A Biased and Personal Description of GR at Syracuse University, 1951-61

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Abstract: In mid century, General Relativity was largely in the doldrums. Though at the time I was completely unaware of it, there were perhaps only four or five active groups around the world working in GR; Hamburg (Jordan), London (Bondi), Princeton (Wheeler), Warsaw (Infeld) and Syracuse (Bergmann). I had the privilege and pleasure of being a member of the Syracuse group working under Peter G. Bergmann. I would like to describe some of the things that took place there, who were the active participants, who we interacted with, what was accomplished and finally conjecture what role we played in the revitalization of relativity in the late 1950s and early 1960s.

1. Preliminaries

As a preliminary remark I want to say that I am not an historian of science - I am (I think) a working mathematical physicist - and I do not know the modus operandi of historians. I once read a history book - the biography of Erwin Schrodinger - and, from it, I thought that historians were only interested in the well known licentious behavior of physicists. But our good friend John Norton quickly and definitely informed me that I had been misinformed. So I came to this meeting with a talk prepared about general relativity (GR) at Syracuse University in the years 1951-61; completely leaving out the rich details that Norton thought inappropriate. But then listening to all the talks in the first 3 conference days, I had the realization that I still did not understand how historians of science understood history. Every talk in the first 3 days dealt with Relativity *before* 1950. So on the night before my talk, thinking that history ended in 1950, I redid my notes so that I could describe to you GR at Syracuse University before 1950. That did present a problem, since there was not much GR done at Syracuse University before 1950 - but I managed to make up some interesting facts. Then to my dismay, this morning, just before my talk, I heard two lectures describing events that took place in the second half of the 1900s. And again I realized that I was wrong. In desperation, in the last 15 minutes, I went back to an earlier and more noble draft of my talk. Unfortunately, by now, it is slightly schizophrenic and disorganized.

But levity aside, the fact is that I really did not know what would be of interest to professional historians. What appears in the following report is my guess - and with it a hope that I have touched on some topics of interest.

2. Subject

I start with the premise that general relativity (GR) in the late 1940s in the U.S. was in the doldrums[1]. This view arose as my personal observation made as an undergraduate student at New York University. At the time, nobody seemed either interested or knowledgeable about GR nor did I find it easy to get information about potential graduate schools that had GR programs. In fact the only school I did find was Syracuse University - which eventually became my academic home for five years. [Jean Eisenstaedt[1] recently pointed out to

[1]dolodrums;.a. A period of stagnation or slump. b. A period of depression or unhappy listlessness.

me a paper of his that gives a much more complete and objective description of this stagnation of the field during this period.]

There are several points that I would like to try to convey to you. The main one being that in the early 1950's, we saw the beginnings of a reawakening of the field - in retrospect one can see that it occurred almost simultaneously in the several different schools (to be discussed below) - but in my inexperienced eyes I only saw it occurring at Syracuse University. The emphasis in my talk will be on the GR group, at Syracuse, under Peter G. Bergmann during the years (1950-61). I will describe the personnel there, a bit of their subsequent careers and some of the external interactions. The second (closely associated) point to be made was the remarkable number and high level of the collaborations that developed during this renaissance, between the different schools from around the world - and the total absence (as far as I could see) of professional jealousies or conflicts. I believe that the very high level of scientific activity in the different groups with the subsequent interactions between groups played a critical role in the rebirth of interest in GR. A prime example of this, (I emphasize and describe in more detail later), is that in the brief period, 1960-62, essentially, the entire theory of gravitational radiation was developed by the strong interaction of many workers from Syracuse, London, Hamburg and Warsaw via personal contacts and word of mouth communication. The published material came afterwards with, to the best of my knowledge, complete attributions and acknowledgments. Though there is no way to prove or document it, I am quite convinced that the high quality of the science came, at least partially, from this free exchange of the ideas.

Though Bergmann was deeply involved in many different research projects (e.g., quantum gravity and the search for observables, gravitational radiation and statistical mechanics; see below) the main emphasis here will be on the development of radiation theory. The quantum gravity aspects of the Syracuse program are better known and have been already reported on[2].

3. Personal Calendar; for Perspective

Since I am describing my own observations, personal experiences and biases, it seemed to me that, for perspective, my personal calender might be of some interest and value.

1. I graduated as a physics major from New York University in the spring of 1951 and went to Syracuse in Sept. 1951 as a grad student to work with

Peter Bergmann. {Anecdote; a few weeks after I arrived in Syracuse I saw that a well-known left-wing journalist, I.F. Stone was giving a public talk. I went to the talk but with considerable trepidation since I had come from a fairly left-wing family background and the time was at the peak of the Joe McCarthy witch-hunt period. In my mild state of paranoia I actually had my collar turned up so that I would not be recognized. As I sat there, to my joy almost the entire Syracuse relativity group openly walked in, talking and laughing happily. From that moment on I felt at home.}

2. Since it was in the midst of the Korean war and I was in danger of being drafted, I stopped for a Master's degree (in 1955) before continuing on to the Ph.D. (1956).

3. I joined the faculty of the Physics Dept., University of Pittsburgh 1956 and remained there until the present - with many leaves of absence.

4. I spent 6 months in Europe (1958); visiting Copenhagen, Hamburg, London, Dublin, Liverpool. This was my first exposure to the European Relativity Community and was, for me, an eye opener and of the greatest importance in my scientific development.

5. I made yearly return visits to Syracuse, 1956-60.

6. In 1961 I returned for the year to Syracuse. For me this was again a period of great importance.

7. Over the subsequent years I have retained a close relationship with the Syracuse relativity group.

4. Participants

Peter G. Bergmann, the leader of the group, though born in Germany, received much of his training with Philip Frank in Prague before coming to the USA as Einstein's assistant. After a brief period on the faculty at Lehigh University, he joined the Syracuse faculty in 1947, remaining there until his retirement in 1982. Though he had many graduate students and post-doctoral fellows over the years, I will list and comment on only those I knew in the 1950s. I will give the names and approximate dates the participants were at Syracuse and then give a few of the salient items of their subsequent careers. Unless stated otherwise, everyone remained as well-known researchers in GR.

The list is divided into two periods; the early years (1951-56) when I was in residence at Syracuse and then the later years (1959-61) when I was a frequent visitor.

I. early years;

a. Josh Goldberg; (grad. student, 1947-52); Wright-Patterson AFB, Syracuse Univ.

b. Jim Anderson; (grad. student, 48-53?), Univ. of Maryland, Stevens Inst. of Tech.

c. Ralph Schiller; (grad. student, 47-53?), Stevens Inst. of Tech. (Switched fields to Biophysics)

d. Ezra T. Newman; (grad.student, 51-56), Univ. of Pittsburgh

e. Irwin Goldberg; (grad.student, 52-57??), Drexel Univ. (He appears to have dropped out completely. I could not find any mention of him any place.)

f. Al Janis; (grad.student; 53-57), Univ. of Pittsburgh

g. Rainer Sachs; (grad.stud; 54-58?), Kings College, London, Berkeley (after an extremely distinguished career in GR, he switched to Biophysics)

h. Jeff Winicour; (grad.stud; 59-64?), Wright-Patterson AFB, Univ. of Pitt.

i. Arthur Komar; (post-doc., 58...?), Syr. Univ., Yeshiva Univ.

II. Later Years ~1959-61 (*=returned, after many years, to Syracuse for a long term visit)

a. Roger Penrose; (Cambridge) - Birkbeck College, London, Oxford Univ., (Rouse Ball Prof of Mathematics)

b. Ivor Robinson; (Cambridge) - Univ of Texas

c. Andrzej Trautman; (Univ. of Warsaw); Univ. of Warsaw

d. Engelbert Schucking; (Hamburg); Univ of Texas, NYU

e. Melvin Schwarz; – Queens College, NY

f. *ET Newman

g. *R. Schiller

h. *A. Komar

i. *R. Sachs returned to Syracuse for short visits in 1961

j. Juergen Ehlers; (Hamburg) Univ. of Texas, Max Planck Inst. Munich, Einstein Institute, Golm, (Director Emeritus)

The intent of this list is to show how the influence of the Syracuse GR Group - the former graduate students, the post-doctoral fellows and long term visitors - spread through the US and Europe. There is no suggested implication that it had a greater or lesser influence than any of the other groups that will be mentioned shortly.

5. Major Research Interests of the Syracuse Group

The major research interest of the Syracuse GR group,- especially so in the earlier years (1949-58), though the interest continued for many more years, - was in Canonical Quantum Gravity. My belief is that the resurgence of world-wide interest in quantum gravity was largely due to the Syracuse group. Someplace in the middle, considerable overlap developed with the work of Dirac and the Princeton group. One of the main contributions made during this period was to the theory of pathological Lagrangians, their related constrained Hamiltonian systems and the search for observables. I myself worked in this area until 1956. The story of this research direction was reported by Josh Goldberg[2] in (1998). I will make no further mention of it other than to say that it was scientific questions concerning quantum gravity that led to the interest in gravitational radiation.

A second research topic of considerable interest to the Bergmann group was the Foundations of Statistical Mechanics. Though there was a great deal of personal interaction between the students doing GR and those doing Statistical Mechanics, it is not clear to me if there was any real scientific spin-off from these interactions.

A third topic of interest was the Theory of Gravitational Radiation & the Asymptotic Behavior of the Gravitational Field. This began with a paper by Bergmann and Sachs in (1958). The balance of this report is devoted to this subject. We will discuss in detail the interactions between members of the different groups and the specific technical ideas that grew from individuals and how they spread.

Remark 1. *I point out that Wright-Patterson Air Force Base provided financial support for the Syracuse and King's College groups (among several other relativity groups) from the mid 1950s to the early 1970s - during a most productive period. A question often asked is why did they do so. Though I was not privy to any internal Air Force information, once, when I spent a three month period working at the base, a full-time base-scientist remarked to me that they hoped to be able to understand and perhaps develop anti-gravity devices. It does seem likely that this idea played some role in their financial support. I have always hoped and believed that someone there understood that fundamental science should be supported and was valuable in its own right. I never saw any pressure from them to develop anti-gravity ideas.*

6. Parallel Developments

At the same time that the Syracuse group was developing, unknown to me was the parallel growth of several other groups. The main ones, from my perspective, were the Princeton Group under John Wheeler - (their overlap with the Syracuse group was in the field of quantum gravity) - and the Kings College, London, group under Hermann Bondi with the overlap being in gravitational radiation. Other groups playing basic roles were the Univ. of Hamburg group under P. Jordan and the Univ. of Warsaw group under L. Infeld.

Though there is not, in any sense, a unique way to organize the associations, I will give a *rough* grouping of the main players with their early close associations;

I. Bergmann, Goldberg, Sachs, Newman

II. Bondi, Pirani, Trautman, Penrose, Robinson.

Referring now only to the theory of asymptotically flat space-times and gravitational radiation, I will briefly describe (mainly from my memory) how these main players interacted with each other and what were the scientific/technical ideas that were developed. [In the appendix, I have included a technical glossary of the terms used. A star (*) near a technical term will indicate that its definition can be found in the appendix.] I find it impossible to know precisely who had what idea first - the publication information of (some of) the main papers [also included in the appendix] are not at all a good indicator of when the ideas were developed or even who was the first to propose an idea.

One of the first papers to seriously approach the issue of gravitational radiation was that of Goldberg in 1955 (following discussions with Bergmann) where it was shown, via the EIH approximation for equations of motion, that there was a radiation reaction term in the force law.

Probably the first major idea, for the direct study of gravitational radiation was that of Felix Pirani, namely, to use the degenerate structure (whose existence was first pointed out by E. Cartan in 1922 and rediscovered by the Russian mathematician Petrov) of the principal null vectors* (pnv) of the Weyl tensor* for the discussion of radiation. This material was eventually referred to as the Petrov - Pirani - Penrose Classification* of the Weyl tensor. (From an historical point of view it is perhaps of interest to note that Cayley, studying the algebraic classification of 6x6 matrices had already found essentially the same classification.) Closely associated with Pirani's work was that of his close collaborator Trautman, who found (by generalizing Sommerfeld's work on radiation in Maxwell Theory) the asymptotic fall-off properties of the Weyl tensor for outgoing radiation.

Bondi made probably the major contribution with his realization that the most appropriate way to study gravitational radiation was to introduce null coordinate systems, i.e., systems where one of the coordinates formed a family of null surfaces. He then applied this idea to obtain the asymptotic solution to the Einstein equations with the assumption of axial symmetry and analyticity in 1/r near infinity. The profound result from this work was the proof of the existence of gravitational radiation and the resulting mass loss from the gravitating system - the Bondi mass-loss formula. It has recently been suggested[2] that at about the same time Trautman had obtained the same result.

Sachs, after his initial work with Bergmann, on the asymptotic behavior of linearized multipole fields, moved to Hamburg and interacting with Ehlers and Schucking, began his generalizations of Bondi's work. First dropping the axial symmetry condition and giving it a covariant formulation, he gave a very general form of the asymptotic radiation metrics. Included there was a geometric formulation of the remaining coordinate freedom (first observed by Bondi) now known as the Bondi-Metzner-Sachs group, the asymptotic symmetry group of a radiation space-time. He also stressed the geometric meaning and importance of several of the relevant variables of the theory, namely the optical parameters*; (the shear, the divergence and the twist of a null congruence) as well as giving their dynamical equations, the optical equations*.

A major result that was developed in this period was the so-called Peeling Theorem*; a powerful detailed description of the algebraic properties of the asymptotic Weyl tensor which associated the different coefficients of powers of (1/r) with the Petrov - Pirani - Penrose Classification*. Though, I believe, much of this insight must have come from Sachs, I do not know exactly who first gave its precise formulation.

Following Ivor Robinson, who first pointed out the importance studying shear-free null geodesic congruences, Goldberg and Sachs, interacting in London with Bondi's group, proved the beautiful Goldberg-Sachs Theorem*. This theorem shows, for vacuum metrics with degenerate principal null vectors, that the degenerate vectors were tangent to null geodesics and had vanishing shear*. Closely related and in the same period, Robinson and Trautman, interacting in

[2] It was P.Chrusciel, a former student of Trautman, who made this suggestion. Its status is, however, not at all clear. Trautman, who is extremely modest, has not entered into the debate. The issue, which is certainly not a contentious one, is often resolved by different authors by just referring to the result as "the Bondi-Trautman Mass Loss Theorem" with no discussion.

London, integrated the vacuum Einstein equations for spaces with a degenerate principal null vector that had nonvanishing divergence but had vanishing twist. These two works stimulated a great deal of activity over the years.

Starting in a different direction, Penrose (1961) reintroduced[3] and greatly extended the use of spinor algebra and calculus into GR. At first many thought it was just another formal method for stating the Einstein equations but with little practical use. However in 1962 Penrose (using spinors) with Newman (working with the tetrad calculus) developed the spin-coefficient formalism which turned out to be a very powerful tool for the study of GR[4]. They obtained all the results of Bondi, Sachs and Goldberg in a much simpler fashion, the Peeling Theorem was almost obvious and they were able to drop the Bondi-Sachs assumption of analyticity for the asymptotic behavior of the radiation fields. Many further details and applications for radiation theory arose from this formalism. Soon after this Penrose extended these ideas with his introduction of Null Infinity and the conformal compactification of space-time. Many of the concepts and ideas arising from the theory of asymptotically flat spaces, the use of spinors or tetrad calculus, the use of conformal techniques, etc. are still, 40 odd years later, in active use.

In 1961 Bergmann invited many of these players (Robinson, Trautman, Schucking, Newman, Penrose) to Syracuse where many of these ideas were extensively discussed and developed.

The point of this brief history was to highlight the remarkable scientific developments coming from so many places and people, that occurred in such a brief period of time. It seems clear that these results played a major role in the revitalization of GR in the second half of the 20th century. I find it difficult to believe that all the effort and money devoted to the detection of gravitational radiation would have been expended without, for example, the Bondi mass-loss theorem.

7. Postlude

I look back on the years (1951-61) as one of the most exciting scientific and personal times of my life. I felt close to virtually all the participants and even up to the present, I keep in close contact with many or most. A sad fact for relativity

[3]For an early discussion of spinors in GR see, for example, W.L.Bade and H.Jehle, Rev. Mod. Phys.**25**, 714, (1953)

[4]In 1981 it received the Citation Index Award for being one of the most cited papers in GR.

is that two of the very best, Ray Sachs and Felix Pirani, with no explanation to the community, simply dropped out of relativity at the height of their intellectual powers. I have asked them both for explanations - with no satisfactory answers. On the other hand they both seem to be perfectly content. Pirani has been writing very successful children's books, popular science books and even a play. Sachs has made a completely new scientific career in Radiobiology, Computational Biology and Mathematical Biology. Most of the others have had excellent productive careers in Mathematical Relativity - with Penrose being probably the world's most eminent or famed relativist. {Anecdote: To illustrate my high regard for both Penrose and Sachs, I wish to recount a slightly, for me, embarrassing tale of many years ago. I had submitted a paper to Journal of Math. Phys. (on what is now known as the Kerr-Newman metric) which came back with some excellent referee critical comments that I completely accepted and agreed with. A short time late, while talking with Penrose, I commented to him about this excellent referee report; telling him that it was such a well written report that only one person in the world could have done it, namely Ray Sachs. Penrose, rather sheepishly, replied "maybe there was someone else who could have done it".}

Final Comment: Though I can not think of a single effect or equation or metric in relativity that I would call or refer to as the "Bergmann ....", nor is there a single paper of his that, I could say created a revolution in thought, nevertheless I believe that he was among the clearest and deepest thinkers in relativity. He played a key role in developing the directions the field took – from Quantum Gravity to Radiation Theory - through his publications, his university courses, his gentle but strong influence on his students during the long talks and walks, his conference reports and lectures. His influence in keeping the field alive was inestimable. In addition, he was one of the kindest, most intellectually honest and honorable scientists I have known - and he emphasized to his students the importance of these attributes. And he was loved by his students, post-docs and colleagues - and they carried on the traditions and love for physics and GR in particular, that he had imparted to them.

8. Apologia

I want to emphasize that the story I have told here, regarding gravitational radiation and asymptotically flat spaces, is largely from memory - dipping on occasions - into books and papers for references and some memory help from a few friends. If I have errors of fact, I do apologize and hope that I can correct them. However the judgments as to the most important and influential scientific contributions were mine - though perhaps easily argued with. I know that I have left out many friends and colleagues whose work did play a significant role in the research directions described here - some whose direct influence I could easily feel and see were E. Schucking, A. Janis, T. Unti - others, whose influence was there, but further afield from me, were A. Schild and R. Geroch. Probably I have slighted and perhaps even hurt colleagues; but they should know it was done unwittingly - they can blame an aging memory.

References

[1] Jean Eisenstaedt, *Low Water Mark of General Relativity, 1925-1955*, Proceedings of the 1986 Osgood Hill Conference, Einstein Studies Vol.I, D. Howard and J. Sachel Editor, Boston; Birkhauser1986

[2] Josh Goldberg, report to the History of GR Conference; Notre Dame, 1998, to be published

9. Appendix

9.1. Technical Glossary

1. The Weyl Tensor; defined from the Curvature Tensor by

$$\begin{aligned} C_{abcd} &= R_{abcd} - \frac{1}{2}(g_{ac}R_{bd} - g_{ad}R_{bc} + g_{bd}R_{ac} - g_{bc}R_{ad}) \\ &\quad -\frac{R}{6}(g_{ad}g_{bc} - g_{ac}g_{bd}) \end{aligned}$$

2. Principal Null Vector (pnv) of Weyl Tensor; Four independent algebraic solutions for l_a;

$$\begin{aligned} l_{[c}C_{a]ef[b}l_{d]}l^e l^f &= 0 \\ g^{ab}l_a l_b &= 0. \end{aligned}$$

sometimes degeneracies (two or more coinciding) => algebraically special.

3. Algebraic Classification; Petrov - Pirani - Penrose Classification

$$\begin{aligned} Type\ I \text{ or } general &= [1,1,1,1], \\ Type\ II &= [2,1,1], \\ Type\ D &= [2,2], \\ Type\ III &= [3,1], \\ Type\ IV\ or\ Null &= [4]. \end{aligned}$$

4. Optical Parameters; for a null geodesic field l_a; $l^a \nabla_a l^b = 0$

$$\begin{aligned} divergence &= \rho = -\frac{1}{2}\nabla_a l^a, \\ shear &= |\sigma| = \frac{1}{\sqrt{2}}(\nabla_{(a} l_{b)} \nabla^a l^b - \frac{1}{2}(\nabla_a l^a)^2)^{\frac{1}{2}} \\ twist &= \nabla_{[a} l_{b]} \nabla^a l^b \end{aligned}$$

5. Optical Eqs. with affine parameter r,

$$\begin{aligned} \partial_r \rho &= \rho^2 + \sigma\overline{\sigma} + Ricci\ component, \\ \partial_r \sigma &= 2\rho\sigma + Weyl\ component. \end{aligned}$$

6. Goldberg-Sachs Theorem; Degenerate pnv l^a, $G_{ab} = 0$; $\Leftrightarrow$

$$\begin{aligned} l^a \nabla_a l^b &= 0; \text{ i.e. null geodesic} \\ \sigma &= 0; \text{ shear free} \end{aligned}$$

7. Peeling Theorem; Alg. Prop. of Asymptotic Weyl tensor, $G_{ab} = 0$.

$$C_{aefb} = r^{-1} C^{IV}_{aefb} + r^{-2} C^{II}_{aefb} + r^{-3} C^{II}_{aefb} + r^{-4} C^{I}_{aefb} + O(r^{-5})$$

8. Bondi mass-loss; the mass term in C^{II}_{aefb} is a monotonically decreasing function in time.

9.2. Major Contributions to Radiation Theory

1. J. Goldberg, PR 99, 1873-83 (1955)

2. F. Pirani, (1956), Bull.Acad. Polo. Sci,III, **5**, p143 (Introduced Petrov Classification of Algebra of the Weyl tensor.)

3. R. Sachs, P.G. Bergmann; 1958, Phys.Rev.112, p674 (linear theory, definition of multipoles)

4. A. Trautman, King's College Notes; Lectures on General Relativity, 1958, eventually revised and published in "Lectures on General Relativity" Vol.1, Prentice Hall, 1965 (Sommerfeld radiation conditions applied to GR, very influential set of notes.) and recently republished as a "golden oldie" in the GRG Journal.

5. R. Penrose, Ann.Phys.,1960, 10, p171. (Major exposition of Spinor Calculus and GR)

6. R. Sachs, Proc. Roy. Soc.,1961, 264, p309 (Introduced optical parameters, shear, divergence, twist, asymptotic structure of curvature tensor)

7. F. Pirani, 1961, King's College Notes; published in "Lectures on General Relativity", Vol.1, Prentice Hall, 1965 (Introduce the Petrov Classification of Weyl tensors)

8. J. Goldberg, R.Sachs, (1962), Acta Physica Polonica,Vol XII, p12
(The Goldberg-Sachs Theorem; Princ. Null Vectors of Algebraically Special Metrics)

9. H. Bondi, M. van der Burg, A. Metzner, 1962, Proc. Roy. Soc. 269, p21, (Introduction of null coordinates, asymptotic solutions of Einstein equations, mass loss Theorem, BMS group)

10. R. Sachs, Proc. Roy. Soc.,1962, 270, p103 (Generalized Bondi work, elucidated the BMS group)

11. E. Newman, R. Penrose, JMP, 1962, 3, p566 (systematic use of tetrad calculus and spinor analysis, Goldberg-Sachs Theorem, Peeling)

12. E. Newman and T. Unti, 1962, JMP, 3, p 892 (Asymptotic Integration of the Einstein Eqs, the BMS group)

13. I. Robinson and A. Trautman, 1962, Proc. Roy. Soc. 265 p463, (Integrated most of the Einstein Eqs. for twist-free algebraically special metrics)

14. R. Penrose, 1963, Phys.Rev. Lttrs, 10, p66, (Introduced Null Infinity and Conformal Compactification of Space-Time)

Interaction Chart for the early days of Radiation Theory - much simplified
(Joint Publications)
1. Bergmann-Sachs
2. Goldberg-Sachs
3. Robinson-Trautman
4. Newman-Penrose
5. Bergmann-Schucking-Robinson
(Institutional Interactions)
1. Bergmann, Sachs, Goldberg, Newman
2. Schucking, Ehlers, Sachs
3. Bondi, Pirani, Sachs