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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2020 February 14 at 16^{h} 00^{m}
in the Geological Society Lecture Theatre, Burlington House

EMMA BUNCE, *President Elect*
in the Chair

The Chair. Good afternoon, everybody, and welcome to the Open Meeting of the Royal Astronomical Society. I am very pleased to welcome a variety of speakers to entertain you this afternoon. First of all, we're going to hear from Dr. Philippe Escoubet from the European Space Agency, and the recipient of the 'G' Group Achievement Award. Philippe is going to talk to us about the 'The *Cluster* mission, the first mission probing the Sun–Earth connection in three dimensions'.

Dr. C. P. Escoubet. The ESA *Cluster* mission was the first mission to fly four identical satellites as a constellation. Its main goal is to study the Sun–Earth interaction in 3D. Since this lecture is to celebrate the RAS Group Achievement Award, special thanks are directed to the *Cluster* PIs, who have built and operated the 44 instruments, the science-operation team at RAL, which has coordinated the instrument operations, the spacecraft-operation team at ESOC, which is taking care of the spacecraft and challenges for operating during so many years, the archiving teams which give the community the best calibrated data possible, and finally the scientific community which published so many papers.

The *Cluster* spacecraft have the shape of a large flat cylinder, about 3 m in diameter and 1 m high. All 44 instruments are located on the top platforms and measure electromagnetic fields and waves and particles. To be able to measure very small electric fields, four very long wire booms, measuring 88 m tip-to-tip, were deployed on each spacecraft. To keep these wires straight the spacecraft makes a full rotation every 4 seconds. Sometimes we brought two spacecraft to within very small distances, down to 3 km, and Flight Dynamics at European Space Operations Centre (ESOC) had to be very careful to manoeuvre them with their cross-section of almost 100 m.

Cluster has been a very productive mission with 2814 refereed papers up to the end of 2019. The publication rate was rather slow at the beginning because people had to understand the data, and to reach an accuracy necessary to enable comparison of four spacecraft measurements involved a challenging calibration.

But from 2003 and then after 2006, with the opening of the *Cluster* active archive, the rate of publication has been above 160 refereed papers per year. If we add *Cluster* papers to those from *SOHO* (which forms with *Cluster* the first ESA cornerstone) the total at the end of 2019 is 8414 papers. If we compared it to the very successful second ESA cornerstone, *XMM–Newton*, this number is 37% higher.

Among the many *Cluster* discoveries during almost 20 years of operations, six of them were selected to highlight the various physical processes addressed by the constellation.

Shocks can be found around any astrophysical objects that produce supersonic winds — around nebulae (*e.g.*, the Cat's Eye, Orion) comprising concentric shells of gas and high-speed jets, young stars (LL Ori) that emit extremely fast solar winds, or supernovae (*e.g.*, 1987A) with ejecta propagation at speeds above 7000 km/s. At Earth, the bow shock is formed in front of the Earth's magnetic field, the magnetosphere, when it encounters the solar wind with typical speeds of 400 km/s. A big mystery involving shock reformation and non-stationarity was recently solved with *Cluster* when they detected an electron structure less than 3·4 km in size.

Plasma turbulence has also been a hot topic of *Cluster* investigations. In fluid dynamics, turbulence is a fluid motion characterized by chaotic changes in pressure and flow velocity. In plasmas, it is characterized by strong fluctuations of the magnetic fields (reaching 80% of its average value) and plasma parameters. New techniques (k-filtering, wave telescopes) have been developed to combine the four spacecraft measurements of turbulence. Such techniques could identify for the first time the various waves present in the shocked solar wind — mirror, Alfvén, and slow modes — that could not be identified with previous single-spacecraft measurements.

Magnetic reconnection is a process that allows two plasmas of different origin, for instance from the Sun and Earth, to couple and allow the exchange of mass, momentum, and energy. This process then leads to solar energy entering the Earth's magnetic field and, under strong conditions, producing geomagnetic storms. Many reconnection papers have been published using *Cluster* data and among them we can highlight the longest reconnection line (2·5-million kilometres) observed in the solar wind, the curvature of the magnetic field measured around the reconnection centre, the cusp spot produced by reconnection at high latitude, and magnetic nulls found at the heart of reconnection.

Kelvin–Helmholtz (K–H) waves are produced when two fluids or plasmas have different velocity. They are usually observed in clouds, the ocean, Saturn's bands, Jupiter's Red Spot, magnetosphere flanks, and the Sun's corona. Using four-point measurements, *Cluster* demonstrated that these waves can roll-up and then collapse. This roll-up can push solar-wind plasma into the magnetosphere and also twist the magnetic field to trigger magnetic reconnection.

Geomagnetic storms are one of the strongest manifestations of the coupling between the Sun and the Earth. They can affect power lines, pipelines, satellites, GPS signals, and communications. They are usually triggered by coronal mass ejections (CMEs), huge dense clouds of plasma ejected by the Sun from its active spots. In 2003 October and November five successive CMEs were ejected by the Sun in ten days, making this event one of the most active for many solar cycles. During this period the radiation belts, a ring of energetic particles around the Earth, disappeared and reformed much closer to the Earth with enhanced flux. *Cluster* showed that this fast acceleration of particles was due to very intense whistler waves, called chorus waves, analogous to the dawn chorus of birds.

Aurorae have been studied for many centuries based on the first observations from the ground. It was, however, with the first global images taken from satellites that their origin and behaviour could be investigated in greater detail. One mystery that lasted more than 30 years was the origin of a special aurora, the so-called transpolar arc or theta aurora. A theta aurora is observed when the auroral oval is crossed in the middle by an additional arc. A few years ago, *Cluster*, together with the *IMAGE* mission, demonstrated that in one event, the theta aurora was produced by electrons originating from the inner region of the tail of the magnetosphere.

Cluster will celebrate the 20th anniversary of its launch in 2020 September with a special symposium to be held at ESOC, Darmstadt, Germany.

The Chair. Thank you so much, Philippe, for a fascinating talk. So many results — what a wonderful mission. We have time for a few questions.

Dr. G. Hutchinson. What's the future for *Cluster*?

Dr. Escoubet. We are preparing a new extension to the mission. Two years ago we proposed the scientific case for a two-year extension, covering 2021/2022, and we are now looking for a confirmation. If nothing happens to the spacecraft or instruments we may get this confirmation up to the end of 2022, but we would like to propose also to work until the end of 2025. And the key aspect there is to collaborate with the new mission that will be launched soon — the *SMILE* mission — because *Cluster* will be very complementary to *SMILE* and also to other missions such as the NASA *MMS* and *THEMIS* missions. So we are preparing that proposal, and the results will be known in June of this year.

Professor Lyndsay Fletcher. You made, at the beginning, and actually throughout your talk, the strong point that these processes you are studying are of universal relevancy to astrophysics. So how do you see the *Cluster* results affecting the rest of astronomy and astrophysics?

Dr. Escoubet. We will know when we see where these papers are published. For instance, the key results on turbulence are published not in our usual journals such as *JGR*, *GRL*, or *Annales Geophysicae*, but they are published in more physical journals — *Physical Review Letters*, or *ApJ* — or wider-remit journals such as *Nature* or *Science*. I think all these papers on key physical processes are seen by many other people because they are published in these specific journals. I think this helps to reach a broader community than just the magnetospheric community.

The Chair. Any further questions? If not, thanks very much [applause].

I am delighted to welcome Professor Jo Bovy, from the University of Toronto, who is going to talk to us about 'The Milky Way in the era of large surveys'.

Professor J. Bovy. Studies of the Milky Way have played a central role in astrophysics for more than a century, but in the last decade and especially in the last few years, our understanding of the structure and evolution of the Milky Way has been revolutionized by large surveys. This is primarily the ESA *Gaia* mission, but also a wide range of other large surveys that include photometry, medium- and high-resolution spectroscopy, and asteroseismic observations for many stars. These surveys have led to an outpouring of new results on the formation, evolution, and dynamics of all of the Milky Way's stellar components: bulge, disc, and stellar halo. During a Specialist Discussion preceding this Ordinary Meeting that I organized with Daisuke Kawata (UCL/MSSL), Denis Erkal, and Payel Das (both University of Surrey), about three dozen experts gathered to discuss new results on the Milky Way that were obtained from the second data release from *Gaia* in 2018 April, and how best to prepare for the third *Gaia* data release scheduled for the autumn of 2020. This discussion for me highlighted again how the large amount of data that we now have access to is allowing us to study aspects of galactic dynamics and evolution that we scarcely believed to be possible just a few years ago. In this talk, I present a few highlights of my group's work on the Milky Way with large surveys, focussing both on new results and on novel methodologies for dealing with large data sets.

We live in a truly amazing era for Milky Way studies. Over the course of just a few decades, we have gone from having high-resolution spectroscopy, from which fundamental stellar parameters such as the temperature and surface gravity and detailed elemental abundances can be determined, for just a few dozen stars, to now more than half a million stars from surveys such as APOGEE and GALAH that cover a large volume of the Milky Way disc and nearby halo. Similarly, in astrometry, which measures parallaxes and proper motions, we have gone from having astrometry for about 100 000 stars with ~ 10 milli-arcsecond accuracy (which allowed detailed studies of a ~ 100-pc sphere around the Sun) to having more than one billion stars with ~ 10–100 micro-arcsecond accuracy from *Gaia*, allowing us to investigate the structure and dynamics of the disc and stellar halo over many kpc. Similar increases in data volumes have come from imaging surveys that provide broad-band photometry now for hundreds of millions of stars, and soon many billions with *LSST*, and in surveys of stellar light-curves such as *Kepler* and now *TESS*, which by using asteroseismology allows detailed information about stellar masses and ages to be obtained.

These large surveys are helping to answer some of the fundamental questions about our Galaxy's formation and evolution, such as: What is the physical state of stars and gas in the early and late history of the disc? What fraction of stars were born outside of the disc and when and in what form were they brought in by external galaxies? What is the chemo-dynamical structure of the inner, barred region and how did it form and co-evolve with the rest of the Milky Way? To tackle these questions with large surveys, we need novel data-analysis tools to get the most out of observational data, in particular: (*a*) fast machine-learning of observational data to process hundreds of thousands (now) to millions/billions of data points (soon), and (*b*) careful tracking of observational biasses in surveying stars (what you don't see is as important as what you do see).

One machine-learning method in particular has seen a meteoric rise in recent years: deep learning. Deep learning refers to the use of many-layered ('deep') feed-forward neural networks in machine-learning tasks. Essentially consisting of layers that perform linear transformations on their input data followed by a non-linear output function, they make up a class of highly-flexible function emulators that can be efficiently trained with large training data sets. In recent years, they have been applied in many contexts, from self-driving cars and novel drug discoveries, to the analysis of particle-physics experiments and data analysis in astrophysics, which I will discuss here.

We have been applying deep learning primarily to the problem of determining chemical abundances for different elements, luminosities, and ages from high-resolution spectra, focussing on the near-infrared *H*-band spectra from the APOGEE survey. Traditionally, high-resolution spectroscopic-data analysis has been somewhat of a dark art, with practitioners poring over individual spectra and by-hand analyzing individual lines. This was possible in the era of dozens of high-resolution spectra, but has become infeasible for the data sets consisting of hundreds of thousands of stars that we have today with APOGEE and GALAH. Deep learning can take a small set of stars analyzed using more traditional methods and use them as a training set to learn the relation between the spectrum of a star and its stellar parameters and elemental abundances. In doing so, it also allows for more precise abundance measurements from lower-quality spectra, where traditional methods fail to give good results.

But deep learning can go further than that: when we have data on properties of stars in the training set that could plausibly be determined from their spectra, but for which no good physical model exists, deep learning can use a training set to determine the effect of mapping of spectra to the property of interest without requiring a physical model. For example, we can now determine luminosities for many stars by combining *Gaia* parallaxes and photometry and thus can train a deep-learning neural network to predict stellar luminosity from stellar spectra using this training set (that there is luminosity information in stellar spectra of the red giants observed by APOGEE is plausible given that the mass dependence of internal mixing creates noticeable trends in the carbon and nitrogen abundances that are observable in the spectra; mass is strongly correlated with age for red giants). Using this approach, we have trained deep-learning neural networks that provide distances better than 10% out to about 10 kpc, which is much better precision than *Gaia* parallaxes on their own can give, and that provide ages for most stars in the APOGEE data set.

These high-precision abundances, distances, and ages for large numbers of stars covering a large swath of the Galactic disc and bulge bring the structure of our Galaxy in sharper relief. As an example, we looked at the structure of the inner Milky Way and in particular the Galactic bar. Originally discovered from the distribution of near-infrared emission in the inner Galaxy and from kinematics of gas flows there, the structure, formation, and evolution of the bar remains mysterious, as the large amount of interstellar extinction towards the central Galaxy makes it difficult to observe. APOGEE with its near-infrared wavelength region is able to penetrate much of the extinction. Our maps of the age and abundances (iron and oxygen) for the first time clearly reveal the bar's shape in these quantities, that is, bar-shaped patterns in the age and abundances are immediately apparent from our maps. These maps reveal that the bar is about 8 Gyr old and that it is metal-poor compared to the surrounding inner disc; we also see that it is enhanced in the relative abundance of oxygen to iron, indicating that the stars that make up the bar were formed in a short starburst in the first few billion years of our Galaxy's existence. We can also make maps of the motions of stars in the bar region, and these maps for the first time clearly reveal the expected motions in a barred gravitational potential; relatively simple modelling based on the continuity equation allows us to determine the speed at which the bar pattern rotates and we find that it has a period of 150-million years, which is fast considering the extent of the bar.

Directing our attention to the Galactic disc next, we can use the traditional technique of star counts that has been used since the earliest studies of the Milky Way's structure (then thought to be the entire Universe!). Using star counts refers to mapping the number of stars as a function of not just position in the Milky Way, but also as a function of velocity, chemical abundances, age, *etc*. These multi-dimensional star-count maps contain a rich amount of information about the physical processes at play in the formation and evolution of the Galactic disc. However, while it may seem straightforward to count stars as a function of observed parameters, in practice most surveys have seriously to down-sample the set of stars they could have observed (for example, APOGEE only observes about 500 000 giants among the ~ 1 billion giants in the Milky Way) and the down-sampling fraction is typically a complex function of position that needs to be taken into account when producing star-count maps or models from observational data. This situation is exacerbated by the fact that interstellar extinction due to dust is highly variable across the sky and can make it appear that a volume of the Galaxy has few stars, while in reality we simply cannot see them due to the large amount of extinction.

Luckily, the APOGEE survey has made a heroic effort to keep to a relatively simple selection algorithm for its main sample of stars that makes it possible to reconstruct the down-sampling biasses in detail as a function of position in the Galaxy, and we can combine this with models for the three-dimensional extinction to account properly for the effects of observational and extinction-induced biasses in star-count maps. Doing this, we have made various interesting discoveries about the structure of the Milky Way disc. Overall, the structure of the disc is a strong function of the elemental abundances of stars. Stars that are old, metal-poor, and enhanced in oxygen relative to hydrogen compared to the Sun are highly centrally concentrated and form a vertically-thick distribution, while younger, more metal-rich stars occupy a set of doughnuts with radius set by their iron abundance (*e.g.*, stars with the same iron abundance as the Sun live in a doughnut with a radius similar to the Sun's distance from the Galactic centre) and they are thinner. This points to a scenario where the early evolution of the Milky Way disc was characterized by a turbulent interstellar medium with strong mixing throughout the disc, while during the later evolution a stratified thin disc forms with chemical evolution occurring mainly in rings (the 'doughnuts'). The evolution of the Milky Way disc appears overall to have been only very minimally affected by outside influences (such as mergers with or perturbations from satellite galaxies). The disc's evolution is largely driven by internal processes.

We can apply similar star-count methods to investigating the structure of the stellar halo with APOGEE. Doing this, we find that the stellar halo in total contains about 3% of all the stars in the Milky Way, but yet it looms large in studies of the Milky Way because it is the repository of ancient merger events that are important in the hierarchical cosmological structure-formation paradigm. Separating the halo into stars likely born within the disc and those likely accreted by mergers, we find that 25% come from the disc, while 75% are accreted; these accreted stars are an amalgamation of multiple systems, rather than a single large system. This is consistent with the quiescent history inferred from the observations of the structure of the Galactic disc.

The data explosion in Milky Way studies will continue in the next decade, with millions more high-resolution spectra coming from surveys such as SDSS-V's *Milky Way Mapper*, billions of low-resolution spectra coming in the third *Gaia* data release, and photometry and light-curves for billions of stars from *LSST*. This will allow us to build an even finer-grained picture of the chemo-dynamical structure of all of our Galaxy's components, provided that we can develop methods to deal with these enormous data sets in a responsible fashion, taking observational uncertainties and biasses into account.

The Chair. Thanks very much for that very interesting presentation. We have time for a couple of quick questions from the audience.

Dr. Chiaki Kobayashi. In the middle of your talk you showed the metallicity, compared to iron, of the bar area. In classical observations with limited samples, I thought the central part of the Milky Way was most metal rich.

Professor Bovy. Yes, that is a good point. The metallicity in the inner Galaxy depends strongly on where you're looking. There is a more metal-rich part — thus, if you're looking outside the bar, but still in the inner part of the Galaxy, then stars are more metal rich and as you go to larger heights above the Galactic mid-plane, like in Baade's Window, it might also be more metal rich. Our findings are very specifically that it is the bar that is metal poor. It is a bit of a puzzling observation, but it does seem to agree with at least some numerical simulations. It is also very hard to see how our results could be wrong: we've tried pushing things to see what we would need to do to make this different, but the observations do not change. It just comes out beautifully that the bar looks like that. But if you just look in a different part of the sky, you might not see this very low-metallicity part of the bulge.

Reverend G. Barber. Early on, you say what you don't see is as important as much as what you do see. With deep-learning techniques, how do you guarantee with the training program that you're not building in your own biasses in to the system, because you don't know what's there.

Professor Bovy. Thank you — that is a great point to make. I didn't really touch upon that. We try to impart our physical knowledge in the construction of the neural networks that we use, as much as we can. For example, when we look at the spectra to determine the abundances of different chemical elements, we don't just take the entire spectrum and say "What is the iron abundance?"; instead, we actually only allow the network to use parts of the spectrum where we know that there are iron features. Similarly, for oxygen, we only use the part of the spectrum where there are oxygen features. And so on for other elements. That way we are trying to make sure that we only use parts of the spectrum where there should be physical information, where there is some sort of plausible physical connection between the spectrum and the abundances, and we're simply using the deep-learning neural networks as a way to build a flexible model. However, with the ages, for example, we don't do that — there we just take the whole spectrum. So how the network determines the age, we don't entirely understand. We're always worried that we're actually using correlations rather than true physical relations there. For this, we test and try to check that things come out independently and that there aren't obvious correlations that we've used. But in general, we try, as much as possible, to use our own physical knowledge as we construct these networks to get around that issue.

The Chair. Thanks very much, I think we'd better move on to our next talk, but thank you again. [Applause.]

I'm very pleased to welcome to Dr. Jasmine Sandhu from MSSL. She is going talk to us about 'Outreach — a postdoc's perspective'.

Dr. Jasmine Sandhu. In this talk I will discuss my personal experience of outreach, to open a conversation around the experience that early-career researchers encounter in our community.

My research explores the terrestrial magnetosphere, which is the region in our near-space environment inhabited by our global geomagnetic field and filled with plasma originating from our ionosphere. The magnetosphere is very strongly coupled to the solar wind. This coupling leads to a highly dynamic environment, and controls how energy and plasma is transported throughout the system. During periods where the coupling is strong, the magnetosphere becomes very active and geomagnetic storms occur. Due to the various different sources of plasma (solar wind and ionosphere), the inner magnetosphere is host to multiple coexisting plasma populations. We can differentiate between these populations according to their characteristic energies. The very-low-energy plasma forms a region of cold, dense plasma around the Earth called the plasmasphere. At higher energies we have the ring-current population, and intensifications in the ring current generate global geomagnetic-field distortions.

At the highest energies we have the radiation belts, which can be accelerated to hazardous levels during storm times. Understanding and predicting radiation-belt dynamics is a key goal of the community. However, these coexisting populations are interdependent and they shape one another through a variety of different wave–particle interactions. Therefore, to be able to understand the radiation belts we need to understand all populations in a cohesive way.

Alongside research activities, I also participate in outreach and public engagement. My experience mainly focusses on school students and children. For example, I participated in a magnetometer workshop with Year-5 students, where we constructed a magnetometer and spoke about magnetism in the context of magnetospheric science. I also lead rocket workshops with students ranging from 6–18 years old, which is run alongside discussions about the aurora. Although these workshops are relatively limited in the number of people engaged, they allow for a relatively long period of engagement with the same group of people. In contrast, larger events such as science fairs allow us to reach much broader audiences, spanning different age groups and backgrounds, but with shorter time-scales for engagement with a given person. Overall, the range of outreach activities, the formats, and the groups who we engage with can be very diverse and wide ranging.

The type of outreach that we participate in and the audiences that we engage with will be largely shaped by our own personal motivations. Personally my primary motivation comes from a desire to change and shape our community for the better. Countless studies have shown that certain demographics are severely under-represented in our research community. Outreach presents a route to encourage people to study physics and join our community in the long run. Research shows that under-representation starts early on, with the Institute of Physics (IoP) reporting that only 22% of UK physics A-level entrants are female. This under-representation continues with career stage: 21% of first-year physics undergraduates were female (IoP, 2012) and 21% of UK professorial positions in Solar System science were held by females in 2017 (RAS, 2018). This is also reflected in other demographics: 11% of first-year physics undergraduates are from BAME (Black, Asian, and Minority Ethnic) groups (IoP, 2012), and only 5% of permanent UK staff positions are held by those from BAME groups. We can use outreach to encourage, inspire, and equip students from all demographics and backgrounds to engage with physics and consider it as a career choice for them. However, in order to retain representation, it is also important to combine these outreach activities with community activities. Research shows that those from certain demographics can encounter a very different research experience than others. A report into the experience of black female professors in the UK reported that "a culture of explicit and passive bullying persists across higher education along with racial stereotyping and racial microagressions" (University and College Union, 2019). A report by Wellcome also shows that certain demographics are more likely to fall victim to bullying and harassment than others. So it is vital that, as well as encouraging those from under-represented demographics into research, we need to ensure that our community is a place that welcomes diversity and is able to retain and support representation. I've spoken a lot about representation but there are also other motivations to participating in outreach. For example, it provides opportunities to develop communication skills. I often have to describe what I do and why it is important using very clear and concise language whilst also keeping it engaging.

Now I am going to talk about the main focus of my current outreach activities.

ORBYTS is a scheme led by University College London (UCL) researchers that brings together PhD and postdoctoral researchers with GCSE and A-Level students, to collaborate on real current research projects. ORBYTS addresses diversity issues by focussing on good representation of various demographics. The interaction also helps to dispel harmful misconceptions and stereotypes that the students may have about scientists by providing them with real and relatable role models. Finally, the scheme provides opportunities for the researchers to develop transferable skills (*e.g.*, communication, leadership, and people management). The ORBYTS scheme has currently worked with 24 schools, reaching about 250 students in total. The researchers are from UCL, Mullard Space Science Laboratory (MSSL), and Lancaster University. The projects that are being undertaken are widely varying, including topics such as magnetospheric science and exoplanetary research to name a few. ORBYTS notes that they are close to diversity targets for some demographics, but they are focussing on increasing representation for others (*e.g.*, disabilities and learning difficulties).

Andy Smith (MSSL) and I are leading a project that explores storm-time variations in the plasmasphere. Observations from the *IMAGE* spacecraft show that following a geomagnetic storm, the plasmasphere is highly distorted and its boundary, 'the plasmapause', moves significantly inwards. We aim to understand how the plasmapause changes statistically during geomagnetic storms. We can investigate this using *in-situ* observations from the Van Allen Probes. The students use PYTHON coding skills to plot time-series of plasma density and identify plasmapause crossings. Using these identifications, they explore how the boundary location and thickness varies during storms.

Some preliminary results from the students' analysis is shown, corresponding to the St. Patrick's Day 2015 storm. Magnetometer indices show global distortions in the geomagnetic field, characteristic of a storm. Observations of the radial location and thickness of the plasmapause are shown in the recovery phase of the storm. The results show that following the storm main phase the plasmapause is located at relatively low radial distances of 3 to 4 Earth radii. Observations like these will allow us to understand storm-time changes in the plasmapause.

Running the project has highlighted several key benefits for the students, namely coding, data analysis, and communication skills. They will also have the opportunity to present their work at an upcoming ORBYTS conference this spring. And finally, they have had the opportunity to work with a range of different scientists. As well as working with me and Andy every session, they had a Skype session with Lauren Blum (NASA/GSFC) who is leading a new inner-magnetospheric space mission. This provides insight into the wide range of careers available, as well as different career paths that researchers can take.

I would like to highlight that the outreach work I do is part of the huge breadth of activities within the MIST (Magnetosphere Ionosphere Solar Terrestrial) community. Overall, my experience of outreach has been very positive. However, the major challenge is time management. In an environment where we are often judged simply on numbers of publications, I sometimes feel that anything that takes time away from my core research activities is setting my career back. However, working with ORBYTS has brought a new perspective on this. The project is strongly aligned with my own research goals and interests, and we are producing new results that will lead to scientific publications. Perhaps projects that bring research and outreach closer together can help address these issues. Finally, I think that to move forward as a community we need to understand the common challenges faced by those who participate in outreach. And perhaps we need to change how we see and treat outreach. It's often viewed as an 'extracurricular' activity, and perhaps our research culture needs to change to acknowledge the benefits that outreach can have for both our community and our research.

The Chair. Thank you very much, Jasmine, that was a really engaging and very clear talk, so any comments or questions from the audience?

Dr. G. Q. G. Stanley. I think you hit it right on, very engaging indeed. I think the beauty of that is the fact that young people see the relationship between the aurora and how it plays, which comes from the bigger picture, so it really does spark an interest in people. I was just wondering, being faced with so much data, how are you picking the right sort of areas so that students who look at this for the first time aren't scared, and start to engage rather than being daunted by too much.

Dr. Sandhu. The Van Allen Probes provide data that I'm already familiar with, and we've already got lists of when storm times are happening; we've rather pre-filtered the data for them, so it's not too much. And we guide them a lot so they know what features they're looking for, so that helps. We don't just throw all the data at them at once and leave them to it. We meet with them every week and the idea of them presenting their results regularly makes sure we're all on the same track and no one is going down the wrong path.

Dr. Stanley. It's nice to know that even if they don't go into further research or anything like that, that they actually understand what's around them and that is a key issue; it's enhancing, it's very good, well done.

Ms. Gail Campbell. Thank you for your very engaging talk, I thoroughly enjoyed it. When you were talking about who you covered, you said small workshops and big gatherings like science fairs, freshers fairs, *et cetera*. Who puts that together for you? Because there is a lot of work needed to establish who it is you're going to cover as well as their appropriate level of scientific expertise. That's a job in itself, isn't it?

Dr. Sandhu. Yes, I work at MSSL and we get a lot of people approach us, and usually it is PhD students and post-docs who take on the task of organizing that when we want to have a group come and visit us, so, for example, we have Scouts and Guides and groups like that who want to come and visit us or they'll invite us to go and give talks. The ORBYTS project is very big and I think it's a huge time commitment and there is a team of people, mainly Will Dunn who leads in organizing that; but yes, it is a huge time commitment and it takes a huge amount of work.

Professor S. Miller. Yesterday I was taking a group of young people who work with the Prince's Trust around the Royal Observatory in Greenwich. Now these are young people with very low social capital; they used to be called NEETs, Not in Employment, Education or Training. I just wondered how your project-based approach could be tailored to people who really know almost nothing, where you're actually just trying to get them some contact with the worlds of astronomy, geophysics, and so on.

Dr. Sandhu. ORBYTS actually does that quite well because they specifically focus on schools where the students don't typically go to university or pursue further education. So I think targeting your outreach activities to those groups is a good way to involve people who don't normally engage with science. I don't know if that answers your question.

Professor Miller. A lot of these people are not in school: they are either excluded, or they kept themselves out of school and so on. They have incredibly tough lives. It's something to think about.

Dr. Sandhu. Yes.

The Chair. Any further questions?

Dr. A. Chapman. The point, I think, is a very relevant one, regarding outreach. I'm also involved with this, and with astronomical societies and things like that, but it goes right across the educational board. You have people not only ignorant in science but ignorant in all sorts of subjects. People who are brought up in communities where it's assumed that they are 'no hopers', that they will either sign on, work for Deliveroo, or something like that — that is where we need to work. This is especially the case in my own native Lancashire where lots of kids are literally on the scrap heap by eighteen or nineteen, and there is no factory work for them. I think what is essential is actually to have an increase of general-education access right across the board, and I say that with personal experience, because I was a kid like that, destined originally for the factory or the pit. That was a long time ago, it's true, but I'm sure you see I resisted it. I do what I can to encourage other boys and girls to do the same, but we need this outreach, especially in poorer communities. It is an excellent job that you are doing, thank you.

Dr. Sandhu. I just would like to say that we get so many requests for doing outreach but really we need to think about how we use our time, because we must make sure the outreach that we do is really going to target those people whom it is going to benefit the most.

The Chair. Thanks again Jasmine, that was excellent [applause].

Our final speaker this afternoon is Dr. Elisa Chisari from Utrecht University, and this is the RAS Fellowship talk. Elisa is going to talk to us about 'Galaxy shapes as a tool for cosmology and galaxy evolution'.

Dr. Elisa Chisari. The successful analysis of the cosmic microwave background radiation in recent years has given us a very precise picture of the composition of the Universe. Only about 5% of energy density of the Universe is comprised of matter we can see and touch. The other 95% is in the form of dark components. Dark matter (~ 25%) is thought to be responsible for generating sufficient clustering of matter in the Universe to enable the formation of galaxies. Dark energy (~ 70%) is a mysterious phenomenon thought to be responsible for the accelerated expansion of the Universe. Understanding the physical nature of these dark components is one of the key goals of on-going cosmological surveys. Current efforts in cosmology are also directed towards elucidating the physics of the early Universe: was there an early phase of accelerated expansion? Is inflation the right model for this phase? Which physical mechanism drove inflation? These are the open questions in high-precision cosmology and answering them requires extracting information from the 'large-scale structure' of matter in the Universe *via* dedicated galaxy surveys.

One of the physical phenomena that allow us to probe the cosmological model is 'gravitational lensing'. Photons arriving at our telescopes from a distant source have their paths distorted by the curvature of space–time. As a consequence, the shapes of galaxies are 'lensed' by the large-scale structure in a weak but coherent fashion. Measuring and modelling such lensing patterns enables us to probe the nature of dark energy and to test General Relativity at large scales. Today, several campaigns are dedicated to performing gravitational-lensing measurements, such as recent analyses of the Kilo-Degree Survey, particularly constraining the energy density in matter in the Universe and the amplitude of density fluctuations. Comparing these constraints to those from early-Universe probes, such as the cosmic microwave background, is necessary to search for departures from the Λ-CDM cosmological model. At the moment, deviations between best-fit results from gravitational lensing of galaxies, and the cosmic microwave background, are below 3σ approximately.

Several surveys will focus their efforts in weak lensing in the next decade. One of them, the Legacy Survey of Space and Time (LSST) at the *Vera Rubin Observatory*, has the potential to constrain the parameter space of the equation of state of dark energy (if described by a dynamical field) to 10–20% precision. This will be achieved by the analysis of a deep and wide data set over approximately half of the sky, which will combine gravitational lensing with other large-scale-structure probes. To prepare for this survey, a number of efforts are being undertaken. Among them, the need for a homogeneous theoretical prediction software to model all probes consistently and to a well-defined accuracy was identified as a priority of the Dark Energy Science Collaboration. This has been the focus of my efforts during this RAS Fellowship, and it has also become a publicly available tool for the community.

Among the ground work needed to ensure the success of the next generation of lensing campaigns is the quest to model the large-scale structure to a controlled degree of accuracy. Gravitational lensing is not the only phenomenon that distorts galaxy shapes in a coherent fashion throughout the Universe. Shapes can also be sensitive to the formation history of galaxies embedded in their large-scale environment. 'Intrinsic' shape correlations have been found to be present at a very significant level at low redshift, where the lensing effect is negligible. Thus, these 'intrinsic alignments' play a role as lensing contaminants.

There are several efforts being made to improve our understanding of this phenomenon. Among them, new observational constraints derived from the Kilo-Degree Survey. These suggest that red galaxies tend to be more prone to align with each other, while blue galaxies are randomly orientated within current error bars. This is consistent with theoretical expectations, which suggest that red galaxies stretch along preferred directions set by the tidal field of the large-scale structure, while blue (mostly disc) galaxies have naturally smaller alignments that arise from tidal torquing of their angular momenta. These advances in our constraining power help us gain better control on the level of contamination of intrinsic alignments to future cosmological constraints from gravitational lensing.

Another possibility for gaining knowledge about the nature of these alignments is the use of cosmological simulations. There are several results obtained from this line of research, which are in agreement with the trends already mentioned. The simulations further allow for making predictions in the regime that current observations cannot reach and which are more representative of future surveys.

Finally, advances are needed in the analytical modelling of this phenomenon, as tools available at the moment rely on a simple linear proportionality of shapes and the projected tidal field of the large-scale structure. In a recent work, my collaborators and I proposed a perturbative approach to describing intrinsic alignments (based on effective field theory) which we aim to test with simulations and existing data in the near future.

Intrinsic alignments are widely regarded as a lensing contaminant. However, given that the phenomenon is measured to high significance, and that the linear-tidal-alignment model provides a good description in the regime where alignments are weak, this suggests that it might be possible to extract useful information from it. There are two possible avenues of exploration. On the one hand, if alignments are truly a consequence of tidal stretching, as current observations suggest, the large-scale alignment of galaxies could provide information about the early Universe. The potential application of this observable could be further improved by the fact that the outskirts of galaxies are more aligned than the inner regions. The combination of two measurements of galaxy shapes would result in potentially better constraints on the inflationary model. On the other hand, alignments can be sensitive to physical processes in galaxy evolution. One example, derived from cosmological simulations predictions, is the fact that feedback from active galactic nuclei (AGN) yields a more prominent red population of galaxies, and thus stronger alignments in the Universe.

It is an exciting time for weak-lensing cosmology. New experiments starting as early as 2021 will deliver stringent constraints on the cosmological model, particularly on the nature of dark energy. This perspective also presents us with the challenge of ensuring models are accurate enough to describe the large-scale-structure data. Intrinsic alignments are one of these challenges, but they should also be regarded as an opportunity for novel studies of galaxy evolution and cosmology.

The Chair. Thank you very much, Elisa; we have time for a couple of questions.

Reverend Barber. Thank you for a very interesting talk. Just a point of information — when you talk about an elliptical galaxy, are you talking about a spiral galaxy that is seen at an angle, inclined, or an actual elliptical?

Dr. Chisari. No, we are only talking about an actual elliptical. We think that the spirals, regardless of how we see them, whether face-on or edge-on, are subject to a different mechanism from the one that aligns elliptical galaxies. We're also always thinking about red galaxies as being ellipticals and discs as being blue galaxies, as we do not have enough data to make that distinction, but we think there should be a distinction to be made.

Dr. Stanley. I'd like to refer back to Professor Bovy's talk earlier where he was saying that the Milky Way had lots of little galaxies in there. Are you seeing that in those bubbles — can you see these small events of mergers, or interactions between galaxies in your model?

Dr. Chisari. That is a very interesting question. Indeed, we know from simulations that the more mergers a galaxy has, the more aligned it is. So in principle, there is a way in which you could connect the strength of alignment that you see to the number of mergers. We're looking into trying to make that statistical connection.

Dr. Stanley. Is that due to the flow of those galaxies, those mergers, causing the alignment, do you think? In that network?

Dr. Chisari. Well, we don't know exactly, because we don't have the time resolution in the simulations necessary to perform that study. The only thing that we know is that the number of mergers determine the amplitude of the alignment. It could also be because they come from particular directions that correlate with the tidal field.

The Chair. It is fantastic to see how productive you've been during your RAS Fellowship, so thank you very much for telling us all about that today, and we wish you all the best for your new position.

Now to close the meeting and to remind you that we have the drinks reception in the RAS library immediately following; and finally I give notice that the next monthly A&G Open Meeting of the Society will be on Friday the 13th of March, so I hope to see you all then.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2020 March 13 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

EMMA BUNCE, *President Elect*
in the Chair

The Chair. First of all, before we begin with our speakers for this afternoon, I just have some announcements. I'd like to announce the first winners from the new RAS GCSE poster competition which has been sponsored by Winton. We have some of the winners here this afternoon so I am going to ask them to come up in a moment. We have three prizes to give. Third place goes to Hannah Mohammed, from Guildford High School for Girls, who is unable to attend because she is on a gap year in India, which is very exciting; and her poster title was 'The Imaging of a Black Hole by the Event Horizon Telescope'. The second place goes to Naomi McWilliam, from Watford Grammar School for Girls, for the poster entitled 'How Strange Are Neutron Stars?' [Applause.] Congratulations Naomi. And the first prize goes to Yana Imykshenova, from Nonsuch High School for Girls, for the poster entitled 'The Cosmic Web: Filaments and Voids.' Congratulations. [Applause.]

Well done. I hope some of you managed to have a look at those posters on display during tea, and congratulations to the students for their excellent efforts. The second announcement I have is to call for applications for the Caroline Herschel Prize Lectureship, and the deadline is the 30th of April for that. So please have a look at the William Herschel Society website for the details of that; I would encourage you to nominate appropriate people for it.

The next item is the AGM: the 200th Annual General Meeting of the Society will take place here at 4 o'clock on Friday the 22nd of May 2020.

The last announcement is to do with the honorary auditors. We are fast approaching the time of year for us to produce the annual report and accounts and prepare for the AGM. You will recall that we appoint two Fellows who are not members of Council to be honorary auditors who are directed by the bylaws to deliver a personal report on the resources, goals, structure, activities, conduct, and general health of the society, but not matters relating to finance, law, or personnel, and they report to the AGM. This year we have Dr. Natasha Stephen and Professor Isobel Hook to undertake this role, and they would welcome any input from the Fellowship. If you need their email addresses please contact Philip Diamond, the Executive Director.

And so to our programme for this afternoon. We've got another excellent programme of speakers, and we're going to start with Dr. Peter Wyper from Durham, who is going to give us the RAS Fellowship Lecture, and will be talking to us about 'Simulations of eruptions from the Sun's corona: what can we learn?'

Dr. P. Wyper. The Sun and its activity have fascinated astronomers for hundreds of years. Satellite observations in extreme ultraviolet (EUV) and X-rays show that the solar corona (the Sun's hot outer atmosphere) is highly structured and often very dynamic. They also show a myriad of different rapidly appearing bright features (indicating energy released impulsively as radiation) from tiny, barely observable, bright points to the spectacular loops that form during the largest solar flares. Many of these energy-release events involve an eruption of coronal plasma but most are not well understood. For the purpose of this presentation I'll break solar eruptions down into four broad observational categories.

(*i*) Eruptions from active regions: originating from the atmosphere above Sun spots, these are the most energetic eruptions from the Sun and typically produce broad, bubble-shaped expulsions of plasma into the heliosphere known as coronal mass ejections (CMEs). (*ii*) Quiescent filament eruptions: eruptions of material from structures that form between or away from active regions. These eruptions are also highly energetic and produce a mixture of bubble-shaped and longer, narrower (known as jet-like) CMEs. (*iii*) Confined eruptions: eruptions of material in the low atmosphere that begin to erupt and then get trapped (and confined) within the solar atmosphere. Broadly speaking, these are usually mid-range in size and energy although some can be very energetic. (*iv*) Coronal jets: currently the smallest known eruptions on the Sun. Some 10 000 times less energetic than a typical active-region flare and CME, these events occur frequently in the low corona and are observed all across the solar surface. Jet eruptions lead to long, tapered ejections (jets) of plasma that shoot up from near the solar surface.

Until recently jets were thought to be separate entities that didn't involve the eruption of a plasma structure, but rather were formed by a simpler burst of energy release. However, around the time my fellowship started this generally accepted picture was being overturned by observations which revealed tiny erupting structures at the base of many jets. Simulating and understanding these tiny eruptions became my main focus, which in turn helped to tie the four broad categories of eruption above together (at least in my mind!).

All eruptions from the Sun form as a result of a rapid reconfiguration of the coronal magnetic field, which entirely dominates the behaviour of the coronal plasma. And all eruptions share a common pre-eruption magnetic structure known as a filament channel. Often these magnetic structures are filled with cool, dense plasma which are observed as dark snake-like filaments in EUV. Filament channels snake across the solar surface and exist in a force balance between the high magnetic pressure within the channel that tries to make the channel expand upwards, and the downward tension of the magnetic field (known as the strapping field) that forms arch-like coronal loops that reach over the top of these channels and hold them down. All solar eruptions are essentially the result of the catastrophic loss of this force balance.

Different theoretical models appeal to different methods of disrupting this force balance. The one I will focus on here is known as 'Magnetic Breakout' which was originally developed to explain eruptions from active regions and their bubble-shaped CMEs. In the breakout picture somewhere high above a filament channel and the coronal loops/strapping field immediately above it there are other coronal loops with a magnetic field of the opposite orientation. The higher loops meet the lower loops where the field forms an X-shape, known as a null point since at the centre of the X the field strength is zero. At the null point magnetic reconnection of the oppositely directed higher field and the lower strapping field can occur, which weakens the downward tension on the filament channel. This in turn lets the filament rise, which feeds back on the reconnection speeding it up and creating a feedback loop that ultimately leads to the eruption of the filament channel.

Simulations of breakout well describe certain active-region eruptions and reproduce the bubble-like shape of their CMEs. The question we asked ourselves was, if tiny eruptions seem to be involved in jets, could a similar process be driving them also?

A key ingredient of the breakout model is the presence of a null point above the filament channel. In the solar community it is actually rather contentious whether nulls exist above active-region-filament channels and if so whether they are involved to any great extent in the eruption. However, for jets there is no question. Essentially all jets are observed above a magnetic field with a null point. There are two key differences though. The first is that rather than the oppositely directed field being formed by arch-like loops high above the filament channel it is formed by magnetic field that is 'open', that is, it is stretches upwards and doesn't locally connect back to the solar surface. The second is that the tiny scale of the jets is such that the magnetic-field strength of this oppositely directed field doesn't drop significantly with height on the scale of the jet-producing region (the jet is small compared to the field's scale height). In the case of active-region CMEs there is a significant drop in field strength which is why bubble-like CMEs expand so much as they erupt.

I set up a simulation with these two key differences included and found that it did indeed produce a jet, and in fact many aspects of the simulation matched observations of these tiny jet-producing eruptions. Analogous to magnetic-breakout-generated active-region eruptions, reconnection at the null point weakens the strapping field and sets off a runaway expansion of the filament channel. However, since the local field strength doesn't drop in height, significant expansion of the filament channel was inhibited in contrast to large-scale eruptions. The upshot of this was that rather than producing a bubble-like expulsion, the erupting filament channel eventually reaches the null and is itself reconnected. This launches the erupting material along the 'open' surrounding field, which stretches upwards to form a jet.

We now have a working (and at this point reasonably well-tested) simulation model that shows that the eruption in jets is triggered by the same breakout feedback mechanism that triggers some large-scale active-region eruptions. This is quite a powerful result. Remember that these events are vastly different in scale and energy, with jets at the smallest and active-region eruptions at the largest ends of the scale of solar eruptive activity. In between is an entire continuum of eruptions that we are now in a position to understand better using these results.

Shown in Fig. 1 are schematics of four scenarios based on the four combinations of the two key differences studied in the jet-simulation model. The top row shows the jet scenario with open surrounding field and a small filament-channel size *versus* field-strength scale height. Plasma in the erupting filament channel is shaded grey. The second row also has a small source region, but the surrounding field is now closed. The third row has closed surrounding field but a large filament channel *versus* the scale height. Finally, the bottom row has a large filament channel but the surrounding field is now open. In all cases an eruption can be triggered by reconnection at the null point (middle column), but the ultimate eruption behaviour is quite different (right column).

Let's return to the four broad classes of observed eruptions I introduced earlier. As discussed already, jet eruptions are well described by the top row and some active-region eruptions are well described by the third row. The mixture of bubble-like and jet-like CMEs produced by quiescent filament eruptions could broadly be explained by some having a configuration like the third row (giving bubble-shaped CMEs) and others having configurations like the bottom row (jet-like CMEs). Certainly, jet-like CMEs are observed to originate from configurations like that shown in the bottom row (known as a pseudostreamer) which is encouraging. Finally, confined eruptions are often quite messy events but at least a sub-set of them involve a null point and a confined jet redirected back to the surface along coronal loops as in the second row.

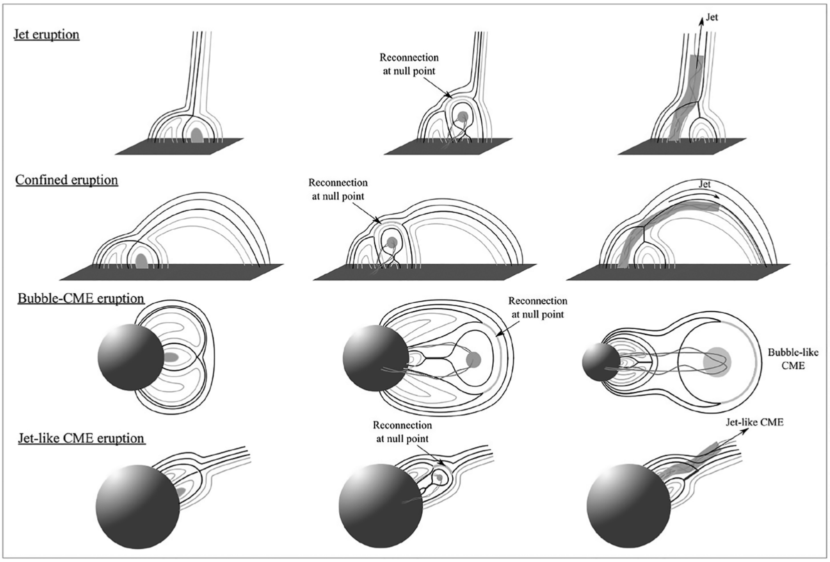

FIG. 1

Schematic of the four theoretical eruption scenarios. Left: the pre-eruptive state with plasma in the filament channel shaded in grey beneath the X-shaped field of a null point. Middle: reconnection at the null facilitates the eruptions. Right: the different eruption morphologies produced.

Obviously, this schematic is high level and by its nature includes a whole host of different kinds of eruptive activity. Within each different eruption category there are plenty of observations that don't conform to this picture and in the case of active-region eruptions it may well be that eruptions involving nulls are the exception rather than the rule. There are also other eruption mechanisms that may be more dominant or that couple to the reconnection at the null point, and this is likely to be situation-dependent. There is a great deal of work still to be done to nail down the specifics. However, theoretically at least, simulations are showing us that a clear link can be drawn between eruptions of different kinds and of vastly different scales in the solar atmosphere, helping us better understand and indeed predict them in future.

The Chair. Thank you so much — it's really great to hear about the work you've done during your RAS Fellowship. We have time for a couple of questions.

Professor T. Horbury. Peter, your jets show a distinct magnetic-helicity signature. Do all of your other events show a similar helicity signature, so is that a universal characteristic of your mechanism in all the different scale sizes?

Dr. Wyper. Good question. I think they should. I haven't looked at it. Assuming the chirality of the filament is the same in the eruptions, yes it should do.

Professor Horbury. As you know, we are always looking for observational signatures to test things, so helicity is an obvious one to me.

Dr. Wyper. Indeed. I will get on to calculating that.

The Chair. Any other questions? Just a quick question from me then. How are the new missions that we're seeing now going to help you progress your ideas about these particular features? What do you need?

Dr. Wyper. I'm actually spread in what I look at. I don't just do eruptions. I was at the discussion meeting today, actually, looking at the solar wind which is what primarily the newest missions are looking at. So we're attacking that in a different way. But, set aside just the features of the wind itself, which is incredibly interesting, considering the eruptions, I would love to see a coronal mass ejection sweep over the top of the *Parker Probe* and get some *in-situ* measurement of electrons and what it is connected to, and see the fields changing directions, because we don't know nearly enough about the inner workings of one of these eruptions nearer the Sun.

The Chair. Thank you very much again for your wonderful presentation. [Applause.]

Our next speaker then is Dr. Timothy Craig from Leeds who is the recipient of the Winton 'G' Award. Timothy is going to be talking to us about 'Bending and breaking subducting tectonic plates'.

Dr. T. Craig. In this talk, I will be focussing on a specific type of earthquake — 'intraplate' events, that occur within subducting oceanic plates. Subduction is the process whereby tectonic plates descend into the Earth's interior, as part of the large-scale heat transfer of the Earth system. At subduction zones — convergent plate boundaries, such as Japan, Tonga, or Chile — two tectonic plates come together; one bends and slides under the other, and then descends down into the Earth interior. The process of the two plates sliding past each other generates the vast majority of seismic activity that we see on Earth, most notably the most devastating events, such as the 2004 Sumatra–Andaman earthquake, and the 2011 Tohoku–Oki earthquake. However, whilst these so-called 'megathrust' events make up the majority of seismic-moment release, and have the potential to be devastating, both in their own right, and through triggered secondary hazards such as tsunamis, they are not the only source of seismicity associated with subduction.

Along with earthquakes that occur at the interface between plates, we also see earthquakes that occur within plates, resulting from the internal deformation of the subducting plate as it bends and warps down into the Earth's interior. These earthquakes occur in two particular classes. I'll start off by discussing earthquakes in what we call the 'outer rise' region — the region just seawards of the subduction trench, where the down-going plate initially bends as it starts to subduct, characterized by a mild topographic bulge — and then I will move to consider earthquakes occurring within the subducted slab at greater depth — typically between 30 and 150 km, as the slab first unbends, to straighten as it descends, and then often deforms in a complex manner during its descent. In both of these cases, we typically see two zones, or planes, of seismicity, associated with the varying plate-parallel extensional and compressional stresses induced by the process of bending or flexing the plate. These two zones are then separated by an aseismic core, where the bending stresses are supported elastically, and are insufficient to reach the yield stress of the plate, and generate earthquakes.

These intraplate earthquakes are critical, for what they can tell us about the rheology, deformation, and stress state of our Earth, and for their implications for seismic hazard. Although intraplate earthquakes, in both the outer rise and in intraslab settings, rarely exceed Mw 8 (where Mw is the moment magnitude, a modern/updated version of the original Richter magnitude scale), where megathrust events can reach Mw 9·5, they still have the potential to be devastating.

In geodynamic context, these intraslab earthquakes result from the combination of bending stresses, arising from the internal deformation of the plate, and the far-field plate-boundary stresses that drive plate tectonics — particularly those from the negative buoyancy of the descending slab, a force termed 'slab pull'. Mapping out in high accuracy — which has been one of my main aims here — the different regions of the plate that (*a*) undergo brittle, rather than ductile, deformation, and so fail in earthquakes, (*b*) where they take up plate-parallel extension or compression, and (*c*) the location and width of the aseismic region that separates these, hence allows us to ask first how the applied stresses are supported by the lithosphere, and how (or if) that mechanism of support is dependent on the rheological structure of the plate (in particular its thermochemical structure, and how this evolves down-dip as the plate subducts and heats up); and then second, the balance between bending stresses and in-plane far-field plate-boundary forces, and how this is reflected in the deformation and seismicity of the plate, provides one of the very few mechanisms for assessing the absolute stress state of the plate tectonic system.

In terms of the importance of these earthquakes for seismic hazard, it is illustrative to consider the case of the South American subduction system, which contributes over one-third of the total global seismic-moment release of the last century. Along this subduction zone, intraslab events account for over 80% of the earthquake-related fatalities, from only 5% of the moment release in this subduction zone, showing the disproportionate impact these earthquakes can have. Despite their greater depth than interface events, they typically occur much closer in three dimensions to major population centres — often occurring directly beneath cities concentrated along the volcanic arc. This also leads to a high proportion of triggered hazards, particularly landslides, given the steep, easily-mobilized nature of volcanic mountain chains. They also commonly have high stress drops, leading to short, sharp ruptures, generating seismic waves dominated by higher frequencies. This leads to enhanced ground acceleration and high damage to man-made structures. But the largest factor in enhancing the hazard associated with these earthquakes is the fact that they are relatively rare — with only a 50-year observational record, and no way to look for geological indicators, we simply have a very incomplete catalogue of where these earthquakes may occur. Combined with continuing uncertainty over exactly what conditions are necessary to allow such earthquakes to happen, it makes mapping out areas that might potentially be affected very difficult, and hinders their inclusion in seismic-hazard models.

The main contribution of the work I've been doing has been the development and application of a range of new seismological techniques, aimed at improving the accuracy of earthquake locations using teleseismic data — data recorded globally, at great distance from the earthquake. These techniques allow us to map out, to a degree not possible before, the seismicity within these intraplate regions, and in particular the separation between planes of seismicity in bending regions, for any plate, anywhere in the world. A particular focus has been on refining earthquake depths using so-called 'depth phases' — near-source surface reflections, which, when they can be detected, provide the high-resolution constraint on earthquake-source depth. Depth is a particularly important parameter — more so than lateral location in this context — because things in the solid Earth (particularly temperature, stress, and rheology) usually vary much faster in the vertical direction than in the horizontal. The techniques I've been working with now allow us to detect depth phases where previously they would have been too small in amplitude to have been picked up, and to determine source depth down to a kilometre-scale resolution, even for relatively small-magnitude earthquakes, down to a Mw of 4·7 or so.

In the outer-rise regions of the world, this accuracy has allowed us to map out the depth extent of the extensional bending region at the top of the plate, to separate out the deeper compressional region — and indeed to determine in which subduction zones there is such a deeper compression zone, as it is not ubiquitous — and to map out the width of the aseismic zone between the two. In some areas, such as Sumba, at the eastern end of the Indonesian archipelago, northwest of Australia, the entire outer-rise region is in horizontal extension, down to brittle–ductile transition, indicating a major contribution from slab pull, putting the entire plate into extension. In other cases, like the Nankai subduction system in Japan, the entire plate is in compression, despite obviously bending, indicative of a limited buoyancy force being transmitted back up the subducted slab, and strong compressive stress being transmitted across the subduction interface. But in the majority of cases — Chile, Tonga, Honshu, the Kurils, *etc.* — we see, and can now map out, shallow horizontal extension, down to some depth, a narrow aseismic zone beneath, and then a zone of deeper horizontal compression. Combining these seismological constraints on the mode of failure of the plate, with bathymetric and gravitational data for the plate geometry, allows us to constrain the rheological properties of the plate, in particular the effective frictional properties of the lithosphere, and homologous temperature that limits the extent of brittle deformation. That the depth-extent of these zones does not seem to vary across the interface seismic cycle also tells us that the stress transmitted across the subduction interface is (usually) small compared to the other plate-driving forces, and therefore plays only a minimal role in the intraplate stress field.

We are now in the process of extending this seismological analysis to cover intraslab regions. So far, catalogues of high-accuracy earthquake locations have been constructed for Northern Chile, as a test case for a more automated, less analyst-intensive, processing routine, and for all of Central America. The aim now is to expand this assessment to cover more region-specific studies, spanning a range of different subduction geometries, slab ages, and stress states. We are also in the process of constructing a new, updated range of geodynamic models for the thermal and rheological structure of subduction zones, drawing on constraints from the outer rise, and from the oceanic lithosphere in general, with the intention to undertake a global, systematic assessment of the controlling factors on intraslab seismicity, hopefully leading into a parameterization of the seismic potential of a given section of subducted slab — *i.e.*, the capacity of a section of slab to generate damaging earthquakes — such that potential intraslab earthquakes can be systematically incorporated into seismic hazard assessment.

The Chair. Thanks very much for a fascinating talk.

Dr. Sue Bowler. I am fascinated the geometry of these slabs. What determines the angle at which the slab descends. Is this the question everyone wants to know?

Dr. Craig. It's a complicated question. Again, the magnitude of slab pull is a critical one, the rheology of the plate, which is a particular function of the age of the plate as it goes in, and also has an impact on its density. Central America, for example, I mentioned, under Mexico, the slab only goes flat; it's a very young slab, compared to a lot of other ones, it is very hot, it's low density, and that potentially has a role in it flattening out and takes off eventually. There is a complexity, there are a lot of parameters that have a role in determining geometry, but it is a critical factor. The big earthquake under Mexico City, for example, occurs right at the edge of the flat-slab region where it then bends again and dips off in to the Earth's interior, so it has a critical role that we don't yet really understand — control on the geometry.

The Chair. I think you partially answered this but you mentioned the challenges of local data *versus* a global dataset that you have compared to what you would need.

Dr. Craig. I perhaps skipped over this. The data I work with is between 30 and 90 degrees, which is roughly 3000 to 9000 kilometres from the earthquake, so you get truly global coverage. We start in northern Chile, as I say, because there was local data so we can check we're getting it right. Now we've got a technique that works we should be able to apply that everywhere in the world, which includes places like the Tonga subduction zone where there is very little local data to provide a constraint.

The Chair. Thank you very much. [Applause.]

The next speaker is Professor Stuart Bale from the University of California, Berkeley. He's going to talk to us about 'The *Parker Solar Probe* — the mission and its first results'.

Professor S. Bale. [No summary of this talk had been received at the time of going to press.]

The Chair. Thanks very much for telling us about the very exciting first results from this fantastic mission. We are really looking forward to seeing what happens the closer you go.

Dr. Susan Ames. You said that energetic particles were being measured but you didn't say anything about them. Has the spectrum been measured and what are the highest energies?

Professor Bale. We're at solar minimum, so we haven't seen any spectacular events where one could put together a nice comprehensive spectrum. The surprising thing is that, so far, the instrument sees very small events. At 1 AU, near Earth, you see the large solar events associated with flares or interplanetary shocks. When you get that close to the Sun you start to see a new population of smaller events emerging, the $1/r^2$ sensitivity, essentially. There are papers in the *ApJ Supplement* and *Nature*.

The Chair. About the thermal testing of the spacecraft, so when you get to the closest perihelion, what is the maximum temperature you're expecting the spacecraft to experience?

Professor Bale. That's a good question. The thermal models say that the front surface of the heat shield should be up above 1500 to maybe 2000 C. The antenna that we build for our instrument, we expect 1300 C, or so. And I should say that on the back of the spacecraft, we have magnetometers on a boom in the shadow and we have to heat them to keep those warm, so the gradient is enormous.

The Chair. The irony of solar space engineering! Thank you so much again. [Applause.]

The final speaker this afternoon is Professor Malcolm Fairbairn from King's College London who's going to talk now about 'Astrophysical probes of dark matter.'

Professor M. Fairbairn. There is a huge amount of evidence for the existence of dark matter. If you look at the approach of M31 to the Milky Way, and take into consideration the age of the Universe, then there appears to be a lot more mass than you might expect.

There is the stability of galactic discs; if there is no dark matter then it is difficult to understand how the discs remain stable for as long as they do. This is in addition to the observed circular velocities discovered by Vera Rubin and others, seemingly requiring the presence of mass which is not visible in the form of gas and stars.

Galaxy clusters were observed by Fritz Zwicky who then used the Virial Theorem to work out the total mass, and this turns out to be much more than the mass tied up in stars and gas. Then there are dwarf galaxies which do not contain many stars, but the speed with which the stars are moving around corresponds to a deep potential well.

Turning to nucleosynthesis, and in the time interval between 1 second and 1 minute after the Big Bang, there are nuclear reactions which are understood very well. By looking at regions of the Universe where gas is left over by nucleosynthesis and seeing how much deuterium and lithium is there, you can figure out the ratio between the number of protons and the number of photons which allows you to measure the total number of baryons in the Universe. It turns out that there is not enough matter in the form of protons and neutrons to explain the way in which the Universe is expanding.

Using observations of the cosmic-microwave-background (CMB) radiation we observe a two-point correlation function which corresponds to sound waves on different scales. The ratio between the first peak and the second peak tells you about the sound waves moving in the underlying gravitational potential — again this must indicate the presence of dark matter. But there appears to be six times more dark matter than normal matter in the Universe, so where is it?

The simplest explanation is a particle which cannot be observed conventionally since it doesn't produce or interact with photons. We know that this dark matter must interact gravitationally or we would be unable to see its effects on its surroundings. The question is whether the dark matter can interact non-gravitationally so that we can detect it in other ways. (For example, neutrinos do not interact directly with photons, but we can detect them through their interactions *via* the weak nuclear force.) The relatively small mass of the Higgs boson is difficult to explain in quantum field theory unless new particles exist around the same energy scale, and we were hoping that one of these could be the dark matter.

If such an interaction or particle does exist then here are three basic ways for humans to interact with dark matter which can be summarized as making, shaking, and breaking.

First, using the *LHC* and colliding protons at ultra-high-energy results in collisions and trying to turn the kinetic energy of the protons into new particles, including, hopefully, dark-matter particles. Cases where there is perhaps just one visible resulting particle, but the conservation of energy tells us that other products were not detected because they did not interact with the detector, is what would be expected from dark matter. These are known as monojet events, but so far there are no unexpected monojet events to suggest that dark matter can be created in this way.

Second, in the centres of galaxies if we wait for dark-matter particles to annihilate with each other then we might expect gamma-rays, synchrotron radiation from spiralling electrons, or neutrinos. There is a possible gamma-ray excess at the centre of the Milky Way but this may be due to a population of millisecond pulsars.

Last, we may try and detect dark matter on the Earth. The *Lux ZEPLIN* detector, which is housed deep underground, consists of a chamber of pure liquid xenon surrounded by detectors. Dark matter interacting with xenon is expected to produce an ionization signal and light. *Lux ZEPLIN* is not yet operational and the current most sensitive detector is *Xenon1T* located in Abruzzo in Italy. This may be our greatest hope for detecting dark matter, but no positive results have been obtained as yet. Is this because dark matter is too light to make the activator material respond?

We can also turn to astrophysics to see if dark matter decays, specifically to look for a 3·5-keV excess, using the *Chandra* and *XMM* satellites, decaying into photons. Another possibility is the self-interaction of dark matter — if dark matter interacts with itself then it will change the way in which it behaves. The dark-matter density in a large galaxy will be highest in the centre. If it reacts with itself then it will form a constant-density core; if not it will be a cusp or rising density as we approach the centre.

If dark matter is extremely light, *i.e.*, a fraction of an eV, it could behave like a Bose–Einstein condensate meaning that you could have a core in the middle of a galaxy because the particles are behaving in a weird quantum-mechanical way. Either way we want to know if there is a core in the middle of spheroidal galaxies. This is a good laboratory for understanding what dark matter is doing. We did work with PhD student Thomas Richardson analysing the shape of radial-velocity profiles. It appears that some dwarf spheroidals do have cusps — in other words the dark matter does not have much self-pressure. Justin Reed considers that star formation most likely is related to the presence of a cusp or a core in the centre of small galaxies.

What happens if dark matter is really large, *i.e.*, the size of an asteroid? Then we can use gravitational microlensing. On Hawaii the *Subaru* telescope is equipped with the *HyperSuprimeCam* CCD array and they have been observing M31 for a long time. They are looking for variations in star luminosity as foreground objects pass in front of background stars, with the results helping to put constraints on the number and size of dark-matter halos within the galaxy.

We can also try to find out how quickly dark matter was moving in the early Universe. We know that it is not moving faster than 500 km/s, which is the escape velocity of the Milky Way, or more than 20 km/s in order for it to be present in dwarf spheroidals, so if it is moving quickly then dwarf spheroidals cannot form.

If we look at the dark-matter distribution inside a galaxy, there is a large overall halo but with lots of little haloes. The mass of the smallest halo will depend on the velocity of the dark matter. We are currently applying machine-learning techniques to lensed images of galaxies to see if we are able to distinguish between fast-moving or slow-moving dark matter in the early Universe.

Particle physicists trying to understand dark matter are becoming more interested in developing aspects of astrophysics which could help understand this mysterious stuff. If we get no further with the *LHC* then we will be relying more on astrophysics. We need to continue to investigate every possible way to search for this weird stuff.

The Chair. Thank you very much — a fascinating talk.

Mr. O. Oji. Given that dark matter does interact with normal matter, gravitationally, could we not, indirectly see the dark matter produced in the crescent or cone by how it's moving normal matter about?

Professor Fairbairn. I think, in a sense, that is what we're trying to do — it's just that there's lots and lots of different ways of doing that. You mean by looking at normal matter and the motion of normal matter to work our where the dark matter is? I think we're trying to do that. And that's why dwarf spheroidals are so good because if you go down to the region of the centre of the Milky Way where there is lots and lots of dark matter, it is insignificant, because there are also a huge number of stars and also, of course, the central supermassive black hole, which is actually not that important because there are so many stars down there. So trying to see the dark matter down there is a no-hoper, so you have to look at the dwarf galaxies where there's really a lot of dark matter and very few stars and the stars are really just tracers. We're trying to come up with new ways of using the very limited data we have about the motion of the stars in these dwarf galaxies to try and figure out what the dark matter is doing and where it is.

The Chair. Any more quick questions?

A Fellow. Is it now generally accepted that dark matter must exist or could you adjust the physical laws?

Professor Fairbairn. I love MOND! If you go back to how Newton discovered the law of gravity, he basically looked at the motion of the planets in the Solar System and used astronomical data to derive the force law, and MOND does that as well, and that is beautiful and I respect that. The trouble is that MOND doesn't work. It doesn't work for clusters of galaxies, it doesn't work for the CMB. If you go to a galaxy conference, and you're just talking about galaxies then there's lots of MOND people there and that's great, but if you actually take into account other observations I think you've got some problems. But I'm open-minded — MOND stands for Modified Newtonian Dynamics, so that is where you say that General Relativity is OK but on very, very small accelerations there are deviations from General Relativity or Newton's Gravity, which are responsible for the anomalous motion of stars. I think there are too many other pieces of evidence that make it very difficult for MOND to survive, but I'm always willing to be proved wrong, as I think we all should be.

The Chair. I think we should wrap up this conversation now; there is obviously lots to talk about. Thank you again for your very interesting presentation. [Applause.]

Just to remind you that there will be a drinks reception in the RAS library immediately following the meeting. Finally I give you notice that the next monthly A&G Open Meeting of the Society will be on Friday the 17th of April, but given the circumstances, which we are experiencing at the moment and how rapidly things are changing, obviously if this does change we will let you know as soon as possible. [Owing to the Covid-19 pandemic, this meeting was cancelled — as was the May meeting. — Ed.]

BEFORE THEY WERE FAMOUS: PRE-1925 PAPERS IN BRITISH JOURNALS THAT TURNED OUT TO BE ABOUT GALAXIES

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Proof of the existence of external galaxies only came at the end of the first quarter of the 20th Century, Hubble's first papers on the distances to M 31, M 33 and NGC 6822 being published during 1925[1,2]. However, many papers appeared before that which — in retrospect — were about galaxies, some, of course, contributing to the arguments for or against 'island universes'.

So where did British astronomers, generally Fellows of the Royal Astronomical Society, publishing in or reading the British 'professional' journals, specifically *Monthly Notices* (and *Memoirs*) of the RAS and *The Observatory*, stand in this? As expected, most papers described below are in *MN*, indeed *Memoirs* hardly figures, but there was also some lively 'Correspondence' to the Editors of this *Magazine*, especially towards the end of the period covered.

I. The Papers

Early history

It would appear that the first paper in *Monthly Notices* to discuss what are now known to be galaxies was 'Suggestions respecting the Origin of Rotatory Movements of the Celestial Bodies and the Spiral Forms of the Nebulae as seen in Lord Rosse's Telescopes. By James Nasmyth, Esq.' in 1855[3]. (Lord Rosse himself published some of his observations in *Philosophical Transactions of the Royal Society of London* in 1850 and 1861.) In this, Nasmyth compared the formation of a spiral nebula to water going down a plug-hole, speculating that gravity was the key in both cases, with the material of the nebula flowing towards the centre, and correctly pointing out that any deviations from perfectly radial infall would lead to rotation, the 'nebulous mass' then spiralling around the nucleus.

The first more observational, or interpretative, *MN* paper with significance for extra-galactic astronomy was actually by an American, Cleveland Abbe's 1867 'On the Distribution of Nebulae in Space'[4]. This was followed by R. A. Proctor's influential 'Distribution of the Nebulae' in 1869[5]. Both papers discussed the avoidance by nebulae (planetary nebulae aside) of the regions close to the Milky Way most occupied by stars and star clusters. Abbe simply tabulated the numbers of nebulae in different areas in RA and Dec, but Proctor, a proficient mathematician, plotted the positions on an equal-area 'isographic' sky projection of his own devising. He concluded that the disjoint distributions "led to the conclusion that the nebular and stellar systems are part of a single scheme. If the nebulae were external star-galaxies (this distribution) would be accidental." He did allow the caveat "unless … there is some peculiarity in the galactic stratum preventing us from looking so far out into space along its length than elsewhere" but then dismissed this for other reasons.

Aside from papers or comments on discoveries of new nebulae, including interesting ones by 'M. Stephan' noting groups of nebulae[6], the only other pre-1880 *MNRAS* papers were another by Proctor describing 'The Rich Nebular Regions in Virgo and Coma Berenices' in 1872[7] and two by Sidney Waters, also on nebular distributions, the following year[8]. Waters' papers used Sir John Herschel's 'General Catalogue of Nebulae and Clusters of Stars', as had Abbe and Proctor, but his drawings utilised more exact positions. He also concluded that the observations implied that the nebulae and stars were separate parts of one overall system. (Note that, despite significant numbers of other papers in *MN* and *Memoirs*, John Herschel never published anything there on nebulae, at least not that is relevant for this article.) In *The Observatory*, in 1878, there was a somewhat testy exchange of letters between J. L. E. Dreyer[9] and W. Tempel[10] on the reality of spiral structure, Tempel arguing that it was merely an artefact of visual observing and sketching.

In 1885 in *MN*, there appeared two papers on the 'nova' (now known to be supernova) in M31: 'Observations of the spectrum of the new star in the great nebula in Andromeda, made at the Royal Observatory, Greenwich' by E. W. Maunder[11] and 'The outburst in Andromeda' by Rev. S. J. Perry[12]. These were followed by 'Observations of the new star in Andromeda made at Mr Wigglesworth's Observatory with the 15·5-inch Cooke Refractor' by J. Gerh. Lohse[13], 'On Hartwig's Nova Andromedae' by Ralph Copeland[14], and 'Nebula in Andromeda and Nova, 1885' by T. W. Backhouse[15]. None of the papers made any comments beyond describing the observations. There was also widespread correspondence and a review of the topic (by Maunder[16]) in *The Observatory*, with further observations of the nova reported therein the following year by Joseph Baxendell[17] and T. E. Espin[18].

In a new departure for *MN*, in 1888 Isaac Roberts supplied 'Photographs of M31, h44 and h51 Andromedae, and M27 Vulpecula'[19]. The image of M31 and its companions (better known as NGC 205 and M32) is reckoned to be the first photograph of a galaxy. Roberts was convinced he was confirming the 'nebular hypothesis', *i.e.*, the formation of a planetary system around a star (he later[20] thought he saw variations in M31 between photographs taken at different times). This was just the start of a string of similar papers by Roberts over the next few years[21], covering amongst others M81, M82, and M51. Roberts' latest photograph of 'The Great Nebula in Andromeda' was reproduced in *The Observatory* in 1889 with a review by A. A. Common[22]. Other photographs of M31 were supplied by E. E. Barnard from Lick[23], while H. C. Russell from Sydney Observatory sent the RAS photographs of Nubecula Major and Nubecula Minor, *i.e.*, the Large and Small Magellanic Clouds[24]. For variety, Albert Taylor contributed a 'Note on Nebulae Spectra at Hurstside Observatory'[25]. This included the spectrum of M31, which he tried to convince himself showed emission lines.

We should add that 1888 saw the publishing of J. L. E.Dreyer's monumental 'A New General Catalogue of Nebulae and Clusters of Stars, being the Catalogue of the late Sir John F. W. Herschel, Bart, revised, corrected, and enlarged' in *Memoirs of the RAS*[26]. His *Index Catalogues* of additional nebulae appeared in *Memoirs* in 1895 and 1910[27].

In *The Observatory* in 1891, W. F. Denning[28] noted that the oft reported variations in the appearance of nebulae, including the nucleus of M31, were in reality likely to be due to varying atmospheric conditions. Returning to Isaac

Roberts, in *MN* for 1895[29] he used his new photograph of M33 as evidence for his theory of its formation in a collision, most likely between "meteoritic streams". Two years later, in *The Observatory*[30], he showed that new nebulae could be discovered photographically, particularly noting the (now known to be interacting) companion of NGC 672 (which became IC 1727), accurately describing the central regions of each as "straight lines of faint nebulous stars".

The new century

Aside from Roberts' photographs already mentioned, there was nothing in *MN* in the 1890s on nebulae now known to be galaxies apart from occasional discoveries or lists of observations. The first decade of the 20th Century did not provide much for *MN* readers interested in 'island universe' theories, either. An honourable mention, though, to J. E. Gore, who described the appearance of every Messier nebula in a series of articles in *The Observatory* in 1902[31]. One new development, brought about by improvements in the photographs, was discussion of the dark lanes seen in spiral nebulae. In 1904, W. S. Franks[32] presented photographs of four 'ray-like' nebulae, which he correctly deduced were edge-on spirals. He interpreted the 'dark nebulosities' along their axes as demonstrating the presence of "certain forms of non-luminous nebulous matter". He thought that this could be due to the cooler outer parts of the nebula absorbing light from the hotter central regions.

G. W. Ritchey returned to this in 1910[33], in two 'Notes on Photographs of Nebulae taken with the 60-inch Reflector of the Mount Wilson Solar Observatory' (extracts from letters accompanying presents of photographs to the Society). He remarked of M33 that "these dark rifts, like those in the Andromeda nebula, (show) a flocculent, curdled structure, strikingly similar to that of the dark rifts which Professor Barnard has photographed in the Milky Way". However, he failed to make the — obvious in retrospect — step of equating the spiral nebulae to Milky Way-type systems. (By 1917 he did, though, agree with Heber Curtis' assessment that the faintness of novae he had discovered in spiral nebulae implied that they were very distant external systems.) He also noted the "bright convolutions which apparently consist of nebulosity which is condensing into thousands of nebulous stars" (which had been counted by his associate Miss Ware). From a photograph of a 'straight-ray' nebula, he too deduced that the dark lane was due to "the relatively cool, thin edge of the spiral projected against the hotter and more brilliant centre".

Two other original contributions were by Max Wolf[34] and H. Knox Shaw[35] in 1908. Following suggestions by H. H. Turner, they had, respectively, compared the axis ratios and position angles of spiral nebulae and looked at the angle between the axes of spiral galaxies and the plane of the Milky Way, but in both cases had had too few measurements to claim a convincing result. The same year, Turner himself explored 'the diminution of light in its passage through interstellar space', hypothesizing that this could occur due to scattering by small particles. While the paper[36] was not directly concerned with nebulae, this idea became important in the context of their apparent distribution.

The years from 1910

Over the remaining years until the Great War, there were several relevant *MN* papers. Arthur Hinks presented two linked contributions[37] in 1911, returning to the question of the distributions of stars and of nebulae. He followed these with a brief note in 1914[38], appended to J. A. Hardcastle's list of nebulae found on the Franklin-Adams plates[39] (which were divided into spirals, 'elongated', 'diffused', and small), where he noted particularly that the distributions varied with galactic longitude as well as latitude. In 1912, Francis G. Brown[40] made the interesting inference that if there was indeed absorbing interstellar material, then more distant nebulae, which would naturally look smaller on average, should also have progressively lower surface brightnesses as the length of the absorbing column increased. Although with many uncertainties, he found that the data did support this.

Perhaps the most intriguing papers, though, were both by J. H. Reynolds. In a 1912 paper[41], he suggested that if the light from the central star(s) in a spiral nebula was scattered by surrounding material, it should appear polarized. Using a 'double-image prism', he found some evidence that this might be the case for M64 (a galaxy with a notable dust lane), the images appearing to change as the prism was rotated. As evidence against spirals being made purely of stars, he also cited the observation that the continuous spectrum of NGC 1068 was "crossed by bright lines of nebulium and hydrogen", as seen in gaseous nebulae.

The following year[42] he determined 'The Light Curve of the Andromeda Nebula'. In more modern parlance, this was what we would call its radial intensity profile. He found that it could be represented by $y = 1/(1+x)^2$, the result that Hubble later confirmed for elliptical galaxies. Reynolds theorized (from its similarity to an inverse-square law) "that the light of the nebula is derived in some measure from a central star". He did note that "the usual alternative hypothesis … is that it is a very distant galaxy of stars" but argued that in that case "we must also make the very improbable assumption that the surface brightness of the stars follows the light curve".

Apart from papers on rotational dynamics and on viscosity, both of which James Jeans[43] proposed might be relevant to the evolution and the spiral shapes in nebulae (and one on relativity; see below), the only matter of note in our context in *MN* in the wartime years was the question of detecting motion within spirals. First (in *MN*) was 'Probable Motions in the Spiral Nebula Messier 51 (Canes Venatici) found with the Stereo-comparator: Preliminary Communication' by S. Kostinsky[44], remarking on "almost indisputable displacements of some characteristic knots lying on the spirals", between plates taken 20 years apart. (Schouten[45] later reported a consistent result in *The Observatory*.) A review[46], 'The Motions of Spiral Nebulae', written by Arthur Eddington, was presented as part of 'The Report of Council' to the 1917 RAS AGM. These reports covered papers in other journals on a variety of topics. Eddington noted the potential usefulness of measuring radial and angular motion for determining radii and distances, and even masses, of spirals. He discussed the known radial velocities and was even prepared to consider a "motion of the stellar system (*i.e.*, the Galaxy) relative to a possible super-system of (external) galaxies". He was dubious of the accuracy of claimed proper motions of spirals but thought that there were "some very interesting results with regard to internal motions" (most famously, those of van Maanen). Jeans meanwhile, in *The Observatory*[47], found support in van Maanen's work for his own theory that material was thrown off the central body of the nebula (which he believed to be small and gaseous) and condensed into stars in the spiral arms. Later in the year L. Becker[48] showed, by comparing the original and a rotated image of M51, that its arms were very symmetrical. He deduced from this that particles of the spiral must be moving outwards uniformly from the centre rather than randomly falling in.

Hidden away in a lengthy 1919[49] 'Preliminary Note on the Application of

Photoelectric Photometry to Astronomy' by 'Messrs Lindemann' was the perceptive comment that if all spirals turned out to have the same colour, while stars changed colour with age, then spirals must be long-lived compared to stars and replenish their stellar content through the birth of new stars.

The cosmology paper alluded to was one of a set of three by de Sitter, which Eddington had asked him to write for *MN*[50] 'On Einstein's Theory of Gravitation, and its Astronomical Consequences'. The third, in 1917, presented Einstein's static, constant-density model of the Universe and de Sitter's own empty (zero density) solution of the field equations, in which distant objects show a (non-velocity) redshift. De Sitter considered that spirals must be (at least reasonably) distant systems and made estimates of the scale factor of Einstein's universe (from the likely mass density) and in his own model (by comparing redshifts to distances), obtaining values from 5 to 250 Mpc and around 1 Mpc, respectively. He remarked that continuing to find a preponderance of redshifts over blue shifts would favour his solution over Einstein's.

There had also been some interesting contributions in *The Observatory*. In 1916, as part XXIII of a long series of reviews covering 'Some Problems of Astronomy', Hector MacPherson[51] summed up the pros and cons of the island-universe theory, following Eddington in his book *Stellar Movements and the Structure of the Universe*, by coming down in its favour. Reynolds responded[52] by repeating some of the problems, including the non-stellar, emission-line spectrum of NGC 1068 (now known to be an AGN, of course), and McPherson issued a rejoinder[53] (reviewing the topic again in 1919[54] and, in the context of stellar evolution, in 1921[55]). In a further contribution the following year, Reynolds[56] was unconvinced by the large radial velocities measured for some spirals. This time it was V. M. Slipher himself who defended their accuracy[57]. At this point he had measured about 30 velocities, which averaged 570 km/sec.

The 1920s

The so-called 'Great Debate' between Heber Curtis and Harlow Shapley on the existence or otherwise of 'island universes' took place at the US National Academy of Sciences in 1920. However, papers of interest in *MN* remained scarce, with just two that year by Reynolds[58], returning to the questions of 'light curves' and the spatial distribution of spirals. The first classified nebulae according to their "stages of nebular condensation". This assumed that unresolved regions were nebulous and that the stars condensed in the outer regions; essentially what we would now call the bulge and the disc, respectively. His classes progressed from "entirely amorphous nebulosity", through the case where "the condensation has advanced considerably towards the nucleus", to objects where "the whole nebula is of the condensed type", the first step towards the Hubble classification system of galaxies (though Reynolds himself still thought that "it seems difficult to reconcile the various stages of condensation with the idea that the spirals are all distant galaxies"). He then presented measurements of the intensities along the axis of typical nebulae from each class, noting *inter alia* that the outskirts were bluer than their centres.

The second paper concentrated on the (rather few) spirals with the largest angular size, which he reasonably assumed to be the nearest. He found that their numbers were different in the northern and southern hemispheres and that their shapes suggested they favoured orientations parallel to the Galactic plane. He acknowledged the suggestion that the distribution on the sky could be due to 'occulting matter' in the plane but felt that the surplus of near edge-on systems indicated a connection between the nearer spirals and the Milky Way.

Also in 1920, but in *The Observatory*, Reynolds reviewed[59] Curtis' Lick Observatory contribution 'Description of 762 Nebulae and Clusters' but was unconvinced that the small faint nebulae were all distant spirals, citing the case of M32, which must by its proximity on the sky be assumed to be at the same distance as M31 but was much smaller and without arms. In a 1921 *MN* paper Reynolds[60] added that there was "evident grouping together of spirals of the same type". (He was, perhaps unsurprisingly, unsure what to do with NGC 5128 = Cen A, so omitted it.) Back in *The Observatory*[61], he compared the degree of condensation in M31 and M33, concluding that the latter must be "a vastly older formation" and that it "seems impossible to admit that the amorphous nebulosity of the uncondensed type actually consists of stars", suggesting that the star-like spectra merely indicated the temperature of the nebulosity. On the other hand, he had to agree that 'Nubecula Major', which clearly contained both normal stars and emission nebulae in the same way as our galaxy, had a remarkable general resemblance to condensed nebulae. (The following year[62] he commented that negative recession velocities were confined to the objects likely to be nearest to the Galaxy.)

Still in 1921, Rev. J. G. Hagen[63] contributed 'A Map of Obscure Nebulae and their Situation towards the Milky Way'. Hagen claimed that "the entire sky outside the galactic plane is covered with dense cosmic clouds", visible by eye but not detectable in photographs as they were "recognised only by the various shadings from one point of the field to another". He suggested that stars had formed from the clouds in the Milky Way leaving a "cavity in the surrounding ocean of primitive matter". Spirals, also seen associated with the obscure clouds towards the poles, were therefore outside our galaxy (though, by implication, only just outside).

The only *MN* paper of relevance in 1922 was a study by C. D. Perrine[64] of the appearance of several spirals, most interestingly a detailed description of the structure of the galaxy pair now known as the Antennae (NGC 4038–4039) and their near neighbour NGC 4027. In *The Observatory*, there was a paper by W. Smart[65], 'An Infinite Universe', reviewing the hierarchical structure ideas of Charlier and attempting to estimate the scale of each level of clustering of galaxies.

1923 was a very active year in *MN* with several on-going debates. First up was Reynolds, back to the distribution of spirals again[66]. Prompted by Shapley's deduction that the Sun was offset from the centre of the distribution of globular clusters, Reynolds suggested that "the galactic plane is situated eccentrically with regard to the spirals", also concluding that the distributions of globulars and spirals were mutually exclusive. He also noted that if, like globular clusters, all spirals had similar physical sizes, then, compared to M31, the smallest seen must "stretch to a vast distance from the galactic plane".

Next was a 'Note on the Constitution of the Spiral Nebulae' by F. A. Lindemann[67], who (having changed his mind since the 1919 paper, above) listed arguments against island universes and set out his own new theory. He proposed that spirals were formed of dust particles driven outwards by radiation pressure from star-light, that these particles condensed into clouds once in a region of low radiation density and were visible by reflected light. Apparent novae had to be explained (rather artificially, as he admitted) as resulting from a collision with a comet-like cloud.

The following month Reynolds responded with his own note[68] on Lindemann's note. He pointed out that stellar radiation driving the clouds might also account for their longitude distribution if the Sun was off-centre of the stellar distribution, but considered that the inverse-square radial fall-off of intensity and the presence of dark lanes were strong arguments against Lindemann's idea as they suggested illumination from the nucleus. Reynolds ended the year with yet another paper[69] on the distribution, this time of 'small spiral, spheroidal and lenticular nebulae'. He pointed out that frequently one or two faint spirals were accompanied by "a number of faint featureless objects, round or oval in form".

Meantime, another argument briefly broke out. Harold Jeffreys[70] drew attention to "certain internal inconsistencies in Jeans' theory of the origin of spiral nebulae" as put forward in Jeans' book *Problems of Cosmogony and Stellar Dynamics*. This theory relied on rotation and tidal effects to shape the nebulae. Although agreeing that the predictions of the theory were "strikingly in accordance with … modern observations", he objected that "the method of transition from the lenticular to the spiral form appears to be dynamically incorrect". Jeans immediately supplied a rejoinder[71] with a more rigorous mathematical treatment, to which Jeffreys had no further objection. Jeans revisited the tidal origin of the spirals as section 42 of a hefty paper in *Memoirs*[72]. Jeans also contributed a further *MN* paper[73] on internal motions. Given that all spirals were 'equiangular' (constant pitch angle), any motion had to preserve this form and Jeans calculated that van Maanen's measurements of motion along and across arms did satisfy this constraint and moreover implied that the spirals were winding up. However, he also found that the ratio of radial motion to rotational motion was incompatible with orbits under gravity and was forced to posit a new unknown force.

The Observatory had even more than the usual number of papers which induced rounds of (sometimes quite feisty) correspondence. First up was E. Öpik[74], commenting on Reynolds' claim of the alignment of large spirals with the Galactic plane, noting that there could be a preference for edge-on systems among such objects as they would have higher surface brightness. Reynolds immediately countered[75] that this implicitly assumed that all spirals were the same (while he divided them by how condensed the nebulosity appeared), also making the point that obscuring lanes in edge-on systems would counteract Öpik's effect. Öpik came back[76] with his own (lengthy) rebuttal, pointing out that for distant, apparently small nebulae, only the central more spherical region would be visible, not the fainter arms, making them look more face-on. He ended with the rather tart comment "Mr Reynolds' (suggestion) appears to me but little more probable than a hypothesis that would claim a systematic orientation of the planes of Algol variables with respect to the observer". Unsurprisingly 'Mr Reynolds' responded again[77], concluding that Öpik's error was in "the implicit assumption that the distribution of spirals is entirely a random one over the whole sky", before the Editors brought the correspondence to a close.

Next in the firing line was Perrine[78] who, as part of a lengthy review, concluded on the basis of mostly large and positive but some negative recession velocities, that the (in his view, relatively small) spiral nebulae "are the offsprings of large stellar systems such as ours", driven away by a process such as Lindemann's light pressure, preferentially towards the galactic poles. However, he found Lindemann's view that the spirals were seen mainly by reflected light to be untenable, as in that case the reflected galactic starlight should have twice the Doppler shift of the intrinsic nebular emission lines in objects such as NGC 1068. A. C. Gifford[79] likewise objected to Lindemann's theory, noting that recent estimates that 700 000 faint nebulae were within reach of large telescopes implied that, even if they were individually small, they contained more mass than the stars in our galaxy. Thus the hypothesis that "the light we receive from spirals is reflected starlight (is) quantitatively impossible". Rather waspishly, he remarked that if "this theory is the only alternative to that of external galaxies, it would seem to furnish some strong arguments for the truth of the latter".

These, unsurprisingly, led to correspondence from both Reynolds[80] and Lindemann[81]. The former reiterated points he had made previously about the distribution of nebulae relative to the poles and the evidence that they were not composed of stars. Amongst other things he cited the steep decrease in intensity from the centre of M31 outwards, which he considered to be unlike that of our galaxy. As to Gifford's arguments, he rejected the assumption that all the small nebulae were spirals, commenting that there was "a regrettable tendency towards over-speculation and insufficient observation". Lindemann countered that both the continuous and emission-line spectra could be reflected light and that "if the alternative hypothesis that the white nebulae are unresolved star clusters is excluded (because of the van Maanen observations) … I suggest that my critics should explain to what their luminosity is to be attributed if it is not reflected starlight". Perrine came back in 1924[82], noting that if all spirals were seen in reflected light from the Galaxy, why did some have emission lines and some not, also noticing that NGC 1068 and 4151 had broader lines than Galactic emission nebulae. W. H. Pickering[83] also disagreed with Lindemann and Reynolds' latest contributions. He suggested that spirals could be placed in an evolutionary sequence from the least to most condensed into stars (effectively smallest to largest disc-to-bulge ratio in modern terms), with our galaxy at the most condensed, therefore oldest, end (such spirals also having only a small decrease in intensity from the centre to outskirts).

By 1924 word on Hubble's measurements of distances to spirals should have started to circulate — his first Cepheid in Andromeda was observed in 1923 October and his results were formally announced at the AAS meeting on 1925 January 1[84]. (In fact it was in the *New York Times* the previous November.) However, there is no hint of this in *MN*, even when Hubble himself talked at the May RAS Meeting. *The Observatory* records[85] that he spoke only on nebular classification and intensity profiles (though he did, tellingly, split nebulae into galactic and non-galactic).

It was, nevertheless, another busy year in *MN*, with seven relevant papers in 1924. The first item, in February, was the *Council Note*[86] on nebulae (this time written by Reynolds) which considered that the outstanding contribution on spirals in the preceding years had been van Maanen's reports of internal motion. It also discussed Jeans' and Lindemann's new theories. Smart[87] carefully examined van Maanen's measurements and methods, stating that "I do not believe that anyone would be so bold as to question the authenticity of the internal motions". J. Jackson[88] similarly concluded that the internal motions were large enough to be secure, but that overall proper motions of nebulae would be compromised by the proper motions of comparison stars. Reynolds himself compared measured separations of condensations to those predicted by Jeans, given the implied angular velocities of the nebulae. He explored[89] the spacing of nebulosities in the arms of various types of spiral, finding typical values of order 15 arcsec or more, but did not actually compute the distances (they would have been about 200pc). In addition, C. C. L. Gregory[90] returned to the question of the orientation of spirals, looking at the distribution of the directions of their poles. He found that these poles avoided the area around the galactic poles, hence that the spirals' planes "avoid parallelism with the galactic plane".

In light of the news about to break, the most up-to-date paper was by Knut Lundmark[91]. This was actually a response to a contribution by Ludwik Silberstein[92] who had noted that in de Sitter's relativistic world model (see his 1917 paper, above), the redshift of a source should vary approximately as r/R where r was the distance and R the curvature radius of the Universe, provided r/R was small. Silberstein attempted to determine R from the distances and radial velocities of globular clusters and then used the value he obtained to determine the distances to spirals from their velocities. He suggested that Andromeda should be 30 kpc away (he ignored the sign of the velocity) and the spirals with the largest velocities 180 kpc away. Lundmark disagreed that globulars were far enough away for curvature effects to be larger than normal velocities and in any case, Silberstein had only used a selection of the available data; using all the objects did not give a consistent value of R. Moving on to spirals, Lundmark reiterated his previous work using the brightness of novae to deduce a distance of 200 kpc, perhaps to be updated to 500 kpc, for M31. From the relative apparent sizes and magnitudes of other spirals compared to M31, he deduced that their distances should be between 2 and 200 times larger (so up to tens of Mpc). Also "Plotting the radial velocities against these relative distances there may be a relation between the two quantities, although not a very definite one." This was the first time such a proto-Hubble's law (now Hubble-Lemaître law) appeared in *MN*, though numerous others appeared elsewhere. Lundmark also had a paper in *The Observatory*[93], specifically on the distance to the LMC which he found from various stellar indicators to be around 35 kpc. Silberstein issued a rejoinder defending his distances at the start of the following year[94].

Postscript

The legend is, of course, that Hubble's paper at the AAS immediately ended the debate on island universes and ushered in extra-galactic astronomy. This was not apparent in *MN*. The section on nebulae in the February *Council Note*[95] still gave pride of place to van Maanen's work and listed the work in the 1924 papers noted above. Reynolds (again the author) did note, though, that "work of first-rate importance has been done by E. Hubble on variable stars in the spirals, which it is hoped will soon be available for publication". Neither is there anything in the 'Notes' section of *The Observatory* for January to April.

It was left for Jeans (RAS President at the time), in April's *MN*[96], to state that "Recent results by Hubble … seem to establish the inaccuracy of estimates I made some time ago of the distances and other quantities associated with the spiral nebulae". The rest of the paper re-did Jeans' earlier calculations on nebular concentrations, removing any reliance on van Maanen's results. Incorporating Hubble's distance of 950000 light years for M31 he found the 'nucleus' (*i.e.*, bulge) must have a mass-to-light ratio similar to that of our galaxy. Hubble himself appeared in print in Britain in May when *The Observatory*[97] republished his original *Popular Astronomy* report of the AAS meeting talk. Reynolds also showed some slides of the galaxies containing Hubble's Cepheids at the end of the RAS meeting in May, as reported subsequently in *The Observatory*[98].

Lundmark returned in June's *MN*[99] with further measurements of velocity against distance and (optimistically) concluded that he could fit an extra quadratic term in the distance, which implied that no velocity would ever be above 3000 km/s. In a lengthy paper, there is just one page on Cepheids, noting that they had recently been discovered by Hubble in M31 and other nebulae, and that they confirmed Lundmark's own nova-based distance to M31. Separately he proposed that the recent nova seen in the 'globular nebula' M87 could be interpreted as an "upper-class nova" like that in Andromeda in 1885, thereby deducing a (rather accurate) distance of about 18 Mpc.

Groot[100] and Reynolds[101] supplied papers on the shapes of spiral arms with only the briefest mention in the latter of the distances to the nebulae; "As Dr Hubble has included this nebula in his programme of work on the Cepheid variables, we may soon get some information of a definite nature on this point". It should be said that even his own observatory apparently made little of it; July's *The Observatory*[102] reprinted Mount Wilson's annual Director's report which gave Hubble just four lines commencing "Among other results of interest …".

Afterword

In 1925 July, the IAU met in Cambridge and the RAS took the opportunity to invite a number of prominent overseas astronomers to a Special General Meeting. Van Maanen gave a talk[103], still convinced of the veracity of his internal motions (which were impossibly large if spirals were as far away as Hubble said). Shapley and de Sitter were there too, but the former talked on dark clouds and the latter on the work of the Leiden Observatory. Hubble and extra-galactic nebule nowhere get a mention.

Neither is there anything else in *MN* for the remainder of the year, though Jeans did speak about a paper of his in *Nature*, at the December RAS meeting (reported in the next issue of *The Observatory*[104]). This concerned the total light that should be produced by the birth of stars, as computed in his earlier *MN* paper, compared to that actually seen from M31. Jeans suggested that the disparity could be accounted for if the missing flux was emitted as 'penetrating radiation', *i.e.*, gamma rays, also managing to rule out the existence of many similar sized objects to M31, and of a very large universe, along the way. (In the discussion afterwards, Turner commented that he was struck most by "the coolness with which our President played marbles with island universes".)

To be fair, the 1926 February *Council Note* on nebulae[105] (by Reynolds again) did begin with a substantial piece about Hubble's work on M31 and M33, though even then, more space was devoted to dark clouds and the *MN* papers noted above. And far from inspiring extra-galactic research (at least in the UK), there were just nine *MN* papers on the topic over the next decade, if we exclude those on cosmology and the expanding universe. Indeed, the next extra-galactic astronomy papers in *MN* by a British professional or academic do not appear until 1937. Maybe Hubble finding the answer had taken the fun out of it!

II. The Authors

James Nasmyth gave his address as "Bridgewater Foundry" (which was in Patricroft, near Manchester). He was well-known in engineering circles for his invention of the steam hammer, and in astronomical ones for the Nasmyth focus. Using his engineering skills, he built a mirror-grinding machine powered by a windmill for his observatory at his home 'Hammerfield'.

Cleveland Abbe was at the US Naval Observatory. He had spent some time at Pulkova Observatory in Russia and in 1868 became Director of Cincinnati Observatory, from where he instigated telegraphic weather reports. From then on primarily a meteorologist, he was the first head of the US Weather Bureau.

Richard Anthony Proctor was effectively a full-time author, popularizing astronomy though a succession of books and articles, particularly on planets, but had no fewer than 84 contributions published in *MN*. He founded the popular magazine *Knowledge* and contributed the astronomical articles for the *Encyclopaedia Britannica*. His daughter Mary also became an astronomer.

Previously assistant to Le Verrier, *Edouard Jean-Marie Stephan* was promoted to director of the Marseilles Observatory in 1872, remaining in post for 35 years. He discovered two asteroids before moving on to studying nebulae, finding Stephan's Quintet in 1877.

Sidney Waters, of Oakridge Lodge, Tufnell Park, was only 19 years old at the time of the quoted paper and was an assistant to his father, a wine and vinegar merchant. He became an RAS Fellow in 1873 and published another *MN* paper similar to his early contribution 20 years later.

Danish by birth, *John Louis Emil Dreyer* was at Rosse's observatory at Birr Castle before moving to Dunsink in 1878 and then Armagh, where he was director until 1916. He remains best known for compiling the *New General Catalogue* and its extensions, the *Index Catalogues*. Also a noted historian of astronomy, he wrote extensively on the work of fellow Dane, Tycho Brahe. He was co-author of the *History of the RAS 1820 to 1920* and the RAS President from 1923 to 1925.

A lithographer who travelled widely through Europe, *Ernst Wilhelm Leberecht Tempel* was employed at the observatories in Marseilles and Bologna in the 1850s, making copies of star charts. Also a keen observer, well known for discovering several comets (Tempel 1 was the target of NASA's *Deep Impact* mission in 2005), he worked in Arcetri from 1875.

Edward Walter Maunder (always referred to by his middle name) was Spectroscopic and Photographic Assistant at Greenwich from 1873 (retiring in 1913). He is now best-known for his later work on the 'butterfly diagram' of sunspot positions and the 'Maunder Minimum' in the sunspot cycle. Much of his work was done in collaboration with Annie Russell, later Mrs Maunder, one of the first female RAS members. He was also one of the founders of the British Astronomical Association.

Father Stephen Joseph Perry was a Jesuit priest who taught at Stonyhurst College in Lancashire and was director of their observatory, which was a prominent establishment in the 19th Century. He was a regular participant in astronomical expeditions, heading that to Kerguelen Island (aka Desolation Island) for the transit of Venus in 1864. He travelled to several solar eclipses (including, coincidently, one with Maunder), sadly dying of dysentery on board ship immediately after heroically completing the 1889 observations in the French penal colony of Iles du Salut, French Guiana (which included Devil's Island which is now a CNES space-agency site).

Gerhard Lohse, though German, spent most of his career in Scotland, first at Lord James Lindsay, the Earl of Crawford's observatory at Dun Echt and later at the Royal Observatory Edinburgh, both times as assistant to Ralph Copeland. He contributed a number of objects to Dreyer's *New General Catalogue*. James Wigglesworth's observatory, where he made the M31 nova observations, was home to *The Great Scarborough Telescope*; Wigglesworth had actually bought out the famous T. Cooke & Sons telescope builders, responsible for the telescope, in 1879.

After an adventurous youth including gold prospecting in Australia and a German expedition to northern Greenland, by 1885 *Ralph Copeland* was senior assistant to the Earl of Crawford at Dun Echt. He was later the Astronomer Royal for Scotland. The Earl, an enthusiastic observer in his own right and president of the RAS in 1878–1880, eventually donated many of the instruments from Dun Echt to the re-founded ROE on Blackford Hill, where Copeland was director.

Thomas William Backhouse, a wealthy land and coal-mine owner from a Quaker banking family, observing from Sunderland, was clearly a remarkably keen practitioner. He published 250 papers in various journals — including many in *Nature*, a few in his own *Publications of West Hendon House Observatory*, and 20 in *MN* — over a span of 52 years.

Joseph Baxendell was an estate agent with his own observatory in Birkdale, Southport. He made contributions to numerous learned societies, particularly the Manchester Literary and Philosophical Society, and became an FRS in 1884. His brother-in-law was Norman Pogson, government astronomer in Madras.

The Reverend Thomas Henry Espinell Compton Espin became interested in astronomy at an early age and was elected an FRAS in 1878 when only 19. He collaborated with fellow clergyman Thomas Webb on the latter's famous book *Celestial Objects for Common Telescopes*. His observatory was at Wolsingham, Darlington.

Welshman *Isaac Roberts*[106] was a building contractor in Liverpool and had his own observatory, at first at his home in Rock Ferry on the Wirrall, then in Maghull outside Liverpool, where his most famous photograph was taken, and finally in Crowborough, Sussex. He published the first ever book of celestial photographs in 1893 and won the Gold Medal of the RAS in 1895.

Andrew Ainslie Common was another pioneer astrophotographer. Making his money working with his uncle in his sanitary engineering business, he was able to acquire larger and larger mirrors for the observatory in his back garden in Ealing, eventually building a 36-inch reflector (now the *Crossley* reflector at Lick). He was President of the RAS 1895–96 and an FRS.

Edward Emerson Barnard, who moved from Lick to the University of Chicago's Yerkes Observatory in 1895, is best known for his photographic studies of the Milky Way, especially of dark nebulae, correctly surmising that they were dust clouds obscuring more distant stars. While still an amateur he had discovered five comets, earning a 200-dollar prize for each, discovering 15 in all during his career, as well as Jupiter's fifth moon Amalthea.

Henry Chamberlain Russell was Government Astronomer in Sydney from 1870 to 1905, having started out as a 'computer' at Sydney Observatory. He supervised Sydney's contribution to the *Astrographic Catalogue* and was also one of Australia's first meteorologists. He was elected an FRS in 1886.

Albert Taylor used Sir Henry Thompson's observatory in West Molesey for the observations noted. Thompson was a distinguished surgeon and later donated his telescopes to the Royal Observatory at Greenwich. Taylor was, though, more associated with A. A. Common's observatory at Ealing, and was also prominent in eclipse expeditions, for instance leading one to Brazil in 1893. He was later an inspector of schools in Wales[106] and attempted to observe the 1927 eclipse visible in North Wales only to be met by heavy rain.

William Frederick Denning, mainly a writer by profession, was an indefatigable amateur observer in Bristol who apparently had no fewer than 1179 published contributions in various journals over a span of more than 50 years, many of them on meteors[107]. Indeed, his fame in that area led to him being name checked by H. G. Wells[108] in *The War of the Worlds*; on the arrival of the first Martian cylinder, he writes that "Denning, our greatest authority on meteorites, stated that the height of its first appearance was about ninety or one hundred miles". Denning also discovered a number of nebulae and four comets.

John Ellard Gore was an Irish amateur who spent some time in the Indian civil service as an engineer in the Punjab. From the 1870s onwards he was well known for his observations of variable stars, mostly made with binocular field glasses. He wrote numerous books, perhaps the most important *The Visible Universe* in 1893.

At the time of the paper noted, *William Sadler Franks* was at Isaac Roberts' observatory at Crowborough, but following the latter's death became astronomer-in-charge of F. J. Hanbury's Brockhurst Observatory in East Grinstead (later run by a very young Patrick Moore). Franks had been active for many years previously, contributing a *Catalogue of the Colours of 3890 Stars* back in 1878, star colours remaining his main preoccupation throughout his career (his final *MN* paper was 52 years after his first one).

George Willis Ritchey, co-inventor of the Ritchey–Chrétien telescope, was an instrument builder who worked at Yerkes, Mount Wilson — where he was responsible for the construction of the 60-inch telescope used in the cited paper, as well as the mirror for the 100-inch — and later in Paris. The 'Miss Louise Ware' mentioned in the paper was a member of the 'Computing Department' at Mount Wilson until 1942, particularly working on solar spectroscopy.

Maximilian Franz Joseph Cornelius Wolf was chairman of astronomy at the University of Heidelberg and director of the Heidelberg–Königstuhl state observatory. He was a pioneer astrophotographer, famous for discovering a large number of asteroids by photographic means, and took the first photograph of a '*nebelhaufen*', a cluster of faint nebulae (the Coma Cluster, in fact). He was also one of the first to propose an optical projection planetarium. He won the Gold Medal of the RAS in 1914.

Harold Knox Shaw (later *Knox-Shaw*), recently graduated from Cambridge, became an assistant at the Khedivial Astronomical Observatory, Helwan, Egypt, in 1908, being the first to photograph Halley's comet on its 1909–10 return. Subsequently superintendent at Helwan, he returned to England as Radcliffe Observer at Oxford and was instrumental in the observatory's move to South Africa. He was RAS president in 1931–32.

Herbert Hall Turner was Savilian Professor of Astronomy at Oxford and director of the Radcliffe Observatory for many years, and president of the RAS in 1902–1903. He had previously been Chief Assistant at the Royal Observatory at Greenwich and published around 300 contributions in astronomy and seismology journals, particularly on variable stars and earthquakes. He also wrote popular books and co-edited the first volume of the history of the RAS.

Arthur Robert Hinks was Senior Assistant at Cambridge University Observatory and was well known for his leading role in determining the length of the Astronomical Unit *via* observations of the minor planet Eros when it passed close to the Earth in 1901, work which won him the 1912 Gold Medal of the RAS. He subsequently changed fields and became secretary of the Royal Geographical Society. He was also secretary to the Mount Everest Committee which organised Mallory's ill-fated attempt on the summit.

Joseph Alfred Hardcastle was the grandson of John Herschel. When his business career was halted by poor health, he took to lecturing on astronomy and later set himself the task of measuring all the nebulae visible on the Franklin–Adams plates of the whole sky, publishing his work not long before his early demise.

While a young clerk to an insurance company, *Francis Gilbert Brown* had constructed his own 12-inch telescope with which he discovered a comet in 1911. He was later a director of a machine-tools business. He spent many years studying the orientation and alignment of galaxies in his spare time, publishing five papers in *MN* on the topic between 1938 and 1968.

John Henry Reynolds (usually known as "J.H.") was one of the final generation of distinguished UK amateurs, being the President of the RAS in 1935 to 1937. A successful businessman in the metal industry, whose father became the Lord Mayor of Birmingham, he had his own observatory at Harborne and also funded the construction of a large telescope at Helwan in Egypt. As will have been noticed, he has more papers mentioned above than anyone else.

One of the most prominent mathematical physicists of his day, *James Hopwood Jeans* wrote key monographs on numerous topics such as *The Dynamical Theory of Gases*, and is remembered for the Jeans mass, the Rayleigh–Jeans law, *etc*. After a relatively short time as a lecturer in Cambridge and Princeton, in 1912 he moved to Surrey to work on his own research and write books. With Eddington, he was among the first to try to model stellar structure, subsequently moving on to cosmology. He was knighted in 1928 and wrote a number of highly popular books including *Astronomy and Cosmogony* and *The Stars in their Courses*.

Sergei Konstantinovich Kostinsky's communication was sent from Pulkovo in Russia. He, and the observatory, were noted for their work on astrometry, particularly proper motions of stars, but he also wrote (in German and French) on planetary satellites, comets, and asteroids, and reported on the 1896 solar eclipse in Novaya Zemlya (later one of the USSR's main nuclear test sites). He remained in post after the Revolution.

Willem Johannes Adriaan Schouten was Kapteyn's graduate student at Groningen with a dissertation 'On the Determination of the Principal Laws of Statistical Astronomy'. He was later a teacher at the Christian Lyceum in Arnhem and an influential figure in the Dutch Reformed Church's acceptance of modern science, particularly evolution.

Though primarily a mathematician, *Arthur Stanley Eddington* briefly worked as Chief Assistant at the Royal Observatory at Greenwich before being appointed Plumian Professor in Cambridge in 1913 and shortly afterwards also Director of the Cambridge Observatories. His lively debates with Jeans at the RAS, particularly on the origin of the luminosity in stars, were legendary. (Retrospect tells us that it was generally Eddington who was correct.) His seminal *The Internal Constitution of the Stars* appeared in *MN* in 1920. A pacifist Quaker, during the Great War he was largely responsible for bringing Einstein's General Relativity to the notice of English-speaking scientists (see below), also organizing the 1919 eclipse expedition which led to the acceptance of Einstein's theory. Like Jeans he wrote numerous books, such as *The Expanding Universe*, which were very popular with the public. He was knighted in 1930.

Ludwig Wilhelm Emil Ernst Becker had moved from Germany to direct Dun Echt observatory in 1885. He was subsequently Regius Professor of Astronomy in Glasgow for 42 years, though he largely exiled himself to the Highlands during the Great War to avoid anti-German feeling. His primary expertise was in celestial mechanics.

Adolph Friedrich Lindemann was a wealthy engineer and businessman of German origin with an observatory at his mansion Sidholme in Sidmouth, Devon. He observed the 1894 transit of Mercury from Sidmouth and co-authored a number of astronomical papers with his son Frederick.

After working at the Royal Aircraft Factory during the First World War, *Frederick Alexander Lindemann* was appointed professor of experimental philosophy in Oxford in 1919. Earlier, while at the Sorbonne he had been the youngest participant invited to the famous Solvay Conference on Radiation and Quanta in 1911. A personal friend of Winston Churchill, he circulated in the country-house set with the likes of the Mitfords and Evelyn Waugh in the thirties. In the Second World War he was Churchill's controversial Chief Scientific Advisor, raised to the peerage as Lord Cherwell and later Viscount Cherwell of Oxford.

Mainly associated with Leiden where he was professor from 1908 and director of the observatory from 1919, *Willem de Sitter* was a Dutch mathematician and cosmologist and one of the first proponents of General Relativity. (He actually published a paper 'On the bearing of the Principle of Relativity on Gravitational Astronomy' in *MN* in 1911.) As the Netherlands were neutral during the Great War, he was able to communicate easily with Einstein in Germany and Eddington in Britain. Later he collaborated with Einstein on the paper presenting what is now called the Einstein–de Sitter universe (*i.e.*, one with critical density and no cosmological constant).

Hector Copland McPherson, then of Juniper Green, Edinburgh, was primarily a writer and historian of astronomy. He wrote a number of books such as *The Romance of Modern Astronomy, Describing in Simple but Exact Language the Wonders of the Heavens*, and was responsible for the *Biographical Dictionary of Astronomy*. He was a minister in the United Free Church of Scotland.

Vesto Melvin Slipher of Lowell Observatory, Flagstaff, Arizona, was responsible for the earliest measurements of radial velocities of spiral nebulae, starting in 1912. He was director at Lowell from 1926 to 1952, supervising the work that led to the discovery of Pluto in 1930, and was awarded the Gold Medal of the RAS in 1932. His brother Earl was also at Lowell and became mayor of Flagstaff.

Austrian–American astronomer *Father Johann* (*John*) *Georg Hagen* was a Jesuit priest, who had been in charge of Georgetown University Observatory in the USA before being chosen as director of the Vatican Observatory in 1906. His chief work was *Atlas Stellarum Variabilium*, in Latin. He also led the Vatican's participation in the international *Astrographic Catalogue*, a group of four nuns aiding with the measurements of the plates.

Charles Dillon Perrine was an American, previously at Lick, who was director of the Observatorio Nacional Argentino in Cordoba. He went on several eclipse expeditions and was best known for discovering several comets and two of Jupiter's moons, though his published contributions (over 49 years) covered a huge range of topics. In 1927, *i.e.*, 'post-Hubble', he had a paper in *MN* claiming that Cepheids were no brighter than the Sun and therefore the distance to M33 was 10 kpc. He had been elected an Associate of the RAS in 1904.

Harold Jeffreys became a fellow at St Johns in 1914 and spent the rest of his career in Cambridge apart from a short spell at the Meteorological Office. He was originally a mathematician, specifically a statistician, then a geophysicist and seismologist, and finally Plumian Professor of Astronomy, though his research remained in Earth sciences; his astronomical papers were largely on planets and their satellites. He was knighted in 1953.

Ernst Julius Öpik was at the Astronomical Observatory Tartu, Estonia. He is most remembered now for his essentially correct 1922 estimation of the distance to M31 based on its rotation velocity and assumed mass-to-light ratio, though most of his papers were on meteors. Escaping Estonia prior to the arrival of the Red Army in 1944, he was the Estonian rector of the Baltic University in Exile, moving to Armagh Observatory in 1948. His published papers remarkably spanned over 70 years.

Algernon Charles Gifford was a New Zealander, in his sixties at the time of his contribution mentioned above, whose most noted work was on impacts forming lunar craters. He taught at Wellington College where one of his students was Bill Pickering, later long-serving head of JPL, in charge of the launch of *Explorer 1* in 1958.

The brother of E. C. Pickering, the director of Harvard College Observatory for 42 years, *William Henry Pickering* was appointed assistant professor of astronomy at Harvard in 1887. He established an out-station of HCO at Mandeville, Jamaica, and moved there permanently after he retired. He discovered the ninth satellite of Saturn, Phoebe, and thought he had discovered a tenth, Themis, subsequently proven not to exist.

Edwin Powell Hubble was actually an RAS Fellow. Previously at Yerkes Observatory, at the end of the Great War he left the US Army in France, with the rank of major, to spend part of 1919 at Cambridge University. Having been proposed in May, he was elected a Fellow at November's meeting, just after obtaining a position at Mount Wilson. And the rest, as they say, is history[109].

Having served as a navigation instructor during World War 1, *William Marshall Smart* was John Couch Adams Astronomer in Cambridge. Later Regius Professor in Glasgow, he wrote several books on navigation including *Astronomical Navigation: A Handbook for Aviators* during World War 2, and is remembered most for his classic textbook *Spherical Astronomy*. An authority on stellar kinematics, he was RAS president 1949–51.

John Jackson, who studied under Becker at Glasgow, had been Chief Assistant at the Royal Observatory at Greenwich since 1914 (famously continuing to observe during a Zeppelin raid). He was appointed to the post of His Majesty's Astronomer at the Royal Observatory, Cape Town, in 1933. Shortly after retiring in 1950 he was awarded the RAS Gold Medal for his work on star positions, parallaxes, and proper motions.

Christopher Clive Langton Gregory (known as Clive) became chief assistant at Helwan Observatory at the end of WW1, but by 1924 was a lecturer at Imperial College, subsequently being instrumental in the setting up of the University of London Observatory at Mill Hill. Despite a lengthy career, he only published five further papers, the last after switching from astronomy to philosophy.

Ludwig Silberstein was born in Warsaw but worked primarily in Italy, holding a position at La Sapienza in Rome from 1904 to 1920 (though he was in England from 1913 and worked for the War Office in WW1). He wrote the first textbook on relativity theory in 1914 and expanded this to include General Relativity in 1924, by which time he was a researcher for Eastman-Kodak in the USA but lectured on relativity in several universities. He had been critical of Einstein's mathematics in the general theory and from time to time returned to his objections, earning him the sobriquet of 'Einstein's antagonist'.

Educated at Uppsala in Sweden, *Knut Emil Lundmark* remained there until 1929, though he spent periods at Lick and Mount Wilson, and in the 1924 papers gave his address as "Greenwich". He was subsequently appointed professor of astronomy and head of the observatory at Lund. His measurements of the brightnesses, and therefore distances, of novae in spirals made him a convinced advocate of the island-universe theory. He continued to work on the distance-scale problem and may have been the first to suggest the use of supernovae. He was the leading popular author on astronomy in Sweden from the 1930s onwards.

On the quoted paper, *Dr. Herko Groot* gave his address as Bussum, as he did in the list of Dutch IAU members. He contributed another paper on spirals in 1926. He was a secondary-school teacher of 'maths, physics and cosmography', whose predecessor at the school had been another astronomer, Anton Pannekoek. He is said to have unwittingly instigated Dutch astrophysics by sending copies of Saha's papers to Pannekoek.

Though Dutch, *Adriaan van Maanen*, a student of Kapteyn, spent virtually his whole career in the USA, briefly at Yerkes and then for 33 years at Mount Wilson. Unfortunately he is most remembered for his measurements, ultimately shown to be erroneous, which appeared to show significant movement of knots within spiral nebulae and were key factors against the island-universe theory. His work on stellar parallaxes and proper motions led to the discovery of the nearest isolated white dwarf, now known as van Maanen's star.

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CORRESPONDENCE

To the Editors of 'The Observatory'

The Accuracy of the Solar Parallax Obtained at the 1769 Transit of Venus: a Correction.

At last October's RAS meeting, Dr. Rebekah Higgitt gave a very interesting diary talk on the historical circumstances surrounding the British overseas expedition to observe the above transit, as reported in April's issue of *The Observatory*[1]. During the questions at the end of Dr. Higgitt's talk, the speaker and I had a significant exchange about the errors of the resulting solar parallax as arising, respectively, from measurement of the terrestrial baseline used and from the transit observations themselves. It has always seemed to me that the difficulties and likely errors of the latter allegedly arising from the notorious 'Black Drop' effect have been greatly overplayed in most accounts in the general literature of astronomy and that something of a persistent myth has become established on the subject.

Be that as it may, I now see from page 40 of that report in April's number that, in citing the relevant figure purely from memory in that discussion, I inadvertently did the accuracy of Thomas Hornsby's result a grave disservice. The truth, on further fact-checking, turns out to be rather remarkable and certainly deserves to be far better known than it would appear to be.

On the evening of 1769 June 3 as first contact approached, Hornsby, who was successor to the incomparable Bradley in the Savilian chair at Oxford, stationed himself to observe the phenomenon[2] in the astronomy professor's quarters at the top of the gatehouse tower of the Bodleian Library quadrangle. Unfortunately he was able only to observe first and second contacts before clouds intervened. Much more significant was the fact that, after the return of Captain James Cook from the South Seas, Hornsby was able to collate and reduce trans-global timings of the 1769 transit from Cook's on Tahiti — which seem to have been the best — to Vardö off the north coast of Lapland, *via* California and beyond, from which he finally obtained a mean solar parallax of 8·78 seconds of arc[3]; the exact figure, as now known, is 8·794... , less than 0·16% different from Hornsby's value. While it is true that some other published reductions of the 1769 observations gave slightly different results, Laplace many years later nevertheless adopted a figure of 8″·81, within 0·35% of Hornsby's[4]. A hundred years later, David Gill obtained almost exactly Hornsby's value "from 350 sets of measurements" by heliometer of the semi-diurnal parallax of Mars at the opposition of 1877, his result[5] being 8″·783 ± 0″·015, while Newcomb's reduction of the transit observations of 1874 and 1882 by Delisle's method[6] gave 8″·794. It is worth noting that the latter method depends on accurate timings of the internal contacts and is even more sensitive to their errors than is Halley's, and yet Newcomb's result is accurate to four significant figures: so much for the 'Black Drop' myth.

In short, the astronomers of the late 19th Century found it impossible to improve significantly on the solar parallax obtained by their predecessors a hundred years before not because their own results were so bad but because the 18th-Century results were so good. It is an interesting story.

Yours faithfully,
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(2) T. Hornsby, *Phil. Trans. Roy. Soc.*, **59**, 172, 1769, gives an interesting account, from which it appears that Oxford was positively bristling with telescopes trained on the Sun that evening. Hornsby cites nine observers by name, at six different locations around the University.
(3) T. Hornsby, *Phil. Trans. Roy. Soc.*, **61**, 574, 1771. The method used was Halley's, requiring timings of the transit-arc from second to third contacts at observing stations widely separated in latitude; that of Tahiti is 17° 35′ south, and of Vardö 70° 20′ north. The full text of these papers is available on-line.
(4) C. A. Young, *A Text-book of General Astronomy* (Boston), 1904, p. 408. Young's text first came out in 1888, only a few years after the 19th-Century transits of Venus, and contains a superb 20-page chapter (Chap. XVII) devoted to the determination of the astronomical unit. It was explicitly on this work that Russell, Dugan and Stewart based their own famous textbook nearly forty years later.
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REVIEWS

Luna Cognita: A Comprehensive Observer's Handbook of the Known Moon, by Robert A. Garfinkle (Springer), 2020. In 3 volumes: pp. 1400, 28·5 × 21·5 cm. Price £64·99/$89·99 (hardbound; ISBN 978 1 4939 1663 4).

By any standards Robert Garfinkle's *Luna Cognita* is a remarkable piece of work, a *magnum opus* in the fullest sense. First conceived in 1989, it has finally seen the light of day as three substantial, large-format volumes — a total of around 1400 pages weighing in at over five kilos. The table of contents and preliminaries alone occupy more than a hundred pages, and it is clear that, although described as an observer's handbook, this is not intended as an aid that will accompany the observer to the telescope!

Luna Cognita consists of three broad parts, although these do not map precisely onto the three physical volumes. The first part presents a series of informative chapters covering topics such as the Moon's cultural significance, the science of the Earth–Moon system, fundamentals of lunar geology, and several helpful chapters on how to observe and photograph the Moon. The second part provides a day-by-day account of the lunar surface at different phases, including detailed descriptions of individual features and of the honorees after whom they are named. This is followed by an analysis of different types of lunar geological formation, as well as of phenomena such as transient events, eclipses, and occultations. The third part, taking up the whole of Volume 3, provides useful ephemerides, tables, catalogues, an extensive bibliography, and a revised version of Edmund Neison's lunar maps.

This summary offers only a hint of the richness of content, and right from the beginnings of this project Garfinkle's aim has been audaciously ambitious: nothing less than the production of a comprehensive compendium of what we know about our natural satellite. There were those of us who feared at times that such an ambitious undertaking might prove to be beyond the capabilities of a single author. It is therefore pleasing to report that in this case the author's reach does not exceed his grasp. It might plausibly be argued that some of the areas covered by Garfinkle have been more exhaustively explored in other, more specialized monographs; but such a view would overlook the immense achievement of bringing together so coherently such a wealth of diverse information and focussing it on the needs of the lunar observer.

The volumes are well illustrated and beautifully produced by Springer, and all at an exceptionally attractive price. This is a work that should be acquired by everyone interested in our Moon, and Robert Garfinkle should be congratulated on an outstanding contribution to the literature on the subject. — BILL LEATHERBARROW.

Evolving Theories of the Origin of the Moon, by Warren D. Cummings (Springer), 2019. Pp. 311, 24 × 16 cm. Price £79·99/$109·99 (hardbound; ISBN 978 3 030 29118 1)

We should rightly be proud of our massive moon. Mercury hasn't got one, neither has Venus, and Mars has only acquired two tiddling captured asteroids. But our moon, at 1.23% the mass of Earth, posses a difficult problem. Where did it come from?

Cummings reviews the history of our attempts to provide an answer. It is a history that easily divides into two time periods, the pertinent date being Sunday 20th of July 1969. Prior to that day we mainly gazed at the Moon from afar. We knew that in the past the Moon had been much closer to the Earth, and over time had moved from an orbit just beyond Earth's Roche limit to its present distance of about 348 400 km. We also knew that the lunar mean density was about 75% of that of the uncompressed Earth — a figure similar to that of the Earth's mantle. On 1969 July 20 the USA's *Apollo 11* mission briefly deposited Neil Armstrong and Buzz Aldrin on the lunar surface and over the subsequent two and a half years about thirty suitcases of lunar material from six different sites were returned to Earth's science laboratories. Geochemists were delighted.

We quickly learnt that the Moon had been with the Earth for around 4·5 billion years, and that both bodies had very similar ages. Also the lunar material was depleted in volatile and siderophile elements. The four, old, origin theories were, however, all still possible. The first was fission. Here the early Earth was spinning about every 4 hours, became unstable, pear-shaped and broke into two bits, the smaller piece becoming Moon. The co-accretion proposal had both bodies being formed at the same time, in a common accretion event. Theory three had the two bodies being formed in different regions of the Solar System, and then the Moon being captured by Earth later on. Theory four involved a glancing collision between Earth and a single Mars-sized asteroid, and the resulting ejecta, a mixture of the incoming object and Earth's surface layers, coalescing to form our satellite. The blossoming of collision modelling using super-computers and theories pertaining to the death of the dinosaurs has done much to popularize this idea.

Cummings has meticulously trawled through all the historical works and the more recent scientific conferences on lunar-origin possibilities. Massive chunks of significant research papers are quoted in full. Even though Apollo was fifty years ago it is clear that the jury is still out. The story is not over yet. There are huge possibilities for future research, and it is far from obvious where new relevant data will come from.

I enjoyed this book greatly. It is well-written, well-illustrated, detailed, thorough, and aimed accurately at a postgraduate level. There were however two slight problems. Cummings was content to act as a reporter and not a critic. Also the type-face of the main text was very small and the type-face of the many quotes even smaller still. — DAVID W. HUGHES.

The Apollo Chronicles: Engineering America's First Moon Missions, by Brandon R. Brown (Oxford University Press), 2019. Pp. 269, 24·5 × 16 cm. Price £19·99 (hardbound; ISBN 978 0 19 068134 0).

Brandon Brown was born early in 1969, the son of Robert Brown, an engineer on the Apollo programme. Brandon became a professor of physics, performed research in superconductivity, and wrote several books on science history. A few years ago he suddenly became interested in spaceflight and his father's career. His father presented him with a box of mementos, and this sparked his curiosity. He started to read everything he could and interviewed dozens who were involved, among them the associates of his father who were still alive. The result of those efforts is this very readable book that, while not an official history, is reasonably accurate in what it covers, but is in no way complete. In addition to outlining the engineering features of Apollo — as the title implies — Brown paints a picture of the human aspects; every engineering project ultimately has people behind it! While technical histories describe the nuts and bolts, often in dry language, the human side is best a narrative. For instance, in describing the building of the Pyramids, how did the thousands of workers live? How many hours did they spend at work? How were their families housed? And many other questions. This book considers the human side of Apollo engineering.

Written in an easy-to-read style, without any typographical errors that this reviewer found, this book could be comfortably read in two or three evenings. The volume is organized into 16 chapters, preceded by a preface and acknowledgements, and followed by notes (which are combined for all the chapters rather than following them individually), a bibliography, and an index. For readers wanting a more detailed history, many of the books in the bibliography are worth pursuing.

A few errors came to my notice. (*i*) On p. 5 I found a common mistake relating to Max Faget (see ref. 1). (*ii*) Because the author concentrated on Apollo, the Gemini missions are not very well covered, and in many cases he does not identify them by number (1–12), which is how they are easily covered historically. On p. 105 he states "Some detractors believed it stretched NASA too thin or was simply a ploy to stay in the headlines". These couldn't be serious engineers. If not for the techniques developed by the Gemini missions, the Apollo programme would probably either not have been successful or would have required many more missions. The ability to keep astronauts alive and well for two weeks of weightlessness, techniques of the rendezvous of two spacecraft, the challenge of being able to enter and exit the spacecraft and perform outside maintenance, and docking and undocking procedures were just some of the skills taught by those earlier missions. (*iii*) On p. 75, a procedure is described for detecting hydrogen leaks. It does not make sense as written. From my early study of chemistry I understand that hydrogen can only be ignited by either an electrical spark, a spark caused by static electricity, or a direct flame. Would NASA, with all its emphasis on safety allow such a dangerous method of testing for leaks? Maybe this is just an old 'space tale'. It might be very interesting to write to NASA's historical division for clarification.

There may be other anomalies in this book that I have missed, but it is still worth reading for all the other information it contains. To paraphrase the great American humourist and columnist Will Rogers who, with some stipulations, wrote "I never met a man I didn't like", I *almost* never met a book I didn't want to read. — LEONARD MATULA.

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Galaxy: Mapping the Cosmos, by James Geach (Reaktion), 2019. Pp. 272, 22 × 17 cm. Price £15 (paperback; ISBN 978 1 78914 133 7).

This is the second edition of a work first reviewed in these pages (**137**, 140, 2017) and the reader is referred to that review for some valid comments on the value of the book. Although the author mentions the latest developments, such as gravitational-wave discoveries, in the introduction to this second edition, I can find no discussion of those topics within the main text, and certainly no inclusion in the index. But then the latter does not even include entries for 'black hole' or 'neutron star', which one might expect in connection with the discussion of gravitational waves.

As pointed out in the review of the first edition, the book is written for the popular market, not astronomers, but the author fails to explain some points, or rather, fails to make various connections that would help readers to understand some of the points being made. For example, there is the statement that "You may be more used to hearing about distances ... in light-years ... Actually ... astronomers tend to use the parsec ..."; and then "The nearest star to the Sun, Proxima Centauri is 1·3 parsecs away ....". What is not said (and it would help comprehension) is "... 1·3 parsecs (4·244 light-years) away ...". Parsecs (or rather kiloparsecs) are used exclusively in the list of distances given on page 254, although equivalents are quoted in kilometres. Similarly, the use of mid- to far-infrared to detect dust is mentioned, and a *Planck* image shown on page 32, where the dust appears white. Again, simply mentioning that the observations were made in the far infrared, would help understanding.

Then again, some statements are simply incorrect. One of the most basic occurs on page 11, where readers are told: "It takes a few minutes for your pupils to dilate ..." and night vision to occur. This is, of course, completely wrong. The pupils react almost instantly to changes in illumination, but it takes time (15–20 minutes or more) for rhodopsin to build up in the retina, enabling true dark adaption (night vision) to take place. On page 11 there is the heading 'Via Lactae'. This incorrect spelling is repeated in the text instead of the correct 'Via Lactea'.

Unfortunately the layout of the material is not particularly reader-friendly. The illustrations are not keyed into the text and although the placement is often reasonably relevant, large illustrations frequently break up the flow of material. There are no directions, so one has to hunt for the continuations of the text. For example, the text jumps from page 35 to page 47 and then again to page 54. The intervening pages contain large images of different types of galaxy. Although the images throughout the book have long, informative captions, these often merely repeat material in the text and the lack of cross-references is annoying. For example, the discussion of barred spirals is not keyed to the illustrations of typical galaxies that are found about 100 pages earlier. In another instance, the Spindle Galaxy (M 102) is mentioned specifically on page 155, but there is no reference to the large image (on unnumbered page 50), in an earlier section.

Although choice of the cover is usually governed by the publisher, it is a little difficult to understand why a photograph of an open cluster is used on the cover of a book on galaxies. I have been favourably impressed with works in Reaktion's 'Nature and Culture' series, such as *Earthquake*, *Volcano*, *Lightning*, *etc.*, but have some reservations about the editing of their later 'astronomical' series.

Not having seen the first edition of this work, I cannot comment on whether any inconsistencies and errors in that edition have been corrected, but the omission of any discussion of the latest techniques, despite their being mentioned in the introduction, is unfortunate, suggesting that this edition adds little, if anything, to the previous version. — STORM DUNLOP.

Advancing Astronomy for All: ASP 2018 (ASP Conference Series, Vol. 524), edited by Greg Schultz, Jonathan Barnes & Linda Shore (Astronomical Society of the Pacific), 2019. Pp. 336, 23·5 × 15·5 cm. Price $88 (about £69) (hardbound; ISBN 978 1 58381 935 7).

There seem to have been problems with encouraging the engagement of people with science for as long as I can remember. I have been involved in 'outreach', now more appropriately termed 'public engagement', for most of my career. In the early stages, it was simply trying to encourage school students to do physics at A-level and then university. With some success on that front, we then tried to address the still-unsolved problem of the relatively low numbers of female students. Now, quite rightly, we also need to think about the involvement of minority groups of various kinds. At the same time, we are being asked to demonstrate the effectiveness and value of what we do and move from what used to be a 'cottage-industry'-type approach to efforts that are more coordinated and of wider scope.

Astronomy has long been perceived to be a valuable tool for STEM (Science, Technology, Engineering & Maths) engagement and is now being utilized to support economic development. In 2018, the Astronomical Society of the Pacific devoted its 130th Annual Meeting to the topic of 'Advancing Astronomy for All'. This volume is the published output of that meeting, which focussed on issues of diversity and inclusion in astronomy. The content covers promotion of equity, culturally sensitive astronomy teaching and learning, multicultural astronomy, including arts in astronomy, and the use of social media to reach new audiences.

The volume is split into four parts: plenary sessions; workshops, discussions and special sessions; short oral presentations; poster sessions — within each is a mixture of topics. There is an enormous amount of valuable material but the completeness of the content is somewhat patchy. For example, some plenary-session papers are just short biographies of the participants, where I was expecting some kind of transcript or summary of the discussion. Other papers in all sections are just abstracts which provide little value. However, where the papers are presented in full, there are many excellent engagement ideas and much to be learned. This wealth of material makes a great contribution on how to engage, what to do, and how to measure it.

There is, inevitably, given the host organization for the meeting, a strong US bias to the material, but much of what is presented can be used in any educational setting and the main messages transcend national boundaries. I would definitely recommend these proceedings as essential reading for anyone doing public engagement in astronomy. I am already thinking about how to use some of the great ideas in my own work, and will be encouraging my colleagues to do likewise. — MARTIN BARSTOW.

Star Maps: History, Artistry, and Cartography, 3rd Edition, by Nick Kanas (Springer), 2019. Pp. 563, 25 × 17 cm. Price £39·99/$54·99 (hardbound; ISBN 978 3 030 13612 3).

The first edition of this work was favourably reviewed by Robert Garfinkle in Vol. 128, p. 318 (2008 August). Considerable changes were made to the second edition, and yet more have been incorporated into this third edition. Apart from shifting to a hardback, one of the main changes is the incorporation of the illustrations (most in colour) into the text, in general in places where they are first discussed.

I started to read this book with eager anticipation, but the more I read the more I struggled, both physically and subjectively. Physically? Unfortunately the binding of my copy is so tight that it takes physical effort to forcibly hold the pages open to read into the gutter, and if one lays the book down, open, the pages and even the cover immediately start to close. As for the content: in the Preface, Kanas says that the book is aimed at amateur astronomers; antiquarian map collectors; and anyone interested in the beauty and history of star maps. Regrettably, the information for the antiquarian map collectors seriously interrupts the flow of the text. Every time a map is described or illustrated, we are given in the text or caption (or both) its physical page or map size in cm (or the diameter of any planisphere), its projection, and sky coverage. Such information would be better (and probably far more usable) tabulated in a separate appendix. The first four chapters are historical in nature, although much of the content will be familiar to any astronomer who has, or has read, anything on the history of astronomy, such as Dreyer's *A History of Astronomy from Thales to Kepler* and, on the constellations, works such as Ridpath's *Star Tales* (especially the revised and expanded edition of 2018). As such, these chapters seem somewhat superfluous. The history of the development of telescopes, described in later chapters, will also be familiar to most astronomers.

The criticism of the information directed at antiquarian collectors particularly applies to the descriptions of all the different publications in Chapters 5, 6, and 7. In general, every author has a heading. This is followed by a sub-heading for 'Life and times', and sub-headings for every publication. Although there is some overlap, most of the information of interest to astronomers (amateur or professional) comes under the first sub-heading, despite much of the interesting biographical information being summarized in Appendix B. Again, much of the information under the sub-headings relating to the publications would be better collected together in a suitable appendix. The same problem also mars subsequent chapters.

Chapter 8 ('Special topics') covers a wide range of topics, including globes, telescopes, playing cards, and illustrations of non-stellar objects, together with a rather cursory examination of lunar mapping.

Then I am not too happy with the quality of the information. There are many typos and other errors (some elementary such as 'principles' for 'principals' or the omission of the umlaut on 'Boötes'), simple spelling mistakes, and instances of careless writing: advances occur by 'quantum leaps', for example. The description on page 313 is of Halley writing a book on parabolic orbits, whereas Halley actually described cometary orbits as ellipses. Then there are some silly errors: 'Coup de Gras' for example, and the caption for Figure 12.27 which states that one of the postage stamps shown commemorates 'the first satellite to take photographs of its dark side', whereas the stamp clearly states (in Russian) 'first photograph of the far side of the Moon'.

Although in general the illustrations are reproduced well, the chance has not been taken to show enlargements of sections of interest. As a result, features are sometimes discussed that are utterly invisible and sometimes significant features are not discussed at all. The caption to Figure 6.4 (page 176) tells us that the path of Halley's Comet is visible at upper right, together with the location of Kepler's 'nova' [*sic*] in Serpentarius and that P Cygni is labelled as a variable. But I am unable to find Halley's Comet's path, and although I do know where Kepler's supernova is located, P Cygni is definitely not labelled at all. But some puzzling features of that plate are not mentioned. What are the parallel lines extending from Pegasus towards Scorpius? Their annotation is illegible. Somewhat similarly, the fact that Figure 4.2 (page 124) shows 'Canobus' [*sic*] as the star at the end of Eridanus, rather than Achernar, is ignored as are the generally linear constellation boundaries in Figure 10.2 (page 388) from *Steiler's Hand-Atlas* of 1905, long before the IAU's decision on the matter in 1922. Another type of inconsistency is shown by the text (page 192) that describes how '... reminiscent of Piccolomini (see Section 5.5) the direction of celestial north was indicated by an arrow ...' But Section 5.5 talks about Piccolomini the man, not about how his charts indicated north.

One annoying quirk is the constant use of the phrase "According to Warner". Although Deborah Warner's *The Sky Explained* is an accepted, reliable, academic work, the repeated use of this term (an indication of inadequate research by the author) suggests that other interpretations are valid. There is also a tendency to rely upon sources that are generally regarded as somewhat shaky in the light of more recent research (Richard Hinckley Allen's *Star Names* comes to mind). Kanas praises Dava Sobel's popular work *Longitude*, but ignores other, more academic works on the 'Longitude Problem', such as Gould, Quill, or Howse.

Chapters 11 and 12 are new to this edition. They are devoted to pictorial maps and to celestial images in paintings. Frankly, I find them both rather odd, in that they include any object that contains an image that might be taken to have any vague connection whatsoever with astronomy. Chapter 11 includes a section on fruit-crate labels (and a historical summary of the Californian fruit industry), while Chapter 12 includes not only Crivelli's *The Annunciation* (where I am unable to find any astronomical or celestial relevance), Holbein's well-known *The Ambassadors* (with its depiction of astronomical instruments), modern, vaguely celestial paintings (of which one can buy giclée prints — Wow!), but also postage stamps, and drawings of an eclipse and Saturn by the author's grandchildren.

Appendix A is on collecting celestial maps, including a list of dealers; Appendix B gives short biographical notes on various individuals (many covered in separate sections in earlier chapters); Appendix C is a listing of the plates in eight major atlases, while Appendix D is a note on the British Library's 'King's Topographical Collection', all followed by a Glossary.

In summary, this is a work that contains many items of relevance to astronomers, who will find the illustrations of atlases and charts of particular interest, albeit with many failings in the text and descriptions. It is probably of more interest to collectors of antiquarian maps (and even they will have some problems using it). — Storm Dunlop.

Astronomy in Focus: As Presented at the IAU XXX General Assembly, 2018, edited by Maria Teresa Lago (Cambridge University Press), 2020. Pp. 605, 24·5 × 16 cm. Price £98/$130 (hardbound; ISBN 978 1 108 48873 0).

I am three-quarters as old (b. 1943) as the IAU, which chose to celebrate its centenary a year early in Vienna, in 2018 August, and scientifically half its age (PhD 1968). In my years of membership (from 1973, but first participation 1970, Brighton, UK), I have seen the formats of its triennial General Assemblies through two major changes. Once upon a time, events were driven by 30 or so discipline-focussed Commissions, which actually did a bit of organizing of international projects (the original stated goal of the Union). Evenings included a couple of invited discourses, and there were symposia before and after the GA in 'nearby' locations. Then the Symposia were crammed inside the GAs, commission time greatly reduced, and most other sessions had to be 'joint commission' sessions, proposed by two or more. The present arrangement is that there are still Symposia (about six in the two weeks), a scrap of time for Commission business sessions (into which many squeeze a bit of science), and a large number of Focus Meetings, which can be proposed by any handful (up to 15 or so) IAU members with the sponsorship of one or more Divisions. There is no longer any requirement for IAU membership or a formal invitation from the Union for speakers or the SOCs of the Symposia and Focus Meetings.

The deadline for proposals for 2018 was 2016 December 15. Thirty-two Focus Meetings were proposed, and 15 selected by the IAU Executive Committee, with advice from the Division Presidents, most of whom polled their own executive committees (including Commission presidents, who in turn often polled their committees). The goal of organizing international collaborations to do astronomy has faded to the point that the topic appeared in only one FM proposal (here reported as FM 13). The voting for proposals to be chosen was supposed to be based on the excellence of the science to be reported, not on an organizational prospect. The instructions also asked for a wide mix of nationalities, generations, and genders both on the SOCs and among the invited speakers.

My goal in organizing this review has been to try to pick out one presentation (or poster) from each Focus Meeting that I wish I had been able to hear or read, but did not, owing to a meeting-wide average of about eight parallel sessions on most days. Here is the list I came up with from what will surely be my last IAU GA.

FM 1. Centenary of discovery of asteroid families by K. Hirayama. A. Kazantsev (Kyiv, Ukraine): Identification of members can be improved by use of albedo data.

FM 2. Material not received by press deadline.

FM 3. Radio Galaxies. N. Birkinshaw *et al.* (Bristol, UK): Extreme distortions, bends, and knots in jets are sometimes due to interaction with gas structures in the IGM *via* magnetic fields.

FM 4. Magnetic fields in star formation. N. Juvela (Helsinki, Finland): Dust is important, and *Planck* polarization data show that magnetic fields are often perpendicular to filaments.

FM 5. Historical observations and transients. Your author was involved in this one and so picks two highlights: A. Hunger (Vienna): There are readable cuneiform records of Halley's comet, meteors, eclipses, and transits (and somebody had to transcribe those tablets!!!); F. R. Stephenson *et al.* (Durham, UK). Yes, old eclipse records show that the rotation of the Earth is slowing down, but not by as much as tidal friction would predict.

FM 6. Galactic Angular Momentum. P. J. E. Peebles (Princeton, NJ): History and current status; in particular why do we see so many bulge-less discs when models predict almost none?; S. Pedrosa (Buenos Aires): How angular momentum evolves in Λ-CDM universes.

FM 7. Composition gradients in galaxies. J. S. Zhang (Guanzhau): O^{18}/O^{17} rises outward in Milky Way ISM from 3 to 20 kpc.; CO data.

FM 8. Extragalactic magnetic fields. K. Subramanian (Pune, India): Origins: Dynamos *vs*. Early-Universe relic still undecided.

FM 9. Solar Irradiation. R. D. D. Sokoloff *et al.* (Moscow): Solar magnetic-field tracers with and without correlations to spot cycle; H. Maehara (Kyoto): Spot-flare correlations in solar type stars.

FM 10. Nanodust. J. Cami (U. Western Ontario): Fullerenes occur in PNe, but only in young ones (why?).

FM 11: *JWST*. B. Meinke (STScI) & S. Milam (GSFC) plus individual abstracts: Status of four instruments to cover 0·6 to 28·8 μ.

FH 12. UVOIR calibration. H. Fukugita (Tokyo & MPIfA Munich): 17 black-body stars in SDSS sample of $0{\cdot}6 \times 10^8$ (presumably DC WDs) show colour calibrations are very good.

FM 13. Global Coordination of Astrophysical and Heliospheric Physics, Space, and Ground: Most major missions were represented. Many of the schedules they presented are now on hold. D. Spergel (Princeton) provided a marvellous summary of the meeting, which the editor felt did not comply with the requirement for individual presentation reports.

FM 14. IAU's role in global astronomy outreach. G. Ifrate (Trieste): On the importance of the Virtual Observatory for this purpose; S. Blickhan *et al.* (Adler, Chicago): Citizen Science (I think this is an unambiguous good) and astrotourism (yes, important for developing countries with few other resources, but ominous for carbon footprints unless can be made virtual — which would not be much fun!)

FM 15. Astronomy for Development. D. Y. Yuna & P. W. Premadi (Lembang, Indonesia): Recommended sites as sustainable astrotourism for Indonesia.

FM 14 and FM 15 (above) were among the proposals for which many voting members of Division and Commission committees said. "Yes! We must do this. It is now an important part of what the IAU stands for!" That both received many submissions shows that there is a significant community that agrees.

Conflict of interest statement: I paid for my copy of *Astronomy in Focus* as part of the registration process for IAU XXX because of having a presentation scheduled in FM 5. My share of the Proceedings is slightly less than one page of the more than 600, and it would have had to have been printed on gold leaf to have been a good investment (adopting the view that most of us buy conference proceedings, if at all, in order to have complete sets of our own publications). — VIRGINIA TRIMBLE.

A PUBLISHER'S NOTE

On page 106 of the 2020 June issue, we published a review of *Origin and Evolution of the Universe*. While the content was generally welcomed by the reviewer, some adverse comments were made about the production. The Editors have subsequently learned that the copy sent for review was an early version that is still being revised and expanded, and is not an actual representation of the intended final version. It is expected that a final copy will soon be available and that our reviewer will be invited to comment on that.

OBITUARY

Eleanor Margaret Burbidge (1919–2020)

Eleanor Margaret (née Peachey) Burbidge, always called Margaret, who died in San Francisco, California on 2020 April 5 was the first B of B^2FH, otherwise known as "the early universe made all the elements up to helium; Burbidge, Burbidge, Fowler and Hoyle made all the rest". Her co-authors on that notable 1957 paper, 'Synthesis of the Elements in Stars', were her husband Geoffrey R. Burbidge, Caltech nuclear physicist William A. Fowler, and Yorkshire co-inventor of the Steady State universe Fred Hoyle. She and Geoff shared the Warner Prize of the American Astronomical Society in 1959 and an RAS Gold Medal in 2005. The second woman Warner winner (say it three times quickly) was Sara Seager in 2007, and the first woman Gold Medallist had been Caroline Herschel (1828), followed by Vera Cooper Rubin (1996).

Other honours and achievements

Margaret had many other female firsts in her career, including co-editorship of *The Observatory* (1948), where her successors have been Katherine B. Gebbie, Carole Jordan, and S. Jocelyn Bell Burnell. Admittedly some pretty impressive guys have also held that position. Another of her UK firsts was as director of the Royal Greenwich Observatory for about 18 months in 1972–73; first woman, but also first in that post not to be designated Astronomer Royal. EMB was the first woman elected to the presidency of the American Astronomical Society (1976–78), at which time she took out US citizenship. Soon after the Burbidge's daughter Sarah was born (1957), she faced an emigration officer who said "what are you doing taking this American citizen out of the country without her father's permission?" because Margaret was still unambiguously British at the time.

While AAS president, Margaret had the chance to introduce the first woman to win the AAS's lifetime career award (the Russell Lectureship), Cecilia H. Payne-Gaposchkin. Margaret was, of course, the second female Russell Lecturer in 1984. She was the first woman president of the International Astronomical Union's Commission on Galaxies (1970–73), and I can still hear the pride in Geoff's voice when he said in Brighton that Margaret was the in-coming president of Commission 28. The second such was (three guesses) Vera Rubin (and I was third).

EMB received many other honours, large and small, but it was one she refused that made the highest waves. The American Astronomical Society inaugurated an Annie J. Cannon Prize (funded by a legacy from Cannon, who classified nearly all the spectra in the Henry Draper Catalogue) in 1934. Cecilia Payne-Gaposchkin was the first winner, and you may or may not have heard of most of the others. But the Committee selected Margaret for 1971. She wrote a very thoughtful letter, declining on the grounds that a prize for which only women were eligible was no longer needed or appropriate. Her stance very much impressed Beatrice Muriel Hill Tinsley, then a fairly new postdoc in Texas. Whether 'appropriate' or 'needed' can still be debated, but the AAS made the decision to ask the American Association of University Women to do the official awarding, with AAS advice, the prize to be limited to young women (under 35), who would need to apply for it, eliminating the possibility of another refusal. The first Cannon Awardee under the new rules (1974) was Beatrice Tinsley, exactly the same age as Payne-Gaposchkin had been when she won.

A subset of scientific societies have designated a separate class of membership for the supposedly especially distinguished. These are called Fellows by the American Association for the Advancement of Science, the American Physical Society, and, more recently by the International Society on General Relativity and Gravitation, the European Astronomical Society, and Sigma Xi. The RAS cannot do this, because we are all Fellows. Perhaps the most impressive should be designated members? In any case, the American Astronomical Society decided to have Fellows, with a legacy class of 233 (chosen by some secret method to which I am not party). But of these, the very first, announced in 2019 as the Inaugural Fellow, was Margaret Burbidge.

Margaret Burbidge, of course, also received many honours for which she was not quite the first woman, though generally among the pioneers. These included election to both the Royal Society (of London) and the US National Academy of Sciences, the Bruce Medal of the Astronomical Society of the Pacific, a US National Medal of Science, and asteroid 5490 Burbidge.

Margaret and the cosmos

We will come to the usual objective biographical data in due course, but the question I have been asked most often about Margaret Burbidge, both before and since her death, is "what does (or did) she really think about steady state cosmology, its later variants, and non-cosmological redshifts?" And the corollary, "to what extent did her views agree or disagree with those of Geoffrey Burbidge?" A third question was one graduate students asked each other long ago, "do the Burbidges talk about redshifts in bed?" Finally comes "and to what extent did their personal and scientific relationships affect one another?" The answers to three and four might well be "none of your bloody business!" By analogy with your present author and her husband, Joseph Weber, I will, however, suggest that the answer to three is "sometimes but not much", and to four "nearly always positively in both directions, for both parties, even when sometimes out of step with the great parade of mainstream opinion."*

Now what about her astronomical and cosmological opinions, and his? It happens that, among many other publications, joint and separate, each wrote an article for *Annual Review* on quasars and galactic nuclei fairly early in the now 57-year lifetime of QSOs and all: EMB — 'Quasi-Stellar Objects' (*ARA&A*, **5**, 399, 1967); GRB — 'The Nuclei of Galaxies' (*ARA&A*, **8**, 369, 1970). Then, at the stage in life that Isaac Asimov called "late youth", each wrote an autobiographical chapter: EMB — 'Watcher of the Skies' (*ARA&A*, **32**, 1, 1994); GRB — 'An Accidental Career' (*ARA&A*, **45**, 1, 2007). The four relevant volumes sit on my desk as I write this.

Back in the dear dead days beyond recall, both wrote that QSO redshifts could be cosmological (but with rapid variations setting a source size that presented very serious energy problems), or Doppler (but then there should be blue shifts if things were being expelled from other things), or somehow intrinsic to the sources, which were close and associated with normal galaxies (*à la* Arp). Perhaps surprisingly, the GRB of 1970 (p. 451) thought the generation of the 2·7-K background radiation in a primeval fireball was the most likely possibility. Both worried about the origins of the necessary magnetic fields for synchrotron radiation and about how the energy supply (from Hoyle–Fowler collapse in the EMB article; black-hole accretion in the GRB article) was transformed into the relativistic particles required. Neither was much impressed by contemporary arguments from M. J. Rees, L. Woltjer, and others that special relativistic effects could alleviate many of the problems with the cosmological-redshift interpretation.

A few decades later, EMB still describes cosmological-redshift QSOs as requiring very large energy releases from 'the central engine' as well as highly relativistic outflow of charged particles, which, of course, is all true. She also describes discrepant redshifts (*i.e.*, different ones for galaxies, QSOs, *etc.*, that seem to be physically connected, as 'well documented' by H. C. Arp and by GRB, A. Hewett, J. V. Narlikar & P. Das Gupta in a 1990 paper). She concludes that section by saying that she finds herself very much attracted by the Quasi-Steady-State Cosmology (Hoyle, G. R. Burbidge & Narlikar) in which material pours out of the centres of radio galaxies and QSOs as 'creation events' in the presence of very strong gravitational fields. She describes the appeal as both philosophical and observational. GRB is, not surprisingly, much firmer, saying that we live in a cyclic universe, in which creation processes take place in the nuclei of galaxies, for instance, *via* the C field described by Hoyle and Narlikar in the 1960s. He believed that this is closer to the truth than is the standard model of cosmology, although it is a very unpopular point of view. He agreed that science is a self-correcting process, but pointed out that it can take a very long time.

It is not customary to attach references to obituaries, and indeed the latter are often cited as sources for articles in encyclopedias and Wikipedia entries. But there is far more to know about EMB that I can possibly tell you here! The first next place to go is undoubtedly her *Annual Review* autobiography, mentioned above, and the second surely GRB's corresponding chapter. Not saturated yet? Then progress to a perfectly marvellous oral history of EMB conducted by David H. DeVorkin of the Smithsonian Institution, in 1978 July. It is to be

*One more analogy. On at least one occasion, GRB expressed the feeling that he had to some extent sacrificed his career for hers. And there is no doubt that, because VT worked on things much more portable than two-ton aluminium bars, JW bore the heavier burden of bouncing between Irvine and College Park for 28 years.

found from the web page of the American Institute of Physics and is the source of some of the more *outré* items and incidents mentioned below. Finally (for the moment at least), I recommend the 1977 oral history of GRB, also conducted by DeVorkin and also to be found from the AIP website.

Early life

But let us now step back in time to 1919 August 12, when Eleanor Margaret Peachey first saw light of day in Davenport, near Manchester, England. This counts as "the North" to the editors of *Nature*, but not to her family. Said family was supposedly of Huguenot origin, the surname Peachey suggesting that they had been fishermen. (Margaret wrote that somewhere, and assumed her audience would catch Peachey as *Pêcheur* without explanation.) They had always intended to call her Margaret, but thought the sequence Eleanor Margaret was more euphonious than Margaret Eleanor. Well, she probably said "sounded better" rather than "more euphonious", because among many other wonderful things, she was not pompous.

Father John Stanley Peachey was a chemist and an instructor at the Manchester School of Technology, where mother Marjorie Stott had been his student, and, of course, much younger. John Peachey died when Margaret was only about 17, and had been ill for several years before that, but one of his chemical patents (for a more rapid way of vulcanizing rubber) was sufficiently successful that the family lived in reasonable middle-class comfort through her time in college. It was Margaret's grandfather who gave her her first telescope; science books and a chemistry set (naturally!) came from her parents. There was a younger sister who did not go on to college but served in the Wrens (correctly WRNS, the Women's Royal Naval Service) during World War II, while Margaret was pursuing graduate education and doing some war work of her own on range finders and such.

Mother Marjorie moved the family down to the outskirts of London, where Margaret and her sister could play on Hampstead Heath (beloved by all readers of regency romances as the place where your carriage could be held up by robbers). Margaret attended first the Heysham School in Hampstead and then the Francis Hallam Church of England School near Regents Park, which she reached by bus from home. These were all-girls' schools of the type called private in the US and public in the UK, and a motherly effort at socialization also put Margaret in dance classes for a while, of the 'free expression', rather than ballet or ballroom sort. On the walks to and from the dance classes, mother got asked about large numbers, and Margaret was both disappointed and unbelieving when she was told there was nothing larger than a quadrillion.

The family had had two maids and a nanny, presumably long gone when Margaret passed the examination for university entrance in physics at age 15. The choice of University College London had been made by Margaret's mother, who did not want her daughter off in either Oxford or Cambridge. Margaret signed up for pure and applied mathematics, physics, and chemistry and began to slip behind as the result of an unhappy love affair with a slightly older guy from geology (which, however, left her with some knowledge of the subject).

The young scientist

Among the books that influenced her early were popular ones by James Jeans (a distant relative of her mother, whom she never met), Eddington, and, especially Max Born's *Restless Universe*. She received her bachelor's degree in

1939, having been mentored by Clive Gregory of UCL, whose obituary for *Quarterly Journal* was written by EMB. She mentions his late-life interest in the 'Fundamental Theory' ideas of Eddington. Elizabeth Williamson also provided guidance to the young Miss Peachey. She was a woman of independent means and an honorary assistant at UCO, starting at least by 1926. She appears in some of the annual reports from the Observatory, which appeared regularly in *MNRAS*. In 1926, for instance, she was "measuring radial velocities from contact prints of Yerkes spectrograms and getting numbers that agreed with published ones." And in 1928 she was part of a small UCO eclipse expedition to a site in Yorkshire. They took plates but got no useful images through the clouds. There had been another young woman, Violet Florence Wood, at the Observatory briefly in 1930 who taught school-level physics and mathematics for most of the rest of her life. C. C. L. Gregory wrote her obituary in 1946 for *MNRAS*. Gregory had already sent a number of (male) students to work with Leslie John Comrie while Comrie was director of the Nautical Almanac Office in London and, from 1937, head of his own Scientific Computing Service, Ltd. Margaret, understanding her mother's feeling that, once she had her degree, she should earn her own living, applied there, was selected, and accepted the appointment. Comrie took her to lunch once, and she fled back to University College and a graduate studentship, to work for a PhD, officially with Gregory, but with a good deal of scientific independence and responsibility for maintaining the small telescopes on the UCL grounds and the reflector at Mill Hill (the lenses of the refractors having been hidden away in case of bombing), because Gregory, though he remained in London, quickly slipped into war work on polar navigation and other items then classified. Just what happened at that lunch has not been recorded, but Comrie clearly did astronomy a great favour by sending Margaret back toward spectroscopy rather than measurements of parallaxes and proper motions (her undergraduate work) and calculation and compilation of tables (Comrie's work, which made a considerable contribution to the British war effort). Comrie was elected to the Royal Society (London) not long before his death in 1950, and the associated *Memoire* is a fascinating read.

Meanwhile, as it were, Ms. then Dr. (1943) Peachey was working on her thesis (the spectrum of Gamma Cassiopeiae — one of Comrie's last papers was a violent objection to the use of the three- and four-letter abbreviations for constellation names), maintaining the UCL and Mill Hill observatories, and contributing to the war effort by assembling range finders and other tasks. She took no classes there during the war, because none was offered, and lived with the Gregorys at Mill Hill, while her mother was an air-raid warden back at Hampstead. When another of the Mill Hill astronomers, R. W. Pring, returned to the fold, she generally did the observing (for parallaxes and such), and he measured the plates, the opposite of the division of labour employed at Harvard College Observatory.

Margaret started attending meetings of the Royal Astronomical Society while working on her PhD and thus encountered some of the noted astronomers whose books and papers she had been reading. She was impressed by E. A. Milne but described Sir Arthur Eddington as "a crabby little man" who gazed into a corner of the room and gave an uninteresting talk in 1941. Egad! "little" is puzzling. A photograph from the 1939 nova meeting in Paris shows him, in comparison to the other participants, as something like 5ft 10in*. And what

*Just possibly it was one of her words. At the 1975 meeting in Venice that celebrated Fred Hoyle's 60th birthday, someone asked what happened when a particular refrigerated case of drinks and snacks was empty. "Oh", said Margaret, "there's a little man who comes and fills it up every day." A moment of consideration, then "well, actually he is normal size."

could that uninteresting talk have been about? Admittedly, no one ever claimed that he was one of the world's ten most dynamic lecturers, but it was his 1919 'Eclipse Expedition' talk that Cecilia Payne said she could write out word for word after hearing it early in her time in Cambridge and which inspired her life-long desire to be an astronomer.

Now the *Magazine* which you now hold in your, I trust, no-longer virus-encrusted electronic or paper hands, has published accounts of RAS meetings ever since the presidency of William Huggins. And editor Stickland has bravely descended into the virtual vaults where reside back copies, finding there that Sir Arthur gave two talks at the RAS meeting on 1941 May 9, and no others that year. So if Margaret's memory of the year is correct, she heard either 'Ionization equilibrium in convective regions' or 'On the cause of Cepheid pulsation', or perhaps both. Each appears under the same title a bit later in *MNRAS*. 'Convective regions' is a sorting-out of some disagreement with Martin Schwarzschild (by then safely at Columbia University, following his German PhD), and Eddington had actually been the submitter of the paper by Schwarzschild that immediately precedes his own in the published version. Miss Peachey was then still working on parallaxes and proper motions, but also on her thesis about the spectrum of γ Cas in outburst, and perhaps had not yet appreciated the significance of convection for the line shapes she would later be analyzing for surface compositions. As for the cause of Cepheid pulsation, the trail that led from eclipses to rotation with spots and on to current understanding is too long and brachiated to follow here; and I am still wondering whether Eddington's proposal of a hydrogen-ionization zone as a leaky faucet or valve to control pulsation was an important step at the time. Ionized helium is now known to be more important.

Meanwhile through the war, Margaret continued to carry out her own research, assume responsibility for the ULO, assemble range finders, and put little dots of black enamel and lavender oil into holes on glass plates, to be used somehow in aerial reconnaissance. She had a position and a salary as first or second assistant at the Observatory, and she continued as second assistant after the end of the war.

Enter Geoffrey R., costumed as a gadfly

There had been, however, no courses, or at least no relevant ones, taught through most of the war at UCL. (Cambridge and Oxford, and for that matter at least some of the German universities were different.) Thus it was that in 1947 she was attending lectures by David R. Bates on the atomic and molecular structure of Earth's upper atmosphere. Among her fellow students was Geoffrey R. Burbidge, born in 1925 in Oxfordshire and a recent recipient of a first degree from the University of Bristol. They found mutual interests in opera, theatre, politics, the Cotswold countryside, and history, though his devotion to sports was always greater than hers. GRB received a UCL PhD in 1950 for work under H. S. W. Massey on the capture of μ mesons (cosmic-ray secondaries) by atoms and the resulting Auger transitions. They lived with the Gregorys at Mill Hill for the first year after their 1948 marriage, and he learned to assist with the observing during that period, becoming increasingly attracted by astronomy in the process.

Soon after their marriage, they attended the first post-war IAU in Zurich in 1948 August, and Margaret was given some observing time at l'Observatoire de Saint Michel, in Haute Provence, France, a much better site for optical astronomy than anywhere in the UK. They met colleagues and made friends wherever they went (indeed that was so throughout essentially the rest of their lives). Two from this period were particularly important. At the Zurich IAU had been Otto Struve, director of Yerkes Observatory (then also the operator of McDonald Observatory with its 82-inch in Texas). And they met Fred Hoyle in France, who was there for a meeting on astronomical turbulence.

The Wanderjahre

The Burbidges left London in 1951, and the next 11 years constituted an odyssey in search of two comparably respectable positions, including access to a decent-sized telescope in a decent climate. First came two years in the US, with her officially at Yerkes and him at Harvard, but each sometimes at the other's place. Then two years back in England, at Cambridge where they met William A. Fowler on his sabbatical, leading to two years in Pasadena, with (well-known story) Margaret holding a theoretical appointment at Fowler's Kellogg Lab and Geoff an observing fellowship at Mt. Wilson. She was the one in the 100-inch prime-focus cage (which she loved), and he was frequently down in the dark room, cutting big pieces of glass, and later film, into little pieces of glass and film, then sloshing them in developer, water, and hypo, after they had spent time upstairs with Margaret. The Burbidges stayed in the unheated Kapteyn Cottage (built for summer stays by Jacobus and Catharina in the years before the First War).

It was during the 'Pasadena period' that work by the Burbidges, Fowler, and Hoyle (who had first written about nucleosynthesis in stars before WW II) came together in the famous paper, written through summer 1957 (while Margaret was increasingly pregnant), and published in 1957 October in *Reviews of Modern Physics*, because the paper was much too long for *Astrophysical Journal* or *Physical Review*. Their daughter, Sarah, was born near the end of 1957, and in the 1978 oral history, Margaret expressed some regret that there had never been the right opportunity for her to have more children. Apart from the scientific issues, she was by then pushing 40, and probably even Margaret had a biological clock, although her long lifespan suggests that hers might have run somewhat more slowly than most!

The 'paper' record

But this is supposedly a scientific obituary, so let us take a look at EMB's publications between 1951 and 1962, when they finally settled together at the newly-formed University of California at San Diego, so called because it is in La Jolla. The Astrophysics Data System records only two Peachey papers in 1942 (γ Cas of course) and 1946 (with C. C. L. Gregory) on the spectrum of T Cor Bor, recorded with a four-prism spectrograph on the 24-inch reflector on an Ilford Zenith plate. Ilford was to astronomical photography in Britain what Eastman Kodak was to the American discipline, and was also the source of 'nuclear' (thick) emulsion plates used for cosmic-ray studies in Europe. From 1951 onward, there are many papers, most with GRB as co-author. The papers migrate from *MNRAS* (and occasionally *The Observatory*) to (mostly) *Astrophysical Journal* when the authors migrated. Most deal one way or another with stellar spectra. Early on EMB is generally the first author, later more often GRB, but also there is some correlation in the sense EMB first = more data, GRB first = more interpretation. ADS records 409 EMB papers from 1951 to

2006, with GRB a co-author on 177. Number two co-author is Vesa Junkkarinen, and these are mostly later AGN-related papers. Hit 'more' often enough, and you will find a list of 171 co-authors; 94 on only one paper (and probably dating from her work on the *Faint Object Spectrograph* for *HST*); Hoyle and Fowler are well down the list, at 11 papers each. And at nine up pops Donna Womble, who was a UCI undergraduate (in one or two of my classes) and who went on to graduate school at UCSD as one of EMB's students*.

Of the early (pre-B^2FH) papers, the most cited are (as of 2020 May 7) — and all with GRB — 1953 on the outer atmospheres of some Be Stars; 1956 an analysis of the magnetic variable α^2 Canum Venaticorum; 1956 on the chemical composition of five stars which show some characteristics of Population II; and (aiming dangerously toward eventual discord with mainstream astronomers) 1957, the source of radio emission in NGC 5128 and 1316, submitted from Mt. Wilson and Palomar for him and Kellogg Lab, Caltech, for her. NGC 1316 is Fornax A and the two sources are of the sort generally called core–halo (which might look quite different if viewed from some different direction). The Burbidges averred (correctly) that the emission is synchrotron, but also that the underlying energy comes from p–$\bar{p}$ annihilation making electron–positron pairs, conceivably from galaxy–galaxy (or galaxy–antigalaxy) collisions. Citation numbers for the three 'star' papers are 55, 66, and 54; and 33 for the radio galaxies. On the same day B^2FH was up there at 2369. These numbers are, naturally, lower limits, since there are publications not known in the world of the Astrophysics Data System.

The years 1957–62 saw them back at Chicgo and Yerkes, with access to the 82-inch telescope at McDonald Observatory (established in 1939). Margaret helped recommission an ageing spectrograph and turned it to stars, star clusters, and, increasingly, galaxies, putting the slit along galactic major axes to record rotation curves. In these five years, she acquired enough spectra that publishing their analysis and implications carried several years into their next location. GRB was a co-author, though not always the only co-author on most of the papers from this period. In 1957 came more items relevant to nucleosynthesis, then star clusters in 1958 (with a good many more citations), and on to the rotation and internal motions of NGC 5128 in 1959. B^2 also acquired a new collaborator.

Kevin H. Prendergast had received a 1954 PhD under Jan Schilt at Columbia, spent a couple of years at Yerkes, overlapping the Burbidges there, one year each at IAS, Princeton, and GISS, New York, and then returned to Columbia in 1963, where he remained. The first collaboration, in 1959, is on the rotation and mass of NGC 2140 (a barred spiral), and papers with parallel titles continued on until 1964, with Prendergast usually the last author. Vera Rubin was part of the team in 1964 (still using 82-inch spectra) before she settled in at Georgetown University.

The 'rotation and mass' papers all made clear that the numbers they found could pertain only to parts of their galaxies bright enough to show up on the spectrograms, still on photographic emulsions. How Vera carried the work on with early photo-electric detectors and sometimes larger telescopes, to find flat rotation curves and large mass-to-light ratios, is part of a different story.

*She moved on to a Hubble fellowship held at Caltech (1992–96), included on one paper an affiliation with MIRA (the Monterey Institute for Research in Astronomy), and currently turns up as a software engineer at Roland and Associates Corporation, somewhere in "the North" (meaning Salinas or Monterey, not Manchester). According to the Corporation web site, much of their work is for the military and probably classified. Well, Donna was always a very reliable gal!

Meanwhile, EMB and GRB (varying the order of their names on papers) had started looking at Stephan's Quintet (in 1959) and other apparently interacting galaxies, often from the Vorontsov-Velyaminov catalogue. Each was, by a wide margin, the other's most frequent co-author — something like 177 papers out of many hundreds over their careers and marriage. Geoff's number two was Adele Hewitt (at about 50 papers) and Margaret's Vesa Junkkarinen (a UCSD student and later colleague).

The California astronomers

Do you think their 1962 arrival at UCSD had solved the telescope-access problem forever? Almost, but not quite! Lick Observatory was deeded to the Regents of the University of California in 1888 and counted as an independent 'campus' (though it had no professors, or students, or classes). Similarly, a marine field station in La Jolla California had been part of UC since 1917, also counting as a 'campus'. It became the Scripps Institute for Oceanography, under Roger Revelle (an old friend of the Burbidges), from 1950 to 1964. He oversaw Scripps becoming part of the new UC San Diego (first graduate students 1960) and was part of the team that organized the Burbidge's status there. Meanwhile, Lick had been declared part of UC Berkeley in 1958, but both research and support staff had continued to live on Mt. Hamilton, with no formal teaching responsibilities. They were firmly opposed to actually moving to Berkeley, where there were already astronomers. Jumping ahead for a few lines, Lick became part of a yet newer campus, UC Santa Cruz (first undergraduates 1965) in 1968.

Meanwhile, not only were some UCB folks competing for Lick time, the 120-inch having gradually come on line over a couple of years from 1959, but UCLA began hiring astronomers, in addition to a meteoriticist named Leonard (after whom prizes are named) who had been there from time immemorial. Daniel Magnes Popper arrived in 1947, George Ogden Abell in 1959, and Lawrence Hugh Aller in 1962. A storm began to gather with UCLA and UCSD using Lick access as a recruiting tool. A Time Allocation Committee materialized, with a majority of non-Lick members, and somebody decided to start organizing all-UC astronomy meetings on spring weekends. And here was a new outstanding observational astronomer, Margaret Burbidge, asking for telescope time. "Grrrrumph", went the director, who shall be nameless here, "all right". "But there shall be only four astronomical campuses in UC — UCSC, UCB, UCLA, and UCSD, whose faculty and students are entitled to apply for Lick time."

In 1965 up popped another UC campus, UC Irvine, whose physics department did indeed attempt to use Lick access as a recruiting tool. And it was only after their first target astronomer was on board in 1971, that the Lick director wrote to her, saying she was not entitled to apply for time at Lick. This did not change until Robert Kraft became director and discovered, in 1981, that a newish 40-inch Lick telescope was undersubscribed and allowed astronomers from outside the UC system to apply for time on it. He also convened a quinquennial review committee for Lick (these tended to happen every 6 or 7 years, giving a new meaning to quin) and asked the UCI astronomer to serve on it. I will say for Bob that he had really not been aware of the situation, was duly embarrassed, and changed the rules to encompass all the UC campuses. UC Riverside, Santa Barbara, Davis, and even Merced now also have astronomers entitled to apply for Lick time. More crucially, when the *Keck* collaboration between UC and Caltech was set up, all UC campuses were firmly included. Margaret therefore became a pioneer in yet another way, for women faculty to use Lick (and its successors) and for all UC campuses to be included. She herself definitely used *Keck* and was rather more tolerant of the altitude than many of her much younger colleagues!

Of course, some folks never learn! Albert Whitford was director at Lick 1958–68 and rightly given credit for completing the 120-inch and its instrumentation and superintending the astronomers' move down from Mt. Hamilton to UC Santa Cruz (where they have some teaching responsibilities). He participated in a 1984 oral history, in which he stated firmly that the UC system had astronomers on four campuses (the astronomer of the fifth wheel had by then been a full professor at UCI for about four years).

Cosmology and controversy

To return to the science story, quasars — originally QSRSs — joined the astrophysical inventory in 1963, and the Burbidges were part of the story from the get-go. They appear as co-authors of three separate items in the proceedings of the first Texas Symposium (held in Dallas, in 1963 December). This is a bit misleading; many items in the proceedings are reprints of previously-published papers. Only she is listed among the participants, and she spoke about the original Hoyle & Fowler theory of gravitational collapse (meant for radio galaxies) and other possible mechanisms to release large amounts of energy in a hurry and get it into the form of radio waves. GRB was apparently also there, however, for a description of the meeting mentions his having proposed a series of triggered supernovae as the primary energy source (you need a few per year).

Much of their later work, both jointly and severally, was focussed on observations and interpretations of QSOs and their relatives. Radio and Seyfert galaxies were admitted into the family, which eventually split into many subclasses, based on radio to gamma-ray spectral distributions and lines, morphology, and types of variability. We have already looked at how Margaret's opinion on the nature of the QSOs and all varied through the years. And as I, and surely many others, have written elsewhere, she contributed more than enough to 20th-Century astronomy to have been entitled to hold firmly to her own views.

Before moving on to 'late-career' issues, let us deal with the directorship of the Royal Greenwich Observatory, which was indeed once upon a time at Greenwich, but had slowly and cumbrously moved to Herstmonceux Castle in Sussex starting in 1948. According to the oral history, it was while the Burbidges were in Cambridge in summer 1971 visiting Hoyle's Institute of Theoretical Astronomy that one of the 'great and good', Sir Brian Flowers (then head of SRC) suggested to Margaret that she should consider taking up the directorship upon the retirement of Sir Richard Woolley. She countered immediately that Geoffrey would make a much more successful observatory director (and his years in charge of Kitt Peak suggest she was probably right). But no, they wanted her — the observer — and promised a suitable SRC senior appointment for him, and "of course" the schooling for Sarah, by then about 14, would be much better. As for medical care, stay tuned … ! Many things went wrong: the promised position for GRB did not materialize and Sarah did not much like the UK. She and her father returned to UCSD (from which both EMB and GRB had requested only a leave of absence, being sensible folks) after only a few months. Margaret was not appointed Astronomer Royal for England, and the outgoing one, Sir Richard Woolley, Hermann Brück (the AR for Scotland), and other senior British astronomers violently opposed moving the 98-inch *Isaac Newton Telescope* to a better northern-hemisphere site, though examination of the observing logs (by GRB) had revealed that the *INT* was actually in use only 600–800 hours per year, *versus* more like 2000 for telescopes in the US and Australia.

EMB had actually already handed in her resignation when, driving from London back toward Lewes (to attend a nephew's wedding — remember that sister!), she was involved in a horrible automobile accident that smashed her *right* hip joint, leading to weeks of hospitalization. Unfortunately, someone had mis-labeled the X-rays, and Margaret spent many more weeks (including time in Australia and the return to California), using crutches to stay off her *left* leg. New X-rays sorted out which hip was which, and mercifully she recovered full use of both! On at least one occasion, safely back in California, she admitted there had been yet another problem with living and working full time in England: the limited opening hours of most shops just about required some sort of 'homekeeper'. In any case, she left England only about 15 months after arriving to take up the RGO job, and we were very glad to have her back!

The later years

The US did not, of course, completely spare her political issues! She was AAS president when the US Postal Service wanted every page of *ApJ* to say "this is an advertisement" because the authors had paid page charges for publication. Then there was the Equal Rights Amendment to the US Constitution, proposed in 1972 and leading to the one actual error in EMB's *ARA&A* autobiography. It's not that the Amendment had been ratified by all but three states; US Constitutional amendments require ratification by three quarters of the states, and the ERA had come within three of reaching that goal. The issue was whether the AAS should meet in states that had not (yet) ratified; and the issue died with the ERA in 1983 when the number of states that had approved was still shy of the necessary 38.

Her years in the presidential line of the American Association for the Advancement of Science (1982–84) also involved her in topics outside astronomy; she was a particular supporter of improving US education at all levels, including Project 2061 (whose goal was something like complete science literacy by the time of the next return of Halley's Comet). NASA's putting UCSD at the head of one of the *HST* teams, the one to build the *Faint Object Spectrograph*, required the creation of an Organized Research Unit — all three words having a hopeful aspect. Under the name Center for Astrophysics and Space Science, it gobbled up a good deal of EMB's time until two years before the launch of *HST* (1988) when X-ray astronomer Larry Peterson took up the burden.

Whatever her private views on the nature of large redshifts and the early Universe, Margaret's contribution to QSO and related spectroscopy remained very much part of the mainstream of the topic, for instance, co-discovery of QSO absorption lines in 1966 and the record-holder redshift of 3·53 for OQ 172 which held pride of place for nearly a decade after the 1973 observation.

Margaret and Geoff were both at Caltech for the 50th anniversary of B^2FH, both in fine and characteristic fettle. Geoff, in a wheelchair, explained that he had ruined his knees playing tennis, and gave a fascinating talk on the background and writing of the honoured paper. He displayed a relatively mild version of customary pique by interrupting my description of what Marietta Blau had written in 1942 (in Spanish) about the origin of helium (my topic was nucleosynthesis before B^2FH) with a loud "Virginia, where do you find these things?" And then, lest this might have been perceived by anyone as a compliment, "Virginia, why do you find these things?" She, delivering the afterdinner talk, was, as ever, the woman about whom I recently wrote in *Nature*, "If there was anyone who got to know Margaret and didn't like her for her intelligence, charm, and patience, I never met them." We shall miss both Geoffrey (who died in 2010) and Margaret, though perhaps in slightly different ways.

Her daughter, Sarah, and grandson were with her when she died in San Francisco on 2020 April 20. — VIRGINIA TRIMBLE.

Here and There

OH NO IT WOULDN'T

"The biggest question is when [Betelgeuse] will explode in a supernova." ... an expert on Betelgeuse ... said on Twitter. "It would be so incredibly cool." — *Daily Telegraph*, 2019 December 31.

INCREDIBLY HUGE

The blue nebula above was ejected in episodes from the single huge star — 200,000 times the mass of the Sun — at its centre. — *A & G*, **61**, 3.6, 2020.