Reflections

This essay was written by A K Raychaudhuri on the occasion of a conference organized in 1995 to commemorate the 40th anniversary of the publication of his paper in which he derived the equation that bears his name.

Editor

A Little Reminiscence

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I feel rather exhilerated by the honour that the organizers of ICGC-III have bestowed on me by arranging a particular session in connection with my works. However, I must confess that I am embarassed to speak at this meeting. Firstly, I believe that a characteristic beauty of science is its objectivity but my presence as a speaker apparently brings in a subjective element. Secondly, in a conference like the present one, the discussions are generally on the pressing problems of the present and possible developments that are likely to take place in the foreseeable future. Frankly, I am an old, out-of-date person who is a misfit to take part in such deliberations. In the circumstances, acceding to your request, all I can do is to travel down the memory lane and tell you something about how I came across the equation that bears my name.

It was early fifties when words like the black hole, static limit or geodesic incompleteness had not entered the scientific vocabulary and quite different types of peculiarities were clubbed under the name 'singularity'. My first interest was in the so-called Schwarzschild singularity where some metric tensor components vanished or blew up. Nevertheless, the metric determinant remained well behaved and the signature condition was nowhere violated. This was an indication that this so-called singularity was a peculiarity of the particular coordinate system, nevertheless, there were some awkward questions to be answered:

Reprinted from *Proceedings of the Symposium on the Fortieth Anniversary of the Raychaudhuri Equation*, (IUCAA, 1995).

(a) What does this peculiarity which has no analogue in Newtonian gravitation signify?

(b) Is this singularity physically attainable?

Bergmann in his book 'Introduction to the theory of relativity' referred to an unpublished work of Robertson on the penetrability of the 'Schwarzschild singularity' and also Einstein's model of a rotating cluster in equilibrium. On the basis of his model, Einstein conjectured that matter could not be compactified to an extent sufficient for the appearance of the singularity. Bergmann's concluding remark that the Schwarzschild singularity is only 'partly singular' intrigued me.

Bergmann was apparently unaware of the work of Oppenheimer and Snyder on the collapse of a spherical dust distribution and so was I. Thus I took up the study of a collapsing homogeneous dust distribution, which could be imagined to be a portion of a contracting Friedmann universe, in an outside empty space. The problem was solved using a comoving coordinate system and provided a counter-example to Einstein's conjecture. In view of the earlier work of Oppenheimer and Snyder, the result was not new but my paper correctly considered the boundary conditions which were overlooked by Oppenheimer and Snyder. This work led me to the ultimate occurrence of the collapse singularity – the singularity with which one is familiar with as the big bang origin of the universe.

But the then prevailing ideas about the big bang were confusing. Einstein speculated that this singularity signalled a failure of the general theory of relativity for high concentrations and intense fields and might be removed in a unified field theory where there will be no separation between field and matter. Others thought that the origin of the singularity lay in the assumptions of homogeneity and isotropy and in a more realistic picture, the singularity will disappear. However, Einstein did not succeed in building up such a unified field theory and Tolman and Omer investigated a non-homogeneous model only to find that the singularity persisted.

True, the steady state theory did away with the singularity but a breakdown of the long cherished conservation principle of energy did not find favour with the average physicist.

In this background came Gödel's famous paper in 1949 – the Einstein issue of the *Reviews of Modern Physics*. To be sure, I did not understand many parts of the paper but it was remarkable that there was no singularity. No doubt

there were undesirable features – the closed time-like lines, absence of expansion and a large cosmological term augmenting gravitational attraction. Nevertheless, there seemed reasons to hope that one could so change the parameters, that an expansion will be there, the closed time-like lines will disappear and what seemed most important to me, the singularity will not appear. With this naive hope I devoted considerable time trying to discover such a solution. Little did I realise that what I was doing was like searching in a dark night for a black cat which probably did not exist.

During this investigations, at same stage, a neat result of Einstein and Pauli influenced me. In the process of proving the non-existence of an everywhere regular solution representing a monopole, they have shown that the time–time component of the Ricci tensor can be elegantly expressed as a divergence. Just out of curiosity, I tried to figure out what this component will be in more general (i.e., non-static) case, spherically when rotation (*á la* Gödel) is introduced. Somewhat to my surprise, I found that the expression was not only fairly simple but one could read the equation physically, namely that the gravitation and shear (i.e., anisotropy of the velocity field) augment collapse while centrifugal repulsion opposes it.

That was the elementary form of the equation. However, I had obtained this by a rather clumsy method so that it might appear that the equation was coordinate dependent. Again I had assumed the velocity field to be geodesic and had consequently missed an important term. All these defects were removed by later researchers. Further, while I had restricted myself to material fluids whose velocity vectors are unit time-like, the case of null propagation vectors was also taken up by later workers. Anyway, in spite of all these developments the relativity community has generously continued to refer to the equation in my name.

To conclude, I may tell you that story of the publication of my paper – this will perhaps not be very boring. The result was first communicated to the Editor, *Physical Review* as a letter and was received by them on April 21, 1953. I stated the equation without proof and set out some of the consequences. The referee's report was dated May 27, 1953 and here is an excerpt from that:

"In spite of considerable efforts on my part, I did not understand this paper. The author interprets two equations ... and I have no idea whence these two equations come from. I have looked up the paper by Gödel ... and also Gödel's article in the Einstein volume of the Library of Living Philosophers... and I cannot find

any relation similar to these two. ...Somehow I have the feeling that I may be terribly obtuse... If the author would be kind enough to enlighten me concerning the derivation of the equation... I would welcome an opportunity to re-read the paper and to advise concerning its publishability. At present, I feel unable to recommend its publication."

Following this I wrote out a full paper giving the derivation and also made a conjecture that there exist solutions in which the velocity brings about a bounce from a collapsing to an expanding phase. Using the values of universal energy density, Hubble constant and galactic spins which seemed plausible at that time, I tried to argue that the conditions at the bounce agreed with the conditions required in the theory of nucleosynthesis by Gamow *et al*. In view of the astrophysical slant I sent the paper to the *Astrophysical Journal*. However, the paper was rejected as the referee considered the astrophysical considerations of dubious value.

After this I cut off the astrophysical part and sent the truncated paper to the *Physical Review*, naming the paper as 'Relativistic Cosmology I': the number I was put in as I hoped that I shall be able to prove the existence of a rotating non-singular model and also to display some astrophysical consequences in a later communication. However, these hopes were never realised. Anyway the paper was received by the *Physical Review* on December 28, 1953. An acknowledgement duly came from the Editors but then there was a long period of silence when all my enquiries could elicit no reply. Then after about fourteen months, in February 1955, came the letter of acceptance. It carried an interesting remark:

"After much prodding we were finally able to recover your manuscript from the referee. We endeavour to choose as referee those colleagues who accept this task conscientiously. We regret that in this case, there was an extensive delay". So at last the paper appeared in May 15, 1955 issue – a little over two years after the first note was sent.