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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2017 November 10 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

J. C. ZARNECKI, *President*
in the Chair

The President. Good afternoon ladies and gentlemen, and welcome to the November meeting of the RAS. Without further ado, I would like to move straight into the programme for today. We start with the James Dungey Lecture for 2017, one of our named lectures. It will be given by Professor Chris Owen of the Mullard Space Science Laboratory, UCL. The title is, 'Manifestations of the Dungey reconnection process within the heliosphere'.

Professor C. Owen. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Geophysics*.]

The President. Chris, thank you very much for delivering the James Dungey Lecture. We have just a very brief time for a couple of questions or comments.

Mr. B. Fernando. With regard to your question about *Cassini*, I have looked at the first 520 crossings of the magnetopause, from Saturn Orbit Insertion to 2008, and that is split half between dusk side and dawn side. We didn't find anything that looked particularly convincing, but there were 26 candidates that roughly matched the magnetic signature. We saw what you would expect: of those 26 candidates, 22 were on the dusk side and four were on the dawn side, and there was a significant bias to the North, but that might have been a seasonal effect, due to the axial tilt. I am not sure if that is particularly convincing, but it supports, perhaps, what you might expect.

Professor Owen. Yes, that sounds reasonably promising to me.

Professor S. Schwarz. You talked a lot about the global aspects of reconnecting fields which are on large scales. There is also a lot of interest in fine-scale reconnection in turbulent regions, and in other places, as a mechanism perhaps for the ultimate depositing point for the energy that has cascaded out at large scales.

Professor Owen. Yes — I think I rather rushed through the little bit about the Kelvin–Helmholtz rolled-up vortices. I think that is the kind of thing you are talking about, where the field is more turbulent.

Professor Schwarz. Anywhere where you eventually generate fine-scale currents, one way to dissipate them is reconnection.

Professor Owen. That is true.

The President. The clock is moving, so I am afraid we too must move on. Can we thank Chris again? [Applause.]

For our second speaker, it is a great pleasure to welcome a distinguished visitor from the United States, Dr. Alan Stern, from Southwest Research Institute. I would just remind you that we awarded Alan an Honorary Fellowship in 2015, for his distinguished body of work in planetary science and also his leadership at a very senior level in NASA. Alan is going to talk to us now on 'The exploration of Pluto by NASA's *New Horizons* mission'.

Dr. A. Stern. [The speaker began by explaining that he wanted to discuss the scientific motivation for the exploration of Pluto, and some of the chief results that have been obtained with the *New Horizons* spacecraft. The project involved 2500 engineers and technicians and 200 scientists, and he felt that his job was to act as principal instigator, rather than principal investigator.

New Horizons reached Pluto on 2015 July 14 after a nine-and-a-half-year flight. The images of Pluto showed that the visible surface was divided into a diverse complement of geologic provinces, and although the team expected something complex, perhaps something dynamic, what was found exceeded wildest expectations.

The story starts with Percival Lowell who founded Lowell Observatory with his own funds and who was convinced that apparent astrometric residuals in the motion of Uranus pointed to the presence of another body further out in the Solar System, the so-called Planet X. Lowell died in 1916 but left his observatory and his fortune so that the search could be continued. In the late 1920s Clyde Tombaugh was hired to carry out a systematic search. He took photographic plates on successive evenings and then used a blink comparator to check for any images that showed the right amount of motion expected from a more distant planet. He discovered Pluto in 1930 and in doing so found the first member of the Kuiper Belt. It was not until the 1990s that the second member was found, and by then large telescopes and CCD detectors were being employed.

Even at that time very little was known about Pluto and the outer Solar System. Only the development of spaceflight has opened up observation of the magnificent worlds there. From infrared spectroscopy in the early 1990s it was known that the surface of Pluto was covered by three active volatiles: CO, N_2, and CH_4. Nitrogen is actually the dominant feature, and changes our ideas about Pluto in many ways. It was also suggested that Pluto might have an atmosphere. To see if it did, it was necessary to use the *Kuiper Airborne Observatory* to observe the occultation of faint stars as Pluto passed in front of them. In 1988 this was achieved and the data returned allowed an estimation of the molecular weight and a rough estimate for the atmospheric temperature. In the 1980s Charon too was discovered and it turned out to have half the mass of Pluto, and it had so much angular momentum that it could only have been created from a glancing impact on Pluto, rather in the way that the Moon was formed from the Earth.

Although there are ices on Pluto, it is not an icy planet by mass. Its mean density is 2 grams per cc. The surface is a thin layer of volatiles, over a mantle of water ice with a rocky core. At that time the best image we had of Pluto was taken from *HST* when the disc of the planet fitted comfortably inside a few pixels. *Voyager 1* could have been directed towards Pluto but it was decided to explore Saturn's moon Titan. In 1989 *Voyager* passed Neptune. That seemed the right time to consider a mission to Pluto, but it was to take another 14 years to find the right combination of spacecraft design, management institutions, and the right scientific case to unlock funding. *New Horizons* cost $728 million overall and it was significantly cheaper than other proposed missions. It was decided by the Decadal Survey of 2003 when only two or four proposals out of the 50–100 submitted could be supported.

In the 1990s the Kuiper Belt was discovered. The first new KBO was found in 1992, four more in 1993, another ten in 1994, and there are now literally thousands known. This outer part of the Solar System represents the third zone after the terrestrial planets and the gas giants, but it is considerably bigger than either. Dwarf planets were found to outnumber all other objects in the Solar System, and it was the large number of dwarf planets that informed the Decadal Survey decision of 2003.

Whilst major projects like *Cassini* and *Voyager* had 12 years to come to launch, *New Horizons* had only four years and two months to make the only available launch slot in the 2000/2010 launch schedule. It was the first planetary spacecraft mission to fly alone, and this was on the longest-ever planned journey. It was about one-third the weight of *Voyager*, it was powered by a plutonium source, and it used a 2-metre dish working at X-band wavelength to send and receive signals from the ground. The scientific instruments, seven of them, were mounted on the outside. These included two spectrometers (one ultraviolet; and one visible–infrared, also comprising panchromatic and four-colour imagers), and two plasma detectors, among other instruments.

The satellite was put on top of the most powerful launch vehicle available, an Atlas 5, and with a relatively small payload (which also contained some of the ashes of Clyde Tombaugh) *New Horizons* became the fastest-ever spacecraft, a gravity assist at Jupiter in 2007 adding 5000 metres per second to its already considerable velocity. The Jupiter fly-by was used an as opportunity to test the spacecraft operation, and Jupiter and its rings and meteorology along with the major satellites were studied. During the long arc to Pluto, the satellite was put into hibernation using autonomous software which also reduced the number of people required for the mission (less than 50, compared with 450 for *Voyager*).

Since Pluto has not yet completed one orbit around the Sun since discovery, one of the challenges was the uncertainty in the planet's position. By using on-board images, the position of the spacecraft was compared with radiometric tracking, and corrections applied to home in on the target from a distance of five billion km. The aim was to fly *New Horizons* through the occultation shadows of Pluto and Charon, which meant hitting a target area at the orbital plane of Pluto of only 40 × 60 km, and to arrive within 90 seconds of the expected time. The spacecraft cannot look at its own data and needed to be told in which direction to point to acquire targets. It eventually arrived at Pluto 86 seconds late after nine and a half years.

Pluto not only has the large satellite Charon, making it in effect a double planet, but there are also four smaller satellites which are irregular in shape and on the surface are made of water ice. The tumble and spin periods are very short compared with the orbital periods. Hydra, for instance, rotates 90 times for every orbital period around Pluto, which is likely to be due to the asymmetric gravity field around the binary. Charon has a diameter of 1214 km and is very reminiscent of an icy satellite of Jupiter, Saturn, or Uranus. It has a large tectonic belt across its equator which is due to expansion forces after liquid water froze. The north pole of Charon has a distinctly red surface which is due to methane flowing from Pluto and caught in a cold trap, then being exposed to solar radiation. An experiment in the laboratory under similar conditions confirmed this. It is called a tholin, and should also be visible at the south pole, but this remains hidden from view as the area will remain in winter for another 30 years.

Pluto is observed to have a rugged topography, and as nitrogen has a very low viscosity it seems unlikely that on its own it could form steep topography: there must be something underneath, which is mechanically strong, supporting it. At one edge of the large feature Sputnik Planitia, where there is a large nitrogen ice field, we see a glacier, which was unexpected. It is the largest glacier known outside Earth and it appears to be dynamic. There is evidence for both young features — flows and avalanches in the recent geological past — as well as features which are as old at Pluto itself; and there appears to be a source of convective heat under the glacier — could it be a global ocean of water which is slowly freezing and so releasing latent heat? That has not been proven. Cryovolcanoes have been seen, the largest of which has a diameter of 150 km, and the central caldera is about 4 km deep. There is a feature which looks like a frozen lake but liquids cannot exist on Pluto as the temperature is nowhere near the triple point of nitrogen. However, evidence for formally standing or flowing cycles may be a result of Milankovitch cycles where the obliquity of Pluto's orbit drives the atmospheric pressure to 200 mbar, greater than that on Mars, and above the triple point for nitrogen.

In conclusion the speaker said that the population of dwarf planets in the Kuiper Belt is at least as diverse as the terrestrial planets. *New Horizons* has only scratched the surface of that area of the Solar System.]

The President. Thank you so much, Alan. We are pretty much up with the clock, but time for two quick questions.

Professor S. Miller. Huge congratulations. I think that you have transformed our understanding, certainly of the outer Solar System and the Solar System as a whole, with big implications, in my view, for understanding exoplanets and so on. You talked about those tholins on Charon. Is there any nitrogen in those compounds, and if so, is there any sign that you have some sort of prebiotic chemical species there?

Dr. Stern. There must be some nitrogen there, but we do not see it in the reflective spectroscopy. Here is why: the nitrogen is 28 AMU and the methane is only 16 AMU, but in Jeans escape, the molecular weight enters the escape-rate calculation in the numerator of an exponential. It turns out when you do the maths, that nitrogen is escaping 1000 times more slowly, only about 10^{22} molecules per second. So, there is very little nitrogen in the mix with the methane, just not enough for us to see. No doubt there is radiation chemistry taking place between carbon, nitrogen, and hydrogen and making nitriles, but they must be a minor constituent.

Dr. P. Allen. I noticed from the picture of Pluto that it was nice and round, as you would expect a planet to be. But the picture of Charon, at the top by the red stain, seems to be really rather wobbly along the edge. Are there any big variations in the radius there, and if so, why?

Dr. Stern. Good question. First, both objects are in hydrostatic equilibrium. When we fit a triaxial ellipsoid to each from all the available imagery and light-curve data, we cannot find any deviation from a spheroid for either object except due to local topography. Charon is also mechanically relaxed, that is what I mean by hydrostatic equilibrium. It has a larger variation in surface topography than we see almost anywhere else among the icy satellites. But that is just to do with the peculiarities of the surface topology — just on the thin rind on the top of the planet.

Dr. Allen. So the variation is basically large mountains.

Dr. Stern. Large mountains and large holes in the ground.

The President I am afraid we do have to finish. I get into big trouble if we run on past 6 o'clock. However, Alan is going across to the RAS library for drinks. I am sure he will be happy to speak to anybody on this.

Dr. Stern. The science gets better over drinks. [Laughter.]

The President. So can we thank Alan again? [Applause.]

Finally, I must give notice that the next monthly A&G Open Meeting of the Society will be on Friday 8th of December.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2017 December 8 at $16^{h}\ 00^{m}$
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C. Barclay, *Vice-President*
in the Chair

The Vice-President. For those that do not know me, I am not Professor John Zarnecki. My name is Charlie Barclay and I am the Junior Vice-President (A) this year. John has very kindly given me the privilege of standing in for him here. John wanted me to read a quick message: "I have been invited to attend a special event tonight, to commemorate 50 years of space research at the University of Kent, where I was for some years. As this only happens every 50 years, I have decided to attend. [Laughter.] I notice that this is the first Open Meeting I have missed in nineteen months of my presidency, so I hope I can be forgiven."

I have some news to break, which I think is exciting and very important for the Society. Council, at its meeting this morning, approved the appointment of Dr. Philip Diamond to the role of Executive Director. He has come from the IoP. I am going briefly to read his CV here: "Philip Diamond is currently Director of Programmes, Policy and Performance at the Institute of Physics. He obtained his BA (Hons) in Physics at Brasenose College, Oxford, and MSc in Nuclear and Particle Physics at Birkbeck. Following his degrees, he worked in nuclear medicine as a scientific officer and then a senior-grade physicist at the Royal Free Hospital. Since 1990 he has worked at the Institute of Physics in the Education Department, the Department of Higher Education and Research, and became an Associate Director in 2007. Philip is very well known to physics departments across the UK and was awarded an Honorary Fellowship at the Institute in 2015 in recognition of his many contributions to the physics community." So, I think we are lucky to have him. He aims to start in early January, so this is a good transition for the Society.

Now the programme for today has had a fairly last-minute change, very sadly, due to illness of Dr. John Veitch, who was going to talk about gravity waves. I know that the organizational people here and I, chairing this, are absolutely delighted that Dr. Sue Bowler has stepped into the breach at the very last minute to talk about 'Fifty years of plate tectonics'.

Dr. Sue Bowler. Fifty years ago Dan McKenzie was among the handful of people, worldwide, who brought about the plate-tectonics revolution. This new paradigm for understanding the Earth came about because of an explosion in the amount and quality of data made possible by new technology — a process that continues in Earth sciences today. McKenzie's landmark 1967 paper with Robert Parker was the first to describe rigid-plate movements, and was one of a flurry of publications in the late 1960s that demonstrated the power of plate tectonics to explain the evolution of the Earth's surface. McKenzie has now donated his extensive archive to the Geological Society of London, where it has been catalogued and can be searched on-line; the Society has also established a dedicated website for a wider public readership (www.mckenziearchive.org).

In 1963 McKenzie started his PhD in the Department of Geodesy and Geophysics at Cambridge, winning, while a research student, a fellowship from King's College that gave him the freedom to travel and pursue his own ideas — and new data. He headed for the United States, where oceanographic research was expanding. Continental drift had never been taken seriously; it was new data from the oceans that changed minds and led to plate tectonics. Technology developed for submarine warfare during the Second World War led to accurate bathymetry, refined in the Cold War. Magnetic methods for tracking submarines picked up remnant magnetism in the rocks of the ocean floor; the flow of heat through the ocean floors was measured. And seismological data, improved by the need to detect underground nuclear tests, improved the accuracy of earthquake locations and gave information about the fault movements generating earthquakes. McKenzie examined the data around the north Pacific, and found large areas with no seismic activity — plates — surrounded by narrow bands with consistent movements — plate boundaries. He saw that the plates themselves were rigid and did not deform; their movement on the Earth could be simply described using spherical geometry. Bob Parker had written a mapping programme — HYPERMAP — that was ideal for presenting these ideas, so together they wrote the paper subsequently published in *Nature*.

But McKenzie & Parker (1967) was not McKenzie's first work on plate tectonics nor, arguably, his most significant. In 1966 he had returned to Cambridge to complete his PhD thesis, *The Shape of the Earth*. While waiting for his examination, he thought about mid-ocean ridges. Seafloor spreading was accepted, but the ridge was thought of as part of a convection cell in the mantle, with the up-welling driving the two sides of the ocean apart. But McKenzie saw that the plates need not have any deep structure — they could be like ice floes moving apart, with magma welling up to fill the gap as a consequence of the movement. At Caltech, he modelled the process by computer, using heat flow and ocean-floor elevation; his models matched the observations far better than those tying ridges to deep-seated convection.

Although he continued to work on mantle dynamics, McKenzie did not work again on marine geophysics after a monumental work with John Sclater on the history of the Indian Ocean. One of McKenzie's most significant areas of work, and one that greatly benefitted the worldwide hydrocarbons industry, arose from the fieldwork on the continents that he began in 1968, trying to understand why the deformation there is so much more complex than in the oceans. He started fieldwork in Iran, moving on to the Aegean in the 1970s, developing from this work his model of the formation of sedimentary basins.

The McKenzie Model applies to basins such as the North Sea, formed by extension of continental crust and characterized by the accumulation of thick sedimentary deposits, all formed more or less at sea level, with the earliest extension by faulting. That was the pattern emerging from seismic exploration of the North Sea in the 1970s, but neither industry nor academic researchers could produce an adequate model for further exploration. McKenzie's years of fieldwork in the Aegean showed him that the region was dominated by extensional faulting, so much so that it must involve the mantle as well as the crust. So, just as he had done for his mid-ocean-ridge modelling a decade before, McKenzie examined what happens if you stretch the lithosphere, then track the thermal consequences. Fault-controlled subsidence is thus followed by thermal subsidence, just as is recorded in the sediments of the North Sea. McKenzie's further work with Andrew Mackenzie of the University of Bristol demonstrated how that model could predict quantitatively the process of maturation to form oil and gas within a basin. After some difficulty in publishing the paper — neither author had a track record in geochemistry — Mackenzie was awarded a prize for the best paper in organic geochemistry that year.

It is difficult to assess the impact of those papers, which together not only gave the hydrocarbons industry a tool with which to seek out basins in similar tectonic régimes, but also allowed the prediction of whether or not organic matter within their rocks was likely to have formed oil and gas. What is clear is that they arose from fundamental, blue-skies research into how the Earth deforms.

That is something that McKenzie has focussed on throughout his long — and continuing — career, as shown in the archive of papers now available at the Geological Society. He has received many awards and prizes and has made major contributions in the fields of geodesy, mantle dynamics, continental tectonics, fluid mechanics, seismology, and more. He looks forward to new techniques and ideas about the workings of the Earth and other planets — and to learning more about the fundamental processes that drive the movements of the Earth.

The Vice-President. Thank you, Sue, very much. We have time for questions.

Dr. A. Chapman. A fascinating lecture, especially at such short notice. It really brought home to me two things that you mentioned. The fitting together of the African/Eurasian plates and the American plates, and magnetism. Edmund Halley in the late 17th Century hit upon both. In the voyage of HMS Paramour 1697–1702, he says at one stage that it is somehow as though the two could be pushed together. And during his studies of the geomagnetic field as far as 52°S, in a 60-foot ship almost overwhelmed by a vast tidal wave, he again mapped the magnetic drift. He was using it for navigational purposes, being able to fix the navigational positions more accurately for the Navy, but he was also working geophysically, which is why I personally consider Halley the first real geophysicist. But the parallels with Halley's remarks are quite extraordinary.

Dr. Bowler. Thank you, Allan. I was aware of Halley's overall maps, but I was not aware of his deeper interest.

The Vice-President. Did the advent of space photography have an impact? When Google Maps came out one could look at things and say, "Gosh, it's obviously how things go together", but until then, one did not really have the same sort of picture.

Dr. Bowler. No. It is very visual, but the good fit does not come from coastlines, which are affected by things like river deltas and volcanic activity. It comes from taking, perhaps, the edge of the continental shelf, so you are mapping the continental rock, not what you are just seeing above sea level. But yes, I could look at maps all day. Space geodesy has made a huge difference.

Looking at the geoid from the surface height of the ocean has been a very powerful technique.

The Vice-President. One more.

Mr. C. Taylor. Mars and Venus had a lot of volcanism in the past, but still apparently no evidence of plate tectonics. Why?

Dr. Bowler. People are suggesting that there might have been, but certainly not now. There are what has been suggested as stripes of magnetism in the Martian crust, and there are some suggested areas on Venus. If there is tectonics there, it is not tectonics like the established system we have on Earth. People are modelling planetary interiors in much more varied ways. People are thinking about the possibilities. In all exoplanet research, it is very tempting to be Earth-centric, human-centric, and to think that our planet is the only way that planets can be, and it clearly is not, because we have Venus and Mars. And that goes right down to the core. One of the crucial things in producing a magnetic field is getting enough energy into the core, but it turns out there are ways you can produce a magnetic field without having a solid inner core and a liquid outer core. You can have iron snow forming at the top of the core. People are expanding the parameters of planetary modelling. One of the things they look at is whether it is possible to return crustal rocks to the mantle through some sort of whole planetary over-turn. You get a stagnant lid, and the heat is still churning there underneath and ... it all goes under. I don't know.

Mr. Taylor. How about super-rotation of the core, if you believe in that?

Dr. Bowler. It seems like anything could happen in the core models at the moment, but they are only models.

The Vice-President. We are going to have to move on. Thank you, Sue, very much indeed. [Applause.] You will see we are not actually running to the order as on the sheet here, but there are good reasons for it. Next up here is Professor Catherine Heymans from the University of Edinburgh, who is going to give the 2017 George Darwin Lecture on 'Observing the dark side of our Universe'.

Professor Catherine Heymans. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Geophysics*.]

The Vice-President. Thank you, Catherine, very much. Super timing, so we have time for some questions.

Professor Carole Jordan. Can I just point out that there was at least one early measurement of the Hubble constant, which nobody ever mentions, that was when the *IUE* satellite observed the double quasar that is gravitationally lensed, so you have two images of the same galaxy, and two ultraviolet spectra of the same object. That was analysed, I think by Wilson, Gondhalekar, Dupree & Burke, and they came up with an answer, let's say 74 km sec^{-1} Mpc^{-1} [actually 67 — Ed.] plus or minus a small error bar. It is consistent with anything that has been done since, and I really think people should look at it again, because it is a unique way of doing it.

Professor Heymans. Thank you. People are actually working on that very question. It is the same lensing technique I was telling you about. The idea is that you have a foreground clump of matter and some variable source behind it, a quasar turning on and off. Light from that quasar takes two different paths to get to you, and one will be slightly longer than the other, so you get a time delay between the two images. That allows you to constrain the Hubble constant. It has had a revival recently, partly to address this question, because we have two different measurements now. The *Planck* measurement is extremely clean, but in order to be able to infer a value of the Hubble constant, you need a cosmological model, you need to have a model for dark matter and dark energy, whereas these direct measurement from the Cepheids and the mega-masers don't need a cosmological model. So people wanted a third method. Using strong-lensing time delays they can measure that. They are finding a measurement that is not inconsistent with the *Planck* results, but in tighter agreement with the Cepheid results. They are now doing very dedicated monitoring campaigns to get more of these lenses. The *LSST* will find the 'killer asteroids', which is going to be brilliant, but it will also find anything that changes with time. It is going to show you where all the quasars are, where anything that is varying is, and it is going to produce all of these strong-lensing features. There is so much science you can do with that project that is going to address those questions.

Reverend G. Barber. When you took your second poll, you said, "vote for only one". It may be that there is a cosmological constant and some modification of GR?

Professor Heymans. For me, cosmological constant equals vacuum energy, and you can argue that or not. I think solving the vacuum-energy problem is easiest by just setting it to zero. There are mechanisms you can do to set it to zero, but setting it to a non-zero-but-small value is hard. Yes, in principle you can have both, but both isn't the best solution or the simplest answer. What I did not talk about today is gravitational waves, because I was hoping we would have another gravitational-wave speaker. Actually the tightest constraints on these beyond Einstein — modified-gravity theories — today come from that amazing result where they saw a gravitational wave coincident with an optical source, which means the gravitational wave is travelling at the same speed as light. That puts extremely tight constraints on different modified-gravity theories that can produce an acceleration like dark energy. A lot of the modified-gravity community at the moment are scrambling to say what viable models are left, that are not ruled out by this gravitational-wave result but still give us acceleration. There are a couple left, but they are extremely complicated models, by far not the simplest. Before that result of the gravitational wave, I would have voted for beyond-modified-gravity theories, but since that result has ruled out a lot of those models I am finding myself turning more towards different scalar-field theories, different dark-energy theories.

Professor I. Roxburgh. Several years ago, when there existed what people call the solar-neutrino problem, one explanation that was favoured in some quarters was WIMPS congregating in the centres of stars. Why should they not?

Professor Heymans. If you look at the profile of dark-matter particles, some of them have what you call a 'cusp' in the middle, so there are lots of WIMPS in the centres of galaxies. But in the centres of stars … I have not thought about that one.

Professor Roxburgh. It would mean that our estimates on mass and things like that and the structure of stars are all wrong.

Professor Heymans. One way to picture it is that of a dark-matter halo around a galaxy — the galaxy you can see is just a tiny pinprick in the middle. The extent of the dark matter compared to the stars you can see in the galaxy is so much larger.

Professor Roxburgh. Is that because there is so much more?

Professor Heymans. Yes, there is more of it, but it is also much more spread out.

Professor A. M. Cruise. In the microlensing surveys the analysis is extremely statistical. If you are measuring a whole lot of probability functions that are not Gaussian, is there any chance that it is skewing the result in a systematic way from one survey to another?

Professor Heymans. I didn't talk about microlensing. I will talk about it very briefly. The idea is that if you look at a patch of sky, if there are any clumps of matter (in between you and the stars in the background) and they move in front of your background light source, they will magnify it. That is the same lensing effect which bends the light. You see distant stars increase in brightness and then decrease again as something goes in front of them. This is the technique that has been used to find exoplanets as well, as something that orbits a star. The microlensing searches try and see if dark matter could be explained by having lots of dark stars in our own galaxy. They really did not find anything, but they paved the way for a lot of those exoplanet searches. So your question was about Gaussian probabilities. I am not sure what you were asking about, but they have been observing for so long now that they must be reaching the point where they really understand their errors and their analyses.

The Vice-President. I have realized there are other questions, but we are going to have to move on again I am afraid. Thank you very much, Catherine. [Applause.] Our final talk this evening is from Dr. Justin Alsing from the Centre for Computational Astrophysics. Its title is 'Mapping the cosmos with weak gravitational lensing'.

Dr. J. Alsing. [No summary had been received at the time of going to press.]

The Vice-President. Because of the delays with the IT we have pushed up against time a bit. We probably ought to move from here over to the reception in the RAS Library. I am sure Justin will be able to answer questions over there, and indeed Catherine and Sue as well. I am going to wrap up now, and give notice that the next monthly A&G Open Meeting of the Society will be on Friday, 12th of January 2018.

THE IMPACT OF WORLD WAR I ON RELATIVITY PART II

By Virginia Trimble
University of California Irvine, Las Cumbres Observatory, and Queen Jadwiga Observatory, Rzepiennik, Poland

We continue by plunging into other work done during 1914–18/19 and the crowd of people who did it, besides Albert's own work (*Entwurf*; the four critical 1915–16 papers; some fine tuning thereof; the 'Foundations' paper emphasizing Mach; and the first mentions of gravitational waves and of cosmological considerations, including λ, which we call Λ). The ordering is from folks (astronomers!) you are expected to have heard of before, to the less famous and perhaps the totally obscure (mostly physicists and mathematicians).

The astronomers

Least surprising is that Americans initially hardly noticed the War (which the US entered only in 1917 April). Thus Vesto Melvin Slipher continued measuring what we would now call galactic redshifts[38] throughout 1914–18. Harlow Shapley began calibrating pulsating variables as distance indicators and getting us out of the centre of the Milky Way[39]. Walter S. Adams looked at the spectrum of the companion of Sirius[40]; Einstein knew of the measurement and regarded it as support for his theory.

More surprising, routine observations of variable stars and comets continued at several places in Germany, the information appearing briefly in a regular astronomy column in *Nature*, funnelled through Copenhagen, where Ejnar Hertzsprung was beavering away on stellar spectra[41]. In Britain, Harold Jeffreys was worrying about the compressibility of dwarf stars and planets[42]. And further east, Ernst Öpik, who graduated from Moscow State University in 1916, wrote that year and published in the *Astrophysical Journal* 'On the densities of visual binary stars'[43]. Of the scientists who have appeared so far, Jeffreys (1891–1989) and Öpik (1893–1985) were the only two I met (in Cambridge and Maryland, respectively).

The eponymous

Karl Schwarzschild (1873–1916) comes first here for '*Über das Gravitationsfeld eines Massenpunktes nach der Einsteinschen Theorie*'[44] and '*Über das Gravitationsfeld einer Kugel aus inkompressibler Flüssigkeit nach der Einsteinschen Theorie*'[45], otherwise known as the Schwarzschild solution and held later to predict something odd at $R = 2GM/c^2$ and Schwarzschild black holes. Notice that he, and many others, write of Einstein's theory rather than of General Relativity.

Those papers were sent to Einstein from the Russian front where Schwarzschild was a volunteer, having done his required year of military service in 1893–94.[46] He was already well known among astronomers for the Schwarzschild criterion for convective instability, the Shuster–Schwarzschild approximation of stellar atmospheres, the concept of LTE*, the velocity ellipsoid as a description of the motions of Galactic stars, and other items. He spent all of 1915 in the field, first in Belgium, then France, and then the Russian front, where he developed pemphigus and was invalided home to die soon after, in May. Einstein was chosen to eulogize him for the Academy, but his remark to Besso (Doc. 219)[50] that Schwarzschild "would have been a gem if he had been as decent as he was clever" ("*Er wäre eine Perle gewesen, wenn er so anständig wie gescheit gewesen wäre*") does not sound to me like unmodified praise in either language!†

Obituaries of Schwarzschild appeared in *The Observatory*[47a], the *Astrophysical Journal*[47b], and *Monthly Notices*[47c] in the same time frame as H. G. J. Moseley's obituary appeared in *Naturwissenchaften*[48]. Here are a few sample phrases:

*A gas in LTE or Local Thermodynamic Equilibrium has all its temperatures the same — radiation, velocity distribution, ionization, excitation, and so forth. This approximation makes calculations enormously easier, but cannot be precisely true or the star would never radiate.

†Two early readers of these pages suggested that Einstein might have been out of sympathy with Schwarzschild either because the latter had volunteered for military service or because he had been unhelpful in trying to arrange a better 'job description' for Erwin Freundlich (of whom more shortly) under Hermann Struve. KS said this was hopeless and AE should try to find EF a job elsewhere.

"The war exacts its heavy toll of human life and science is not spared. Now from the enemy comes news of the death of Schwarzschild in the prime of his powers … . The world loses an astronomer of exceptional genius."[47a]

"… loss to science … at the summit of productivity. If he had known that his life would be so short, he could not have used it to better purpose … . At his loss we feel poorer."[47b]

"*Tod dieses außergewöhnlich befähigten Mannes, von dem man noch viele wichtige Entdeckungen erhoffen durfte, bedeutet für die Naturwissenschaften einen schweren Verlust.*"[48]

But another point is that information was moving across the battle lines even then, death notices as well as observations of variable stars and comets, and (next paragraph) Einstein to de Sitter to Eddington. Also Britain apparently bought some German binoculars through Switzerland early in the war; at least the money was sent, though whether the binoculars ever arrived is not clear from the item in *Nature*.

Willem de Sitter (1872–1934) was a Dutch mathematical astronomer who spent much of his career on celestial mechanics and astrometry[49], including the small deviations in the motions of the Moon and planets from Newtonian dynamics as understood in 1911. This put him in a position to appreciate quickly the potential importance of General Relativity for astronomy, though he first appears[50] (Doc. 227, 1916 June) as the recipient of a letter from Einstein, responding to one from de Sitter (which has not survived), concerning coordinate systems and the possibility of gravitational radiation. In 1913 he had provided evidence for the constancy of the speed of light from binary-star observations (that is, support for Special Relativity) and so was an early recipient of the 1915–16 GR papers. More than 20 further communications in one direction or the other survive (sometimes postcards, which apparently took less time in censorship across borders). There was an on-going disagreement about possible solutions of the GR field equations that could describe the Universe as a whole[51].

What is now called de Sitter space is empty and expands.* The Einstein–de Sitter model[52] is the one with $k = \Lambda = 0$, flat space, $a(t) \propto t^{2/3}$ and age $= 2/(3H_0)$. It came a bit later and was widely used because the calculations were so easy, though they always made the Universe seem worrisomely young.

For our purposes, however, the important point is that de Sitter cast AE's ideas into Dutch and English, submitting the latter to the Royal Astronomical Society, where the papers[53,54] were received by the RAS secretary, Arthur S. Eddington, one of rather few people ready to understand the ideas and to think of how GR might be used to help heal the wounds of war. Critically, the Netherlands remained neutral, so that information travelled more or less freely to and from both Germany and Britain, though Einstein found difficulties in visiting colleagues there (but also in Switzerland). Eddington of course appears again 'afterwards' concerning the 1919 eclipse expedition.

The *Lense–Thirring* effect, widely known as dragging of inertial frames, is generally said to have been seen in the context of the Earth's rotation and rapidly-rotating accretors in X-ray binaries[55–57]. Josef Lense (1890 Vienna – 1985 Munich) and Hans Thirring (1888–1976 Vienna) both lived long enough that

*The referee points out that the idea was not as empty as the space, as it provided much of the motivation for early thoughts about the unexpected large redshifts of galaxies. As for anti-de Sitter space, which is used in theoretical particle physics, if you want to read more about it, you must find an article by V. anti-Trimble.

one could have met them. Both were trained in the Viennese school, Lense as an astronomer and then mathematician, Thirring as a physicist. He nominated Marietta Blau for the 1955 Nobel Prize in Physics for “her pioneering work in the study of high-speed atomic particles made visible in photographic plates”, as did Erwin Schrödinger, twice. But that prize had gone in 1950 to Cecil Powell (another of those sad ‘No-belles’ stories along with Lise Meitner and Rosalind Franklin). Lense and Thirring were at the University of Vienna when Friedrich Hasenöhrl (1874–1915) died on active duty (like Schwarzschild, a volunteer), and Thirring wrote to Einstein in 1917 July (Doc. 361, ref. 50) about the problems this presented for the young teachers and researchers (though the letter is mostly about rotating bodies). Thirring was also a volunteer for the Austrian army, but had been assigned to develop photoelectric detectors rather than to be shot at. Einstein’s response (Doc. 369) is purely scientific and advises a focus on the ‘Coriolis-field’.

The *Reissner–Nordström* solution is two separate papers[58,59] and deals, more or less, with charged particles. Hans Jacob Reissner (1874–1967) submitted his paper from Charlottenburg, Germany. He received an Iron Cross 2nd class for civilian work on aircraft design. He left for the US in 1938, having been declared insufficiently Aryan to remain safely in Germany. Gunnar Nordström (1881–1923) was Finnish with a Swedish mother. He went to Leiden in 1916 on a Russian passport to work with Ehrenfest. There is Einstein correspondence with both of them, and they too write of Einstein’s theory or Einstein’s gravitation, rather than General Relativity. Reissner circumvented Russian censorship of German post to Finland with the help of Bohr.

Level B or thereabouts

As I was retyping somewhere around the middle of this, there appeared Hubert Goenner’s ‘General Relativity and the growth of a sub-discipline “gravitation” in Germany’[60], which also briefly expresses surprise at how much had been done before the end of the War, including by authors on active duty. Here are the ones whose papers seem most closely connected with Einstein’s own work.

Johannes Droste (1886–1963) in Holland published three papers in Dutch[61–63], part of his Leiden PhD thesis under Hendrik Lorentz. Starting with the *Entwurf* theory and moving over to GR, he derived, in the third paper, ‘The field of a single centre in Einstein’s theory and the motion of a particle in that field’, the same solution that Karl Schwarzschild had found slightly earlier. His version was applied by de Sitter to some astronomical problems[64] and some of the results were rederived by Einstein, Infeld, and Hoffmann much later[65].

Ludwig Flamm (1885–1964) earned a PhD in Vienna with a thesis ‘*Das Relativitätsprinzip*’. His 1916 ‘*Beiträge zur Einsteinschen Gravitationstheorie*’ treats light-bending as a special case of perihelion advance[66], and Besso wrote to Einstein (Doc. 283) mentioning Schwarzschild–Flamm space. He married Boltzmann’s youngest daughter, Elsa, and Hans Thirring wrote of him as “*Mein Freund* Flamm” (Doc. 361).

Adriaan Fokker (1887–1968) was one of Einstein’s relatively few co-authors[67], and a student of Lorentz at Leiden (and, as mentioned in Part I, the brother of the celebrated aircraft designer Anthony Fokker). There is an Einstein–Fokker equation referenced in MTW[27] as equation 17.24 and shown (as an exercise for the student) to be the equivalent of a pre-GR Nordström equation[68].

Erwin Freundlich later *Finlay-Freundlich* (1885–1964) (see H. Kragh, p. 757 in BEAII = ref. 84) was perhaps the second-unluckiest of Einstein's collaborators (Leopold Infeld counts as number one, but belongs to a later period). His Göttingen PhD in 1910, with Felix Klein, was for a thesis on analytic functions. He had a heart condition that had quickly ended his work in a dockyard and kept him from eligibility for war service. His position at the Berlin Observatory was directed at traditional positional astronomy, though he longed to work more closely with Einstein on ways of testing the various iterations of relativistic gravity, and Einstein repeatedly attempted to persuade the director to relieve Freundlich of routine responsibilities. We follow him to the end here.

(*i*) A non-Newtonian orbit for Mercury, which offended his director without confirming the GR value[69].

(*ii*) An attempt to measure the gravitational redshift of the Sun[70], which is still rather difficult.

(*iii*) The K-term as gravitational redshift[71].

(*iv*) Novae as star–star gravitational lensing[72].

(*v*) The hope that light bending by Jupiter might be usefully invoked to support GR[73].

(*vi*) And of course he was the leader of the 1914 August eclipse expedition to the Crimea[72] funded by Krupp and advocated by AE himself to look for bending of light by the Sun, but which was captured and interned when Germany declared war on Russia. He was part of a group exchanged after about a month, through Switzerland, for Russians who had been captured by the Germans; August Köhl, less lucky and '*Assistent an der Münchner Sternwarte*', spent a whole year in captivity[74]*.

Back home, Freundlich wrote one of the first GR texts[75] of which you will meet many more 'afterwards'. In 1918, he finally got a promise of the observatory he had been wanting and started work toward the Einstein Tower, to look for gravitational redshift in the solar spectrum. That facility was to be financed by the Kaiser Wilhelm Institute (a remarkable investment for wartime!)[72]†. A relativistic German eclipse expedition was not in the realm of practical politics for 1919, but he attempted 1922, 1926 (both clouded out), and 1929 (Sumatra), where he thought he had recorded a deviation of star positions significantly larger than predicted by GR[72], from which he never recovered. As the grandson of a Jewish woman, he went to Turkey in 1933 (someone has surely written at length about the role of Turkey in the lives of Jewish scientists, but I don't know who or where), to the Charles University in Prague in 1937, soon thereafter to the Netherlands, and then to the University of St. Andrews (Scotland), where he was charged with building up an astronomy department and observatory. His first (indeed apparently *the* first) Schmidt–Cassegrain telescope was a great success, later efforts not, at which point the university had very little use for him.

Freundlich (by then Finlay-Freundlich, incorporating the first part of his mother's maiden name) resigned the Napierian Professorship in 1951 and was succeeded by D. W. N. Stibbs (whom I knew). He finally retired to Wiesbaden, Hesse, and was honorary professor at University of Mainz at the time of his death in 1964 July. Unlucky, and not even best of friends with Einstein after about 1922.

*The instruments captured with the people were finally recovered in spring 1923 (Doc. 504, Note 2).

†Additional funding came from a 1919–1921 concerted drive, enabling construction, and use of the observatory by 1924. As a sad attestation of humanity's perennial insanity, the observatory was badly damaged by Allied bombing in WWII, finally undergoing a full renovation in 1999.

Friedrich Kottler (1886 Vienna – 1964 Rochester NY). He spent the year 1906 as a volunteer in the Imperial Royal Artillery, then served as a 2nd lieutenant in a motorized unit at Namur, Maubeuge, Antwerp, and Ypres 1914–16, and commander of a Howitzer unit in Romania and East Galacia 1916–17. Despite all this he found time to contribute to the development of Special Relativity using absolute differential calculus. He later disagreed with Einstein about the equivalence principle[76,77]; (Einstein's response also survived[78]). His letter to AE (Doc. 495) deals largely with Kottler's own theory of gravity and is a rebuttal to a number of criticisms made by AE in an earlier letter to FK which apparently does not survive.

In 1929, Kottler was a "chair holder at an invited university"* and nominated Heisenberg and Schrödinger for the physics Nobel Prize (Heisenberg won in 1932, Schrödinger in 1933). But in 1938 (May) he was part of a large group of Austrian professors relieved of university jobs to "preserve peace among faculties" (meaning they were insufficiently Aryan). Having received an Iron Cross, 2nd class, he may actually have been the only such decoration holder to be expelled, after which Kottler was employed by Eastman Kodak 1939–55, returning to Austria and resuming his professorship (a nearly unique experience for an Austrian scientist!) in 1956. Because he brought the mathematics of Ricci and Levi-Civita to express Maxwell's equations in generalized coordinates, Gutfreund & Renn[3] count him as influential in the beginning of General Relativity, but not in its later development[14,64].

Erich Justus Kretschmann (1887–1973). He claimed that general covariance has no physical significance, since any physical theory can be written in covariant form[79]; Gustave Mie, at least, was persuaded (Vol 8, Doc. 465, note 12). The note is perhaps a little unfair to EJK, describing him as a substitute high-school teacher. In fact he received a 1914 PhD from Berlin, working with Max Planck and others, and was later professor of theoretical physics at Königsberg and Halle. AE disagreed vigorously[80]. Kretschmann is listed as significant in both refs. 14 and 64; a 1915 paper was submitted from Königsberg, indicating that he was not (just?) a high-school teacher and that he was probably not on active duty.

This completes the group of scientists whom I identified as having written, during the Great War, significant papers concerning General Relativity that did not result in eponyms.

And the rest

This last step takes us to all the rest, if not thousands, at least dozens, picking up people who wrote GR-related papers, those mentioned in the 1914–18 Letters[50], and those whom Gutfreund & Renn[14,64] declare were important. These are 'people', not, as you might have supposed, 'men', because the list includes both Lise Meitner and Emmy Noether. Here they are, in one monster alphabetical list, with 'I' meaning 'influenced', 'L' meaning appears in the Letters, BEAII means an entry in Hockey *et al.*[81], and with somewhat abbreviated forms of references to other papers related to relativity. I have not been consistent about scientists whom Einstein writes to, hears from, or mentions in non-gravitational contexts. Paschen, whose work AE mentions he likes, makes the cut; Michael Polanyi (thermodynamic issues) does not.

* Professor of mathematical physics in the Philosophical School of the University of Vienna, in the days when invitations to put forward Nobel Prize nominations were much less widely distributed than today. (I write just as people in Europe are receiving their 2018 invitations, but Americans may not have. Mine came on my 74th birthday.)

What might you do with this list, apart from moving quickly on to the *post-hoc* and *propter-hoc* in the next section? Check for how many of these scientists you have heard of, sometimes in different contexts. Be astonished at how many there are given the popular impression that Einstein was a fairly solitary worker, even by the standards of his time, which did not include papers with 10^3 or more authors. Do some statistical investigation (yes, birth years are clustered, and, yes, many are Jewish).

Abraham, Max. (1875–1922) I, L: An earlier competing theory of gravity, *Physikalische Zeitschrift* (hereafter *Phys. Zeit.*), **13**, 1, 1912, '*Zur Theorie der Gravitation*', which AE initially regarded as a possible alternative to his own, using Minkowski's formalism. They disagreed about ether and electrons, and Abraham was an early opponent of GR.

Adams, Walter S. (1876–1956) I, BEAII: His (1915 and thereafter) attempts at measuring a gravitational redshift for Sirius B were regarded by AE as confirmation of GR, but the result was badly affected by scattered light from Sirius A, leading to a value of +19 km s^{-1} rather than 60-something.

Adler, Freidrich (1879–1960) L: extensive correspondence leading up to 1920 book *Ortszeit, Systemzeit, Zonenzeit und das augezeichnete Bezugssystem der Elektrodynamic, Eine Untersuchung über die Lorentzsche und die Einsteinsche Kinematik*, (Wiener Volksbuchhandlung, Vienna). AE wrote to the emperor of Austria, asking that Adler not be executed for murder (yes, he was guilty, but was not executed).

Arrhenius, Svante (1859–1927) L: AE responded to a post-war greeting from SA, who was a mover and shaker in the Nobel-Prize establishment, not particularly favorable to AE's winning. Nobel Prize in 1903 for Chemistry.

Baade, Walter (1893–1960) L, BEAII: Appears peripherally in correspondence with Felix Klein, to whom WB was mathematical assistant after receiving his PhD at Göttingen in 1919, but this is 'our' Baade, of supernovae, optical identification of radio sources, *etc*. His Mt. Wilson images of the Crab Nebula were used extensively by Jeffrey Scargle and yours truly in our Caltech PhD dissertations on the nebula.

Banachiewicz, Tadeusz (1882–1954) L, BEAII: An assistant at Göttingen (Doc. 188 from Karl Schwarzschild note 3) engaged in some task that might have been relevant to looking for bending of light by Jupiter, but BEAII puts him at Engelhardt Observatory near Kazan 1910–15 and at Dorpat (Tartu, Estonia) 1915–18. He returned to Poland, and Poland returned to being a country in 1918; he survived three months at the SS concentration camp at Sachsenhausen near Berlin, returning to Cracow, participating in the 1946 organizational meeting for the IAU in Copenhagen, after serving as IAU vice-president 1932–38. In the same letter, Schwarzschild reveals that he hasn't much use for Freundlich and AE should try to find him a job away from Struve!

Bauer, Hans L: '*Über die Energiekomponenten des Gravitationsfeldes*', *Phys. Zeit.* **19**, 163, 1918.

Bernays, Paul (1888–1977) I, L: Suggested in 1914 to Einstein & Grossman that they use variational calculus in *Entwurf*; later collaboration with Hilbert.

Bloch, Werner L: Had been AE's student. His 1918 *Einfüring in die Relativistätstheorie* (Teubner, Leipzig/Berlin), was a popularization, which AE had seen in early draft and praised (Doc. 358a of Vol. 8 , but appears in Vol 10 of the *Collected Papers of AE*, dated 1917 June 17) as "thorough, easily comprehensible, and well arranged." AE later recommended WB for some position (Doc. 203 in Vol. 12 of *CPAE*).

Bohr, Niels (1885–1962) L, BEAII: Bohr's basic atomic model was completed in 1913 and described at the end of the war, in 1918 in 'On the Quantum

Theory of Line Spectra', *Proc. Royal Danish Academy*, **8**, No. 41, 3. We, with perfect hindsight, know that AE would gradually diverge from Bohr's atom and its implications, but in 1916 he assured Sommerfeld that it was exactly through S's work that Bohr's idea had become entirely convincing (Vol. 8, Doc. 246, 3 August 1916). Nobel Prize in 1922.

Born, Max (1882–1970) L, I: '*Einstein's Theorie der Gravitation und der allgemeinen Relativität*', *Phys. Zeit.*, **17**, 51, 1918, though of course known primarily for work on atomic and quantum theory, and to Americans for the Born–Oppenheimer approximation. Left Germany for Britain in 1933, and received his Nobel Prize very late (1954), and, in need of funds, he had sold some of his books to the University of Maryland shortly before.

Budde, Emil L: '*Kritisches zum Relativitätsprinzip*', *Deutsche Physikalische Gesellschaft Verhandlungen*, **16**, 586 & 914, 1914. Budde and the paper are mentioned in a letter to AE from Friedrich Adler (Doc. 582), written *intra muros* in 1918 July. Adler really had assassinated an Austrian prime minister, received and imperial amnesty for that crime, and had been transferred to a military fortress in Stein on the Danube. He believed that there exists a preferred coordinate system and had written of 'Einstein optical clock', which puzzled AE (who also reports that by 1918 July prisoners were receiving better food than most civilians).

Burgers, Johannes (1895–1981) L: Dissertation, University of Leiden, *Het Atoommodel van Rutherford–Bohr*, 1918. AE mentions to Ehrenfest that he looks forward to seeing JB's work. Burgers later had a vector, and I met him very late in his life.

Caratheordory, Constantin (1873–1950) L: Professor of mathematics at Göttingen. Several letters, with lots of equations, concerning canonical transformations, the possibility of closed time-like curves, and the Hamilton–Jacobi theorem. AE apparently initiated the correspondence.

Campbell, William Wallace (1862–1938) I, L, BEAII: Director of Lick Observatory. Some of his stellar spectra were used by Freundlich in his attempt to demonstrate a gravitational redshift of starlight (it's there; just not big enough to dominate other effects except for white dwarfs and such). Led a 1922 eclipse expedition to the north-west coast of Australia, which confirmed the 1919 bending-of-starlight discovery.

Cassirer, Ernst (1874–1945) I: A neo-Kantian German philosopher, whose work was "appreciated by Einstein".

Christoffel, Elwin Bruno (1829–1900) I, L: Ricci and Levi-Civita used Christoffel symbols; Grossman told AE about them, and they were essential in formulating the field equations of GR.

Dällenbach, Walter (1892–1990) L: His GR thesis, begun in 1916 with Weyl, was interrupted by war service in the Swiss army (guarding the frontier, which, as AE discovered, was not always easy to cross), completed at ETH Zurich in 1918, and published in Leipzig by Barth. '*Die allegemein kovarianten Grundgleichungen des elektromagnetischen Feldes im Innern ponderabler Materie vom Standpunkt der Elektronentheorie*', *Annalen der Physik* (hereafter *Ann. Phys.*), **58**, 523, 1919. His work addressed generally covariant electrodynamics and contraction of the Riemann tensor.

Debye, Peter (1884–1966) L: Involved in effort to raise money to start X-ray scattering experiments at Zurich; AE praised his work (Doc. 403) on the somewhat fraught topic of atomic structure, '*Der erste Elektronenring der Atome*', *Phys. Zeit.*, **18**, 276, 1917. You perhaps know him for his length (not 1·7 metres, but the shielding length in a plasma), and there was a brief fuss a few years ago about taking away his degree or the name of his institute on the grounds that he had been inadequately anti-Nazi.

De Donder, Theophile (1872–1957) L: Extensive discussion of energy–momentum tensor and on GR as gravitation. *Sur les equations differentielles du champ gravitique, Proc. Koninklijke Akademie von Wetenschappen te Amsterdam*, **26**, 101, 1917. AE informed editor Hendrik Lorentz that there was an error in the paper and asked that it be corrected, preferably by De Donder himself. That never happened.

Duhem, Pierre (1861–1916) I, L: His 1915 essay against relativistic physics was one of the first, but it is an earlier 1908 book that AE mentions in a letter to Eduard Sturdy (Doc. 864).

Ehrenfest, Paul (1880–1933) I, L: Early letters reflect extended friendship and regard for each other's work, but PE's was largely in early quantum theory and later quantum mechanics, and their views gradually diverged. (My impression is that if someone wanted to be a self-confident physicist, AE was not the best possible choice for a good friend.) Ehrenfest's widow (née Tatiana Afanasieva, also a physicist, and they published at least one paper together) was in Holland in the 1950s and gave Joe Weber the photo of Einstein that appears as the frontispiece in Weber's text (see Part I ref. 37). Ehrenfest wrote 'On adiabatic changes of a system in connections with the quantum theory', *Kon. Akad. Von Wet. te Amsterdam*, **25**, 412, 1916.

Ehrenhaft, Felix (1879–1952) L: Correspondence largely concerns Brownian motion, electrons, and other topics extending from AE's earlier work '*Über die Teilbarkeit der Elektrizität*', *Ann. Phys.*, **56**, 1, 1918 (and earlier papers).

Eötvös, Lorand (*Roland*) (1848–1919) I, L: Appears in the correspondence only in connection with filling a vacant chair at a geodetic institute. But that AE insisted upon an equivalence principle, for which Eötvös's work was primary evidence, was a key part of the development of GR. Modern Dicke–Braginsky–Eötvös (*etc.*) experiments are, of course, enormously sensitive. But it seems to me that one carries out a crude form by swinging two pendula of the same length but with bobs of different material and masses (and compare the experiment that Galileo probably did not perform at Pisa).

Epstein, Paul (1883–1966) L: '*Zur Theorie des Starkeffektes*'; '*Zur Quantentheorie*'. *Ann. Phys.*, **50**, 489; **51**, 168, 1916. A Russian national, Epstein worked in Germany as an enemy alien under surveillance in Sommerfeld's lab in Munich. AE and others attempted to get government permission for him to leave Germany for Switzerland, which turned out not to be easy! His later career was spent at Caltech.

FitzGerald, George (1851–1901) BEAII, L: Mentioned in a long letter from Lorentz. You know them together for their contraction, which, writes Lorentz, is not the only possible interpretation of the data.

Förster, Rudolf (1885–1941) L: Discussions of boundary conditions finite *versus* infinite, aether, early interest in unified field theories. In 1917 November AE writes that he has already expended much effort in trying to express electromagnetism and gravitation in a single set of $g_{\mu\nu}$ tensors and hopes Förster will have better luck. Published as Rudolf Bach, '*Die Anziehung eines unendlichen Sternsystems*', *AN*, **206**, 165, 1918. He was an engineer, engaged in military work at Krupp, though not, he writes to AE, directly concerned with cannons to be fired at people.

Gehrcke, Ernst (1878–1960) L: '*Zur Kritik und Geschichte der neueren Gravitationstheorien*', *Ann. Phys.*, **51**, 119, 1916, and a 1913 attack on the special theory. AE wrote disparagingly of him several times, and the sentiments were reciprocated (Doc. 267). His 1916 paper claimed that AE's work had been guided by that of Paul Gerber, who had derived an expression (numerically correct) for the perihelion of Mercury using a velocity-dependent gravitational potential in 1898.

Grommer, Jacob (1879–1933) L: AE acknowledges him in one of the 1917 papers. In 1914 wrote a book on transcendental functions; later AE & J. Grommer '*Allgemeine Relativitätstheorie und Bewegungsgesetz*', *Sitzber. Preuss. Akad. Wiss., Berlin, Klasse*, Nr. 1, 2, 1927, on the problem of motion. AE describes him as a marvellous mathematician from Göttingen, with enormous head and hands*, who would very much like to obtain a mathematician's position in Russia, being a Jew and a true Russian (Doc. 362 to Ehrenfest). The 1912 AE & Grommer paper on Special Relativity was published also in Hebrew as part of a project coordinated by Immanuel Velikovsky (yes, that one) that contributed something to the founding of the Hebrew University in Jerusalem.

Guillaume, Édouard (1881–1939) I: '*Les bases de la physique moderne*' and '*Sur le possibilité d'esprimer la théorie de la relativité en function du temps et des longuers universal*' in *Archives des Sciences Physiques et Naturelles*, **44**, 28 and **43**, 5; 89; 185; 1917. AE disagreed with much of the material in letters to EG, but continued to end his letters "*mit besten Grüßen*" (Doc. 38). They had been colleagues in the Swiss patent office.

Haber, Fritz (1868–1934) L: Well, you know him for nitrogen fixation and poison gases, but he was also instrumental in bringing AE to Berlin, through his positions in the Kaiser Wilhelm Institute and the Deutsche Physikalische Gesellschaft. He also offered temporary lodging to Mileva Marić and the sons when she and AE were in the process of separating. He necessarily left Berlin for Switzerland in 1933, and his own last years were not happy ones, personally or professionally; Nobel Prize in 1918 for Chemistry.

Hale, George Ellery (1868–1938) I, BEAII: As early as 1913, AE asked him about the possibility of detecting light deflection in the Sun's gravitational field.

Hartmann, Eduard and Johannes (1874–?, EH; 1865–1938 JH) L: '*Einsteins allgemeine Relativitätstheorie*', *Phil. Jahrbuch der Görresgesellschaft*, **30**, 363, 1917. EH was co-editor of that journal, and AE wrote to him refuting Gehrcke's objections to GR. JH also appears in the letters as a candidate for the directorship of the Astrophysical Observatory at Göttingen.

Harnack, Adolf von (1851–1930) L: Of the Kaiser Wilhelm Inst. Involved in bringing AE to Berlin. Historians of GR at Berlin today speak of a Harnack principle — when starting a new organization: appoint an excellent person to head it and let 'him' carry on unimpeded.

Hasenöhrl, Friedrich (1874–1915) L: Professor of Physics at Vienna. Volunteered (same age as Karl Schwarzschild) and died on active duty. Schrödinger, who had been his student, served as an artillery officer for Austria. Thirring took over FH's classes, but told AE that the younger Viennese physicists had been left leaderless (Doc. 361). FH was Boltzmann's successor. Thirring also volunteered for Austria, but was put to work constructing photoelectric devices.

Heckmann, Otto (1901–1983) I: Definitely 'out of period', but described by Gutfreund & Renn[64] as his work having triggered the Einstein–de Sitter cosmological model. I encountered him thrice. First, when I attempted to translate his 1942 *Theorien der Kosmologie* to fulfil the Caltech language requirement (passing the exam turned out to be easier); second, when Kip Thorne got a laugh at a GR colloquium by saying that some particular solution

*Part of an acromegaly-like syndrome that made it difficult for him to find employment and which contributed to his death in 1933[17].

of the Einstein equations was not a Heckmann–Schücking–Baer cosmology (the speaker was Baer); and third, when he refereed the first paper to come out of my PhD thesis in 1968. He was a founder and first Director-General of the European Southern Observatory and President of the IAU 1967–70 (alright, four encounters, since he was the retiring president at my first IAU General Assembly, 1970 in Brighton).

Herglotz, Gustav (1881–1953) L: '*Zur Einsteinschen Gravitationstheorie*', Proc. of another of those Königl Academies, this one in Leipzig, 1916.

Herzfeld, Karl (1892–1978) L: Also volunteered, served, and survived; student of Hasenöhrl. 1912 paper on statistics of radiation, mentioned in two letters from Ehrenfest (Docs. 9, 11). Came to US in time to be John Wheeler's advisor at Johns Hopkins University and went on to the Catholic University in Washington DC where he taught many of the physics classes Joseph Weber took. I met him shortly before his death.

Hertzsprung, Ejnar (1873–1967) L, BEAII: Though Danish, he was an observer at Potsdam during the war. Appears in a 1916 February letter from Karl Schwarzschild (Doc. 188) in connection with possible measurement of bending of light by Jupiter.

Hertz, Gustav (1887–1975) & *James Franck* (1882–1864) L: Franck & Hertz *Deutsche Physikalische Gesellschaft Verhandlungen*, **16**, 457 and 512, 1914, deal with electrons in molecules and/but AE spoke highly of the work in a couple of 1914 letters. Frank wrote a 1947 biography of AE that is praised in many later ones. Hans & Paul Hertz (1881–1940) also appear in correspondence sounding somewhat hostile. Nobel Prize in 1925.

Hertz, Heinrich Rudolf (1857–1894) I, L: AE notes that Hertz's demonstration of electromagnetic waves in his laboratory considerably clarified Maxwell's equations, with the implication (Doc. 44, 1915 January) that something similar could be hoped for in the case of his theory of gravitation. We are still hoping for it, the waves having been seen in laboratories, but not generated there.

Hilbert, David (1862–1943) I, L: Two parts of *Die Grundlagen der Physik* in *Nach. Kongl. Gesell. d Wiss. Göttingen*, 1915, 1917. AE first regards him as one of the very few people who has any idea of what he is on about, then as a competitor (possibly not a completely honest one), then again as a friendly colleague. Mathematicians particularly write of the Einstein–Hilbert equations, and for some feel of the intensity of the issues, note that Goenner[60] lists six references for "an introduction" to the priority debate. He left 20 mathematical problems, progress on which has sometimes been taken to mark the progress of the field (not all yet solved). And if you suppose that mathematics is a young person's game, note Hilbert's dates.

Humm, Rudolf: (1895–1977) L: He was still a student at the time of his exchange of letters with AE concerning boundary conditions at infinity, prompting AE specifically to invoke Mach's principle, which name Einstein coined in 1918. Humm left a diary covering from 1913 to his death.

Jahnke, Paul (1861–1921) L: He appears briefly in connection with plans to celebrate the 60th birthday of Max Planck in 1918. His full name was Paul Rudolf Eugen Jahnke; the first edition of his tables of functions appeared in 1909; and if you feel something is missing, you know the later editions as Jahnke & Emde, not to be confused with Emden, who lives with Lane in an equation of stellar structure.

Jeans, James (1877–1946) L, BEAII: Important to us astronomers, but appears (Doc. 334, note 9) only in the context of black-body radiation not showing a Rayleigh–Jeans spectrum.

Kamerlingh Onnes, Heike (1846–1931) L: No real connection to GR, but did win a Nobel Prize in 1913, and is somewhat known to astronomers for trying to discourage Zeeman from carrying out the experiment that won his Nobel Prize in 1902. AE extended greetings to him *via* Lorentz (Doc. 47).

Kapteyn, Jacobus (1851–1922) L, BEAII: Appears in letters from Freundlich about measuring bending of light by Jupiter using Kapteyn's (1898) method for determining stellar parallaxes, which relied primarily on proper motions, so he would have been a plausible person for AE to ask about or mention in connection with the idea that the Universe is static on average. He continued work throughout WWI, though his annual trips to Mt. Wilson Observatory came to an end. One of his daughters married Ejnar Hertzsprung (not happily) and he was so firmly opposed to the brand-new IAU excluding Germany and other Central Powers that he tried to persuade the Netherlands not to join as soon as neutrals were admitted (he failed). His 'Kapteyn Universe' notoriously ignored interstellar absorption, though he had thought of it earlier.

Klein, Felix (1849–1925) L, I: He wrote on non-Euclidean geometry as early as 1871, and all is over Volume 8[50] concerning de Sitter's work, energy-momentum tensors, Hilbert's equations, and much else in *Nach. König. Gesell. der Wissensch. Göttingen*, **8**, 171 and 394, 1918, 'On aspects of Einstein's gravitation theory and cosmological solutions'. AE suggested to him (Doc. 322, 1917 April) that, just as Newton's theory gave way to $g_{\mu\nu}$, so some day $g_{\mu\nu}$ "will have to give way to some principally different approach". Freundlich was his student at Göttingen. He was definitely too old to serve in WWI, though his bottle is ageless, never having been blown.

Küstner, Karl Friedrich (1856–1936) L, BEAII: AE supported him to become director at Potsdam, but he remained at Bonn where he determined the aberration of starlight and detected a variation in latitude later better described by Seth Chandler. His son was a U-boat commander lost in WWI, and he was the 'representative' German given an honorary degree at the Leiden IAU in 1928 (more than 25 years before Germany became a member). The 'representative' Allied astronomer was H. Deslandres of Paris (IAU Vice-President 1992–98).

Laue, Max von (1869–1916): Known best for discovery of refraction of X-rays by crystals (Nobel Prize 1914). Papers 1907, 1911, 1913 on Special Relativity principle and at least one defence of GR, '*Die Fortplanzungsgeschwindigeit der Gravitation, Bemerkungen zur gleichnamigen Abhandlung von P. Gerber*', *Ann. Phys.*, **52**, 214, 1917. Lifelong friend of AE from their first meeting in 1906; he was 'von' only after 1913.

Levi-Civita, Tulio (1873–1941) I, L: '*Sulla espressione analitica spettante al tensore gravitazionale nella teoria di Einstein', Rend. Reale Accademia dei Lincei*, **26**, 381, 1917. Some of his mathematical ideas were critical to the final development of GR. AE expressed pleasure at receiving a letter from L-C in Italian, saying it reminded him of pleasant times with his family in northern Italy, but regretting his own inability to respond in Italian. Their 1914 disagreement about what is or is not a covariant tensor belongs to the *Entwurf* period, and AE was not always right.

Lindemann, Frederick Alexander (1886–1957) L: A. F. Lindemann & F. A. Lindeman wrote 'Daylight photography of stars as a means of testing the equivalence postulate in the theory of relativity', *MNRAS*, **77**, 140, 1917. FAL was working at the Royal Aircraft Establishment in England and had been a student of Nernst. You may know him as a WWII advisor to Churchill (whose advice was not as good as that of Patrick Blackett, who had served in the Battle of Jutland, at Cape Verde, the Falklands, and was in Harwich when the German submarines surrendered in late November 1918). The father (AFL, 1846–1931) is a *BEAII* astronomer partly for the invention of an electrometer that could be used to observe stars by daylight. The elder Lindemann was Jewish, and the younger became Lord Cherwell.

Lorentz, Hendrik Antoon (1853–1928) I, L, BEAII: 'On Hamilton's Principle in Einstein's Theory of Gravitation', *Proc. Kon. Akad. Wetens. Amsterdam*, **19**, 751, 1915. 1916–17 series of four papers 'On Einstein's Theory of Gravitation', *Proc. Kon. Akad. Wetensch. Amsterdam* in English Vols. **19** and **20**, 1916–18 (slightly earlier in Dutch). Lorentz & Droste wrote 'The motion of a system of bodies under the influence of their mutual attraction, according to Einstein's theory', *KAW Amsterdam*, **26**, 392, 1917–18. His version of electrodynamics involved an ether. Correspondence with AE dealt with coordinate systems, transformations, covariance, and such, but also with political issues. Nobel Prize in 1902.

Ludendorff, Friedrich Wilhelm Hans (1873–1941) L, BEAII: Chief observer (later director) through the war at Potsdam. '*Bemerkungen über die Radialgeschwindigkeiten der Helium-Sterne*', *AN*, **202**, 75, 1916, did not confirm the correlation of residual redshift with stellar luminosity (hence presumably mass) that Freundlich had claimed, slightly knocking one support out from under GR. The Ludendorff you have heard of was his brother.

Mach, Ernst (1838–1916) I, L: Was Einstein a Machian? Well, he wrote an obituary in *Phys. Zeit.*, **17**, 101, 1916, and Doc. 165 to Moritz Schlick also suggests "yes", with the most nuanced view coming from Gerald Holton in '*More on Mach and Einstein*', in J. Blackmore (ed.) *Ernst Mach* (Dordrecht), 1992, p. 263. Contrarily, Robert Dicke felt that the Brans–Dicke scalar–tensor theory of gravity was needed to bring the Universe into accord with Mach's principle (roughly that the inertia of a single object depends on the distribution of mass–energy of the whole Universe). John Wheeler, on the other hand, said that both Mach's principle and the equivalence principle were signposts left from an old road that had been replaced by the superhighway of General Relativity.

Meitner, Lise (1878–1968) L: She appears in the correspondence because AE had suggested that she might repeat an experiment by Edgar Meyer on the absorption of X or gamma rays from the decay of radioactive elements, and then decided that it would not be necessary (Docs. 615, 616, & 642). Better known for having been excluded from the Nobel Prize for fission.

Meyer, Edgar (1879–1960) L: AE supported him for an appointment at Zurich, and they were in friendly correspondence from before 1914 through to at least 1918 (Doc. 644), in which AE agreed with Meyer's interpretation of cathode rays, but disagreed on other topics about radiation and such.

Michelson–Morley: (AAM 1852–1931, Nobel 1907; EWM 1838–1923) L, BEAII: The eponymous experiment appears in a handful of letters, but this is not the place to look if you want to know when AE learned about it relative to his development of Special Relativity

Mie, Gustav (1868–1957) I, L: '*Bemerkungen zur Einsteinschen Gravitationstheorie*', *Phys. Zeit.*, **15**, 112 and 169, 1914, contained negative reactions to the *Entwurf* theory. '*Die Einsteinsche Gravitationstheorie und das Problem der Materie*', *Phys. Zeit.*, **18**, 551, 574, and 596, 1917, did the same for GR. In between (notes to Doc. 346), he had put forward his own theory of gravity. He was interested in unified theories (whether Weyl or Hilbert), but astronomers know him for Mie scattering (by a uniform sphere). Long letters from 1918 deal with cosmological solutions, gravitational waves, and such.

Minkowski, Hermann (1864–1909) I, L, BEAII: He invented the phrase space–time in '*Raum und Zeit*', *Phys. Zeit.*, **10**, 104, 1909; and invented some of the ways students are now taught to draw special-relativistic problems (Minkowski diagrams 1908). Minkowski space is flat; the uncle of our Rudolph Minkowski.

Nernst, Walther (1864–1941) I, P: Mostly thermodynamic issues, but he suggested that a uniformed expert be sent to Odessa (in spring 1918) to check out the instruments captured from Freundlich's 1914 eclipse expedition (Doc. 505, note 2). These were finally returned in 1923. He played a decisive role in Einstein's election to the Prussian Academy and his move to Berlin. Nobel Prize in 1920 for Chemistry.

Noether, Emmy (1882–1935) I, P: Her spectacular contribution on the association between conservation laws and invariances, '*Invariante Variationsprobleme*', is a bit later than 'our' period; *Nach. Kön. Ges. Wissen. Göttingen*, **37**, 235, 1918. But she worked on conservation of energy in Hilbert's unified field theory and was invited to Göttingen by Klein and Hilbert to replace *Privatdozenten* on military leave and to work on that UFT (Doc. 222). AE's last letter of 1918 (27 Dec. 677) was to Klein and dealt with the effort to get a habilitation or *venia legendi* for her; Olga Taussky Todd, whom I met, had studied with her, which gives me Emmy in one 'step'! [A 'step' is an intermediate mutual contact. Thus Sir William McCrea was just three 'steps' from Sir Isaac Newton. — Ed.]

Paschen, Friedrich (1865–1947) L, BEAII: '*Bohrs Heliumlinien*', *Ann. Phys.*, **50**, 901, 1916. AE said he liked the paper. Paschen gave his name to the series of hydrogen lines ending on $N = 3$ (*Ann. Phys.*, **27**, 832, 1908) and to the Paschen–Back effect (*Physica*, **1**, 261, 1921), which is what happens to the Zeeman effect for magnetic fields reaching above a megagauss. His grandfather, who shared two of his personal names, was also a *BEAII* astronomer.

Planck, Max (1859–1947) L: Initially very skeptical of the *Entwurf* theory; by 1916 February AE thinks he had come to accept GR (Doc. 191) along with Ehrenfest, Lorentz, Born, and Hilbert. You know him for radiation and his constant. It was not until after WWII that the Kaiser Wilhelm Institutes were renamed Max Planck Institutes. He was involved in very many issues of who was to get which jobs and political matters in 1914–18. Nobel Prize in 1918.

Poincaré, Henri (1854–1912) L, I, BEAII: Kottler, Lorentz, and Sturdy invoke his name in 1914–18 documents; AE does not, but critics have suggested some of the credit for Special Relativity should go to him.

Prandtl, Ludwig (1875–1953) L: Appears only once and peripherally, but he seems to have been the only 'connection' whose work was secret enough not to publish until after WWI was over (Doc. 495 note). He had a number, which lives in turbulent fluids.

Regener, Erich (1881–1955) L, BEAII: He appears once, peripherally, but he was the one who showed very early that the energy density in cosmic rays in the disc of the Milky Way is about the same as that of starlight (and, we would now say, magnetic field, turbulence, and the CMB) at 1 eV/cc.

Riemann, Bernhard (1826–1866) I, L: He, his tensor, and his geometry appear in the letters, but no citation of his papers. You will meet his tensor if you decide to calculate things using GR. He considered curved spaces in three and higher dimensions. MTW[27] cite his collected works, as did Förster and others you are meeting here. R. Elves, in *Mathematics in 100 Key Breakthroughs* (Quercus, 2015, Ch. 50), thinks his zeta function is even more important. He was Gauss's student.

Ricci-Curbastro, Gregorio (1855–1925) I: Ricci does not appear in Vol. 8 of the AE papers, and MTW cite none of his publications. But if you settle down to calculate anything in GR, you will eventually encounter his tensor, scalar, curvature, *etc.* Say his whole name three times quickly to see why it is just the Ricci tensor. Levi-Civita was his student, and it was Grossmann in 1912 who realized that their language of absolute differential calculus would be useful in formulating a general theory.

Röntgen, Wilhelm (1818–1925) I: What has survived is a three-sentence note from AE to WR, responding (negatively) to an invitation (1916 November) to participate in some grand occasion celebrating science and scientists. He was by then professor at Munich, and you know him for X-raying his wife's ringed hand. Nobel Prize in 1901, the very first.

Runge, Carl (1856–1927) L, BEAII: Gave a lecture '*Über den Satz von der Erhaltung der Energie in der Gravitationstheorie*'. He discussed energy issues with Klein, who bore the brunt of AE's disagreements, and most of the discussion papers were not published (Doc. 487, note 20), which presumably is why none of his papers are cited in Vol. 8. In particular, Runge doubted AE's assertation that energy would be lost from a system due to gravitational waves (Doc. 492, note 7). We know him as the Runge–Kutta integration for the differential equations of stellar structure, see, *e.g.*, p. 338 of C. J. Hansen, S. D. Kawaler & V. Trimble, *Stellar Interiors* (Springer, 2004).

Rutherford, Ernest (1871–1937) L, BEAII: Appears in association with alpha particles and Geiger (who helped an interned British colleague in Germany get hold of some equipment and carry out experiments, but that is part of a different story). Nobel Prize for Chemistry 1908. He generally supported scientific reconciliation after the Great War but found himself unable to forgive Haber for poison gases.

Sackur, Otto; Stern, Otto (Nobel Prize in 1943); *Tetrode, Hugo*: Other contemporaries mentioned in the letters whose involvement with AE dealt with statistical mechanics, specific heats, zero-point energies, and other parts of thermodynamics. (If Tetrode isn't a tensor, then he must surely be a vacuum tube with one more grid than a triode.)

Schlick, Moritz (1882–1936) I, L: Known primarily as a philosopher and wrote on both special and general relativity from that point of view, but had earned a 1904 PhD in physics at Berlin under Planck. AE praised his writings to his (postal) face (Doc. 165, *etc.*). Several of his publications carry titles beginning '*Raum und Zeit…*'

Schrödinger, Erwin (1867–1961) L, BEAII: *Phys. Zeit.*, **18**, 4 and 20, 1918, on energy and covariance in gravitation. Served as an artillery officer for Austria. Defended a non-zero-but-small cosmological constant from 1917 to a 1950 book. P. Halpern's *Einstein's Dice and Schrödinger's Cat* (Basic Books, 2015) is both fun and informative. Nobel Prize in 1933.

Schottky, Walter (1886–1976) L: A 1914 letter from WS to AE concerns emission of electrons by a filament. He worked in a Siemens lab and is eponymized in Schottky diodes, *etc.*

Schweydar, Wilhelm (1877–1959) L: A geodecist who had been sent from Potsdam to Romania to carry out gravitational-acceleration measurements with an Eötvös torsion balance and expressed willingness to travel to Odessa to try to retrieve the 1914 instruments (Doc. 504). '*Über die Elastizität der Erde*', *Naturwiss.*, **38**, 593, 1917. Other correspondence with AE dealt with various appointments.

Seeliger, Hugo von (1849–1924) I, L, BEAII: A maven of stellar statistics and violent opponent of Einsteinian relativity. '*Über die Gravitationswirkung auf die Spektrallinien*', *AN*, **202**, 83, 1916; '*Über die Anomalien in die Bewegung der inneren Planeten*', *AN*, **201**, 273, 1915. Admittedly he was right and Freundlich was wrong about some of the relevant calculations (Doc. 186, note 6; see also ref. 72).

Selety, Franz (born *Jeiteles*) (1893–1933?) L, BEAII: Proposed a Newtonian, hierarchical universe in papers 1917–1924 after exchanging long letters (not all extant) during 'our' period with AE. The hierarchical idea was found also in the work of Carl Charlier and preferred frames in papers by Felix Adler. FS showed that, contrary to a claim by AE, a cluster of stars in an otherwise empty universe could hold together by its own gravity. AE thought this not very important and terminated the correspondence. FS was in France and then disappeared completely from science and philosophy. He may have died in 1933 (Kragh cited in *BEAII* but with no indication of the evidence for this; 1933 in Paris was not yet dangerous for Jews). Last paper, '*Unendlichkeit des Raumes und allgemeine Relativitätstheorie*', *Ann. Phys.*, **73**, 291, 1924.

Silberstein, Ludwig (1872–1948) L, I, BEAII: He wrote *The Theory of Relativity* (Macmillan, 1914) with alternatives to both SR and GR. He really belongs to the 'who discovered Hubble's Law?' story because he tried to correlate nebular and cluster distances with velocities, including negative values for some globular clusters. He moved from the University of Rome to Eastman Kodak in Rochester, NY, in 1920, which was probably a good choice.

Smoluchowski, Marian von (1872–1917) L: '*Drei Vortrage uber Diffusion, Brownsche Molekularbewegung and Koagulation von Kolloidteichen*', *Phys. Zeit.*, **17**, 500, 1916. Professor at Cracow, he died 1917 September 3 of dysentery. AE thought very highly of him (Doc. 380), wrote an obituary for *Die Naturwissenschaften*, and spoke in his memory at a meeting of the Division of Particles and Fields of the American Physical Society (notes to Doc. 397, a letter from MvS's widow to AE thanking him for his kind letter of sympathy, and recalling their earlier acquaintance). Physicists at Cracow organized a centenary meeting there, 2017 September 3–8, recalling his contributions to statistical mechanics and related subjects.

Sommerfeld, Arnold (1868–1951) I, L: Held (up to 1960) the record for the largest number of nominations for the Physics Nobel Prize (84) without winning. Sixteen of his papers are cited in Vol. 8, and he approved, at least initially, of the *Entwurf* theory, and applied the Special theory to a variety of physics problems. He also accepted Weyl's version of unified field theory.

St. John, Charles (1857–1935) I, L, BEAII: In 'The principles of general relativity and the displacement of Fraunhofer lines toward the red' he reported that he had not been able to see any gravitational redshift in solar spectrograms, *ApJ*, **44**, 249, 1917. He later thought he had seen the expected shift, equivalent to about 1·2 km/sec, but separating that out from solar convection, rotation, Evershed effect, *etc.*, was done by others later.

Struve, Hermann (*Karl Hermann or Hermann Ottovich*) (1854–1920) L, BEAII: Part of the well-known dynasty of astronomers. One of several people who did not succeed in retrieving the 1914 eclipse instruments (Doc. 504). Here mostly in connection with AE's attempts to get a suitable position for Freundlich (*e.g.*, Doc. 151, 186, *etc.*) of whom HS was not entirely supportive, having noted, with Seeliger, EF's mistaken calculation of gravitational redshift.

Sturdy, Eduard (1862–1930) L: Professor of mathematics at Bonn, another who disputed aspects of GR in correspondence with AE (Docs. 618, 624, 627), but with the tone on both sides remaining friendly and respectful, as far as I can tell, necessarily trusting to the translations.

Tolman, Richard Chase (1881–1948) L, I, BEAII: 'The Principle of Similitude', *Phys. Rev.*, **3**, 244, 1914. The idea is that you could construct a miniature universe out of the same constituents as our real one. "No", would have said D'Arcy Wentworth Thompson, whose *On Growth and Form* was written during

WWI, and its second edition during WWII. Tolman belongs to a later era as far as GR is concerned. Perhaps one reason to note him here is that he was a pioneer in publishing in *Physical Review* rather than in one of the European journals. AE writes to Ehrenfest (Doc. 112) that the Tolman principle is just the requirement of covariance under a similarity transformation.

Weyl, Hermann (1885–1955) I, L, BEAII: '*Zur Gravitationstheorie*', *Ann. Phys.*, **54**, 117, 1917, and many later papers and books describing aspects of '*Raum, Zeit, und Materie*'. Discussed with AE elliptical *versus* spherical spaces for cosmology (*e.g.*, Doc. 551), gauge invariance (Doc. 661), and many other topics. Attempted a unified field theory of gravitation and electromagnetism in 1918–19, but gave it up. Was apparently the first to write equations of cosmology in co-moving form. He and his family came to the US and to the Institute of Advanced Studies in 1933 or 1934.

Wien, Wilhelm (1864–1928) L: He attempted to "penetrate the secrets of gravitation theory" (Doc. 15, June 1914); most of that letter and many more items pertain to academic politics. You know him for the Wien displacement law of black-body radiation; Nobel Prize in 1911.

Zeeman, Pieter (1865–1943) L, BEAII: Wrote to AE (Jan. 1918, Doc. 432) to report that he had done a torsion-balance experiment with two different uranium (radioactive) compounds and found only an upper limit to violation of the equivalence principle, though not so tight as for some other substances. AE had initially thought this important (Doc. 141, Nov. 1915) but later decided there was no need; Nobel Prize in 1902.

Looking ahead

Part III will explore some of the things that happened to General Relativity after, and sometimes because of, the Great War. Well-known items include the founding of the International Research Council (now ICSU) and the International Astronomical Union (still IAU) and the response of the Deutsche Astronomische Gesellschaft; the 1919-eclipse expeditions; an outburst of both enthusiasm for GR (to which the lifting of paper rationing must have been a contributor!) and also of opposition to it, which went back to the earliest days; discussions pro and con about the cosmological constant (for which there was always at least one believer from 1916 to the present!) and of gravitational radiation (ditto, though whether a system with no forces except gravity could emit energy this way remained disputed, with always at least one disbeliever, at least up until 2017 October).

Few now living remember that war, nor is my typewriter equal to describing the absolute horror. But if you find time between now and the appearance of Part III, do watch King Vidor's 1926 *The Big Parade*, starring John Gilbert (1898–1935) and Renée Adorée (1898–1933). It is, of course, silent (though there is a modern musical sound track on the DVD) and told with prejudice from the American point of view, but worth noting, I think, is that you cannot tell the American soldiers from the Germans by looking at them. And if you have kept track of the dates for the physicists and mathematicians above, you might conclude that postwar Germany (*etc.*) was a safer place to live than post-war Hollywood. Part III will probably not address that issue!

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 260: HD 3454, HD 63107, AND HD 69662

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Orbits are presented for three quite bright stars, whose identities appear in the title of this paper. Their periods are about 8 days, one year, and half a year, respectively. HD 63107, only, is double-lined; it has components that are almost equal.

HD 3454

HD 3454 is a $7½^{m}$ star of HD type F5, to be found near the southern boundary of Cassiopeia, about 5° north of the Andromeda Nebula. It has a substantial presence in the astronomical literature (not all of it deserved! — for example, *Simbad* cites papers by Eggen[1] and by Chapman *et al.*[2], both of which were actually referring to *HR* 3454). The star features in several photometric papers, which give for its magnitude and colour index values close to $V = 7^{m}{\cdot}53$ and $(B - V) = 0^{m}{\cdot}47$, respectively. It also appears in several papers that refer to elemental abundances; even where there are no authors in common, they seem to exhibit a remarkable unanimity in giving [Fe/H] = –0·58, with some other elements less under-abundant. There was for quite a time an analogous consensus on the radial velocity of the star, with the value of +31·5 km s^{-1} first given by Nordström[3] being favoured, though Gontcharov[4] ventured the very slightly different result of +31·1 km s^{-1}.

Gorynya & Tokovinin[5], however, observing with the 1-m telescope in the Crimea and pursuing a more independent line, found the radial velocity of HD 3454 to vary over a range of more than 30 km s^{-1}, the orbit being circular and having a period of only about 8 days. Their 33 observations spanned little more than a year, starting in 2012 October. The present writer failed to notice

TABLE I

Radial-velocity observations of HD 3454

2012–2013: Gorynya & Tokovinin[5]; 2014–2016: this paper

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2012 Oct. 21·97	56221·97	+52·0	0·786	+0·2
25·87	225·87	47·1	1·258	+0·2
25·88	225·88	47·4	·259	+0·6
29·88	229·88	47·0	·744	–0·1
31·91	231·91	64·4	·989	–1·1
Nov. 3·90	234·90	37·5	2·352	+0·3
5·86	236·86	33·0	·589	+0·4
9·87	240·87	63·3	3·075	–0·3
9·87	240·87	63·0	·075	–0·6
9·90	240·90	63·5	·078	0·0
2013 Aug. 23·08	56527·08	46·6	37·741	–0·2
28·10	532·10	37·7	38·348	+0·3
29·06	533·06	30·9	·465	+0·5
30·09	534·09	33·6	·590	+0·8
31·06	535·06	43·0	·707	0·0
Sept. 1·02	536·02	56·2	·824	+0·4
2·09	537·09	65·2	·953	+0·4
3·08	538·08	63·2	39·073	–0·6
5·05	540·05	41·2	·311	+0·1
6·11	541·11	31·0	·440	–0·3
7·09	542·09	30·7	·559	–0·5
9·04	544·04	53·1	·795	+0·4
9·99	544·99	63·1	·910	+0·3
11·08	546·08	65·0	40·042	0·0
Nov. 1·89	597·89	41·0	46·317	+0·5
6·89	602·89	63·6	·924	0·0
7·93	603·93	64·6	47·049	–0·2
9·90	605·90	44·8	·288	+1·3
10·92	606·92	32·6	·412	0·0
11·82	607·82	31·2	·520	+1·1
14·84	610·84	61·1	·887	–0·1
15·75	611·75	64·3	·997	–1·2
16·81	612·81	59·4	48·125	–0·9
2014 Aug. 13·13	56882·13	47·2	80·745	0·0
Sept. 23·09	923·09	42·9	85·706	–0·1
25·11	925·11	64·9	·951	+0·2
Oct. 8·03	938·03	29·7	87·516	–0·4
10·07	940·07	49·3	·763	+0·1
19·88	949·88	64·4	88·952	–0·4
21·99	951·99	52·7	89·207	+0·2
22·12	952·12	50·7	·223	–0·1
24·97	954·97	31·4	·568	–0·2
27·99	957·99	64·2	·934	+0·1
30·94	960·94	43·0	90·290	–0·3
31·94	961·94	32·4	·412	–0·2
Nov. 1·96	962·96	30·4	·535	0·0
2·94	963·94	37·5	·655	–0·2
3·96	964·96	51·1	·778	+0·2
5·97	966·97	65·6	91·022	+0·2
7·98	968·98	46·4	·264	+0·2
9·96	970·96	30·2	·505	+0·2
12·87	973·87	58·6	·857	–0·2
20·89	981·89	+56·3	92·829	+0·1

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
2014 Nov. 23·97	56984·97	+53·3	93·202	+0·2
24·85	985·85	41·3	·308	−0·1
29·93	990·93	64·1	·923	+0·6
Dec. 8·83	999·84	65·7	95·002	+0·1
13·82	57004·82	34·0	·605	+0·3
19·80	010·80	39·3	96·330	+0·1
2015 Jan. 16·78	57038·78	44·4	99·719	0·0
30·81	052·81	32·0	101·418	−0·3
Feb. 3·74	056·74	61·8	·895	0·0
Sept. 7·13	272·13	66·0	127·982	+0·5
Dec. 7·90	363·90	62·3	139·098	0·0
2016 Jan. 15·75	57402·75	53·6	143·804	−0·1
20·75	407·76	32·2	144·410	−0·5
Feb. 3·76	421·76	61·6	146·107	−0·1
Sept. 12·06	643·06	+62·2	172·911	−0·7

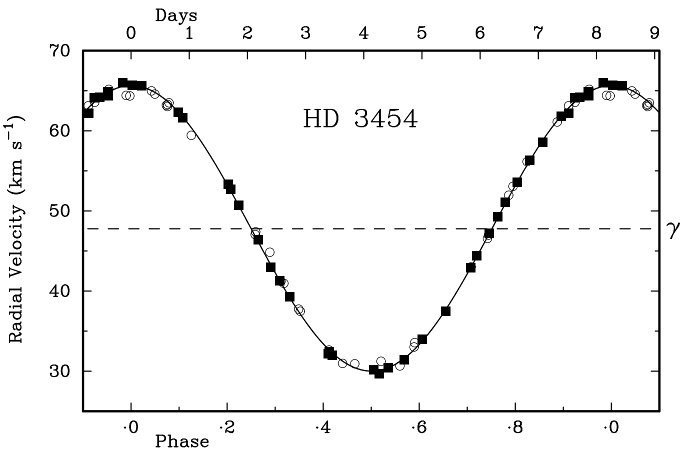

FIG. 1

The observed radial velocities of HD 3454 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The orbit is circular, as nearly as the observations can tell. The writer's radial velocities are plotted as filled squares; the open circles are those published by Gorynya & Tokovinin[5], which were attributed a relative weight of ¼ in the solution of the orbit.

the publication of that orbit when he became interested in HD 3454 in 2014 after finding his first two measures, made about six weeks apart, to be mutually discordant by more than 4 km s^{-1}. Two more observations in the next 15 days increased the range of the discordances to as much as 35 km s^{-1}, which is in fact just about the whole compass of the radial velocities exhibited by HD 3454. The star was then observed relatively assiduously (20 measurements in the ensuing two months), and the 8-day period quickly became apparent.

The radial velocities obtained by Gorynya & Tokovinin and by the present author mesh together rather well, the former covering the autumn observing seasons of 2012 and 2013, and the latter picking up in 2014. They are all set out in Table I. The numbers of the observations from the two sources are almost the same as one another — there are 33 in the former set and 35 in the latter. Solved independently for the orbit of the star, the two series of measurements yield mostly very similar elements, apart from a difference of about 2·2 km s^{-1} in the γ-velocities, no doubt reflecting a discrepancy in the zero-points. Without particularly wishing to apportion blame for the discrepancy, the writer points out that at least 0·8 km s^{-1} of it can be admitted to arise from the slightly idiosyncratic zero-point adopted in Cambridge. The zero-point of the velocities is actually not of great interest or significance as far as the orbit is concerned, but to prevent the difference between the two sources from worsening the orbital elements unnecessarily the writer has entered the Gorynya radial velocities in Table I with an empirical adjustment of +2·2 km s^{-1}. Among the orbital elements, the only one whose discrepancy between the two data sources gives cause for concern is the radial-velocity amplitude, for which the Cambridge value differs from the Russian one by 0·5 km s^{-1}, a surprisingly large amount that is more than three times the mathematically determined standard deviation of the difference between the two values. Table II presents the orbital solutions for HD 3454 from each of the two independent data sets on its own, and then the one — adopted as 'final' here, and illustrated in Fig. 1 — obtained by pooling the data. To obtain the best joint solution, the Cambridge velocities have been attributed four times the weight of the Russian ones, to bring the weighted variances of the two series into approximate agreement. Such a weighting is implicit in a comparison of the standard deviations of the various elements obtained in the separate solutions, which will be seen from Table II to differ (remarkably uniformly) by a factor of two.

TABLE II

Orbital elements for HD 3454

Element	*Gorynya*[5]	*This paper*	*Together*
P (days)	8·2559 ± 0·0005	8·25627 ± 0·00024	8·25613 ± 0·00013
T (MJD)	56463·669 ± 0·010	57049·356 ± 0·005	56933·770 ± 0·005
γ (km s^{-1})	+47·66* ± 0·09	+47·75 ± 0·05	+47·77 ± 0·04
K_1 (km s^{-1})	17·41 ± 0·13	+17·91 ± 0·06	+17·81 ± 0·06
e	0	0	0
ω (degrees)	—	—	—
$a_1 \sin i$ (Gm)	1·976 ± 0·014	2·033 ± 0·007	2·022 ± 0·007
$f(m)$ ($M_\odot$)	0·00452 ± 0·00010	0·00493 ± 0·00005	0·00484 ± 0·00005
R.m.s. residual (wt. 1) (km s^{-1})	0·48	0·25	0·27

*Adjusted to the Cambridge zero-point

HD 63107

HD 63107 is a 7^m late-type star in the constellation Canis Minor; it is to be found about 4° north-following Procyon. *Tycho 2* photometry has given $V = 7^m{\cdot}09$, with a colour index of $(B - V) = 0^m{\cdot}52$ that is in agreement with the *HD* type of F8. Its brightness has resulted in its featuring in a quite a number of catalogue-type papers. Eleven of the 17 papers retrieved for it by *Simbad* list more than 10000 objects each, and the numbers of objects mentioned in the

TABLE III

Radial-velocity observations of HD 63107

All the observations were made with the Cambridge Coravel

Heliocentric Date	*HMJD*	*Velocity Prim.* km s^{-1}	*Velocity Sec.* km s^{-1}	*Phase*	*(O–C) Prim.* km s^{-1}	*(O–C) Sec.* km s^{-1}
2005 Nov. 5·21	53679·21	+23·4	+1·9	0·855	+0·1	−0·1
14·16	688·16	+25·9	+0·5	·880	+0·2	+0·9
25·14	699·14	+28·3	−3·0	·910	+0·1	0·0
Dec. 18·09	722·09	+30·8	−6·1	·974	−0·2	−0·4
2006 Jan. 29·05	53764·05	+23·8	+1·8	1·091	0·0	+0·3
Feb. 8·96	774·96	+20·7	+4·9	·122	+0·3	−0·1
16·01	782·01	+18·0	+7·4	·141	−0·1	+0·2
Oct. 27·20	54035·20	+21·9	+2·6	·847	−0·6	−0·2
Dec. 9·16	078·16	+30·6	−5·9	·967	−0·3	−0·3
2007 Jan. 14·12	54114·12	+25·9	−1·3	2·067	−0·4	−0·3
23·03	123·03	+23·7	+1·9	·092	0·0	+0·3
Mar. 21·94	180·94	+6·7	+18·2	·254	−0·4	−0·1
Apr. 1·91	191·91	+5·1	+20·7	·284	+0·1	+0·2
10·89	200·89	+3·8	+21·8	·309	+0·2	−0·1
Nov. 24·19	428·19	+30·0	−5·5	·943	−0·2	−0·6
Dec. 17·06	451·06	+30·5	−4·8	3·006	0·0	+0·4
2008 Apr. 17·86	54573·86	+1·8	+23·5	3·349	0·0	−0·2
2009 Jan. 14·08	54845·08	+22·8	+3·2	4·105	+0·5	+0·2
2010 Apr. 12·89	55298·89	+1·2	+24·7	5·370	+0·1	+0·3
Dec. 6·13	536·13	+29·5	−3·0	6·031	+0·3	+1·0
12·18	542·18	+28·0	−3·6	·048	0·0	−0·8
2011 Jan. 19·08	55580·08	+17·1	+8·6	6·154	+0·4	0·0
Dec. 18·15	913·15	+24·4	+0·6	7·082	−0·4	+0·1
2012 Apr. 10·88	56027·88	+0·4	+25·0	7·402	0·0	−0·2
26·86	043·86	−0·6	+25·7	·446	−0·5	+0·1
Dec. 2·16	263·16	+27·2	−1·6	8·058	0·0	+0·3
2013 Feb. 2·97	56325·97	+8·8	+16·7	8·233	0·0	+0·1
Apr. 30·86	412·86	0·0	+25·5	·475	+0·1	−0·1
2014 Nov. 6·19	56967·19	+30·2	−4·5	10·020	+0·3	+0·1
2015 Dec. 9·20	57365·20	+19·5	+5·9	11·130	0·0	0·0
2016 Feb. 18·98	57436·98	+2·8	+23·3	11·330	+0·2	+0·4
Mar. 21·91	468·91	−0·2	+25·7	·419	−0·3	+0·3
2017 Jan. 25·02	57778·02	+5·1	+19·7	12·280	−0·2	−0·5

other six range from 291 to 2473, so it is evident that the individual interest of HD 63107 has not been recognized until now. Indeed, the only information that the present writer found to be of much interest in the literature on the object was a measurement of the radial velocity, +11·1 km s^{-1}, published by Nordström *et al.*[3] in 2004 (but I did not seem able to discover the date of the observation).

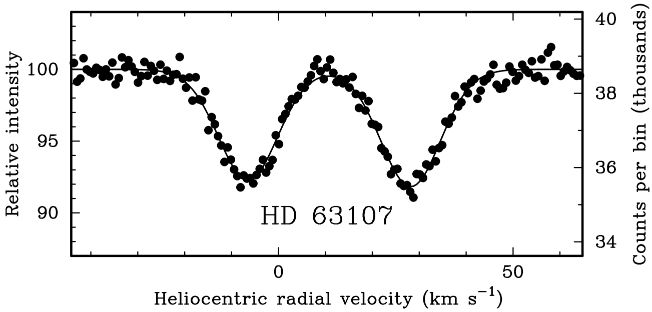

FIG. 2

Radial-velocity trace of HD 63107, obtained with the Cambridge *Coravel* on 2014 November 6 and illustrating the double lines at practically their maximum separation.

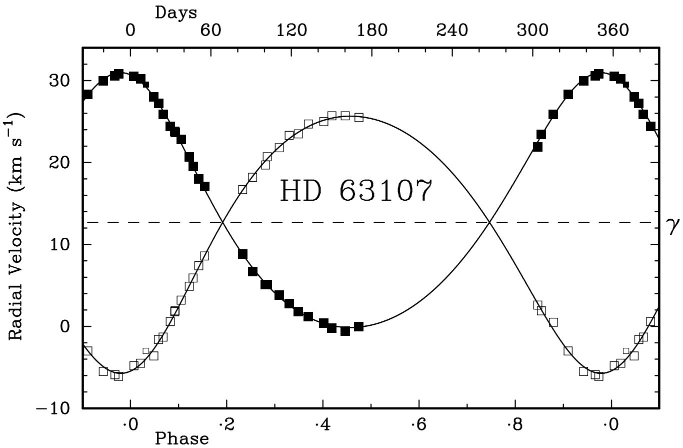

FIG. 3

The observed radial velocities of HD 63107 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Velocities of the primary star are plotted with filled symbols, those of the secondary with open ones. The large gap in phase coverage is caused by the close approximation of the orbital period to a calendar year, and the near-equatorial declination of the star preventing observations being made far from the meridian.

TABLE IV

Orbital elements for HD 63107

P	=	358·75 ± 0·06 days	T_5	=	MJD 55166·2 ± 1·3
γ	=	+12·70 ± 0·04 km s^{-1}	$a_1 \sin i$	=	75·5 ± 0·4 Gm
K_1	=	15·55 ± 0·08 km s^{-1}	$a_2 \sin i$	=	76·2 ± 0·4 Gm
K_2	=	15·69 ± 0·08 km s^{-1}	$f(m_1)$	=	0·1335 ± 0·0021 $M_\odot$
q	=	1·009 ± 0·007 (= m_1/m_2)	$f(m_2)$	=	0·1372 ± 0·0022 $M_\odot$
e	=	0·178 ± 0·004	$m_1 \sin^3 i$	=	0·544 ± 0·007 $M_\odot$
ω	=	11·0 ± 1·4 degrees	$m_2 \sin^3 i$	=	0·539 ± 0·007 $M_\odot$

R.m.s. residual (unit weight) = 0·31 km s^{-1}

The star was placed on the programme of the Cambridge 36-inch reflector and *Coravel* in late 2005, and straight away gave a double-lined trace, with the two dips being nearly equal to one another. The velocities were about 22 km s^{-1} apart, but their mean was close to the published value. A second observation was taken about a week later; it already differed significantly from the first one, the velocity separation of the components having increased to about 25 km s^{-1}. The object was then scheduled for routine observation, and 33 pairs of radial velocities of it have been accumulated. They are presented in Table III. A trace showing the two dips at nearly their maximum separation of about 35 km s^{-1} appears here as Fig. 2. The measurements show HD 63107 to have an orbital period of about a year. In fact, the period is so inconveniently close to the exact year (differing from it by less than a week) that after an observational campaign that has lasted more than a decade there is still a considerable gap in the phase coverage of the measurements, as is all too apparent from Fig. 3, which illustrates the orbit. (Although the star is modestly (9°) north of the celestial equator, it is 11° south of the ecliptic, making it inaccessible to the 36-inch telescope from the beginning of May until late October.) Despite the phase gap, the radial velocities now available define the orbit quite well, and yield the elements set out in Table IV. Although the components of the binary appear to be very similar, at least as judged from the 'dips' that they give in radial-velocity traces, the r.m.s. residuals of the velocities of the secondary are appreciably higher than those of the primary (0·35 km s^{-1} against 0·26). There is no obvious reason for that difference, and in the solution of the orbit set out in Table IV below the velocities of the two stars have been treated as if they merited equal weights.

HD 69662

HD 69662 is in the constellation Cancer; it is about 2° north of β Cnc. Its magnitude and colour index have been derived from *Hipparcos* photometry as $V = 8^{m}{\cdot}65$, $(B-V) = 1^{m}{\cdot}08$; the latter could be considered to be in agreement with its *HD* spectral type of K0. Its parallax, however, is only about 2 milliseconds of arc, putting it at a distance of the order of 500 pc or a distance modulus of eight or nine magnitudes; the star evidently has the luminosity of a giant, and in the absence of an actual classification its MK type might be expected to be K0 III. The bibliography retrieved by *Simbad* is extraordinarily niggardly, consisting of just one paper[6], 35 years old, entitled 'New optical positions and proper motions of late type stars associated with SiO masers'.

The star was placed on the observing programme of the Cambridge *Coravel* in 2013 (no significance is to be attached to the fact that the first observation was made on April 1!). The next observation was not made until the end of the same year, and disagreed by about 20 km s^{-1}, so the star was watched reasonably attentively (22 observations in that season, 43 altogether now). The results are set out in Table V, and yield the orbit which is shown in Fig. 4 and whose elements are given in Table VI.

The period is seen to be extremely close to half a year (it is 0·5003 Julian years), but (unlike the preceding case where it was a whole year) no difficulty arose in obtaining velocities at all phases of the orbit.

TABLE V

Radial-velocity observations of HD 69662

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2013 Apr. 1·92	56383·92	−22·9	0·299	+0·5
Dec. 29·04	655·04	−42·0	1·783	−0·1
2014 Jan. 5·09	56662·09	−44·6	1·822	−0·2
13·14	670·14	−46·6	·866	+0·1
27·09	684·09	−47·8	·942	+0·1
Feb. 3·03	691·03	−46·9	·980	−0·5
12·96	700·96	−41·4	2·034	+0·7
20·94	708·94	−37·6	·078	0·0
22·92	710·92	−36·5	·089	−0·1
25·93	713·93	−35·0	·105	−0·3
27·92	715·92	−33·7	·116	−0·1
Mar. 1·96	717·96	−32·5	·127	+0·1
3·92	719·92	−31·8	·138	−0·2
7·92	723·92	−30·1	·160	−0·4
9·98	725·98	−28·5	·171	+0·4
11·91	727·91	−27·8	·182	+0·3
13·90	729·90	−27·2	·193	+0·2
15·97	731·97	−26·9	·204	−0·2
18·96	734·96	−25·7	·221	+0·2
21·86	737·86	−25·0	·236	+0·1
Apr. 4·95	751·95	−23·3	·313	−0·1
14·86	761·86	−23·0	·368	+0·1
18·89	765·89	−23·4	·390	−0·1
Oct. 28·25	958·25	−24·9	3·443	−0·6
Nov. 6·22	967·22	−25·3	·492	+0·4
24·19	985·19	−29·9	·590	0·0
Dec. 6·20	997·20	−33·0	·656	+0·5
30·12	57021·12	−42·0	·787	+0·2
2015 Jan. 11·09	57033·09	−46·2	3·852	−0·1
17·07	039·07	−47·6	·885	−0·2
23·01	045·01	−47·7	·917	+0·3
Feb. 6·99	059·99	−45·0	·999	+0·1
18·02	071·02	−39·8	4·060	−0·3
Mar. 31·92	112·92	−24·0	·289	−0·4
Apr. 11·87	123·87	−23·1	·349	−0·1
22·89	134·89	−23·3	·409	+0·3
23·88	135·88	−23·6	·415	+0·1
Nov. 28·25	354·25	−31·1	5·610	−0·2
Dec. 9·19	365·19	−34·6	·670	−0·2
21·12	377·12	−38·8	·735	−0·1
2016 Mar. 5·00	57452·00	−30·9	6·145	+0·1
Apr. 6·91	484·91	−23·3	·325	−0·2
2017 Jan. 14·06	57767·06	−46·9	7·869	0·0

TABLE VI

Orbital elements for HD 69662

P	=	182·72 ± 0·11 days	$(T)_1$	=	MJD 56877·4 ± 0·8
γ	=	−33·77 ± 0·05 km s^{-1}	$a_1 \sin i$	=	30·88 ± 0·17 Gm
K	=	12·50 ± 0·07 km s^{-1}	$f(m)$	=	0·0352 ± 0·0006 $M_\odot$
e	=	0·184 ± 0·005			
ω	=	220·1 ± 1·8 degrees	R.m.s. residual (wt. 1)	=	0·27 km s^{-1}

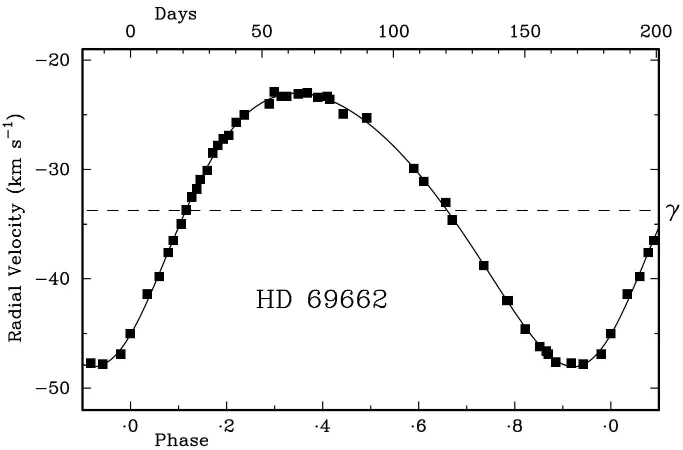

FIG. 4

The observed radial velocities of HD 69662 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them.

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CORRESPONDENCE

To the Editors of 'The Observatory'

On the Velocity of Gravitational Waves

Gravitational waves are a fundamental prediction of Einstein's General Relativity, and the question of the velocity c_g with which those waves propagate has long been of interest. As early as 1913 Einstein was aware that his field equations had propagating-wave solutions which propagated at the speed of light c (ref. 1, introduction to Chapter 35), but the physical reality of those solutions was not established until the Feynman–Bondi 'two beads on a stick' *gedanken* experiment in 1957. In General Relativity (although not in some other relativistic gravity theories) $c_g = c$; both speeds (and more generally propagation directions in space–time) encode the same information about the structure of space–time⋆.

Van Flandern[3] claimed that the observed absence of gravitational aberration implies that $c_g > 2 \times 10^{10}$ cm s^{-1}, but Carlip[4] showed that aberration in General Relativity is almost exactly cancelled by velocity-dependent interactions so that $c_g = c$ remains consistent with experimental data. More recently Fomalont & Kopeikin[5] [hereinafter FK] claimed to have observationally determined that $c_g = c$, but Carlip[6] found flaws in FK's theoretical analysis such that no conclusions can be drawn about c_g from FK's observational data.

Taylor[7] has recently described a *gedanken* experiment which is claimed to demonstrate that $c_g = c$. Taylor's argument is essentially as follows:

(*i*) A key foundational principle of Special Relativity is the existence of (to use today's terminology) a Lorentz-invariant velocity.

(*ii*) Special Relativity can be formulated — and remains a valid description of the Universe in which we live — with the 'Lorentz-invariant velocity' being either c (the usual formulation), or c_g.

(*iii*) One can (*gedanken*) experimentally measure physically-real things (the Lorentz–Fitzgerald-contracted sizes of moving objects) which depend on the Special Relativity 'Lorentz-invariant velocity'.

(*iv*) There is (we assume) a unique physical reality, so measurements of physically real things made using the c and c_g variants of Special Relativity must be consistent with each other.

(*v*) In order for the c and c_g variants of Special Relativity to be consistent with each other, we must have $c_g = c$.

This argument does indeed establish that (at any time and place in space–time) there is a single *unique* Lorentz-invariant velocity, which must equal c. However, *a priori* we have no knowledge of whether or not c_g is in fact a Lorentz-invariant velocity, and without knowing that, step (*ii*) of Taylor's argument isn't justified: if c_g is *not* Lorentz-invariant, then Special Relativity can *not* consistently be formulated using c_g. Therefore, without additional *a-priori* knowledge of gravitational waves' properties, Taylor's argument isn't valid.

Another way to see the limits of Taylor's argument is to observe that its logical structure and validity are unchanged if we replace 'gravitational waves' by 'neutrinos' throughout the argument. Yet we know that at least some neutrinos have non-zero mass, and hence propagate at a speed that is less than the speed of light. (Such a propagation speed is not Lorentz-invariant.)

In conclusion, the question raised by Taylor (what is the propagation speed of gravitational waves?) is indeed an important one, but Taylor's *gedanken*-experiment analysis doesn't resolve this question. Instead, we must seek further experimental data.

Fortunately, such data have recently become available: the *Advanced LIGO* and *Virgo* gravitational-wave detectors recently observed gravitational waves from the binary neutron-star merger GW170817, which was also observed

⋆Here we focus on 'weak' gravitational waves, whose own mass–energy does not appreciably perturb the overall structure of space–time. This permits us to model the waves as small perturbations of the space–time metric. Low[2] discusses some of the mathematical subtleties involved in defining the 'propagation speed' of gravitational waves which are not weak.

electromagnetically as the short gamma-ray burst GRB170817A. Analysing the observed time delay between the gravitational-wave and gamma-ray signals, Abbott *et al.*[8] derived the constraint $-3 \times 10^{-15} < (c_g - c)/c < +7 \times 10^{-16}$ showing that c_g and c are almost exactly equal. (As noted earlier, in General Relativity c_g and c are precisely identical.)

Yours faithfully,
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REVIEWS

Radio Astronomer: John Bolton and a New Window on the Universe, by Peter Robertson (University of New South Wales Press), 2017. Pp. 421, 24 × 16 cm. Price £61·95 (hardbound; ISBN 978 1 74223545 5).

Peter Robertson is an Honorary Research Fellow at the University of Melbourne. Having spent most his career in publishing for CSIRO he is immersed in the history of astronomy in Australia. Writing *Beyond Southern Skies* (CUP 1992), the story of the Parkes 210-ft radio telescope, introduced him to John Bolton, one of the pioneers of radio astronomy, who was Director at Parkes from 1961 to 1971. He realized that Bolton's outstanding research career and his personality deserved a full biography; furthermore, he had access to the extensive CSIRO archive, and he was able to interview Bolton at length before he died in 1993. The result is an outstanding biography encompassing his research and his colourful life.

Bolton was one of the pioneers of radio astronomy who built upon experience in the development of radar during World War II. Robertson traces his life from Sheffield through Trinity College Cambridge to Radar Officer in the Royal Navy, working on airborne radar at the Telecommunications Research Establishment (TRE) and at sea in the Pacific on an aircraft carrier. In 1946 he joined E. G. Bowen and J. L. Pawsey at CSIRO in Sydney following up J. S. Hey's discoveries of radio emission from the Sun and from an unidentified source apparently in the Milky Way galaxy. Locating and identifying that source, and discovering the Crab Nebula as a radio source, started his life's work. Discovering, accurately locating, and optically identifying radio sources became his speciality, bringing him into collaboration with the leading Californian astronomers and resulting in a six-year appointment in Caltech during which he was involved in the identification of quasars. He developed a position-finding interferometer and became Director of the new Owens Valley Observatory. Returning to CSIRO he oversaw the completion of the Parkes telescope and became the first Director of Parkes Observatory.

The many and sometimes complex personal relationships involved in this eventful career are sympathetically and entertainingly described by Robertson. He conveys well the personality of this tough Yorkshireman, competitive and outspoken, determined and energetic, and sometimes abrasive. He does not attempt to place Bolton alongside his contemporaries in the UK and USA, except in an unfortunate discussion of the possibility that he might have achieved a Nobel Prize, including unnecessarily relaying disparaging remarks about an actual winner. It will take a historian who is not based in CSIRO to make a better-informed judgement. What we do have, however, is a very entertaining and readable biography of a major contributor to a golden age of astronomy. — FRANCIS GRAHAM-SMITH.

Blockbuster Science: The Real Science in Science Fiction, by David Siegel Bernstein (Prometheus), 2017. Pp. 336, 23·5 × 16 cm. Price $24 (About £18) (hardbound; ISBN 978 1 63388 369 7).

So yeah — right — like you know how that Gravity stuff (probably named after the movie) and all that Quantum do dah is really difficult — right? WRONG. It's pretty simple — well not that simple obviously — but YOU can get a handle on it — like anyone can — just your basic dude (and dudes, natch), anyone who goes to the movies can understand it.

OK, so Bernstein's prose isn't as annoying as my pastiche but it is quite high energy and aimed at people who are linguistically about 25 or under. But other than that there isn't too much wrong with this book — as Douglas Adams might have said, it is mostly harmless. Well, I was bothered by a couple of trivial things: the explanation of Han Solo's silly description of the *Millennium Falcon* in the very first of the *Star Wars* movies — "the ship that made the Kessel run in less than 12 parsecs" — is not at all satisfactory, and the famous 'Tears in the Rain' speech from a doomed replicant in *Blade Runner* doesn't even get a mention — you know the one — "… attack ships on fire on the shoulder of Orion…"; but then it is a book about science and not poetry. The science is casually explained, mostly at a very superficial level — think tabloid-newspaper science, but with a better grasp of the underlying physics. The entire spectrum of science from the origin of the Universe to its final death, along with human evolution, consciousness, artificial intelligence, fossils, and lots more is covered in 21 brisk chapters. Each subject thus gets at most a couple of paragraphs, but only once did I think that Bernstein had not been as careful with concepts as he might have been — the acceleration due to gravity, *g*, is described as a sort of pressure (necessary for the correct functioning of our Earth-evolved bodies) but correctly given the units of acceleration. That is a rare example as generally care has been taken to be faultlessly, scientifically correct.

Most explanations are at a non-challenging level and do not go very deep, although occasionally some very nice insights are included — I particularly liked the author's explanation of relativistic time dilation which he then uses in relation to the twin paradox. For those wanting to dig deeper there are references to further reading — often an original paper, as well as a comprehensive more general 'further reading' list which includes sci-fi novels. There is also a list of relevant movies, cross-referenced to the explanatory chapters, and even a listening list of technology-related pop music.

I suppose one responsibility of adults in a civilized society is to pass on to the young the acquired knowledge of that civilization. A few thousand years of thought and enquiry have given us a lot to pass on. For most of human history, wisdom and myth have been transmitted by story-telling — sitting around a camp fire perhaps. For the last few decades story-telling has been replaced by film and the camp fire by TV and, Lord help us, YouTube. The aim of this book is to separate the wisdom from the myth in today's stories as relayed by science-fiction films, books, and TV series, and in a similar story-telling way it does a pretty good job. Each chapter deals with a concept to be found in science fiction — time travel, teleportation, parallel worlds, mechanically and genetically enhanced humans, the nature of existence, *etc.*, *etc.*, and the science behind each is explained along with the current limits of knowledge, as well as some speculation as to how far a particular technology may be pushed.

This book is a means to show the science concepts behind some of the whacky-seeming technology to be found in science fiction, and by illuminating those concepts and existing technology with simple explanations, provides a fun way to feed an interest in science or even just inspire a rational way of thinking. For the young reader who just wants to know if *Star Trek*'s teleporting transporter might be feasible, or exactly how does Doctor Who's *TARDIS* get around, and how can it possibly be roomier on the inside than out, then this book will provide some clues in a chatty and not-too-difficult way. — BARRY KENT.

Wormholes, Warp Drives and Energy Conditions, edited by Francisco S. N. Lobo (Springer), 2017. Pp. 303, 24 × 16 cm. Price £88/$139 (hardbound; ISBN 978 3 319 55181 4).

Two recurrent sci-fi dreams survive on the edge of our scientific knowledge, refusing to be declared impossible: *time machines* and *faster-than-light travel*. Within General Relativity, those ideas are realized by strange space–times — wormholes and warp drives — that require exotic matter with negative energy. This book reviews the main efforts to understand those weird spaces, and examines many of the mechanisms by which their exotic matter might arise.

Be warned, though: this is not a pedagogical introduction to the field. (A reader in search of such a book should obtain Matt Visser's *Lorentzian Wormholes* instead.) Most of the chapters on *Wormholes* are mini-monographs, something between a review and an original research paper. Each topic is taken to great depth, and a well-equipped reader will be able to dig down to cutting-edge results. But while some chapters develop geometric intuition and physical understanding, many others have a rather technical flavour, and it is easy to become bogged down in mathematical minutiae. In the last third of the book, we move on to *Energy Conditions* and *Warp Drives*. Those topics do not appear in many textbooks, so I was glad to find that a rather more systematic and introductory approach was taken here.

Many of the wormhole chapters invoke a particular theoretical phenomenon (*e.g.*, gravitons, modified gravity, phantom fields) to provide the exotic matter required. My main scientific criticism of the book is that these speculative ideas are introduced without much reflection on the broader problems they would cause. For example, there is no mention of the quantum instability of phantom fields, nor much concern for the validity of a semi-classical treatment of gravitons in a Planck-scale wormhole. A small dose of scepticism would have conveyed a more balanced view of wormhole physics, without diminishing the value of the work the authors describe.

In general, the majority of chapters work best as jumping-off points: state-of-the-art studies designed to bestow expertise, inspire the reader, and form the basis of some new research project. The ideal reader would be an established researcher with a strong background in General Relativity and field theory. The sections on energy conditions and warp drives should also serve as a useful introduction and reference for graduate students interested in those topics. — LUKE M. BUTCHER.

Stars: From Collapse to Collapse (ASP Conference Series, Vol. 510), edited by Yu. Yu. Balega, D. O. Kudryavtsev, I. I. Romanyuk & I. A. Yakunin (Astronomical Society of the Pacific), 2017. Pp. 594, 23·5 × 15·5 cm. Price $88 (about £67) (hardbound; ISBN 978 1 58381 904 3).

On handling a new book, I am often struck by a disordered string of thoughts both directly but often quite indirectly related to the book's subject matter. This volume was no exception. The first thought went back to early 1953 March and a playground at the Grammar school in Ashford in Kent. For reasons never known to schoolboys like myself, the wall opposite the physics classroom had a blackboard which was never used, but on 1953 March 6 the board contained the message in white chalk 'Stalin is dead'. The last thought in the string released by the volume under review concerned the 1985 IAU meeting in New Delhi which was attended by very few Russian participants and, hence, it was difficult to appreciate the range of observational and theoretical astrophysics activities undertaken within the Soviet Union. Both those thoughts are firmly set in the era when Russia was encircled by an almost impermeable membrane inhibiting even the flow of astronomical information out of and into the Soviet Union.

With the dissolution of the Soviet Union in the late 1980s, the information flow has increased as is especially highlighted by this 2017 volume in the *ASP Conference Series* with its title *Stars: From Collapse to Collapse*. In nearly 600 pages, Volume 510 in the ASP series covers the 2016 conference celebrating the 50th anniversary of the Special Astrophysical Observatory (SAO) of the Russian Academy of Sciences. The SAO's principal telescopes consist of the 6-metre *Large Altazimuth Telescope* (*BTA*) and the *RATAN-600* radio telescope. The SAO was founded in 1966 in the Greater Caucasus in what was then the USSR but is now the Karachay–Cherkess Republic. The *BTA* saw first light in 1975 and the present borosilicate mirror was installed in 1978. As the editors explain in the Preface, "study of stellar physics is one of the main research directions in our observatory", and so appropriately the 50th anniversary was celebrated with "a discussion of a wide range of topics related to astrophysics of stars and stellar systems". The conference was held at the SAO but its proceedings are published by the Astronomical Society of the Pacific.

The editors tell us that at the conference there were seven invited plenary talks, 120 short talks, and 60 posters and "most of them are published in this volume". Published contributions total 146 for an average of four pages per paper with a greater than average length for what I assume were the plenary contributions. The volume is divided into six parts beginning with 'Star formation and interstellar medium' (the first 'collapse') and followed by 'Stellar atmospheres and stellar magnetism', 'Stellar activity', 'Multiple star systems and exoplanets', 'Stars after the nuclear burning' (the second 'collapse'), and concluding with 'Methods and instruments of stellar astrophysics'. Contributions in the main discuss or interpret observations obtained with the *BTA* (or the *RATAN*) but there are exceptions. I was greatly impressed by the breadth of the contributions which, of course, was anticipated by the phrase "Collapse to Collapse". In particular, papers in the final section suggest that new instrumentation will ensure an exciting future for optical studies of stellar physics at the *BTA*.

While almost no subfield of stellar physics goes without mention in this volume, there is one lacuna in my opinion. The volume would have been of even greater interest to the inquisitive reader had it included an account from an observer's perspective of using the *BTA* to pursue stellar physics. Perhaps, when the next anniversary for the SAO arises there will be an opportunity for a spectrum of observers to describe their experiences at the *BTA*. In the meantime, pick up the volume and dip into it and all stellar physicists will find something to engage their interest. — David L. Lambert.

Gravitation, by Charles W. Misner, Kip S. Thorne & John A. Wheeler (Princeton University Press), 2017. Pp. 1280, 26 × 21 cm. Price £49·95/$60 (hardbound; ISBN 978 0 691 17779 3).

This 2017 edition of the classic 1973 book includes a new and rewarding foreword and preface, with an interesting historical perspective on the book, and how it was received in the 1970s. Otherwise it is unchanged, with its famous Track 1, Track 2, and multi-page explanatory boxes and inimitable layout. Most of the book is just as relevant as it was in 1973, with the main exceptions being in our understanding of cosmology, and, of course, the discovery of gravitational waves. The additional material makes clear which parts of the book still stand up, and ample references are given to direct the reader where to go for a more modern perspective where needed. At the time of first publication, the main competitors were Steven Weinberg's book *Gravitation and Cosmology*, and *The Large Scale Structure of Space-Time*, written by Stephen Hawking and George Ellis. *Gravitation* was something of a marmite book — people tended to react strongly one way or another. I confess that as a student I was in the Weinberg camp, preferring its field-theoretic approach to the geometric viewpoint of *Gravitation*, but the coordinate-free geometric description is elegant and illuminating, and I'm not sure now which I prefer — they are both wonderful books. I would do calculations *à la* Weinberg, but conceptually think about them in the way *Gravitation* advocates. Indeed, in its 1280 pages it provides innumerable conceptual insights that will be a revelation to readers coming across them for the first time. Everyone interested in gravity should have this on their shelf (strengthened, if necessary), and at under £50 for the hardback, there really is no excuse. — ALAN HEAVENS.

Data Analysis Techniques for Physical Sciences, by Claude A. Pruneau (Cambridge University Press), 2017. Pp. 704, 25 × 19·5 cm. Price £69·99/$89·99 (hardbound; ISBN 978 1 108 41678 8).

I'm of a generation raised as frequentists, largely as a consequence of the practical limitations of computing power in the last century. I'm now a happy convert to the Bayesian side, so when there was an opportunity to review a text on data-analysis techniques I took it, anticipating that a modern book with this title may broaden my Bayesian horizons. However, it turns out that I'd misjudged the intended meanings of both 'data analysis' and 'physical sciences'.

The text opens with an introductory discourse on 'The scientific method'. I could well have done without this, and particularly its intrusive and discordant Dawkins-esque diatribe against religious faith (I wondered if the author might be annoyed by proselytising neighbours?). The following, and greater, part of the book then presents a 'Foundation in probability and statistics' (370 pages). This develops largely traditional stats material systematically, clearly, and thoroughly, building from the fundamental principles of probability, through standard frequency distributions, to 'classical' (frequentist) approaches to estimators, optimization, and hypothesis testing. The final, substantial, chapter in this section is titled 'Bayesian inference', but the approach seemed unorthodox to this journeyman astronomical-data analyst. There's nothing here (or anywhere else in the book) on Hastings–Metropolis or Markov-chain Monte-Carlo, and rather than maximizing probability, the focus seems to be on minimizing χ^2 — philosophically different, even when functionally equivalent. It's in this section that the simplex and Levenberg–Marquardt algorithms are introduced (among others), and the latter is explicitly identified with 'damped least squares', which, to me, is redolent of a basically frequentist approach.

The next 250-odd pages fall under the heading of 'Measurement techniques'. The averred intent of this section is to introduce "a variety of measurement techniques commonly utilized in nuclear and particle physics" — which is indeed, very specifically, what it does. It's a stretch to generalize the content even to other experimental physical sciences, and on browsing through this material I couldn't find anything that struck me as particularly useful in the setting of largely observationally-driven astrophysics. (Curiously, there exists an ISBN for a recently-published but apparently unavailable book by the same author, in another publisher's catalogue, with the somewhat narrower title of *Data Analysis Techniques for Nuclear and Particle Physicists* — coincidence?) The concluding section, entitled 'Simulation techniques' (~ 40 pp.), comprises a short general introduction to Monte-Carlo methods, including MC integration; and a chapter on 'Collision and detector modelling', again largely cast in the context of particle colliders.

I ended up with ambiguous feelings about this book (which might have been more accurately titled 'Statistics for Particle Physicists'). On the plus side, it's nicely produced, reasonably priced, carefully crafted, and well organized, with a clear and authoritative didactic style. There are useful exercises, a thorough index, and a list of almost 200 references to primary sources and technical monographs. However, the systematic development of everything from the ground up is a double-edged sword. On the one hand, knowledgeable users looking for a quick 'how-to' may be frustrated by the slow build-up (*e.g.*, least squares isn't discussed until p. 190); on the other, those looking for a primer may be deterred by the rigour (*e.g.*, the basics of probability are set out as axioms in set theory).

This is a book that I really wanted to like more, and if I were to have to give lectures on any aspect of basic statistics I would certainly refer to it, probably extensively. However, while I would happily point HEP postgrads in its direction, I couldn't recommend it as a text to undergraduates; and as an astronomer I'm hard pressed to identify anything in it, other than elementary summary statistics, that would be useful for practical data analysis in my field. — IAN D. HOWARTH.

The Art of Astrophotography, by I. Morison (Cambridge University Press), 2017. Pp. 270, 24·5 × 19 cm. Price £29·99/$44·99 (paperback; ISBN 978 1 316 61841 7).

Modern cameras, optics, and software mean that astrophotography is much more accessible than it was ten or twenty years ago and this is evident from the number of very-high-quality images that appear in the popular media. The author's aim here, as stated in the introduction, is to provide enough information to allow budding imagers to produce "beautiful astrophotos". I think he achieves that quite well, although, like all recipe books, there are often multiple ways to the same end and the author's approach is strongly affected by his own equipment, experience, and preferences. This isn't a particularly bad thing. It is clear that the author has actually tried nearly everything set out in his book and his personal experience is clear from the narrative. It just means that readers should be aware that there may be other, possibly better, ways of doing things.

The book covers the whole range of astrophotography from star trails and sky-scapes using a fixed camera to high-resolution planetary and lunar imaging and cooled-camera, multi-filter, deep-sky imaging on a tracking mount. It contains quite a few good ideas and clever tricks, although many are very specific to the named software tools that the author uses. Unfortunately, the publisher has spoiled a lot of this by printing many of the images far too dark (at least in my copy). That is a common problem with this kind of book but I would have hoped the production team at the publisher would have done better than this.

The word "art" in the title is very apt since many of the images are composites, where foregrounds and backgrounds are processed differently or even taken at different times. I have no problem with that as long as the photographer makes it clear that that is what they have done in the caption. One of the curses of the Internet is unacknowledged composite images and knowing what is reasonable and what is not.

There are a few areas where the author's knowledge goes a bit off-piste (the description of FITS files and the image calibration process set out in appendix E for instance), but they aren't key aspects and they don't really detract from the book. Despite those minor gripes I think this is an excellent introduction to astro-imaging and I would recommend it to anyone starting out along this long and addictive path. — NICK JAMES.

The Limousin Asteroid Impact of the Triassic Rhaetian Age, by Neil R. Taylor (Observatoire Solaire), 2017. Pp. 71, 21 × 15 cm. Price £6·99 (paperback; ISBN 978 1 9999 044 1 8).

Dinosaurs died out about 66 million years ago. I well remember the famous American astronomer Fred L. Whipple telling me he had thought of at least thirteen things that could have caused this disaster. However, since the 1981 pioneering work of Luis and Walter Alvarez that led to the discovery of an iridium-enriched layer at the geological C–P boundary, together with the 1991 discovery of a huge impact crater on the edge of the Yucatán Peninsula in Mexico, modern opinion has centred on the impact of a fifteen-kilometre-wide asteroid as the cause. This impact resulted in the extinction of 75% of Earth's mammals, amphibians, and plants. Also the Cretaceous age ended and the Paleogene started.

But the death of the dinosaurs is only one of the many disasters that have befallen our planet. There was a 'Great Dying' about 251 million years ago, and a similar mass extinction about 201 million years ago. The latter again came with a geological age change, this time the Triassic switching to the Jurassic. Astronomers, hopefully being economical with their hypotheses (and remembering Ockham's Razor), turn to similar causes. An asteroid did it, or at least it contributed massively to contemporary plate-tectonic activity and associated volcanism. So there must be another huge crater somewhere. And in the book under review the town of Rochechouart in the Nouvelle-Aquitaine region of west-central France is the proposed location.

The big problem is recognition. Craters in the Earth's surface are being progressively erased as they age. After a mere 65 000 000 years the 180-km-diameter Chicxulub crater in Mexico was so hard to find that we did not stumble over it until 27 years ago. Just how difficult will it be to find a crater that is smaller and three times as old? It could be anywhere.

In this well-written, authoritative, and beautifully illustrated book Neil Taylor explains how what is left of the Rochechouart crater was discovered in 1969 by the French geologist François Kraut. Geological maps together with Bouger maps of gravity anomalies indicate that the crater was about 50 km in diameter. Its impact nature was confirmed by the discovery of local deposits of shocked quartz, lithic breccias, and suevite. Detailed $^{40}Ar/^{39}Ar$ isotopic measurements indicate that the crater formed around 201 ± 2 million years ago.

This delightful book is a superb introduction to the effects of an asteroid impact with Earth. It also is a great encouragement to visit this picturesque region of our European neighbour. I can hardly resist the suggested joys of wandering around the Église Saint Sauveur in Rochechouart and picking out the examples of the distinctly reddish Montoume suevites, the bluish-grey Chassenon suevite, and the brownish-grey local breccias in the church walls, followed by a coffee and pâtisserie in the local café on Place de Dr. Octave Marquet. — DAVID W. HUGHES.

Visually Observing Comets, by D. A. J. Seargent (Springer), 2017. Pp. 276, 20·5 × 12·5 cm. Price £19·50/$34·99 (paperback; ISBN 978 3 319 45434 4).

There is a panel on the front cover of this paperback which says, "Astronomers' pocket field guide". I suspect it would get tatty very quickly. Having said that, the book is full of very useful information for the dedicated comet hunter, not just for the comets that attain visual magnitudes, but for those that are no more than a smudge, barely visible through a small telescope against the stellar background. The writer discusses historical comet observing, speculations as to what they are, and how they formed and where they originate; is there a reservoir of comets out there? Is there still room for the visual comet observer in the digital age? The writer thinks so.

The book is full of information on observing and estimating the magnitudes of the coma, using different formulae to deduce as accurate an estimate as possible. The formation of ion and dust tails is discussed in some detail. The importance of accurate recording and reporting of your observations is stressed. Observing the return of short-period comets is given some prominence, with useful ephemerides and finder charts for some of the better-known ones.

An extensive list of short-period comets should ensure that any comet hunter is never wanting for an observing target. Any serious comet observer will want this book in their arsenal, but, rather than take it into the field, bone up on the bits one wants for that observing session and leave the book at home. Recommended. — Malcolm Gough.

OBITUARY

Donald Lynden-Bell (1935–2018)

Donald Lynden-Bell died peacefully at home on 2018 February 5. He suffered a stroke in December and spent some weeks in Addenbrookes Hospital in Cambridge. As anyone who ever met him will attest, Donald was something of an astronomical phenomenon: brilliant, insightful, generous, inspiring, never dull — a character succinctly captured in the by-line which accompanied the excellent obituary in *The Times* on February 9: “Titan of astrophysics with an impish sense of delight”.

He was born at Dover Castle where his soldier father was stationed at the time. In the course of his education at Marlborough College, and later in Cambridge at Clare College, Donald found outstanding teachers who nurtured his obvious mathematical talent and encouraged his wider interests in the natural world. Half way through his undergraduate course he was advised to switch from mathematics to natural sciences if he wanted to learn physics from real physicists, and this he did, attending lectures by many of the famous names who were then present in Cambridge: Fred Hoyle, Hermann Bondi, Otto Frisch, Paul Dirac, Martin Ryle, John Ziman, Nevill Mott, among others. As an undergraduate he thought deeply about Mach’s Principle and inertia, interests that were to endure. In the summer of 1958 he attended the vacation course at Herstmonceux, and it was here that Richard Woolley sparked his interest in stellar dynamics, studies which became the subject of his thesis completed in 1960.

With the award of a Harkness Fellowship, Donald went to the Mount Wilson and Palomar Observatories in California where he came into contact with some of the leading observational astronomers of the day: Maarten Schmidt, Jesse Greenstein, Allan Sandage, Olin Eggen and Fritz Zwicky. Woolley had suggested that Donald should collaborate with Sandage, and their work, together with Eggen, resulted in a seminal paper on the formation by collapse of the Milky Way galaxy, one of the first such analyses to be firmly based on observational data. Fellow UK post-doctoral students in California at that time included Wal Sargent, Roger Griffin, and Nick Woolf (more of whom below). It was also at that time that Donald married Ruth Truscott who joined him in California. Returning briefly to Cambridge *via* a six-week stay in Chicago to visit the legendary Chandrasekhar, they moved to Sussex: Donald to the Royal Greenwich Observatory (RGO) at Herstmonceux; Ruth to the Chemistry Department at the University of Sussex.

Donald's RGO years (1965 to 1972) saw his energy and enthusiasm in full flow (he himself said "much of my best work was done at Herstmonceux Castle") and it gave an enormous boost to the astrophysical life of the Observatory, further enhanced by the arrival of his three young PhD students Michael Penston, Brent Wilson, and Russell Cannon, themselves a welcome tonic. I was privileged to work for Donald for the first three years of his stay (before I was seconded to the Radcliffe Observatory, Pretoria in 1968) and it was an exciting ride! His principal interest was in the thermodynamics and evolution of self-gravitating gas spheres, for which I did the computing. But he collaborated with others studying spiral structure in galaxies, the relaxation of stellar systems, and radio emission from Jupiter. His other significant contribution at that time was to the infant Astronomy Centre at the University of Sussex, a theoretical, post-graduate department complementing the largely observational work at the RGO. He, Bernard Pagel and Richard Woolley shared the lecturing load with Sussex staff and 90 per cent of the first intake of students came from the RGO. Theorist Donald and observer Bernard taught each other much in their car journeys to and from work, and in the lunchtime walks in the castle grounds (or back and forth in the castle cloisters if it was raining!). It was during that time that Donald was an Editor of *The Observatory* (1967–1969).

His most widely recognized achievement was the 1969 suggestion that there is a massive black hole at the centres of all galaxies and that accretion onto black holes is the energy source of quasars. For this work he, together with Maarten Schmidt, received the inaugural Kavli Prize (which many consider only second in prestige to the Nobel Prize) for Astrophysics in 2008.

Donald returned to Cambridge as Professor of Astrophysics in 1972 and spent the rest of his working life there. His outstanding international reputation continued to grow as he applied himself to a broad range of problems in General Relativity, stellar dynamics, galaxy formation, quasars and relativistic jets, as well as novel telescope design. Despite his general dislike of administration, he became the first director of the new Institute of Astronomy and was instrumental in attracting Martin Rees back to Cambridge as Plumian Professor. He served as President of the Royal Astronomical Society and was the recipient of its Eddington Medal in 1984 and its prestigious Gold Medal in 1993. Other awards followed: he was elected to the Fellowship of the Royal Society in 1978 (Ruth became Professor of Chemistry at Cambridge and was herself elected in 2006) and was appointed CBE in the 2000 New Year's honours list. He also had an asteroid named after him.

In 2015 he, together with Roger Griffin, Nick Woolf and Wal Sargent (his companions in California 50 years before) were featured in a film called *Star Men* in which they retraced their steps from those early days and reminisced about the Universe, their lives, and their contributions to science. The film was widely distributed and is an excellent glimpse into the world of science.

Although astronomy has lost one of its brightest exponents, he leaves behind an impressive legacy and a legion of those inspired by his zest for life. There is a lot of material about Donald on the web — I particularly recommend his own account in *Annual Reviews of Astronomy and Astrophysics* (**48**, 1, 2010) entitled 'Searching for insight'. — ROGER WOOD.

Here and There

CARDINAL MISTAKE

If you follow the western (left side from the UK) side of the Great Square south, you'll arrive at the low and bright star Fomalhaut. — *Daily Telegraph*, 2017 September 4, Night Sky in September.

A VICTORIAN SPACE AGE?

2·8 miles — the width of the asteroid Florence that flew past the Earth at 4·4 million miles, the largest asteroid to pass so close to our planet since NASA started recording such occurrences in 1890. — *Time Magazine*, 2017 September 19, p. 9.

AN INEXPENSIVE SPACE OBSERVATORY

The observatory … is located on the Pastukhov mountain at a height of 2100 km. — *ROOM, The Space Journal*, No. 1, 11, 2017, p. 102.

EXTRAGALACTIC MIGRANTS?

Yet the brightest stars that are visible in the night sky don't necessarily belong to the Local Group. — 'Extreme Earth', *Reader's Digest (Australia)*, 2007, p. 21.