J. Astrophys. Astr. (1996) **17**, 77–79

Neutron Stars Before 1967 and my debt to Chandra

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At least in his later years, Chandra was particularly famous for General Relativity, and throughout his brilliant career he was a model of mathematical rigor and elegance. I have never had a strong interest in General Relativity, I am mathematically about as sloppy as one can get away with and I have spent little time in Chicago. Because of this orthogonality, I have probably had less overlap with Chandrasekhar than most theoretical astrophysicists, and yet even in my case he has had a strong influence. I will illustrate this with a purely personal essay on my own work on equations of state and compact objects, especially neutron stars.

It is interesting to speculate on why some topics are studied when they are, and I have put "neutron stars before 1967" in the title, because the reasons for 'why' are clear after 1967: Pulsars were discovered (Hewish *et al.* 1968), it became clear that they are rotating neutron stars (Gold 1968) and radiation mechanisms were discussed even just before the discovery (Pacini 1967). In the 40 years before this, on the other hand, there were few practical reasons to study neutron stars, except for the prescient suggestion of neutron stars in supernova remnants (Baade & Zwicky 1934). When I was a graduate student in the 1940s I was unaware of this paper, but my interest was aroused in a very indirect way by the earlier controversy between Chandrasekhar and Eddington on the equation of state for relativistic white dwarf stars. In astrophysics circles this controversy is usually described in terms of Eddington as a great man with deep philosophical beliefs and unorthodox views on how the laws of science might change — i.e., it was not clear whether he was morally right in "putting down" a young man so thoroughly and consistently, but it was not clear either till much later that he was scientifically wrong. However, in 1946 I was a graduate student in physics, not in astrophysics, my thesis advisor was Rudolf Peierls and it *was* clear that Eddington was wrong right from the start! At least this was the situation with two very specific papers of Eddington's.

These two papers (Eddington 1935a and 1935b) were mainly concerned with the laws of physics in existence at the time, especially quantum mechanics and special relativity, not with philosophy or the future (in one of them there was one delightful digression into the "magic numbers" in astronomy and physics which was vintage Eddington, but this did not impinge on the main text). There were two aspects to these papers: (i) they pointed out genuine difficulties that would be faced if one wanted to carry out very rigorous and very accurate calculations, and (ii) an explicit calculation of the equation of state for relativistic electrons as Fermi-Dirac particles which not only gave the wrong result but consisted of sheer nonsense or double-talk or both! An example of (i) was how to treat Dirac electrons under high pressure, when they are not free particles but are confined by a strong gravitational field. My thesis advisor had solved this problem within a year (Peierls 1936), although it was not a trivially simple calculation. And I have worried off and on over the last 50 years about (ii). Eddington was a great man and on some level of consciousness he must have known he had written nonsense — how could he live with himself and how could two respectable journals publish such papers? I have felt that much of the answer stems from the genuine problems in (i) obscuring the treatment in (ii). I consider the juxtaposition of macroscopic and several microscopic complications in one problem a particularly exciting challenge for a theorist.

Some of the questions raised in the two Eddington papers had to do with interactions between particles, directly and through Coulomb forces, i.e., forerunner questions for the combination of plasma physics and quantum mechanics. I have worked on this combination off and on since then, stimulated not only by the negative influence of the two Eddington papers, but also by the positive influence of Chandra's numerous papers in the 1930s on the equation of state and white dwarf star structure. These papers (e.g., Chandrasekhar 1935), and my thesis advisor's paper on Dirac electrons in a large-scale potential field, actually were not easy reading and required appreciable effort on the part of a young and inexperienced graduate student to absorb. However, they were so methodical, detailed and logically constructed that, once absorbed, they acted as models for how even a youngster could write papers in the future. To digress on contrasting styles — Landau (and, in other areas, Fermi) had written brilliant papers which seemed to be easy reading at first sight but were not easy to use as role models for common mortals. Oppenheimer was smart enough to use Landau's classic paper (Landau 1932) as the starting point for his own work on neutrons stars (Oppenheimer & Serber 1938; Oppenheimer & Volkoff 1939), but I would not have been. This difference in scientific styles might also be the reason why Landau and Oppenheimer gave so little credit in their papers to Chandra's classic white dwarf papers (Chandrasekhar 1931).

My own first foray into equations of state was not really related to either white dwarfs or neutron stars, but to the plasma physics that goes into the electron screening for thermonuclear reactions (Salpeter 1954). Although I have not discussed this point directly with Schatzman, chapter 4 in his **White Dwarf** book (Schatzman 1958) suggests that he also had been drawn into plasma physics by the Chandra-Eddington controversy on particle interactions (and he worked on electron screening even earlier than I did). Given the absence of any neutron star observations, there was surprisingly much activity on neutron matter and its equation of state (e.g., Harrison *et al.* 1958; Cameron 1959; Salpeter 1960; to name just a few). This work started to blur the division between white dwarfs and neutrons stars or, rather, it provided a region of instability at intermediate densities. More specifically, inverse beta-decays change the charge of nuclei and lead to a maximum white dwarf mass occurring at finite rather than infinite density (e.g., Hamada & Salpeter 1961). Chandrasekhar & Tooper (1964) then showed that General Relativity would also have given instability above a finite density even if nuclei were unchangeable (a similar suggestion had already been made in an earlier paper (Kaplan 1949), which was missed by most of us in the west). There was also a brief flurry of activity on neutron stars before the first Dallas Relativity Symposium in December 1963, just in case quasars turned out to be neutron stars, but this false alarm was soon laid to rest. More details will be found in Harrison *et al.* (1965) and in Shapiro & Teukolsky (1983).

My own interest in neutron stars waned somewhat even before neutron stars became a reality, but not my interest in studying multiple problems, stimulated by Chandra's example of working in many different fields: Three of his many books, on three very different topics, had already appeared well before 1960 and he was well on the way to combine relativity and astrophysics into a new science of relativistic astrophysics. My own excursions into plasma physics plus ionosphere, solid state physics plus interstellar molecules or accretion flows plus black holes all were pale imitations of this, but all were helped by Chandra's books and by his example.

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