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REVIEWS

General Relativity: A Graduate Course, by Horaţiu Năstase (Cambridge University Press), 2025. Pp. 401, 29 × 18·5 cm. Price £18·5 (hardbound; ISBN 978 1 009 57575 1).

The author describes the mathematics in the book as "physics-friendly" and "not exaggerated in its use". While both might be true, it is one of the most mathematical books I've read. I assign it a relativity-book signature of 0+−++−.[*] The assumptions and lack of derivations

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[*] The characters indicate maths-first or physics-first, breadth, depth, level, amount of maths, mathematical detail shown. With regard to the first, this book falls neatly into neither category. Although Chapter 2 provides some mathematical background, in general the necessary maths is introduced with each topic. Thus, the book

allow the author to present "various topics that many, if not most, textbooks consider too advanced, despite their being around for some time and being much used in current research" and "more modern topics that one cannot find in the standard, but now somewhat dated, books of Landau and Lifshitz; Misner, Thorne, and Wheeler; and Wald". On the other hand, the description of General Relativity (GR) is self-contained in that little or nothing about it is assumed, but little time is spent on the foundations compared to the applications. The author, originally from Romania, is a researcher and tenured asistant professor at the Institute for Theoretical Physcis of São Paulo State University, where he has been since 2010. This book corresponds to his one-semester graduate course.

With 33 chapters, the number of topics is greater than in most GR books I've read, but only the essentials of each are presented. Each chapter ends with a list of 'Important concepts to remember' followed by 'References and further reading' and a few problems (without solutions). This is not a book to learn GR from the ground up, at least not for most readers; rather, it is more like a detailed formula collection where one can quickly find the important details and a bit of context. In that sense, it is like a longer version of another book[1] reviewed[2] in these pages, though that book has less breadth and more depth than this one (as well as very detailed solutions to the problems) and makes the common mistake, avoided here, of calling the Lorenz gauge the Lorentz gauge.

Though brief, the discussion of various forms of the Robertson–Walker metric is useful and helpful for avoiding confusion; there is a similar discussion in another book[3] reviewed[4] in these pages. Of course, there are many cases of confusion where the same things are denoted by different terms or the same terms denote different things, but in cosmology that seems to lead more often to lasting misconceptions than in other fields.

I do have to point out some inaccuracies with regard to cosmology, but at least this time I now have a suspicion about the source of one type of confusion. Something similar to the following can be found in many cosmology books.

> Space then expands uniformly throughout the Universe, which leads to the "Hubble Law", that because of this expansion, the velocity of the receding away of a *nearby* point (a fixed star) is proportional to the distance r to the point, $v = Hr$. [Emphasis in the original.]
>
> To picture this, we imagine a two-dimensional closed Universe like a balloon, and filling it up with air, so that it expands. Then every point moves away from every other point, and the relative speed is proportional to the distance between the two points, as in Fig. 27.1.

Of course, as the terms are usually used, a fixed star does not partake of the Hubble flow, but that is a harmless minor point. More serious is the obvious contradiction between the two paragraphs. The second gives an essentially correct picture. In particular, the law holds for all distances and all velocities. However, the first paragraph claims that it holds only for small r. (Red herring: Could the difference be due to the fact that the speeds of dots on the balloon are small compared to the speed of light, whereas that is true in cosmology only for 'nearby' objects? That is a (wrong) claim which is often made (with the 'not even wrong' solution that one should use the relativistic Doppler formula), but that is not the problem here.) The distance r has not yet been defined, but one would naturally assume that it corresponds to the distance between the dots along the surface of the balloon, the so-called proper distance D^{P} (which is the main distance used in GR). However, on the next couple of

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covers a wide range of topics, but not in great detail (about 10 pages per chapter and topic; one could write, and indeed many have written, very thick books about most or all of the topics); the level is relatively advanced ("[a]ssuming knowledge of classical mechanics and electrodynamics at an advanced undergraduate level . . . something like Goldstein's book. . . the full 2 volumes of J. D. Jackson's book. . . a classical field theory course as well"; some familiarity with tensor calculus is also required); the presentation is very mathematical; the focus is on results rather than derivations.

pages, in the (very good) discussion of the Robertson–Walker metric, r is used to indicate what in the case of the balloon is the perpendicular distance from a dot on the surface to a diameter of the three-dimensional balloon. We have $r = R \sin \chi$, where $\chi = D^{\mathrm{P}}/R$, R being the (three-dimensional) radius of the balloon. Of course, $r \approx D^{\mathrm{P}}$ only for small r, so if the Hubble law holds exactly for D^{P} and its derivative with respect to cosmic time v, then obviously in general it doesn't hold exactly for r and v. Note that r is the more important distance in observational cosmology: r is the proper-motion distance, $r/(1+z)$ is the angular-size distance (z is the redshift), $r(1+z)$ is the luminosity distance, and so on.* The idea that the Hubble Law holds only in the limit of small distances (or velocities) might, in some cases, come from thinking that it applies to r and not to D^{P}. (The misconception regarding the relativistic Doppler formula is, however, certainly responsible for a large part of the confusion. See ref. 6 for more on the redshift–distance and velocity–distance laws.) On the other hand, separated only by two pages of Christoffel symbols and Ricci tensors, H is *correctly* defined as $\dot{a}/a$, where the scale factor $a = R/R_0$, which is equivalent to the formulation with D^{P} above since χ is constant for co-moving objects. (He also points out that it is called the Hubble constant because it is constant in space (at a given time), not, in general, constant in time — it's thus not a misnomer and if one would prefer to call it the Hubble parameter that is not a valid reason.) But there is more confusion on the very next page:

> Note that only for small distances, where $H(t) \approx$ constant as light traverses the distance from a star (or galaxy, etc.) to get to us, is
>
> $$v = \dot{a}r = H(ar) \approx H_0(ar)\cdot$$

However, as in the definition $H = \dot{a}/a$ and as explained well by Harrison[6], v refers to the *instantaneous* velocity (in the sense of the derivative of D^{P} with respect to cosmic time), rather than some sort of average.

Another common statement is that the static cosmological model which Einstein put forward in the first paper on relativistic cosmology[7] is unstable, as pointed out by Eddington[8], but also that the Einstein–de Sitter model[9] is an appealing model because slightly different models are unstable, and it is only due to observations that the Einstein–de Sitter model is ruled out. Mathematically, both are unstable in the same sense.† As idealized cosmological models, that is a non-issue, because there is no way to disturb them. If one sees them as approximations, then both are unstable in exactly the same sense. As far as I know, during the time that many considered the Einstein–de Sitter model to be a good approximation to our Universe no-one raised Eddington's objection (which applies just as much to the Einstein–de Sitter model as to the Einstein static model) against it.

Note that the chapter on classical cosmology (*i.e.*, Friedmann–Robertson–Walker (FRW) models) is only a dozen pages, almost just a footnote to the book, and my complaints refer to just a couple of pages; the density of confused or confusing passages in the rest of the book is much lower, essentially zero. As I've remarked in other reviews in these pages, certain misconceptions in cosmology tend to be repeated.

I have fewer complaints than usual about typos, matters of style, editing, and diction; it is a well-written book. There are only a few figures, most of them Penrose diagrams. (The only real goof is that the caption for figure 28.1 mistakenly repeats that of figure 22.6.) There are no footnotes. The main text is followed by six pages of 106 references; some are mentioned in the main text, but mainly they correspond to sources mentioned in 'References and further

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*For more on distance definitions, see ref. 5.

†Both are unstable fixed points (and moreover repulsors) in the dynamical-systems parameter space of the normalized cosmological constant $\lambda = \Lambda/(3H^2)$ and the density parameter $\Omega = 8\pi G\rho/(3H^2)$ (where ρ is the usual physical density of nonrelativistic matter — 'dust' to cosmologists).

reading'. (Unusual is that if a reference is cited in more than one chapter, then it gets an additional entry in the reference list, so actually there are fewer than 106.) A four-and-one-half-page index ends the book. The book is well produced with a design familiar from other CUP textbooks; my only serious complaint is that the wide margins, which some readers might find useful, are always on the left, thus the inside margin on odd pages, which is not very practical. The even-page headers contain the chapter, the odd-page headers the section; that should be the case for all books. (Despite that difference, the layout of the headers is the same, with the page number always at the upper left, above the wide margin; here also it would sit better on the right on odd pages, at the outside edge.)

This could be a useful book for those who want a concise mathematical description of many topics in GR. — PHILLIP HELBIG.

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The Royal Observatory, Greenwich, 1881–1939, by Lee T. Macdonald (UCL Press), 2026. Pp. 321, 23 × 15·5 cm. Price £40 (paperback; ISBN 978 1 80665 0449).

Having worked for several years at the Royal Greenwich Observatory at Herstmonceux in Sussex, I always had the fond notion that my spiritual home was the Royal Observatory at Greenwich, founded in 1675 by order of King Charles II for the practical purpose of helping British mariners to determine their longitude at sea. While we have all heard stories about Astronomers Royal from long ago — Flamsteed, Halley, *et al.* — much less was ever heard about those in more recent times. The present book does much to redress the balance, giving copious and well-researched (and well-referenced) details about the work done under William Christie, Frank Dyson, and Harold Spencer Jones in that period between the retirement of George Airy and the start of the Second World War. The records have been scoured for details of the staffing of the Observatory with emphasis on the work of Chief Assistants, Assistants, and 'computers' of various types in the numerous departments which greatly increased in number during this period. While the 'bread and butter' work for navigation — the provision of accurate time and the *Nautical Almanac* — was of high priority, other work blossomed beside it in the Solar Department, the Magnetic and Meteorological Department, and in the nascent astrophysical studies permitted by a range of newer and larger telescopes. International collaboration was always important, such as the *Carte du Ciel* project, and various solar-eclipse expeditions were mounted, including the famous 1919 'relativity' one. Ultimately, of course, the unstoppable growth of London (Cobbett's Geat Wen) put paid to the Royal Observatory, and after the period covered by this book the work was transferred to the wonderful location of Herstmonceux. It was fascinating to read of several personalities who worked at Greenwich and who were still busy in Sussex when I arrived (including Alan Hunter, Donald Sadler, and Humphry Smith).

This is a fine account of the 'official' work of the Royal Observatory but I was disappointed not to learn more about the personalities of those who worked there. Certainly we discover the special interests of the Astronomers Royal, but little about their characters. Similarly, I suppose, we discover little about the part this *Magazine* played, despite the fact that it was founded at Greenwich in 1877 and was based there until the move to Sussex, and many of the Editors came from the Observatory staff. And now for the future. Well, there is now a fine museum at Greenwich but of the 'real' Observatory, just read Ian Robson's fascinating account of the 'Observatory Wars' in this issue to see that the shambles that is British science has lost one of its great institutions. — David Stickland.

Discovering Quarks: Remembering Feynman, Gell-Mann, and Tollestrup, by George Zweig (Cambridge University Press), 2025. Pp. 201, 26 × 21 cm. Price £39·99 (hardbound; ISBN 978 1 009 47350 7).

This is a fascinating book that charts the history of the discovery of quarks, written by one of their co-discoverers. It is a rich and multi-faceted work, approaching the subject from many different angles. Part autobiography and part a history of the field, it offers numerous insights into the main characters in the story, illuminated by a fair bit of physics along the way. Zweig studied with Richard Feynman and Alvin Tollestrup, and the book includes chapters devoted to each of them, along with a chapter on Murray Gell-Mann. Richly adorned with quotations, personal reminiscences, and anecdotes, it provides a vivid sense of their personalities, the very different ways in which they worked, and how the community viewed the emerging ideas at the time. The book also touches on questions of recognition and whether some of the less-well-known figures received the credit they deserved. Zweig himself, although now widely regarded as a co-discoverer of quarks alongside Gell-Mann, has not received the same level of recognition. I would not say that that issue is the main focus of the book, as its scope is much broader, but it does offer some insight into possible reasons. Among those are the fact that Zweig's work initially appeared as a CERN report (reproduced helpfully in an appendix) rather than in a regular journal, the apparent lack of support from some senior figures at the time, and perhaps also the simple fact that the term 'quark' caught on, whereas Zweig's 'aces' did not. The book also contains a substantial amount of physics, which helps the reader appreciate how particle physics developed during the 1960s and 1970s, with some false starts, and it offers insights into the ways of thinking and influence of some of the field's major personalities. Students of physics, as well as readers interested in the history of science, will find it a fascinating and rewarding read. — Alan Heavens.

Probability Theory for Quantitative Scientists, by L. Leuzzi, E. Marinari & G. Parisi (Cambridge University Press), 2025. Pp. 412, 26 × 18·5 cm. Price £54·99 (hardbound; ISBN 978 1 009 58069 4).

This book grows out of a long-running course on probability theory at the University of Rome La Sapienza. It is readable, accessible, and sufficiently deep to satisfy a scientist who wants to understand the mathematical foundations of probability without going all the way to measure theory. In that sense, the title is an accurate description of what the book delivers.

The treatment is grounded in mathematics, but enriched with many examples and perspectives drawn from physics. In particular, it highlights connections with statistical mechanics and introduces topics that are rarely encountered in standard texts. Large-deviation theory, for example, appears surprisingly early and serves as a good illustration of the book's willingness to explore less conventional but important ideas.

One of the strengths of the book is the way it interweaves applications with fundamental theory, and mathematical development with physical insight. The scope is quite varied: some topics are treated in considerable depth, while others are covered more briefly, giving a sense of a guided tour through the subject. The final chapter provides a useful synthesis, drawing together themes introduced earlier.

Overall, it is clearly written and engaging, and will reward both students and researchers in the quantitative sciences. For readers who want to move beyond simply applying probabilistic techniques to understanding where they come from and how they connect to broader ideas, this is an excellent and illuminating resource. — Alan Heavens.