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Chandrasekhar and the history of astronomy

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Abstract. Chandrasekhar's own books, papers, and oral history interviews make clear that he was generally more interested in the present and future of astrophysics than in its past. Nevertheless, late in his life and after his death, historians of science have somewhat entangled him in two supposedly controversial issues, one concerning precursors of his mass limit for degenerate stars and the other his relationship with Eddington. Neither story is an entirely happy one.

1. The fate of famous scientists

Biographers write biographies and historians write books and papers, and no one, living or dead, has much defense against them. Among scientists I've known, Richard Feynman, Fred Hoyle, and Carl Sagan have each been the subject of at least three. Thus no one should be surprised that there are biographies and encyclopedia articles about Chandra (Wali 1991, 2008) and Eddington (Douglas 1956; Stanley 2007, 2008), and indeed even a biography of Eddington by Chandrasekhar (1983). Not that you had any doubts before, but you cannot come away from any of these without realizing that each made both extraordinarily many and extraordinarily important contributions to 20th century astrophysics. But be thou chaste as ice, as pure as snow, thou shalt not escape calumny (Hamlet, Act III, Sc. 1, to save you looking it up).

2. Degenerate stars, or, who discovered the Chandrasekhar limit?

I first encountered this issue more than 30 years ago (Trimble 1979) when I reviewed a semi-popular book by I.S. Shklovskii (1978) called 'Stars: Their Birth, Life, and Death' and claimed to have learned from it that the Chandrasekhar limit was really discovered by Yakov (variously Jacov) Frenkel in 1928. Very soon after that issue of Sky & Telescope hit the newsstands, there arrived a manilla envelope from the University of Chicago, in which Chandra had enclosed copies of his 1931 papers and a hand-written note pointing out that

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these were the first papers to use explicitly an equation of state with pressure proportional to $(\text{density})^{4/3}$, although this is implicit in the Frenkel paper. Indeed so he had said on page 409 of his stellar structure book (Chandrasekhar 1939).

First I apologized and then I sat down to read the Frenkel paper with a German born friend who was a professional interpreter. She read the short words and I the long ones. She quickly concluded that German was not Frenkel's first language and I that he had not discovered the Chandresekhar limit. Rather, he was somehow addressing degenerate baryons, though the neutron had yet to be found (Chadwick 1932) and protons never get a chance to be degenerate. And there, I supposed, the issue would rest.

It did not and has been raised again by Nauenberg (2008) and again by Nauenberg (2011) and Blackman (2011) in connection with the Chandrasekhar centenary, with a rebuttal from Wali (2011). There is no disagreement about what the several relevant papers say, but only about what Chandra knew and when he knew it, and how credit should be divided, and eponyms awarded. All agree that Fowler (1926) was first to derive an equation of state, $P = K_1(\rho)^{5/3}$ for completely degenerate matter, neglecting any possible effects of special or general relativity. With this EoS, you can build configurations of any total mass, and Chandra had read the Fowler paper before leaving India. Next, Edmund C. Stoner (1929) and Wilhelm Anderson (1929) looked for deviations from Fermi-Dirac degeneracy implied by large densities and so by occupation of momentum states close to $E = p^2/2m = m_e c^2$. Stoner reported an upper limit to densities and Anderson a modification of the EoS in the direction toward $P = K_2(\rho)^{4/3}$ which we now associate with completely relativistic degeneracy. His tables and formulae imply an upper limit to degenerate masses, but explicit calculation of this limit was left to Stoner (1930). Chandra had not had access to those papers before leaving India.

Both approximated white dwarf stars by uniform density spheres. This is not actually a foolish thing to do, and still has pedagogical value (Hansen, Kawaler & Trimble 2004, p. 16). Israel (1987) affirms that $P = K_2(\rho)^{4/3}$ is and should be called the Stoner-Anderson equation of state. Chandrasekhar (1931a,b) famously, perhaps even notoriously did his critical calculation on board ship in 1930, and Wali (2011) has concluded that he was not aware of either Stoner's or Anderson's work at the time. His work was therefore independent, but, more to the point, he adopted Eddington's (1926) polytropes for his models which could, therefore, be in hydrostatic equilibrium, which constant density stars cannot, and real ones must be. A very similar limiting mass was derived by Landau (1932, but paper submitted February 1931), but he is not mentioned by either Nauenberg (2011) or Blackman (2011).

Did Chandra give adequate credit to his predecessors? Simply reading his 1939 book one would think so, though his student, Guido Munch, said very much later that the Stellar Atmospheres book credits him only with drawing figures and not for the couple of chapters he wrote. In any case, Chandrasekhar carried on work largely on stellar structure, especially degenerate stars until 1935. And then significant portions of the roof fell in.

3. Chandrasekhar and Eddington

In the interim, Eddington had apparently been concentrating on his 'fundamental theory' (Israel 1987), and so, although he was in regular touch with Chandra and his ongoing work, he had perhaps not immediately thought what the consequences would be (Wali 1982). And when he did, he put in a paper on 'Relativistic Degeneracy' to be read at the January, 1935 meeting of the Royal Astronomical Society immediately after Chandra's presentation of extensive numerical analysis indicating that the fate of massive stars must be something other than gradually cooling white dwarfs. The next part of the story can be read by anybody with access to old journals, because Observatory, then as now, published more or less verbatim accounts of the RAS meetings, and January 1935 appears in Volume 58, page 37ff. Eddington announced that there is no such thing as relativistic degeneracy and that Dr. Chandrasekhar had rubbed in his result to a reductio ad absurdum, leaving among most hearers the impression that Chandrasekhar had made a simple mistake in his calculations.

Chandra was given no opportunity to respond at that meeting, nor was he later in the year when Eddington spoke at the Paris IAU on the non-existence of relativistic degeneracy. Eddington's toes remained dug in for a number of years thereafter, despite support for the $(\rho)^{4/3}$ equation of state from physicists (Wali 1982) and some observational confirmation (Nauenberg 2011). The questions we might reasonably ask are:

1. Was Eddington's behaviour outside of tolerances? Not, one must conclude, by Eddington's standards. This was, after all, the person who said he did not think it necessary to read a paper by Professor Milne, because it would be absurd for him (Eddington) to pretend that he (Milne) has the remotest chance of being right (Wali 1982). On other occasions, he said things equally harsh about James Jeans and others (Stanley 2008), and his remark at another meeting about generation of subatomic energy in stars, "If the honorable gentleman does not think the center of the sun is hot enough, then let him go and find a hotter place", has joined the body of folklore we share with students.

2. Why did the Eddington toes remain so firmly buried? Stanley (2008) has pretty firmly ruled out the unpleasant suggestion that racial prejudice entered into it. Israel (1987) worked carefully through Eddington's scientific output from the time of his 1923 paper 'The Mathematical Theory of Relativity', and concluded that Eddington had gradually become so wedded to his own definition of the stress tensor and its imbedding in his 'fundamental theory' that he simply couldn't conceive of the Stoner-Anderson equation of state describing anything in the real world.

3. What were the consequences, especially for Chandra? Remember the title of his 1983 book, 'Eddington: The Most Distinguished Astrophysicist of His Time'. If you are thinking that nobody writes a book just to say nasty things about someone else, you need to think again. But, more to the point, Chandra need not have written an Eddington biography at all. Nor would he have needed to have given the obituary and centenary talks cited by Wali (1982). Was Chandrasekhar's acceptance of a position in the United States partly a reaction to Eddington's attitude and the expectation that it might interfere with a successful career in the UK? Perhaps. But more firmly (Wali 2008), the Eddington controversy entered into Chandra's decision to write up his work on stellar structure in 1939 and move on to stellar dynamics. This set the pattern for much of the rest of his career, during which, as virtually everybody has noticed, he worked intensely on a topic until he felt he had learned what he had set out to learn, wrote it up, and moved on to something else, rarely looking back. Thus, just possibly, Eddington's behaviour helped nudge his younger colleague in the most productive possible direction (Dyson 2010).

4. In their own words (and deeds)

What Eddington said to Cecilia H. Payne when she expressed a strong desire to become an astronomer was, "I see no insuperable obstacle", but he advised her to go to the United States, which she did, finding the obstacles from 1925 to 1956 (when she was finally appointed to a professorship at Harvard) high, but indeed not insuperable.

What Chandra said to me when I was passing through Chicago in early May 1968, en route to a brief postdoc at Cambridge University was, "You must give a colloquium", which I did, to a nearly-filled room, despite the detail that it was Saturday and that the topic was 'Motions and Structure of the Filamentary Envelope of the Crab Nebula'. Some of the interesting things he said to other colleagues are included in five reminiscences in the December, 2010 issue of Physics Today.

What of whimsy? Eddington had his cycling number, x , the largest number such that he had cycled at least that many miles on at least that many days. It had reached 75 when he wrote to Chandra in 1938, and the concept will be recognized as the ancestor of the Hirsch index, h, having to do with citations of papers. My own piece of Chandra whimsy was his response to my question about why he had never been on one of the decadal survey committees used, in the US, to set equipment and other astrophysical priorities, starting in 1962. He responded immediately, "No one ever asked me", and, after a moment's cogitation, continued with a verse or two of an English folk rhyme ending with a pompous young man saying to a farmer's daughter, "Then I cannot wed you my fair young maid. Nobody asked you sir, she said." Not surprisingly, he gave "said" the Yorkshire pronunciation required to sustain the rhyme.

A widely reproduced Eddington quote came from his 1935 RAS talk in opposition to relativistic degeneracy: The star has to go on radiating and radiating and contracting and contracting until, I suppose, it gets down to a few km radius, when gravity becomes strong enough to hold in the radiation, and the star can at last find peace. This is surely as good a prediction of black holes or at least horizons, as is to be found in the 18th century writings of John Michell and Pierre Simon de Laplace.

Somewhat less well known is the last line of Chandrasekhar (1932) submitted during his brief stay in Copenhagen: "Given a container containing electrons and atomic nuclei (total charge zero), what happens if we go on compressing the material indefinitely?" Equally clearly, this is a prologue to neutron stars. James Chadwick announced neutrons in February, 1932 and the paper was submitted on September 28th, but in fact neutron stars had to wait another year for Baade & Zwicky (1933), about whom there are also many stories, but they must wait for another book.

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