekhar

SC:gw