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Reflections

Standing on giant shoulders: a personal recollection of the lives and achievements of eminent animal scientists 1965–2015

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Abstract. This article is a compilation of pieces that are part biographical sketches and part personal recollections of 18 scientists with whom the author was acquainted in three continents over almost 50 years. The subjects, from Australia, the United States and the United Kingdom, will be recognisable to many in the field, especially more experienced scientists. For younger scientists, the article also is intended to put a human face on a generation of famous researchers who otherwise would be familiar only as somewhat anonymous authors of classic papers and reviews.

Additional keywords: biography, research leadership, science history.

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Foreword

Like most people who have had long scientific careers, I have been fortunate to have worked with, or otherwise known, many brilliant scientists who have made outstanding contributions to knowledge in their disciplines. Perhaps I am unusually impressionable, but, as I enter my eighth decade, I still have the utmost admiration for the achievements of a group of colleagues in the broad field of animal biology and production who have influenced me directly as well as the wider scientific community and world at large. This article is a compilation of my very personal recollections of the human and professional qualities of these extraordinary people; it certainly is not meant to provide authoritative biographies, however brief. Wherever possible, I have sought to illustrate these qualities by an anecdote from direct experience. For established scientists, this should be an appealing reminder of people, places, times and scientific issues familiar to them. For younger scientists, I hope my writings will humanise a generation of famous researchers who otherwise would be known to them only as author names on classic papers and reviews.

Alan Bell, Woolgoolga, 2018

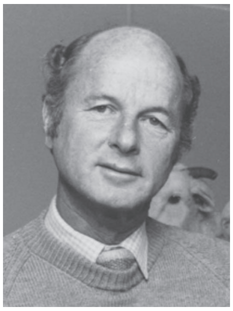
Introduction

> *If I have seen further, it is by standing upon the shoulders of giants*
> Sir Isaac Newton (1675)

This article consists of a compilation of pieces that are part biographical sketches and part my personal recollections of 18 scientists with whom I was acquainted as a student and career scientist in three continents over almost five decades. Some of these associations were short-lived and somewhat remote, others were close working relationships over a sustained period, and yet others were based on recurring contact, sometimes widely separated in time and place. The common theme is that each of these individuals not only influenced me personally but also made major contributions to the animal sciences, often with a great impact on livestock productivity and, in some cases, human health and well-being.

There is an obvious chronological aspect to my association with the subjects of this article, related to different stages of my own career, first as an undergraduate at the University of New England (UNE), Armidale, then in various scientific roles with the Commonwealth Scientific and Industrial Research Organisation (CSIRO), Australia; the Hannah Research Institute, Ayr, Scotland; La Trobe University, Melbourne; the University of Colorado Health Sciences Center, Denver, Colorado; and Cornell University, Ithaca, New York. However, for ease of reference, I have organised the compilation alphabetically by name and, within each piece, I have tried to emphasise a sense of time and place as well as the relationships of the subject not only with myself but with other subjects discussed in this collection. Selected references are provided to support biographical details and to give a flavour of scientific achievements.

George Alexander, FASAP 1926–



George Alexander was born into a family of well known Melbourne drapers. Holidays on his maternal relatives' farms in central Victoria and Gippsland drew him to study agriculture and he graduated with a first class honours degree in Agricultural Science from The University of Melbourne in 1947. George once told me that, as a student, his primary interest was in the plant sciences, but in those early post-war years, there were few jobs available for budding agronomists. He, therefore, took a position with the CSIR (later CSIRO) Division of Animal Health and Production at Parkville in Melbourne to assist in research on reproductive wastage in sheep under the leadership of Dr R. H. Watson. The Australian sheep industry should be very grateful that he made this choice and continued to work on the same large problem for his entire career with CSIRO.

George's first research achievement was the development of an assay for the oestrogenic activity of subterranean clover (Alexander and Watson 1951), which led to conferral of his MAgrSc (The University of Melbourne) degree in 1952. He must have shown exceptional research promise because, in 1949, he was awarded a CSIRO studentship to study fetal physiology, first with C. W. Emmens at the University of Sydney in 1950 and then with D. H. Barron at Yale University in 1951–1952. Barron was perhaps the most famous protégé of the great Sir Joseph Barcroft, with whom he had worked at Cambridge University, UK, before World War II, and was unquestionably the father of fetal physiology in the USA. While at Yale, George worked on maternal–fetal oxygen transport in sheep (Barron and Alexander 1952) and developed a life-long interest in placental function and its influence on fetal development. One of his Yale colleagues was Giacomo Meschia, a young medical researcher newly arrived from Milan, who was to become a preeminent prenatal physiologist (see later in this article). Many years afterwards, Giacomo told me that he considered George's later development of a simple surgical technique for manipulating placental size to demonstrate its influence on fetal growth (Alexander 1964) to be one of the great achievements in prenatal research. Since then, the so-called pre-mating carunclectomy technique has been widely used by biomedical researchers to explore mechanisms and influences of placental transfer of gases and nutrients in sheep (Morrison 2008).

George returned to Australia to resume working with Rodger Watson at CSIRO Parkville on the problem of another important source of reproductive wastage, neonatal lamb mortality. Diverse aspects of this research question were to consume him for the rest of his career, during which he became an international leader in the environmental physiology, energy metabolism and behaviour of newborn lambs as well as in development of management practices to reduce neonatal mortality.

In 1954, George transferred to the newly established CSIRO Sheep Biology Laboratory (later renamed the Ian Clunies Ross Animal Research Laboratory) at Prospect in western Sydney. By the time I joined his group as an Experimental Officer (graduate research assistant) in early 1969, he was well established as one of the leading scientists among a group that had quickly achieved international eminence in several areas of sheep biology and production. Unlike most of his colleagues, George did not have a PhD but had circumvented this doctoral deficiency in 1964 by becoming the first Melbourne agricultural science graduate to be awarded the degree of DAgrSc (Melbourne) for his exceptional research achievements.

In addition to his wide-ranging investigations of neonatal metabolism and physiology, ewe–lamb behaviour and causes of lamb mortality, George and my predecessor, David Williams, showed that the low birthweights of lambs born to ewes exposed to chronic heat stress during pregnancy are unequivocally due to retardation of placental growth (Alexander and Williams 1971). For producers, this work answered a practical question and defined the amount of heat stress likely to be problematic. It also offered another model of placental insufficiency and intrauterine growth retardation to biomedical researchers (Wallace *et al*. 2005; Morrison 2008). In particular, this was taken up by the Denver group during my sabbatical leave with them in 1983–1984 and has continued to be used by them to great effect over the ensuing three decades (e.g. Regnault *et al*. 2013).

After completing my degree in Rural Science at UNE, Armidale in November 1968, I was offered a PhD scholarship to work with Ron Leng, a rising star who had succeeded Frank Annison and Derek Lindsay (both reviewed in this article) as the research leader in ruminant nutrition and metabolism at UNE. However, I was unsure about embarking on a research career and decided to test the water by taking the CSIRO job with George Alexander. I also thought it would be good to gain experience away from UNE and, not least in my decision-making was the fact that my soon-to-be wife was teaching in western Sydney. I have never regretted this decision because, in my 3 years with George, I was exposed to a breadth of disciplines and research approaches, all directed at the problem of lamb mortality, that I never could have experienced in a university PhD program. Equally important was the example George set in work ethic (he firmly believed that success in research, as in other endeavours, is due to 90% perspiration and 10% inspiration) and devotion to improving agricultural productivity.

In the late 1960s and early 1970s, George was heavily committed to fundamental research on physiological limits to cold tolerance in lambs that included the anatomical and metabolic elaboration of the role of brown adipose tissue in the neonatal thermogenic response to cold (Alexander 1981). However, the research program each year included at least one major field trip to south-western Victoria, New England or New Zealand, to study factors contributing to lamb mortality under commercial farming conditions. Unlike some of his highly accomplished Prospect colleagues, George never forgot his responsibility to the sheep industry and firmly believed that regular interaction with producers was necessary to ensure that his research priorities were relevant and feasible.

Among the practical outcomes of George's field research were the introduction of sheep weather alerts by the Bureau of Meteorology and demonstration of the efficacy of grass shelter belts in reducing lamb mortality owing to cold exposure (Alexander *et al*. 1980). The latter work was mostly conducted with Justin Lynch at CSIRO's Chiswick research station outside Armidale, New South Wales. Another field-research highlight was a 1971 trip to the CSIRO National Cattle Breeding Station outside Rockhampton, to study genetic influences on heat tolerance of Brahman, British breed and cross-bred newborn calves. Unfortunately, that November was one of the most temperate on record in central Queensland and we saw little evidence of heat stress even in the pure *Bos taurus* calves. Little was I to know that, 35 years later, as Chief of CSIRO Livestock Industries, I would have responsibility for both the Armidale and Rockhampton laboratories and field stations.

George's field work convinced him of the need to better understand the behaviour of periparturient ewes and their offspring, leading to a sustained period of research that remains the basis for much of our present knowledge of lambing behaviour and the cues leading to establishment of the ewe–lamb bond (Walser and Alexander 1980). Although never formally trained as an animal ethologist, George's achievements have been acclaimed by other behavioural researchers and recognised through his appointment as a long-time editor of the *Journal of Applied Ethology* and his invitation to review the first 25 issues of the journal (Alexander 1982).

George had little time for self-promotion in himself or others, being a strong proponent of the 'deeds not words' philosophy. This and his down-to-earth, sometimes crusty, manner perhaps denied him the public acclaim sought by some of his peers. For example, during my time in the Prospect laboratory, there was a strict colour-coded hierarchy in the wearing of protective clothing. Thus, research scientists not working with animals donned white laboratory coats; when handling animals or performing surgery, they would deign to wear white overalls, whereas research support and farm staff wore brown overalls. George always wore brown overalls and, for the most part, ate and socialised in the general canteen rather than the upstairs 'blue room' restricted to the research scientists. As these habits suggest, he had little time for bureaucracy or institutional politics. However, towards the end of his career, he took on some important administrative tasks on behalf of the Division and the broader animal research community. These included formation and chairing of the Prospect Animal Welfare Sub-committee in 1986 and his inaugural Presidency of the Australian Federation for the Welfare of Animals in 1987 (Alexander 1988). After his retirement in 1989, George served on the Prospect Animal Care and Experimental Ethics Committee as an external member for several years.

Later in his career, George was disappointed that, despite the obvious practicality of much of his research, its uptake by the sheep industry could have been greater. There were many contributing factors, not least of which was the widespread resistance among woolgrowers to increasing labour and other costs during a prolonged period of price instability in their industry. In particular, the payback for increasing weaning percentages of Merino lambs was perceived to be low. Today, the situation is very different, with a marked shift within the sheep industry away from wool and towards lamb-meat production, and widespread recognition of the value proposition for increasing the size of the national flock. This has refocussed attention on improving lambing and weaning percentages via increasing twinning rates and decreasing lamb mortality (Schmoelzl *et al*. 2015). Much of George's pioneering work remains highly relevant to these goals and I predict that, in the long-term, his efforts will be vindicated.

Finally, as I reflect on the present state of the animal sciences in Australia in general, and in CSIRO in particular, I cannot help thinking that if more of our talented, scientifically productive researchers had, like George Alexander, been more responsive to the concerns of animal producers, things might have been different. However, being an eternal optimist, I am encouraged that industry funding bodies and some research providers are beginning to address the need for two-way communication between researchers and their industry stakeholders in a way that George would endorse and applaud.

Ernest Frank Annison, AM, FRSChem, FNSA, FASAP 1926–

Frank Annison was born in London and graduated with a BSc (Hons 1) in chemistry from the University of London in 1946 (Bryden *et al*. 1993*a*). He was awarded a PhD by the University of London in 1951, for his studies on the chemical nature of human blood-group gene products, under the supervision of Professor W. T. J. Morgan at the Lister Institute of Preventive Medicine (Annison and Morgan 1952). Soon after, he was appointed as a scientific officer at the newly established Agricultural Research Council (ARC) Institute of Animal Physiology at Babraham, Cambridge, UK. At the end of his first meeting with the Director, Professor Ivan De Burgh Daly, he asked what research he should undertake, to which Daly replied, 'You're the scientist, that's your decision'. How things have changed!

In any case, Frank began to study the biochemistry of rumen fermentation in collaboration with the ruminant nutritionist, Dyfed Lewis. Their separate and joint contributions to knowledge of volatile fatty acid and nitrogen metabolism in the rumen during the 1950s are neatly and informatively summarised in their 1959 monograph, *Metabolism in the Rumen* (Annison and Lewis 1959). Early in his tenure with the ARC, Frank was seconded for a year to the Rowett Institute at Aberdeen. There he had the good fortune to work with the eminent ruminant physiologist, Andrew Phillipson, and the Nobel laureate, Richard Synge FRS, on ruminal ammonia production and protein nutrition of sheep (Annison *et al*. 1954).

After returning to Babraham, Frank began what was to become a close, long-term collaboration with a newly appointed young biochemist, Derek Lindsay (see later in this article). Together, they used radiolabelled acetate to conduct a pioneering study of acetate kinetics in the sheep (Annison and Lindsay 1961), an approach they later exploited to investigate the flux rates and metabolic fates of other nutrients and metabolites at UNE in Australia.

In 1958, Frank was recruited by Dean Bill McClymont (see later in this article) to the position of Senior Lecturer in the Department of Biochemistry and Nutrition within the Faculty of Rural Science at UNE. There, an early research achievement was his development of a technique for measuring plasma non-esterified fatty acids in sheep (Annison 1960). These important vehicles of lipid transport and metabolism had been discovered in humans only a few years before and Frank's seminal paper was to be the forerunner of innumerable subsequent publications on non-esterified fatty acid metabolism in ruminants. This and other early achievements led, in 1961, to him being the first member of staff at UNE to be promoted to the rank of Associate Professor.

One of Frank's first PhD students at UNE was a young Yorkshireman, Ron Leng, who, after completing his doctoral studies in 1963, was appointed as Lecturer in the same department. During the early 1960s, Frank, Ron and Derek Lindsay, who had followed Frank from Babraham to UNE in 1960, conducted a comprehensive series of studies that provided much of the foundation of knowledge of the post-absorptive metabolism *in vivo* of energy substrates in sheep (e.g. Annison *et al*. 1963, 1967*a*). This impressive body of work resulted in Frank's award of a DSc from his alma mater in 1967 and would, alone, have been sufficient to cement his reputation as an international leader in his field, but much more was to come.

In 1964, Frank returned to his homeland to direct research on livestock nutrition at the Unilever Research Laboratory at Colworth House, Sharnbrook, UK, just a few months before I arrived at UNE to study Rural Science in early 1965. As detailed elsewhere in this article, there he conducted an extremely fruitful collaboration with his former Babraham colleague, Jim Linzell (see this article), on mammary metabolism of lactating goats, cows and pigs. Unfortunately, this partnership was cut short by Linzell's untimely death in 1975. During his 9 years at Colworth House, Frank also collaborated internally and externally with numerous other colleagues on topics that included poultry nutrition and metabolism (Annison *et al*. 1968; Bickerstaffe *et al*. 1970), amino acid metabolism and protein nutrition of sheep (Armstrong and Annison 1973) and lipid digestion in pigs (Freeman *et al*. 1968). At the same time, he mentored and helped develop the research careers of numerous colleagues who would go on to be successful in academia, government and private industry.

I first met Frank Annison not long after he had returned to Australia to take up the Chair of Animal Husbandry (later Animal Science) at the University of Sydney in 1974. We both attended the EAAP Energy Metabolism Symposium at Vichy in 1976, which was my first international conference. During the afternoon of the first session, I was on a high having survived presentation of the first two papers of the meeting under the chairmanship of the redoubtable Kenneth Blaxter (see later in this article), and had the temerity to question another speaker about some aspect of fatty acid metabolism. This was a topic about which Frank knew more than anyone else in the room and, possibly, the world. With typical bluntness, he took me to task and reduced me to silence for the rest of the session. However, realising that he may have been a bit tough on the upstart scientist, he immediately approached me afterwards and invited me for a beer. We were to become friends from then on and I especially appreciated his support and mentorship during my sometimes difficult 8 years at La Trobe University. Therefore, I had no hesitation in asking Frank to referee my application for a faculty position at Cornell University in 1984. The Chairman of the Search Committee and my new colleague, Dale Bauman (see later in this article), later told me that this support was a key factor because of his admiration for Frank.

During his 20 year tenure at the University of Sydney, Frank continued to be actively involved in research despite considerable responsibilities for teaching and administration, including long stints as Department Head and as Pro-Dean of the Faculty of Veterinary Science and also the Faculty of Agriculture. He supervised a steady stream of research trainees, many of whom went on to have successful academic careers, including Wayne Bryden, Christine Gow, Esala Teleni, Hutton Oddy, Peter Wynn, Glenys Hough and Mark Jois, some of whom later became academic colleagues and collaborators. He also engaged in productive collaborations with established scientists such as Graham McDowell, Jim Gooden, John Black (see later in this article) and John Nolan and influenced the research careers of many through his Directorship of both the Dairy and Poultry Research Foundations associated with Sydney University. Frank's influence on agricultural research policy and priorities was significant through his membership and chairmanship of various advisory boards and research councils (Bryden *et al*. 1993*a*). In particular, he championed the notion that pasture-fed dairy cows were failing to achieve their genetic potential for milk production, which helped lead to the major increase in supplementary feeding practices in the Australian dairy industry in recent decades.

Frank Annison's research and service achievements have been recognised by numerous prestigious awards including the International Roche Research Prise for Animal Nutrition (1990), Fellowships of the Nutrition Society of Australia (1991) and Australian Society of Animal Production (2002), and his award of Membership of the Order of Australia (2004). In July 1992, ~150 of his colleagues and friends from Australia and around the world gathered at the University of Sydney for a Festschrift Symposium to honour him and his long-time collaborator, Derek Lindsay. I greatly regret that I was unable to attend this notable occasion, proceedings of which were published in a special issue of the *Australian Journal of Agricultural Research*, including an insightful, futuristic review by Frank himself (Annison 1993). He retired from the University of Sydney in 1994 but continued to be engaged professionally for some years afterwards, including authorship with his former colleague, Wayne Bryden, of two substantial reviews on ruminant digestion and metabolism, published in the late 1990s (Annison and Bryden 1998, 1999).

Ransom Leland (Lee) Baldwin V 1935–2007

Lee Baldwin grew up on a dairy farm at Meriden, Connecticut, which, no doubt, contributed to his lifelong interest in lactation biology and dairy cattle nutrition and production. He obtained a BS in animal industries and an MS in dairy nutrition at the University of Connecticut, before completing a PhD in biochemistry and nutrition at Michigan State University, East Lansing, Michigan, in 1963 (Baldwin *et al*. 2010). His doctoral research, under the supervision of Professors W. A. Wood and Roy Emery, used ^{14}C-labelled substrates to define pathways of carbohydrate fermentation to volatile fatty acids in the rumen (Baldwin *et al*. 1962) and cemented an interest in rumen metabolism and function that continued throughout his career (Baldwin and Allison 1983).

Soon afterwards, Lee was hired as an assistant professor in the Department of Animal Science at the University of California at Davis, where he was to remain until his retirement as Sesnon Professor in 2001. He quickly established himself to be in the vanguard of a new generation of American dairy scientists who were seeking to understand metabolic processes and their regulation in key tissues of the lactating cow. These were to include Dale Bauman (see later in this article) at Illinois, Don Palmquist at Ohio State, Don Beitz at Iowa State, and others. At Davis, Lee's early research activities extended from rumen microbiology and metabolism to the intermediary metabolism and bioenergetics of adipose, mammary and other key tissues (Yang and Baldwin 1973; Baldwin and Louis 1975). I am not sure when his mind turned to mathematical modelling and computer simulation as a means of integrating physiological and metabolic data to predict production outcomes. However, as early as 1970, he had co-authored a conference paper on computer simulation of feed-energy utilisation in ruminants (Reichl and Baldwin 1970). This would be the first of many modelling publications (e.g. Waghorn and Baldwin 1984; Baldwin *et al*. 1987) including a widely used, authoritative monograph (Baldwin 1995) that established Lee as an international leader in the field.

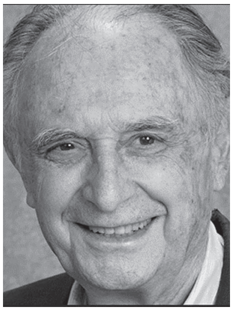
Lee was not content to use modelling only as a means of better integrating and understanding tissue functions; a major objective of the work was to develop a practical tool for predicting cow performance from nutritional and other inputs. The ultimate expression of these efforts was 'Molly', a dynamic model based on biochemical equations describing rumen fermentation and metabolic transactions in body tissues (Baldwin 1995). The model is named after a favourite cow on the Baldwin family farm (Baldwin *et al*. 2010). It continues to be a work in progress a decade after its founder's death, in the capable hands of several of Lee's former students and colleagues (e.g. Hanigan *et al*. 2013).

Lee was a committed teacher of courses on nutritional energetics and lactation biology to undergraduates and a diligent mentor of postgraduate students throughout his career. He was an early believer in using his own and others' research findings as a teaching tool, particularly to illustrate quantitative and analytical principles (e.g. Baldwin 1968; Koong *et al*. 1975). As his reputation grew, he attracted not only numerous talented postgraduate students and postdoctoral associates, but also established scientists from around the world. One of these was John Black (see later in this article) from CSIRO Animal Production at Prospect who, in the 1970s and 1980s, was developing models for ruminant and pig production in Australia. In 1975, Lee spent a sabbatical leave with John at Prospect, developing new concepts for modelling the growth and nutrient utilisation of specific tissues and organs (Baldwin and Black 1979). This was to be a pivotal experience and the basis for a longstanding international friendship and collaboration.

I had been introduced to Lee at ASAS/ADSA conferences in the 1980s, but I did not really get to know him until we spent a week together at the EAAP Energy Metabolism Conference in Lunteren, Netherlands, in 1988. It was then that I began to understand why Lee's colleagues and students were so in awe of his intellect and ability to relate complex physiological and metabolic functions to dairy cow production. This rare capacity was one he shared with his fellow National Academician and my close colleague, Dale Bauman. I continued to enjoy catching up with Lee at meetings over the next decade. The most notable of these was as part of the team he chaired for an in-depth review of the USDA/ARS Nonruminant Nutrition Research Group at Beltsville, Maryland, in 1994. Although more than two decades have passed, I will never forget the intense scientific discussions we had during a day together after the review.

Lee Baldwin was one of the international giants of the animal science community for much of the latter half of the 20th century. Among his many awards and recognitions was his election to the National Academy of Sciences in 1993, an honour rarely given to practical animal scientists. His legacy will live on in the literature and the achievements of the many students he trained and associates he mentored at the University of California, Davis.

Frederick C. Battaglia 1932–



Fred Battaglia was born in Weehawken, New Jersey, just across the Hudson River from Manhattan, New York. His degrees are from Cornell (BA 1953) and Yale, New Haven, Connecticut (MD 1957) and he interned in paediatrics at Johns Hopkins University, Baltimore, Maryland (Jones 2005). As a medical student, Fred worked in the laboratory of the eminent Donald Barron where he was to develop a life-long passion for prenatal physiology. There, he first met Giacomo Meschia (see later in this article) who was to become his long-time collaborator and later, brother-in-law (Battaglia 2005). During his internship at Hopkins, Fred's commitment to clinical paediatrics was cemented. This experience was interrupted by a fellowship to study metabolic biochemistry with Philip Randle at Cambridge University, UK, for a year. Randle (later Sir Philip) was a protégé of the pre-eminent biochemist, Sir Frank Young, and already an international leader in his field. These early influences on Fred were to shape a stellar career that blended seminal research on prenatal metabolic physiology and fetal nutrition, with major contributions to clinical neonatology.

Fred's determination to combine research with clinical practice was demonstrated by his willingness to travel by train from Baltimore to New Haven on weekends to continue research with Barron while completing his residency at Johns Hopkins. He then spent 2 years at the NIH-funded Laboratory of Perinatal Physiology in Puerto Rico, before taking a faculty position at the Johns Hopkins School of Medicine in 1963. This could have been a career appointment but, in 1965, he was recruited to the University of Colorado School of Medicine in Denver, Colorado, with faculty appointments in paediatrics and obstetrics and gynaecology. There, his drive and organisational talent led to the formation of the Division of Perinatal Medicine in 1970 that brought together clinical researchers from the Departments of Pediatrics, Obstetrics and Gynecology, and Physiology. These included Giacomo Meschia who had accompanied Fred to Colorado. Together, Fred and Giacomo, with other faculty colleagues and a long list of research fellows, were to embark on a collaboration lasting more than 30 years that centred on the use of sheep as a model for *in vivo* studies of placental function and quantitative metabolism of the conceptus.

In the late 1970s, I embarked on studies of the effects of undernutrition and exercise on maternal and feto-placental metabolism in pregnant ewes at La Trobe University in Melbourne. I had previously been trained in the requisite surgical and experimental techniques by George Alexander (see earlier in this article) while on study leave from the Hannah Research Institute in 1976. These were the same techniques pioneered by Meschia and Barron at Yale in the 1960s and adopted soon afterwards by Geoff Thorburn (see later in this article) at CSIRO Prospect. By 1983, I was eligible for a year of sabbatical leave and could not think of a better place to spend my leave than working with Battaglia and Meschia in Denver. This was to be a crucial, career-changing decision.

My family and I arrived in Denver in September 1983 and I was quickly immersed in experimental work. For some time, Fred and Giacomo had been planning to extend their research focus from late pregnancy, when the fetus is approaching its birth weight and rapidly maturing in preparation for postnatal life, to mid-pregnancy when the fetus has achieved only ~5% of its birthweight and has not long emerged from its embryonic state. They must have thought I had some capability because they offered me the opportunity to manage this challenging new project. Their faith was vindicated because the work went well and yielded a series of papers that continue to be widely cited to this day (e.g. Bell *et al*. 1986). In addition to managing this project, I was able to introduce the laboratory to the use of maternal heat stress to provide a model of intrauterine growth retardation, as pioneered by George Alexander. The ensuing study is detailed in the piece on Giacomo Meschia because of his closer involvement in the initial phase of this work.

While I was in Denver, Fred Battaglia's capacity for work across a diverse range of responsibilities never ceased to amaze me. In addition to being the Directorial glue that bound the multidisciplinary research of the Division of Perinatal Biology, he chaired the very large Department of Pediatrics from 1974 to 1988. Because of his administrative load and commitment to clinical rounds every morning, he was able to take part in sheep surgeries only in the afternoon, which required him to drive to the sheep research facility ~10 miles from his Denver office. At the same time he continued to teach and write at a prodigious rate. He once told me that it took him only an evening to write a full first draft of a paper, providing the data had been appropriately collated and analysed. In addition to his lead authorship of many original papers, Fred was a penetrating and feared critic of manuscripts first-authored by his research fellows and other collaborators. He also found time to co-author with Meschia a much-cited monograph (Battaglia and Meschia 1986) and to write numerous authoritative reviews on fetal nutrition and neonatal clinical practice (e.g. Battaglia and Meschia 1978, 1988; Battaglia and Thureen 1997).

Apart from a flurry of correspondence about publications after I returned to Australia in 1984, my contact with Fred became at best sporadic. However, as a proud alumnus of Cornell University, he was obviously pleased to learn that I had accepted a faculty position in the Animal Science Department of his alma mater. I cannot remember whether he was one of my referees for the job, but there is no doubt that my research successes in Denver, due in large part to his mentorship and support, contributed to the Cornell opportunity. Once I was established at Cornell, our paths rarely crossed because of our divergent societal and academic affiliations. However, I was delighted when Fred accepted my invitation to speak at a symposium on 'Amino acids in meat animal production' during the Annual Meeting of the American Society of Animal Science at Laramie, Wyoming, in 1991. His review of fetal and placental metabolism of amino acids in sheep clearly showed the relevance of his work not only to human paediatrics but also to the animal science community (Battaglia 1992).

Later, I occasionally saw him at Cornell reunions and he was as affable and enthusiastic about his research as ever. I will always think of Fred Battaglia with the greatest respect and gratitude for taking on an unknown young Australian animal scientist and giving him a career-changing research opportunity.

Dale Elton Bauman, FADSA, FASAS, FASN 1942–

Dale Bauman was born in Detroit and grew up on his family's dairy farm at Brown City, Michigan. His parents were progressive farmers and the farm frequently was involved in field research trials run by scientists from Michigan State University (MSU). As an undergraduate at MSU, Dale was a student employee at the university's research dairy, assisting in field research projects and working as a certified milk tester. These experiences gave him an early appreciation of the variation in the quality of management on dairy farms as well as in productive efficiency among farms and individual cows. Dale received both his BS (1964) and his MS (1968) degrees from MSU. His Master's research, supervised by J. W. (Bill) Thomas, examined the differential passage of dry matter and chromium in the sheep gastrointestinal tract. He then went to the Department of Dairy Science at the University of Illinois, Urbana-Champaign for PhD training in the laboratory of Richard Brown and Carl Davis. His doctoral research on pathways of mammary fatty acid synthesis in the rat, sow and cow (Bauman and Davis 1970) was to set the scene for the next decade of his research career.

On the award of his PhD in 1969, Dale was appointed as assistant professor in the same department. He then embarked on a series of systematic studies of the pathways and regulation of lipid metabolism in adipose and mammary tissues of ruminants that would distinguish him from the mainstream of animal scientists in the USA and establish his national and international reputation. The key findings of this research are summarised in two seminal reviews published in the mid-1970s (Bauman and Davis 1974, 1975). Of particular significance was Dale's identification of the isocitrate pathway as a significant alternative to the pentose pathway for production of reducing equivalents needed for *de novo* lipogenesis in ruminant tissues (Bauman and Davis 1970).

In 1978, Dale returned to his alma mater at MSU for a sabbatical leave with Allen Tucker, intending to enhance his understanding of the endocrine regulation of metabolic processes. There, he worked with Tucker and his students on the role of prolactin in mammary development during the periparturient period (Akers *et al*. 1981). Around this time, senior faculty members in the Department of Animal Science at Cornell University, led by Murray Elliot, were becoming concerned that their traditionally strong department was losing its research edge. They persuaded their Chairman, Bob Young, and Dean, David Call, to pursue a targeted recruitment of Dale Bauman, the young, emerging star from the University of Illinois. Dale had been treated well at Illinois and felt loyal to both the institution and highly esteemed colleagues such as Carl Davis and Jimmy Clark. However, the Cornell offer was compelling and Dale and his family moved home to Ithaca, New York, in late 1978.

Soon after taking up his Cornell appointment, Dale met Bruce Currie, a young New Zealander who had been appointed to the Animal Science faculty only months before. I had become friendly with Bruce when he commenced his PhD studies with Geoff Thorburn (see this article) at CSIRO Prospect in 1971. After graduating, Bruce moved to North America where he undertook postdoctoral training at Ottawa and St Louis, before his Cornell appointment. Despite (or perhaps because of) their different academic backgrounds, Dale and Bruce immediately hit it off and soon agreed that the time-honoured concept of homeostasis was inadequate to account for the regulation of longer-term metabolic adaptions that enable the transition from one physiological state (e.g. pregnancy) to another (e.g. lactation). Their intense discussions led to their publication in 1980 of a review that proposed the concept of homeorhesis to explain chronic shifts in metabolic regulation (Bauman and Currie 1980). This landmark paper continues to be highly cited after almost 40 years. A central feature of the new concept was the endocrine coordination of disparate metabolic adaptations in multiple tissues to achieve a prioritised functional outcome. Several pieces of evidence pointed to growth hormone (also known as somatotropin) being a putative homeorhetic influence on lactation performance, including war-time studies in England. However, the limited availability of the native bovine hormone made this a difficult hypothesis to test. Dale managed to overcome this impediment by persuading the NIH to supply him with a large fraction of their stocks of pituitary-derived bovine growth hormone to demonstrate an impressive efficacy (Peel *et al*. 1981). This, in turn, attracted the interest of corporate scientists who quickly generated a recombinantly derived molecule that was chemically almost identical to the native protein and just as effective at stimulating milk yield in long-term studies (Bauman *et al*. 1985). There followed a plethora of papers on mechanisms of action co-authored by a succession of bright, highly motivated graduate students and postdoctoral associates, which were later reviewed by Dale and our mutual friend from the Hannah Institute, Dick Vernon (Bauman and Vernon 1993).

I was aware of Dale's growing reputation through his publications during the 1970s. I also knew about the early, unpublished success of the growth-hormone study from one of its key participants, Colin Peel, who had returned from Cornell to the Dairy Research Institute at Ellinbank, Victoria, Australia, after working on his PhD with Dale. However, I did not meet Dale until 1984 when, towards the end of my sabbatical leave in Denver, Colorado, I visited Cornell for the first time. My contact there was Dale's colleague and my former Prospect friend, Bruce Currie. I quickly discovered that they both were on a search committee, Dale as chair, for a faculty position in nutritional physiology. I never saw the actual job advertisement but before I returned to Denver I left them a copy of my CV. Several months later, after my family and I had returned to Melbourne, I received a phone call from Dale at 0300 hours (he had mixed up the time-zone difference) inviting me to come to interview at Cornell. This I did in November 1984 and was offered the job soon after. However, I was advised to not emigrate until I had been issued a green card, which took almost exactly a year. In the meantime, Dale and I renewed our acquaintance at the EAAP Energy Metabolism Symposium at Beltsville, Maryland, in September 1985. Afterwards, he drove me back to Ithaca, a trip punctuated by numerous stops to fill his bottomless coffee mug and much discussion of science and many other topics. This was the beginning of a close scientific and personal friendship that has lasted over 30 years.

When I finally took up my position at Cornell in late 1985, Dale had recently received one of the most prestigious honours offered to US agricultural scientists, the Alexander von Humboldt Award for research of the greatest significance to US agriculture. Many other important awards were to follow, including his election to the National Academy of Sciences in 1988. However, Dale's research success with the first application of a recombinantly derived product to animal agriculture also aroused vehement criticism, some of which was personalised and extremely hurtful to him and his family. He did not allow this to affect his team's research progress or his active involvement in other academic duties. Nevertheless, there was a cost that manifested as a sudden inability to engage in public speaking. This phobia, which lasted for several years, was all the more unfortunate because Dale was (and has again become) a powerful and compelling speaker in fora that ranged from extension updates to farmers to plenary speeches at international scientific meetings.

I have long maintained that Dale Bauman has had three research careers, each of which alone would have made him famous. The first two, on pathways of fatty acid synthesis in ruminants, and use of exogenous growth hormone to increase milk yield, have been discussed. The third, begun in the late 1990s, involved a huge volume of work and hundreds of publications on the multiple and disparate biological properties of various isomers of conjugated linoleic acid (CLA) and *trans* fatty acids (TFA) found in ruminant tissues and their food products. These include the anti-carcinogenic (Corl *et al*. 2003) and anti-diabetogenic (Ryder *et al*. 2001) effects of the most abundant CLA isomer, *cis*-9, *trans*-11 CLA, in rodent models of human pathobiology, and the lack of adverse effects of natural TFA in dairy products on cardiac health, in contrast to those elicited by TFA generated by industrial biohydrogenation of polyunsaturated fatty acids (Lock *et al*. 2005). Dale and his team also demonstrated the involvement of the *trans*-10 C18:1 isomer in the aetiology of the low milk-fat syndrome in dairy cows fed high-concentrate, low-forage diets. The latter research led to postulation of the 'biohydrogenation theory' to explain milk fat depression, which has convincingly supplanted previous hypotheses (Harvatine *et al*. 2009), including those proposed by several other people in this article. Dale's late-career involvement in research that is highly pertinent to human health has led to an explosion of biomedical interest in his work, such that he has a career total of over 30 000 citations with a current *h*-index approaching 100. Dale's final major research contribution, with Judith Capper and others, has been to demonstrate the substantial environmental benefits of increases in animal productivity, such as those achieved in the dairy industry by genetic selection over six decades or by treatment of cows with a recombinant growth hormone (Capper and Bauman 2013).

In addition to his stellar research career, Dale made important contributions to the teaching and mentoring of undergraduate and graduate students at Cornell University. He was a long-time instructor of an upper-level undergraduate course on nutritional biochemistry and various specialist graduate courses, including 'Bioenergetics and Nutritional Physiology' that he and I co-taught for many years. He also advised 40 graduate research students and mentored 19 postdoctoral fellows and visiting scholars. Dale always took great pride in the achievements and career development of his advisees who, at last count, include a university Vice Chancellor, at least two Deans, and numerous Department Chairs and corporate senior executives. A protégé of whom Dale and I are jointly proud is Frank Dunshea, Redmond Barry Distinguished Professor and Associate Dean of Research in the Faculty of Veterinary and Agricultural Sciences at The University of Melbourne. Frank arrived at Cornell in the late 1980s, after completing his PhD with me at La Trobe University, intending to undertake postdoctoral research on lactating cows, with Dale as his mentor. However, Dale and our colleague Dean Boyd had commenced studies on the endocrine regulation of growth in pigs and Frank was given responsibility for managing much of this work. This he did with great success (e.g. Dunshea *et al*. 1992) and, after returning to Australia, soon became an international authority on the nutrition and growth biology of swine.

No account of Dale Bauman's career would be complete without mention of his professional and public service. He excelled at extension presentations to dairy producers and explanations of science and technology to the general public because of his ability to distil scientific complexity into simple, clear messages for lay people. He also served on numerous national committees and advisory groups and provided great service to several scientific societies, including Presidency of the American Society for Nutrition (2002–2005). Most nationally significant was his membership (1990–1997) and chairmanship (1993–1997) of the Board on Agriculture and Natural Resources of the National Research Council. During Dale's tenure, the Board advised the President and Congress on issues that included the environmental and public health effects of agricultural practices, drug use in food animals, the food-safety system, sanitary and phyto-sanitary standards in international agricultural trade, the future role of agricultural colleges in the Land Grant System and investing in the National Research Initiative. In recognition of these efforts, the Board took the unusual step of publishing a laudatory report on Dale's contributions as Chairman (NRCBA 1997).

Finally, as a long-time colleague and friend, I must make some personal comments about Dale. First, his astonishing intellect and scientific insights are disguised by a very human persona. I cannot number the times that this led me to 'why did I not think of that' moments. Second, Dale has unusual self-belief and confidence in his ideas, once formed. This, combined with his ability to devise deceptively simple experiments, accounts for much of his ability to assemble compelling evidence for his hypotheses. Third, Dale is an optimist who continues to believe strongly in the future of animal agriculture and its contributions to human nutrition and well-being, and environmental sustainability. I consider my association with him to be the highlight of my academic career.

John Langtree Black, AM, FTSE, FAIAST, FASAP, FNSA 1942–

John Black was born in Omeo, East Gippsland, Victoria, where his father was the Shire Engineer. His maternal grandparents were farmers at Ensay, south of Omeo, and he, his mother and brothers lived with them during and after World War II until John was about five. He and his family then moved to Yarram in South Gippsland, which was a predominantly livestock (especially dairy) farming community. John spent much of his youth on the farms of friends and his ambition was to buy his own property after he left school. However, that proving to be unrealistic, he instead enrolled in Agricultural Science at The University of Melbourne, graduating in 1964.

Soon after graduating, John was appointed by the Dean of the School of Agriculture, Derek Tribe, as a Demonstrator to assist with the practical teaching of second year Agricultural Science students at the newly established Mount Derrimut field station, on the western outskirts of Melbourne. After completing a Diploma of Education in 1965, this appointment and later support from the Australian Wool Corporation enabled him to undertake PhD studies under the supervision of Tribe and Geoff Pearce. His topic was the utilisation of protein and energy in growing lambs. This choice, together with the novel methodological approach he devised and the results he obtained, was to have far-reaching effects on both his career development and the understanding of energy–protein interactions in growing animals.

To avoid the digestive inefficiencies associated with carbohydrate fermentation and protein degradation in the rumen (Black 1971), John infused synthetic liquid diets directly into the abomasum of lambs. Responses of nitrogen balance to various combinations of digestible energy and digestible protein were used to determine optimal protein requirements at different stages of growth, yielding what is now regarded as a classic family of 'hockey stick' curves (Black *et al*. 1973*a*). These and additional data were later analysed to extend and more precisely define the influence of dietary energy on the relationship between nitrogen intake and nitrogen balance (Black and Griffiths 1975). John's work using functionally non-ruminant lambs presaged similar studies on baby, growing and finishing pigs by Roger Campbell and others (see Campbell 1988), which provided the basis for computer models of pig growth later developed by John and others, discussed below.

Another important aspect of John's PhD research was his demonstration that abomasal infusion of milk in lucerne-fed lambs caused them to grow three times faster than did similarly fed lambs infused with an equal quantity of milk into the rumen. This work highlighted the importance of the site of digestion in determining the efficiency of protein and energy utilisation in young ruminants and the essential role of the reticular groove during the milk feeding phase in all animals with fore-stomach fermentation (Black and Sharkey 1970). It also helped stimulate John's long-term enthusiasm for comparative biology.

The examiners of John's thesis were a formidable trio, consisting of Kenneth Blaxter (see later in this article), Director of the Rowett Research Institute in Scotland, Ian McDonald, Chief of CSIRO Animal Physiology, and Bruce Stone, Reader in Biochemistry at Melbourne University and, later, Foundation Professor of Biochemistry at La Trobe University. Blaxter and McDonald, as products of the British academic system, insisted on holding oral examinations, although this was not a Melbourne requirement. I was working at CSIRO Prospect in July 1969 when John gave a seminar after his viva with Ian McDonald. I do not remember the event, possibly because, as John recently reminded me, it was on the day the Apollo 11 crew first walked on the moon and most of the laboratory was glued to a tiny black and white television in the canteen! However, he must have impressed McDonald because, soon afterwards, he was awarded a CSIRO postdoctoral studentship to spend a year in 1970–1971 at the National Institute for Research in Dairying at Shinfield, near Reading. This made it possible for him to travel to Aberdeen to be examined by Kenneth Blaxter, which must have seemed a daunting prospect. However, John now remembers only a congenial discussion lasting 6 h!

John's PhD, accordingly, was awarded in 1971 and, after returning to Australia that year, he was appointed as a research scientist in the CSIRO Division of Animal Physiology (later Animal Production) at Prospect. He quickly initiated studies on effects of energy and protein intake on wool growth (Black *et al*. 1973*b*). Soon afterwards, his continuing interests in effects of nutrition on lamb growth (e.g. Black and Griffiths 1975) led him to develop a simulation model for milk-fed lambs, to settle the then controversy over the effects of nutrition on body composition of animals (Black 1974). This was followed by collaborative involvement in the further development of computer models to simulate observed effects of nutrition on body growth and composition (Graham *et al*. 1976). About this time, his interest in modelling of growth processes was reinforced by the opportunity to work with Lee Baldwin, who took sabbatical leave at CSIRO Prospect to develop mechanistic approaches to simulation of tissue and body growth, as discussed earlier in this article.

Between the mid-1970s and early 1980s, the focus of the Prospect modelling team, led by John, Graham Faichney and Norman Graham, was on simulation of rumen function and nutrient absorption in sheep (Black *et al*. 1981). The obvious next step was to model the tissue utilisation of absorbed nutrients, which John initiated in collaboration with David Beever and his colleagues during a study leave at the Grassland Research Institute, Hurley, UK, in 1981 (e.g. Gill *et al*. 1984; Black *et al*. 1987). By the mid-1980s, it seemed reasonable to expect that this work would be applied to replace the static, tabular form of feeding standards for sheep and other livestock with dynamic computer models that could be continually updated as new information became available. Unfortunately, the Standing Committee on Agriculture, which commissioned the publication of *Feeding Standards for Australian Livestock*: *Ruminants* in 1990, thought otherwise.

However, the potential utility of modelling for prediction of nutrient requirements and performance was not lost on leaders of the Australian pig industry who were concerned that the traditional approach could not keep up with rapid changes in genetics and management in their industry. This led the Pig Research & Development Corporation (now Australian Pork Limited) to convene a meeting in December 1985 between John and a group of leading Australian pig nutritionists. After agreeing on information needed, the new team swung into action and had a working model by April 1986, which was published later that year (Black *et al*. 1986). If only all scientific progress was so swift!

The model accurately predicted performance responses to nutritional and environmental variables. However, to improve industry utility, it was expanded to include ration formulation software and a whole-farm optimisation system; later, after industry testing, expert systems software was added to enable prioritisation of management changes. The complete package, named AUSPIG, was licenced by CSIRO to BP Nutrition for use within its US and European companies and released to the Australian pig industry in 1989. AUSPIG was internationally recognised as the best technology of its type in the world, and enthusiastically embraced by leading agribusiness corporations. It proved to be an excellent tool for strategic decision-making, enabling major changes in ration formulation, feeding and housing strategies for pigs in Australia, USA and Europe, with substantial increases in enterprise profitability. However, its use as a day-to-day decision-making tool in commercial piggeries has not been widespread. John recently cited reasons for the limited industry penetration of AUSPIG and other models for every-day management, including operational complexity, the number of accurately measured inputs required, and inadequate monitoring of productivity gains (Black 2014). Being an optimist, he also envisaged a future when such models will be indispensable to intensively managed animal enterprises.

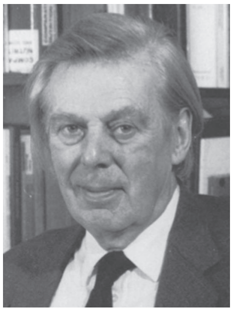
Although heavily committed to model development and application during the 1980s and early 1990s, John continued to lead experimental work on diet selection and regulation of feed intake in sheep (Kenney and Black 1984) and on pathophysiological responses of growing pigs and lactating sows to heat stress and disease (Black *et al*. 1999). However, his personal research involvement and output began to suffer after he was appointed Assistant Chief of Division in 1992. This would have been an onerous responsibility in the best of times. However, the early 1990s were a period of great financial instability for CSIRO in general and its divisions serving the livestock industries in particular (Brown 2010). The Division of Animal Production had never fully recovered from its loss of major block funding from the Australian Wool Corporation, beginning in the early 1970s. It also suffered from the inability of CSIRO to reconcile its increasing dependence on industry funding with its declared mission of independent strategic research of the highest quality. The consequence was a series of involuntary redundancy programs that had begun soon after the appointment of Oliver Mayo as Division Chief in 1989 and continued well into the 1990s. As Assistant Chief from 1992 to 1996, John took responsibility for implementation of many redundancies, including communications with affected staff members and industry stakeholders. This depressing experience weighed heavily on him and lost him the friendship of several long-standing colleagues. He also was becoming frustrated by his lack of time for personal research and increasingly disquieted about CSIRO's schizophrenic mismatch of its funding and science policies. Therefore, his decision in 1996 to leave the organisation he had served for 25 years was not difficult.

John's decision to become an independent consultant on agricultural research management and policy may have seemed risky at the time. Certainly, he had few successful Australian role models to emulate. However, he embraced his new role with characteristic enthusiasm and, in the two decades since, has established himself as the go-to person for numerous academic, government and industry organisations. His varied consultancies have included development and coordination of a large, multi-institutional research program to improve the quality and marketability of feed grains, on behalf of the Grains Research & Development Corporation and the animal industries; development of a research and adoption program on honeybee nutrition for the Rural Industries Research & Development Corporation; management of the National Livestock Methane Program for Meat & Livestock Australia; and numerous other nationally important engagements. At the same time, he has continued to be an active participant in scientific conferences and a prolific writer of research reviews and technical reports.

John has received many awards and honours for his distinguished contributions to science, including fellowships of several scientific societies, the American Medal of Honour for Contributions to Animal Science, the Centenary Medal of Australia and Membership of the General Division of the Order of Australia (AM). The latter recognised not only his contributions to science and the livestock industries, but also to the Rural Fire Service as an active local brigade leader and contributor to policies and practices for preventing and combatting bushfires.

In February 2016, John's colleagues, friends and family gathered at the University of Sydney for a Festschrift Symposium to celebrate his exceptional, multi-faceted career. The widespread respect and positive regard for John were very much in evidence and a particular highlight of the symposium was the honouree's presentation on 'Perspectives of animal research and its evaluation' (Black 2018). In addition to numerous other papers on topics researched by John, the symposium proceedings include an authoritative and comprehensive overview of his contributions to Animal Science by his colleagues, Roger Campbell and Ian Williams (Campbell and Williams 2018).

Sir Kenneth Lyon Blaxter, FRS, FRSE 1919–1991



Kenneth Blaxter was born and raised in Norfolk, near Norwich, UK. Although some of his forebears had been farm workers, his introduction to agriculture did not occur until he was into his teens, after his parents had withdrawn him from school because of his indifferent scholastic motivation and performance (Waterlow and Armstrong 1994). Although the employment chosen for him was as a clerk in an insurance office, he was drawn to the nearby Norfolk Agricultural Station where he eventually was allowed to volunteer on weekends, helping with livestock husbandry and other tasks. As noted by his Royal Society biographers (Waterlow and Armstrong 1994), this experience was crucial, leading to his enrolment in agriculture at the University of Reading in 1936 and, soon after his graduation in 1939, the position of research assistant at the National Institute for Research in Dairying at Shinfield near Reading. At Shinfield, KB, as his friends and colleagues knew him, was involved in research on urea supplementation, alkali treatment of straw to improve its digestibility and, most notably, the feeding of thyroactive proteins to stimulate milk production. The latter research yielded Blaxter a paper in Nature (Blaxter 1943) but, although effective in stimulating milk yield, the strategy was inapplicable because the treated cows lost condition and rapidly declined in milk production after the treatment ceased. At the same time, others at Shinfield were experimenting with growth-hormone treatment of dairy cows to boost war-time milk production. This, too, was efficacious but impractical because of inadequate supplies of bovine pituitary growth hormone. Another 40 years would pass before my Cornell colleague, Dale Bauman (see earlier in this article), successfully pioneered the use of recombinant bovine growth hormone to enhance milk production.

Blaxter's budding research career was interrupted by a year's military service, but, in late 1941, he was co-opted to return to Shinfield to conduct experiments on the use of home-grown feeds for dairy cattle that yielded him several papers (e.g. Blaxter 1944) and a PhD. During 1944–1946, he was seconded to the veterinary laboratory of the Ministry of Agriculture at Weybridge where he worked on lead toxicity in farm animals and discovered the excretion of lead in bile, leading to a series of papers published in 1950 (e.g. Blaxter and Allcroft 1950). Blaxter was then awarded a fellowship to work with Professor H. H. Mitchell at the University of Illinois, which further broadened his experience in nutrition and metabolism and introduced him to animal calorimetry (Waterlow and Armstrong 1994). While still working at Illinois, he was offered the position of Head of the Department of Nutrition at the Hannah Research Institute in Ayrshire, Scotland which he commenced in 1948 and was to hold for the next 17 years.

This was to be an enormously productive period for KB, during which he published almost 200 papers, initially on nutritional deficiencies of calves (e.g. Blaxter *et al*. 1952; Blaxter and Rook 1954). One of his PhD students involved in this work was John Rook who was later to become Director of the Hannah Institute from 1971 to 1981, encompassing my time at the Institute (1972–1977). What followed was an astonishing output of publications on the energy metabolism of ruminants (see Waterlow and Armstrong (1994) for a full bibliography) that led to Blaxter's proposal of the metabolisable-energy (ME) feeding system (Blaxter 1962*a*, 1962*b*). Although I was not fully aware of its significance at the time, his monograph on the subject (Blaxter 1962*b*) was one of the few books I bought as an undergraduate at UNE that was not a recommended text. Blaxter's ME system continued to be refined after he left the Hannah to become Director of the Rowett Research Institute at Aberdeen in 1965 and, eventually, was to form the basis of the Agricultural Research Council's energy-feeding recommendations published in 1980 (Agricultural Research Council 1980). Although the ME system differs from the net energy system of the American National Research Council, in terms of units of feeding value and some other elements, the underlying principles of the two systems are almost identical and have provided the foundation for practical recommendations on the feeding of ruminants for many years. Blaxter's research at the Hannah Institute on energy requirements was extended to make allowances for influences of the thermal environment (Joyce and Blaxter 1964), voluntary feed intake (Blaxter *et al*. 1966) and the chemical composition of nutrients absorbed from the reticulorumen (Armstrong *et al*. 1961); all of these factors are incorporated into existing feeding systems. His work with John Clapperton on methane production of ruminants (Blaxter and Clapperton 1965) also has enjoyed renewed interest and citation as the contribution of enteric methane to global greenhouse-gas emissions has come to be understood.

Soon after assuming Directorship of the Rowett Research Institute, in 1967, KB received the significant honour of being elected a Fellow of the Royal Society. By this time his administrative responsibilities and increasing demands for his advice on agricultural policy and other matters of national importance had curtailed his personal involvement in research. Nevertheless, he continued to publish prolifically on issues as diverse as deer farming (Blaxter 1972), food security (Blaxter 1975) and global use of resources (Blaxter 1976), the latter two being controversial at the time, but surely prescient in light of issues perceived to be important in the 21st century. For these and his earlier major contributions to British agriculture and society, he was knighted in 1977.

I will not comment on KB's 17 year tenure at the Rowett other than to say that the cited comments of colleagues who knew him much better than I (e.g. Waterlow and Armstrong 1994) are largely consistent with my impressions of him. In short, I believe he was essentially a rather shy and private person who, perhaps unconsciously and often unsuccessfully, strove to hide his unusual intellect and force of personality. This would explain why his management style could be described as 'rather remote'; yet, he apparently enjoyed administrative battles both internally and with those holding government purse strings (Waterlow and Armstrong 1994).

When I arrived at the Hannah Institute in 1972, KB had been gone for 7 years, taking with him to the Rowett most of the closed-circuit sheep calorimeters he had built at the Hannah. However, his presence still loomed large in the minds of his former colleagues, including my boss, the Head of the Department of Physiology, Jimmy Findlay. Findlay regarded KB with an odd mixture of respect and frustration, having had many inter-departmental battles with him and, I suspect, having lost more than he won. Not only were their research priorities often at odds, their personalities and demeanours were diametrically different, with the tall, patrician Englishman opposed to the short, feisty Scot.

I had met KB briefly during several visits to the Rowett in the early 1970s. However, I was surprised when he immediately greeted me by name at the start of 1976 EAAP Energy Metabolism Symposium in Vichy, France. This was my first international conference and I was petrified, not only because I was to deliver the first two papers of the conference (one on behalf of my PhD supervisor, Gordon Thompson, who was ill) but because the daunting KB would be chairing my session. I also was conscious that my papers on organ metabolism would be regarded as unusual by an audience devoted to calorimetry and comparative slaughter techniques to study energy metabolism. Blaxter, who had come into the meeting room where I was fumbling with slides before the session, quickly sensed my discomfort and announced that he would take the chair's prerogative and ask the first two questions after my papers. He then asked me what those questions should be and, when that was decided, ran me through the answers. This kindly act transformed my impression of the great man and, when we sat together on the flight back to Scotland, I was much more at ease with him.

Another anecdote that is revealing of KB's sometimes hidden character concerns a presentation he was invited to give to a Yorkshire Veterinary Society meeting in Scarborough. I do not recall the year, but it was long after he had become famous. My informant, who was staying in the hotel room below Blaxter's, told me that the night before KB's presentation he was kept awake for hours by constant pacing up and down and speech rehearsal in the room above that included pauses for effect and other oratorical devices. This greatly surprised me because KB's public speaking style always seemed very natural, almost languid at times, and the meeting concerned would hardly have been a banner event in his speaking calendar.

After retiring in 1982, KB continued to be involved in advising on matters of national and international importance relating to agricultural and food policy, and in writing, including his acclaimed monograph on energy metabolism in animals and man (Blaxter 1988). At the time of his death in 1991, he was working on a history of science in agriculture that was later completed by Noel Robertson (Blaxter and Robertson 1995).

Kenneth Blaxter truly was one of the giants of the animal sciences whose research and administrative achievements will continue to have an impact for generations to come. I feel extremely fortunate to have known him.

Robert Hutchinson Foote 1922–2008

Bob Foote grew up on a dairy farm in Gilead, Connecticut. His degrees were earned at the University of Connecticut, Storrs (BS, 1943) and Cornell University (MS, 1947; PhD, 1950; Greve and Hasler 2002). His postgraduate training was delayed because of his war service in Europe when he was commissioned as a lieutenant in the famed 'Go for Broke' 442nd Regimental Combat Team. This unit consisted of Japanese-American soldiers who then were not permitted to be officers. It suffered unusually high casualty rates and was among the most highly decorated US units in World War II. Bob himself received a Bronze Star after he was badly wounded in 1944.

After the war, Bob took advantage of the GI Bill to undertake graduate studies at Cornell. His Masters research, under the supervision of Glenn Salisbury, was on factors affecting the viability of sperm and control of bacteria in diluted bull semen (Foote and Salisbury 1948). Following Salisbury's departure from Cornell, Bob undertook PhD studies with Bob Bratton on wider aspects of semen quality (e.g. Foote and Bratton 1950) which, in addition to his later interests in other areas of reproductive biology and advanced technology, remained a favourite topic throughout his research career (e.g. Tardif *et al*. 1997).

After completing his PhD in 1950, Bob was appointed to the faculty of the Department of Animal Science at Cornell where he rose through the ranks to become the Jacob Gould Schurman professor in 1980. During the 1950s and 1960s, he continued to pursue various aspects of male reproduction, gamete biology and promotion of semen quality, to optimise the success of artificial insemination, particularly in dairy cattle (e.g. Hahn *et al*. 1969; Foote 1970) but also in sheep, pigs, rabbits (Foote 1962) and dogs (Gill *et al*. 1970). The combination of technical advances in handling semen with powerful new statistical methods for genetic evaluation and selection of elite dairy bulls developed by Bob's Cornell colleague, Chuck Henderson, led to extraordinary improvements in dairy cow performance during the 1950s (Van Vleck and Henderson 1961) that have continued unabated in the decades since.

Bob's research on embryo development *in vivo* and *in vitro* began in the mid-1960s (Greve and Hasler 2002). His early work on oogenesis, superovulation and embryo developmental capacities in rabbits (e.g. Maurer *et al*. 1970) led to similar, much-cited studies in calves (e.g. Seidel *et al*. 1971). This, in turn, provided the basis for subsequent development of embryo transfer technologies, now widely used in the livestock industries, and studies of embryogenesis *in vitro*. Bob's broadening interest in advanced reproductive technologies also turned to sexed semen, of which he was an early proponent (Foote 1972).

By the time I arrived at Cornell in late 1985, new genetic and reproductive technologies such as mammalian transgenesis and cloning were on the horizon. Bob was an enthusiastic advocate for our department to immediately and substantially invest in these areas. However, other senior faculty members, including Dale Bauman (see earlier in this article), were more cautious about transgenic work, preferring to work on functional genomics and manipulation of phenotypic outcomes. Undaunted, Bob pressed on, aided by his associate, Jerry Yang, and others, with refinement of micromanipulative techniques, including nuclear transfer, that he hoped would lead to cloning of cattle (e.g. Yang *et al*. 1993). However, although he and his team came close, they were not to achieve this goal.

Bob was forced to officially retire at age 70 in 1993, just months before mandatory retirement ceased at Cornell. He remained annoyed about this as long as I knew him, although, as an active professor emeritus, he was able to maintain an office and laboratory in Morrison Hall long after 'retiring'. Even at this late stage of career, he prided himself on being the first to arrive and the last to leave the building each working day. This led to some mischievous pranks by the graduate students, one of whom left a message on Bob's office door in the early hours of the morning stating that he had stopped by to chat and regretted that Bob must have left early.

As Department Chair from 1997 to 2007, I kept a tally of the publications of my colleagues. Always a prolific writer, Bob continued to publish frequently for most of this period, including original papers on findings mined from his store of unpublished data. This took his career publications total to well over 500. I am not sure whether he knew, but there were a couple of years when his individual productivity was second only to that of his departmental rival, Dale Bauman – not bad for someone approaching his ninth decade!

Finally, Bob's involvement in public discourse on reproductive technologies, especially cloning, must be mentioned. After the successful cloning of the sheep, Dolly, at the Roslin Institute in Scotland in 1996, Bob warned the New York State Senate against rushing to pass a bill to ban cloning research, as had been attempted in other states because of fears about human cloning. His advice was heeded and the bill did not pass. However, he also was outspoken in his opposition to human cloning, citing not only technical but also ethical challenges, and, in 1998, he publicly denounced the plans of Chicago scientist Richard Seed to clone humans.

Bob Foote was a major international figure in the development and application of reproductive technologies in the livestock industries. He deserves special credit for contributing to the success of artificial insemination in advancing genetic improvement of dairy cattle and for the groundwork he provided in the development of embryo transfer and cloning in livestock species. His reflections on the history of these advances, including his own role, during the second half of the 20th century are well worth reading (Foote 1999).

William Hansel, FAAAS, FASAS 1918–2017

Bill Hansel was raised on a dairy farm at Vale Summit, Maryland. He graduated from the University of Maryland with a BS in dairy science in 1940, before being drafted into the US army. He served for 5 years, rising to the rank of major and company commander in the 94th infantry division of Patton's US Third Army. Bill was severely injured, earning two purple hearts, during the Battle of the Bulge in early 1945. After his repatriation and discharge, he started graduate studies, while still on crutches, in the Department of Animal Husbandry (later Animal Science) at Cornell University. There his major professor was the eminent reproductive physiologist, Sydney Asdell; his graduate committee also included H. H. Dukes (famed author of *The Physiology of Domestic Animals*) and Kenneth Turk, Head of the Department of Animal Husbandry, who, incidentally, had been Bill's undergraduate advisor at the University of Maryland before the war (Hansel 2013).

Bill's MS (1947) and PhD (1949) research was on signs of oestrus and physiological changes during the oestrous cycle in dairy cows (Hansel *et al*. 1949; Hansel and Asdell 1951). He, then, was appointed as assistant professor of physiology in the Department of Animal Husbandry at Cornell and commenced what was to become an illustrious and very long academic career. With George Trimberger, he investigated the use of exogenous progesterone to synchronise oestrus in dairy cows, with the goal of greatly increasing the labour efficiency of artificial insemination (Trimberger and Hansel 1955). By Bill's own admission, these early experiments were not very successful, but provided the foundation for much subsequent and, ultimately, fruitful work (Hansel 2013). About the same time, Bill collaborated with colleagues in the Cornell Veterinary College to show that the causative agents of a vexing disease, bovine hyperkeratosis, were highly chlorinated napthalenes found in bread crumbs, wood preservatives, fire-retardant paints and heat-resistant lubricants (Hansel and McEntee 1955). Once these contaminants were removed from the environment, the disease disappeared. This was one of the first demonstrations of the pervasive distribution and negative biological effects of environmental toxins.

One of Bill's early and highly esteemed faculty mentors was Clive McCay, who is renowned for his demonstration in the 1930s that chronic, moderate energy restriction of rats substantially increased their longevity. In the 1950s, Bill and colleagues extended this work to dairy cattle, focussing on the influence of plane of nutrition of prepubertal heifers on physiological development, especially age at puberty. They found a striking inverse relationship between the level of energy intake and age at first oestrus (Sorenson *et al*. 1959) to establish a concept that is now regarded as fundamental in the management of dairy cattle and other livestock. Incidentally, Bill once told me that he felt that widespread knowledge and acceptance of this work was hampered by its publication in an Experiment Station Bulletin rather than as a peer-reviewed paper in a widely read scientific journal.

During the 1960s and 1970s, Bill's research program mushroomed to include continued work on oestrus synchronisation (Hansel 1966), provision of evidence for the existence of a hypothalamic peptide that controls pituitary release of luteinising hormone (LH) and ovulation (Malven and Hansel 1964) and an exceptional, sustained body of work on regulation of the formation, function and regression of the bovine corpus luteum. Among the most important findings of this work was demonstration that LH is the major luteotropic hormone (Hansel and Seifart 1967) and the identification of a luteolytic substance, arachidonic acid, in the bovine endometrium (Hansel *et al*. 1975). Although Bill and his colleagues were beaten by Australian scientists in the race to identify prostaglandin $F_{2\alpha}$, a product of arachidonic acid, as the ultimate luteolytic stimulus, their contribution was a very important piece of the puzzle. Later, Bill and other Cornell scientists were actively involved in the application of this knowledge to devise successful strategies for oestrus synchronisation (Smith *et al*. 1984).

In 1978, Bill left the Animal Science department, his academic home for more than 30 years, to become Chairman of the Department of Physiology in Cornell's College of Veterinary Medicine. He stepped down from this role in 1983 and was appointed Liberty Hyde Bailey Professor of Animal Physiology. This prestigious appointment is named after the famous agricultural scientist and foundation Dean of the College of Agriculture at Cornell. Bill's office in the veterinary school was only a few hundred metres from Morrison Hall and after I arrived there in late 1985, he was often to be seen at departmental seminars and in informal discussion with animal science faculty and students. I cannot recall the time or purpose of my first meeting with him, but I do remember his friendliness and genuine interest in who I was and why I had chosen to come to Cornell.

As his 70th birthday approached, Bill became concerned about the prospect of mandatory retirement from Cornell, a policy that was not to be repealed until several years later. This caused him to leave the university in 1990, to take up the Gordon D Cain Professorship of Animal Physiology in the Department of Animal Science at Louisiana State University (LSU) in Baton Rouge, Louisiana. There, with Bob Godke and others, he continued his research on fundamental aspects of reproductive biology and technology, including optimisation of techniques for *in vitro* fertilisation and culture of bovine embryos. A most significant finding was that removal of nitric oxide from, or inhibition of the action of nitric oxide in the culture medium significantly enhanced development of embryos to the blastocyst stage (Lim *et al*. 1999).

In addition to being a superb researcher, Bill was a committed teacher and student advisor. At Cornell, he taught an upper-level undergraduate course in Endocrinology for 30 years and, in 1985, initiated a lecture and laboratory course in Animal Biotechnology. At LSU, he continued to teach Animal Biotechnology and a course in Gamete Physiology to undergraduate and graduate students. Over his career he also had major responsibility for training more than 100 research students and postdoctoral fellows, the majority of whom went on to successful careers in academia, government or the corporate world.

In 1994, Bill invited me to present a seminar to the Department of Animal Science at LSU. This was the first time I had seen him in his new academic home and I was impressed by the obvious respect, even reverence, with which he was regarded by his new colleagues. Soon afterwards, he relocated to the Pennington Biomedical Research Center that recently had been built in Baton Rouge with the aid of a large bequest from local oilman and philanthropist, C. B. ('Doc') Pennington and his wife, Irene. Bill again invited me to Baton Rouge in 1996, this time to present at a conference he had organised on nutrition and reproduction. The proceedings, edited by Bill and George Bray, Pennington Director and eminent nutritionist, include a review by Hansel and Lim on *in vitro* fertilisation and embryo development (Hansel and Lim 1998). This was my first and only visit to the Pennington Center which, at the time, was still adding facilities and staff. Today, it has over 600 staff dedicated to research on the aetiology, prevention and treatment of chronic diseases of humans.

In 1997, Bill's wife, Milbrey, died of ovarian cancer. This loss affected him deeply, causing him, at the age of 79, to abruptly shift his research focus and apply his deep knowledge of ovarian biology to search for an effective treatment for this disease. The approach Bill and his colleagues took involved linkage of anticancer drugs with receptor-binding segments of gonadotrophic and hypothalamic peptides, knowing that the tumour cells overexpress receptors for LH, follicle-stimulating hormone, chorionic gonadotropin and LH-releasing hormone. The early focus on ovarian cancer was extended to breast and pancreatic cancers, with promising preclinical results (Leuschner and Hansel 2004). This work led to several patent applications and clinical evaluation of a product, EP-100, which is expected to advance to Phase IIb human ovarian cancer trials in late 2018.

Bill's honours and awards are too numerous to list here. However, it is particularly notable that he received the highest accolades from four different scientific societies, including the Borden Award of the American Dairy Science Association (1972), the Morrison Award of the American Society of Animal Science (1979), the Carl G Hartman Award of the Society for the Study of Reproduction (1980) and designation as a Renowned Physiologist by the American Physiological Society (2014).

During my decade as chairman of the Animal Science department at Cornell, Bill made a point of meeting with me during his annual summer visits to Ithaca, to get an update on the state of his former department. These briefings usually ended with him telling me, with mock seriousness, the only problem with the department was that it had always been run by nutritionists. There was no denying that I and my eight predecessors carried that professional label. However, this was an in-joke because each of us had spent a good part of our careers working at the interface of reproduction, development and nutrition. Nevertheless, I assume Bill was pleased that my two successors, Ron Butler and Pat Johnson, are card-carrying reproductive physiologists. More significant is the fact that in 2013, 110 years after the department was founded, Pat became its first female leader.

Bill Hansel died at age 98 in January 2017. Only weeks before he had been coming to work almost every day and still was publishing regularly into his mid-90s (see Hansel 2013). I feel privileged to have known this true gentleman who, during his incredibly long career, made such important contributions to animal agriculture, fundamental knowledge of reproductive biology and, latterly, human health.

Derek Barber Lindsay, FNAS 1927–

Derek Lindsay was born at Wrexham in North Wales. On completing his secondary education at the local grammar school in 1945, his ambition was to study biochemistry at Cambridge, one of only two British universities then offering a degree in this still-new discipline. However, his preparation was marred by a lack of qualified teachers and other war-time exigencies and he did not gain immediate acceptance to Cambridge. He, therefore, chose to study for the entrance exam to Oxford where he was accepted and went up to Trinity College in 1946. Despite having a strong Biochemistry Department, Oxford did not offer a biochemistry degree so Derek opted to read chemistry, graduating with honours in 1950. His honours research project had been supervised by the well known biochemist, R. B. (David) Fisher, and he applied to study for a DPhil in biochemistry with Fisher. However, the Head of Biochemistry, Professor Sir Rudolph Peters, felt Derek needed to improve his knowledge of animal biology and offered him a research scholarship on the condition that he first completed a degree in physiology. This he did, with first class honours, in 1952 (Bryden *et al*. 1993*b*). He then proceeded to doctoral research on regulation of glucose metabolism in the isolated rat heart. His work was significant because it showed that the stimulation of glucose utilisation by insulin occurred over the same hormone-concentration range as that for stimulation of glucose and galactose transport across the cell membrane (Fisher and Lindsay 1956).

In 1954, Peters retired from the Chair of Biochemistry at Oxford to establish the Department of Biochemistry in the ARC Institute of Animal Physiology at Babraham near Cambridge. His very first appointee was Derek Lindsay, who had yet to complete his DPhil research at Oxford. This Peters allowed him to do, on a scientific officer salary, deferring his arrival at Babraham for a year. Derek was then introduced to the special features of the digestion and intermediary metabolism of ruminants by Frank Annison (see earlier in this article) and Dyfed Lewis. They challenged him to address the unresolved issue of the significance of glucose metabolism in ruminants in relation to the apparent importance of ruminal acetate as a metabolic fuel.

With characteristic rigour, Derek investigated the literature and wrote what was to be the first of several insightful and authoritative reviews on carbohydrate metabolism in ruminants (Lindsay 1959). This article was very well received and widely read because it not only provided the best summary of its topic yet available, but it also pointed to fruitful avenues for future research. The young author was astonished by the hundreds of reprint requests he received, being considerably more than for any paper he published subsequently.

Derek's review clearly identified the need for quantitative measurement of glucose and acetate metabolism. Accordingly, he and Frank Annison were among the first scientists to use radiotracers to measure acetate flux rates in sheep (Annison and Lindsay 1961), work which Derek was to extend with E. J. H. Ford (Lindsay and Ford 1964) after Frank left Babraham in 1958.

In 1960, Derek also left Babraham to join Frank Annison in the Department of Nutrition and Biochemistry at UNE in Australia. There, with Ron Leng and others, they continued the collaboration they had started at Babraham to systematically quantitate the whole-body metabolism of volatile fatty acids, glucose and other substrates in sheep (e.g. Annison *et al*. 1967*a*). In addition to his research contributions, Derek was heavily involved in undergraduate teaching of biochemistry and, in 1966, was appointed chairman of his department.

I had arrived at UNE to study Rural Science the year before but did not really get to know Derek until 1968 when he took me on as a fourth-year honours student. Despite his busy schedule, Derek generously gave me the time necessary to discuss and set up my project on sheep nutrition and digestion. However, not long after it was up and running, he announced that he would be returning to Babraham in mid-1968. At the same time, he took steps to ensure that my maiden foray into research would be overseen by John Davidson, with support from research students in Ron Leng's laboratory, including Bob White and John Nolan.

Back at his former research home, Derek established collaborations with Jim Linzell and Brian Setchell (both reviewed in this article) and others on novel aspects of organ metabolism, including hepatic metabolism of amino acids (Lindsay *et al*. 1975) and cerebral metabolism of glucose and ketones (Lindsay and Setchell 1976) in sheep. The latter study extended earlier investigations of sheep brain metabolism by McClymont (see later in this article) and Setchell to show that, unlike the human brain, the sheep brain has not evolved the ability to oxidise ketones when glucose is in short supply. They surmised that this was due to mass of the sheep brain being less than 10% that of the human brain, with a proportionately smaller demand for glucose.

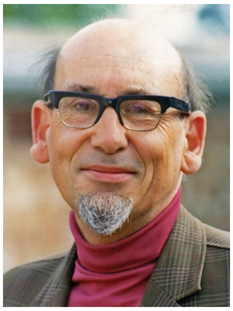
During my 5-year stint at the Hannah Institute, my colleagues and I often visited Derek, Brian Setchell, Jim Linzell and other luminaries at Babraham, including for the famous 'Babraham demonstrations' to the Physiological Society. They always made us welcome and sometimes co-opted our help in setting up the demonstrations. More importantly, they were an inspiring source of novel research findings and discussions about future opportunities. I was not to meet with Derek very often after my return to Australia in 1977, and later, during his time with the CSIRO at Rockhampton (see below), I was working in the USA. However, I always felt comfortable in making the occasional contact, sure of a friendly and thoughtful reply.

During his second sojourn at Babraham, Derek became renowned for the breadth and depth of his grasp of the literature and his ability to distil apparently disparate and complex research findings into succinct, coherent syntheses. This made him a popular choice to present reviews on wide-ranging aspects of ruminant metabolism to the Nutrition and Biochemical Societies (e.g. Lindsay 1975, 1978, 1980) as well as to prestigious international meetings (e.g. Lindsay and Oddy 1985). It also attracted some talented PhD students, including David Pethick and Hutton Oddy, both of whom have since gone on to have highly productive and influential careers in their native Australia. Much of Pethick's doctoral research on fatty acid and ketone metabolism in splanchnic tissues, muscle, pregnant uterus and lactating mammary gland of ewes has been reviewed (Pethick *et al*. 1984), as has Oddy's on whole body and muscle amino acid and protein metabolism in growing lambs (Oddy and Lindsay 1986).

In 1982, Derek spent a brief study leave with Frank Annison at the University of Sydney. This renewed contact with the Antipodes, together with his growing concern about the prospect of being forced to retire at 60, led him back to Australia in 1986 where he spent the remaining 7 years of his career with the CSIRO Division of Tropical Agriculture at Rockhampton. There he worked on regulation of growth in tropically adapted cattle and, more specifically, means of manipulating the maintenance-energy requirement of stock during the dry period. This must have been a very different research experience from that in the temperate and hallowed environs of Cambridge. However, he was much admired and appreciated by his Rockhampton colleagues, including Bob Hunter, who cites Derek's previously mentioned encyclopaedic knowledge of the literature and gentle guidance in developing ideas, designing experiments and making sense of the results.

In 1992, Derek's wonderful career in two continents was fittingly recognised at a Festschrift symposium held in his and Frank Annison's honour at the University of Sydney (Bryden *et al*. 1993*b*), at which he made a valedictory address on the importance of quantitative nutrition (Lindsay 1993). A year later he retired from CSIRO and returned to live in Cambridge where he continued with some collaborative research and his long-time editorial duties with *The Journal of Agricultural Science*. He also was an Honorary Professor in the Division of Nutritional Sciences at the University of Nottingham, Sutton Bonington, where he was an occasional lecturer and research mentor. In 2005, Derek published possibly his last original paper, on carbon recycling among organic intermediates in sheep (Lindsay *et al*. 2005). Now 90, he still dwells on unresolved aspects of this and other research, and recently speculated to me that the use of glucose carbon for synthesis of non-essential amino acids in ruminants may be considerably greater than generally accepted.

James Lincoln Linzell 1921–1975



Jim Linzell was born in Barnet, outside London. Although interested in biology as a boy, he apparently did not decide to become a veterinarian until persuaded by a school friend (Anon. 1977). He enrolled at the Royal Veterinary College and qualified as MRCVS with a BSc in physiology in 1943. He was then employed as a house surgeon (large animals) at the College for a year, before entering private practice in 1944. He quickly discovered that much of veterinary clinical work was routine and not very challenging. Therefore, in 1946 he accepted an Agricultural Research Council (ARC) traineeship to study under Professor Ivan de Burgh Daly in the Department of Physiology at the University of Edinburgh. It was on Daly's advice that Jim began to study mammary biology, a pursuit he enthusiastically and successfully followed for the rest of his career.

While at Edinburgh, Jim discovered the presence and contractile role of myoepithelial cells in the mammary gland (Linzell 1952), which contributed to the award of his PhD in 1952. In the meantime, a new ARC Institute of Animal Physiology was established at Babraham outside Cambridge and Daly was appointed as its inaugural Director. At Daly's invitation, Jim became one of the first appointees and moved to Babraham after laboratory space and staff housing were ready in 1950. Other new staff arrived soon after, including Frank Annison (see this article) who was to become one of Jim's closest collaborators as well as a good friend and neighbour in the community of scientists who lived on the Institute estate. Despite many offers of University Chairs and other prestigious jobs elsewhere, Jim chose to remain at Babraham for the rest of his life.

According to Frank Annison, Jim's closeness to the new Director may have hampered his early career at Babraham because Daly used him as a personal technician, no doubt taking advantage of his excellent surgical and experimental skills, without recognising him as a co-author. Nevertheless, Jim's energy and commitment to mammary biology soon began to pay dividends. Much of his earlier work is summarised in his excellent review of mammary physiology published in 1959 (Linzell 1959), including his development of surgical methods for catheterisation of mammary blood vessels to enable measurement of arteriovenous (AV) differences in blood concentrations of oxygen and metabolites (e.g. Linzell 1960*a*) and a novel thermodilution technique for measurement of mammary blood flow (Linzell 1953). Jim was especially conscious of the complexity and variability with parity and other factors of mammary venous drainage, including its 'contamination' by skin and other non-mammary tissues (Linzell 1960*a*, 1960*b*).

In the late 1950s, Jim and D. C. Hardwick developed an isolated, perfused caprine mammary preparation that, under optimal conditions, produced appreciable amounts of milk. This work and subsequent refinements were summarised a decade later by Jim and colleagues (Linzell *et al*. 1972). One of the most important findings of this research was the dependence of milk volume on mammary supply of glucose via the osmotic influence of its main synthetic product, lactose (Hardwick and Linzell 1961). Thus, in 1960, when Derek Lindsay (see this article) was sailing to Australia to take up his new position at the University of New England, he received the following pithy telegram from Jim: 'Dear Derek, no milk without sugar. Jim.'.

In 1964, Jim began an extremely fruitful collaboration with his former Babraham friend and colleague, Frank Annison, who had returned to England to become Director of Animal Nutrition at Unilever's research facility at Colworth House, Sharnbrook. Their studies employed a powerful combination of Jim's AV difference and blood flow methodology with radioisotope dilution techniques for measuring nutrient fluxes, pioneered by Frank and colleagues at the University of New England in Australia. This approach greatly extended Jim's previous data on mammary net uptake of milk precursor substrates (e.g. Linzell 1960*a*) to quantify the partitioning of glucose and acetate metabolism between synthesis of milk constituents and oxidation (Annison and Linzell 1964). It also enabled measurement of the relative contributions of *de novo* synthesis of long-chain fatty acids from acetate and mammary uptake of preformed triacylglycerols to milk fat synthesis (Annison *et al*. 1967*b*). About the same time, Jim completed his cataloguing of the metabolic sources of the major organic solids in milk, through his work with Ben Mepham to quantify the contributions of individual blood amino acids to milk protein synthesis (Mepham and Linzell 1966).

Although Jim made great use of the goat as an experimental animal, he always was interested in comparative aspects of mammary biology. Thus, early in his career, he had studied the mammary anatomy and physiology of cats, dogs, rabbits, rats and even humans (Linzell 1952). Later, he and his collaborators extended the techniques he had developed in goats to describe the mammary uptake and metabolism of milk precursor substrates in the lactating sow (Linzell *et al*. 1969) and cow (Bickerstaffe *et al*. 1974). About the time of the latter studies, I was visiting Babraham for the 'Babraham demonstrations' that were a feature of Physiological Society meetings held at Cambridge. There, Jim Linzell elegantly demonstrated the real-time measurement of mammary AV difference in blood glucose in a very pretty, demure Jersey cow, which was a revelation to the hard-core medical physiologists in attendance.

Jim's interests in comparative biology were shared by his close colleague at Babraham, Malcolm Peaker, who moved to the Hannah Institute some years after I was there, and later became its Director. Another person close to Jim was his senior assistant, Ivan Fleet, whom he regarded as a friend as well as a skilful and dedicated technician. Ivan remained at Babraham for many years after Jim's death and was instrumental in transferring some of his and Jim's unique techniques to other scientists, including Derek Lindsay.

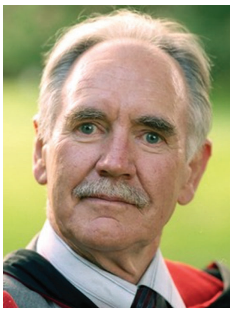
Jim was a practical, working scientist, with little time for bureaucracy or administrative trivia. For example, Derek Lindsay recalls that when he, Jim and Brian Setchell (see later in this article) discussed an experimental approach to measure hepatic metabolism, Jim went ahead with developing a perfused sheep liver preparation (Linzell *et al*. 1971) without ever formally proposing a project to the Babraham administration. Nevertheless, his natural scientific leadership and authority were recognised when he was appointed Head of Physiology in 1971 (Anon 1977). By all accounts, his management style was very hands-off and, in those days before email, most memos that reached his desk were quickly transferred to the circular file.

I first met Jim at Babraham in 1972 where, belying his daunting reputation, he was more than willing to offer advice and encouragement to me as a neophyte embarking on studies involving AV difference and blood-flow techniques. His generosity continued as I began to gather data on hindlimb metabolism in young cattle, including his presence at my very first scientific presentation to the Physiological Society, at Southampton in 1973. I will be forever grateful to Jim for being one of only 8–10 attendees at my talk, while, in the next room, his good friend Professor Henry Barcroft (son of

Sir Joseph) was presenting his valedictory paper to the Society and an audience of more than 200.

I am very grateful to have known Jim Linzell, whose untimely death in 1975 robbed us of a wonderful scientist and generous-spirited colleague. For a comprehensive account of his major contributions to the understanding of mammary physiology, metabolism and milk synthesis, readers are referred to his review in the book edited by Larson and Smith (Linzell 1974).

Gordon Lee (Bill) McClymont, AO, FASAP 1920–2000



Bill McClymont was born and raised in Sydney but spent his school holidays on relatives' farms around Orange, New South Wales (G. L. McClymont, unpublished memoir). These early experiences stimulated his interest in agriculture, particularly livestock production, and desire to go on to university. However, this ambition became possible only after he was offered a bonded traineeship with the New South Wales (NSW) Department of Agriculture on the condition that he study Veterinary Science rather than Agricultural Science at the University of Sydney (G. L. McClymont, unpublished memoir). The architect of this plan was Colonel Max Henry, the Department's Chief Veterinary Surgeon (later, Chief of the Division of Animal Industry), who wanted to strengthen the expertise in livestock nutrition in his organisation. Bill graduated with first class honours and the University Medal in 1941, after completing a degree that was compressed because of World War II and punctuated by duty with the Australian Army Veterinary Corps and the Volunteer Defence Corps (G. L. McClymont, unpublished memoir).

Bill's subsequent career was remarkable for its breadth in the application of science to improve the productivity of livestock beyond a narrow focus on animal health and to understand inter-relationships among the animal, plant and soil sciences, as well as their integration with the social sciences. His broad interests in agriculture and related biology, initiated during his early farm experiences at Orange, were reinforced by his first job as an animal nutritionist with the NSW Department of Agriculture. In particular, the latter gave him a great appreciation of the complexity of agricultural systems, the importance of application of science through extension and the need for involvement of producers in setting priorities and evaluating outcomes for the agricultural sciences (G. L. McClymont, unpublished memoir).

Bill was an accomplished and innovative researcher whose achievements covered multiple species and areas of nutrition and metabolism. His early research with the NSW Department demonstrated the association between feeding low-roughage diets and milk fat depression in dairy cows (McClymont 1950). At a time when PhDs were not offered by schools of agriculture in Australia, he was awarded a Walter and Eliza Hall Veterinary Research Fellowship for doctoral study at the University of Cambridge from 1947 to 1949. There he built on earlier work on rumen fermentation and post-absorptive use of volatile fatty acids, led by Sir Joseph Barcroft (Barcroft *et al*. 1944), to demonstrate that circulating acetate was a principal source of milk fat synthesis in dairy cows (McClymont 1951). This led to his development of the so-called glucogenic hypothesis for the metabolic origins of diet-induced milk fat depression (McClymont and Vallance 1962) that stood the test of time for almost half a century, until new evidence led to its replacement with an alternative explanation by Dale Bauman (see earlier in this article) and his colleagues.

My former La Trobe University boss, Bob Reid (see later in this article), was a student contemporary of Bill's in Barcroft's laboratory at Cambridge. These were heady days for both young Australians, each of whom would become leaders of the emerging field of nutritional physiology in their home country. Bob had many anecdotes about their experiences and often mentioned Bill's PhD research on bovine mammary metabolism. More than 20 years later, Bill's work would be extended by two other people in this article, Frank Annison and Jim Linzell, at the nearby Babraham Animal Research Institute.

Bill's research after he returned to Australia included studies at Glenfield Veterinary Research Station on pregnancy toxaemia and brain metabolism in sheep (McClymont and Setchell 1955, 1956) with another young Australian veterinary scientist, Brian Setchell (see later in this article), and on drought-feeding with M. C. Franklin from the CSIRO McMaster Laboratory (Franklin *et al*. 1955). Although fully committed to research during this period, he and others were becoming increasingly concerned about the separation of education in agricultural science, focussed on plants and soils, and veterinary science, focussed on animal health, in Australian universities, without consideration of the complex array of disciplines that underpin pastoral animal production. In 1953, this led to a vigorous debate via Letters to the Editor of the *Australian Veterinary Journal*, including Bill's contribution in the June issue (McClymont 1953), which came to the notice of leaders of the newly established UNE at Armidale. To my knowledge, this was Bill's first public articulation of the case for creation of a new degree that would bridge the gap identified by him and others, a contribution that was influential in his appointment as the Foundation Dean of Rural Science at UNE in 1955. These same principles would be further elaborated in Bill's inaugural lecture that year, entitled 'All flesh is grass' (McClymont 1996).

At UNE, Bill managed to continue collaborative research on a range of topics, including poultry nutrition, with Rob Cumming and others (Sathe *et al*. 1964), and the pharmacology and toxicology of carbon tetrachloride in sheep, with Alex Kondos (Kondos and McClymont 1961). However, notwithstanding these and his earlier research achievements, Bill McClymont is best remembered as a visionary educator who, through his initiation and leadership of the Faculty of Rural Science at UNE, influenced the lives and successful careers of many hundreds of students, including myself.

When I enrolled in Rural Science in 1965, Bill was about halfway through his 21-year tenure as Dean and at the height of his powers as a teacher and administrator. As a lowly undergraduate I did not get to know him well personally. However, despite his lofty aura, he was very obviously a kindly man who derived great satisfaction from his involvement with students in the classroom and elsewhere. He was not a great lecturer in the classical sense. Nevertheless, his passion and the power of his message, especially about his concept of agricultural ecology, penetrated even the densest undergraduate skulls and, with hindsight, most of us feel privileged to have taken his courses. I am not sure whether Bill actually coined the term 'sustainable agriculture', as has been claimed, but the idea surely was deeply ingrained in his thinking at a time when the environmental consequences of agricultural practice were recognised by few of his contemporaries.

Bill McClymont's educational philosophy was summarised in what he termed the 'perpetual pentagram' of evolution, ecology, economics, ethics and education (McClymont 1970). Its successful implementation in the Rural Science degree program depended greatly on his ability to inspire similar thinking among a group of mostly younger scientists with strong affiliations to traditional disciplines. My impression as a Rural Science undergraduate during the 1960s is that the spirit of interdisciplinary thinking and integration of the animal, plant, soil and social sciences was a tangible and distinctive feature of the teaching program. However, with growth of the faculty came fragmentation and the creation of multiple departmental fiefdoms, a process that accelerated after Bill stepped down as Dean in 1976. This was associated with progressive departures from the ethos and integrated delivery of the Rural Science degree and increased opportunities for specialisation within that degree or in newly created alternative degrees in agriculture. These developments and the subsequent decline in student enrolment during the 1980s greatly dismayed Bill who, before and after his retirement in 1980, continued to press for a return, with modifications, to the model he had created (Ryan 2007).

An aspect of Bill's professional life that is perhaps less well known is his involvement with industry through promotion of extension and outreach and the participation of producers in setting and implementing the agricultural research agenda. Despite an incredibly busy schedule of teaching, research and administration, he was always keen to engage with the practical beneficiaries of his academic efforts, not only to inform them but to seek their advice. It occurs to me that with his early experience in extension with the NSW Department of Agriculture and some familiarity with the American Land Grant system gained during a US tour in 1949, Bill might have missed an opportunity to formally incorporate agricultural extension as a university responsibility after his appointment as Dean in 1955. However, considering the already monumental early challenges he faced and the adherence of Australian universities to the British model of restricting academic pursuits to in-house teaching and research, this may have been a step too far. Despite the efforts of individual scientists to engage with industry stakeholders, this unfortunate attitude still persists in Australian academia. There are modestly encouraging signs of change in a handful of institutions. However, more widespread and meaningful change in the academic culture may need a reincarnation of Bill McClymont to lead the charge!

Giacomo Meschia 1926–

Giacomo Meschia was born in Milan, Italy. When he was 11, his parents decided he should have a classical education based on ancient Latin and Greek, and Italian literature (G. Meschia, unpublished memoir), apparently with little consideration of mathematics or the sciences. Thus, he was not to follow in the footsteps of his engineer father who, incidentally, helped build the prototype of the modern espresso coffee machine. Nevertheless, he chose to study medicine at the University of Milan, graduating in 1951 with little idea of what to do next. During his final year, he had worked on a research project to develop a method for measuring blood lead concentration in victims of lead poisoning. His chemical aptitude was noticed by the Chairman of Physiology, Rodolfo Margaria, who had persuaded relatives of the great Italian–American conductor, Arturo Toscanini, to set up a fellowship in honour of the maestro to enable young Italians to study science at Yale University. Giacomo was chosen to be the first awardee but, since the fellowship was not yet available, the deal was that he would spend 2 years working in Margaria's laboratory before going to Yale.

This delay had a positive aspect because Margaria was an internationally regarded respiratory and exercise physiologist with a strong quantitative bent, a characteristic that Giacomo was to emulate throughout his own career. He eventually arrived at Yale in 1953 and was assigned to work with the distinguished physical chemist, David Hitchcock. However, Hitchcock was nearing the end of his career and, after a couple of months, Giacomo was transferred to the adjacent laboratory of the eminent prenatal physiologist, Donald Barron. This was to be a pivotal moment in his career because Barron had been the star pupil of the great Sir Joseph Barcroft during the 1930s and, after returning to the USA soon after World War II broke out, had become the undisputed American leader of research on fetal and placental physiology.

In hindsight, Giacomo realised that Barron's strategy in enlisting his assistance was to assign him to a project on determination of the influence of the gradient between maternal and fetal plasma colloidal osmotic pressures on placental water transport, knowing Hitchcock's interest and expertise in the colloidal properties of proteins (G. Meschia, unpublished memoir). This led to the first of Giacomo's many papers on placental transport (Meschia 1955). It also helped convert his 1-year fellowship into a 12-year relationship with Barron that was to have an enormous impact on the field of prenatal physiology and its translation into clinical practice. This influence was to be greatly amplified by his early meeting and subsequent collaboration with Fred Battaglia (see earlier in this article) who, as a first-year medical student, was working in Barron's laboratory.

Having convinced himself that maternal–fetal gradients in plasma osmolarity alone could not explain water transport across the placenta, Giacomo turned his attention to the placental transport of oxygen and carbon dioxide, using the expertise in respiratory physiology he had gained in Margaria's laboratory. This was an important, but poorly understood, issue, considering the vital role of the placenta as the 'fetal lung'. Despite the collection of much novel and useful data, including description of the very different oxygen–haemoglobin binding characteristics of fetal and adult sheep (Meschia *et al*. 1961), Giacomo and Barron increasingly became convinced that their inability to satisfactorily explain placental transport of respiratory gases was due to abnormal conditions caused by the stresses of anaesthesia and surgery. This led to their concerted effort to develop a more physiologically normal preparation in which conscious ewes and their fetuses were studied after recovery from catheterisation surgery, as first reported by Meschia *et al*. (1965). The attainment of this 'holy grail' of prenatal experimentation, which had eluded Barcroft and his other successors, including the eminent Geoffrey Dawes, was to open the door to a greatly increased understanding of fetal and placental physiology and metabolism, and amazing advances in obstetric and paediatric practices. Fittingly, it has been included in the 'classic pages of obstetrics and gynecology' published by the *American Journal of Obstetrics and Gynaecology* (Meschia 2006).

In addition to documenting ground-breaking methodology, the 1965 paper conclusively disproved the long-held notion, originated by Barcroft, that fetal blood oxygen saturation declines as term approaches because fetal demand for oxygen outstrips the placenta's ability to supply it (Meschia *et al*. 1965). Earlier attempts to answer this question included a 1958 expedition to the Peruvian Andes to study limits to fetal oxygenation at altitude, thereby directly addressing Barcroft's allusion to late fetal life as resembling 'Mount Everest *in utero*'. This heroic endeavour, led by Barron and including Giacomo and several other researchers, turned out to be a great adventure, but scientifically inconclusive (G. Meschia, unpublished memoir).

In 1965, Giacomo moved to Denver to help form a new perinatal research team with Fred Battaglia and others (G. Meschia, unpublished memoir). There they continued to make extensive use of the pregnant ewe as their experimental model and to exploit the preparation that Giacomo had helped pioneer at Yale. Giacomo's primary interest in the early years was a systematic exploration of placental transport functions, including passage of ^3H_2O and other highly diffusible molecules (Meschia *et al*. 1967), oxygen (Rankin *et al*. 1971) and later, glucose (Simmons *et al*. 1979). At the same time, he collaborated with Fred and a series of talented research fellows to build a metabolic balance sheet that related umbilical uptake of organic nutrients to fetal requirements for energy and growth. The principal findings of numerous studies, as summarised in a seminal review (Battaglia and Meschia 1988), were, first, that in well fed ewes, energy requirements of the fetus are entirely accounted for by its uptake and oxidation of maternal glucose and amino acids, as well as lactate derived from placental glycolysis. Their demonstration of extensive catabolism of amino acids in a rapidly growing mammalian organism in positive energy and nitrogen balance was especially novel. Second, they found that, in late pregnancy, the placenta consumes a large fraction of the oxygen, glucose and, to a lesser extent, amino acids taken up by the gravid uterus. In later years, these observations, particularly those on inter-relationships between fetal and placental metabolism of amino acids, were extended by use of radioactive and stable isotopic tracers.

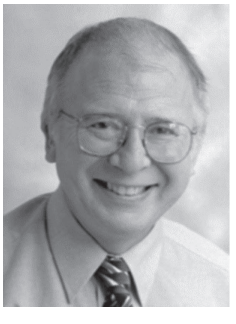
As discussed earlier in my recollections of Fred Battaglia, I was lucky to arrive in Denver in 1983 when he and Giacomo were preparing to commence unique studies of conceptus metabolism and physiology in mid-gestation. Fred's time for active research was constrained by his major responsibilities for administration and clinical service. However, Giacomo had more discretionary time for involvement in our experiments and we spent many enjoyable hours together discussing science, politics, philosophy, literature and other topics while travelling together to and from the research farm outside Denver and jointly sampling our unique preparations. Giacomo's gentle, reflective manner made him the perfect foil for his busy, type-A brother-in-law. This appeared to be tacitly understood by him and Fred and was an important aspect of their obvious mutual respect and affection.

Giacomo was aware of George Alexander's research on chronic maternal heat stress as an effector of placental and fetal stunting in sheep (see earlier in this article). While I was in Denver, we decided to conduct an exploratory study of the effects of maternal hyperthermia on uterine and umbilical blood flows and placental transport of oxygen and glucose. Because appropriate facilities were not available in Denver, I arranged for us to use the environmental chambers of my friend, Don Johnson, in the Department of Animal Science at Colorado State University in Fort Collins, ~100 km up the interstate highway. I still have vivid memories of trucking our pregnant ewes up the highway in freezing winter conditions, only to deposit them in sweltering temperatures after they arrived!

Although our animal numbers were few and there was individual variation in the magnitude of response, our pilot study was largely successful (Bell *et al*. 1987). This encouraged Giacomo, Fred and their colleagues to adopt heat stress as a reliable means of creating a model of placental insufficiency and intrauterine growth retardation that is still being exploited by the group at Denver (e.g. Regnault *et al*. 2013).

More than 30 years on, I consider my year in Denver to be among the most enjoyable and productive of my career, not least because of the astute mentorship and friendly company of Giacomo Meschia whose intuitive understanding of complex physiological processes still amazes me.

Peter John Reeds 1945–2002



Peter Reeds was born in Bath, England, just a couple of months before the VE Day. He graduated from the University of Southampton in 1968 with a BSc (Hons 1) in physiology and biochemistry and went on to PhD studies at the same university, graduating in 1971 (Davis *et al*. 2003). His doctoral research, under the supervision of M. R. Turner and K. A. Munday, investigated the separate and interactive roles of insulin and growth hormone in the regulation of muscle protein synthesis (Reeds *et al*. 1971).

Thus began an interest in the regulation of growth processes that he was to pursue for the rest of his career.

Peter then joined the Tropical Metabolism Research Unit (TMRU) in Jamaica for 3 years as a postdoctoral fellow, working under Professors John Waterlow and David Picou. His mentors would become famous for their research that led to saving the lives of millions of severely malnourished children in the Caribbean and Africa. This surely must have influenced Peter's drive to understand and apply fundamental principles of nutrition to solve important, practical problems. While at the TMRU, he began to use variously labelled isotopic tracers to investigate the nutritional regulation of branched-chain amino acid catabolism in skeletal muscle. He also became initiated in the use of stable isotopes to measure metabolic processes *in vivo*, techniques that would continue to be part of his experimental repertoire for decades to come. A spin-off of this technology was Peter's development, with Alan Jackson, of a novel ^{15}N creatine dilution technique to estimate skeletal muscle mass in malnourished human infants (Reeds *et al*. 1978).

Peter's West Indian experiences with infant malnutrition led to his spending a year in 1975 at the University of Ibadan in Nigeria, studying the influence of infection on the aetiology of kwashiorkor and marasmus in children (Laditan and Reeds 1976), thereby contributing to knowledge of the complex, multifactorial nature of these diseases.

In 1976, Peter was recruited to the Rowett Research Institute in Aberdeen, Scotland, to work on the nutrition and growth of farm livestock. This might seem to have been an abrupt switch in career direction, at least in terms of the application of research findings. However, he was impressed by the Rowett's Director, Kenneth Blaxter (see earlier in this article), for whom his mentor, John Waterlow, had great respect. Also, he saw an opportunity to refine his skills in quantitative metabolism of species such as the pig, for which Rowett nutritionist, Malcolm Fuller, and others had precisely defined the net requirements for individual amino acids and other nutrients.

By the time I first met Peter in 1982, at the EAAP Energy Metabolism Symposium in Lillehammer, Norway, he was already well known in the animal science community for a series of papers co-authored with Fuller, Gerald Lobley and others on effects of age, feed intake and other factors on protein turnover in pigs (Reeds *et al*. 1980) and on rates and energy costs of whole-body and tissue protein synthesis in cattle (Lobley *et al*. 1980). The bar at the conference hotel was prohibitively expensive for impoverished scientists and we spent most evenings in Peter's room drinking weak, tepid beer that we had smuggled in from a local supermarket. Our host was great company, with a mischievous sense of humour and a fund of stories on all sorts of topics. This was the beginning of a friendship that was to grow after we both emigrated to the USA several years later.

At the Rowett, Peter also returned to his early interest in more fundamental aspects of the regulation of skeletal muscle protein turnover, including the role of eicosanoids in mediating the effects of insulin and glucocorticoids (Reeds and Palmer 1983), and he was one of the first scientists to explore mechanisms underlying the potent effects of β-adrenergic agonists on muscle growth (Reeds *et al*. 1986). Peter Garlick, a close friend and colleague from the London School of Hygiene and Tropical Medicine, with similar interests in the endocrine and nutritional regulation of protein synthesis, followed Peter Reeds to the Rowett Institute in the early 1980s. Although they ran largely independent research programs, they, together with Gerald Lobley and Malcolm Fuller, made the Rowett an international powerhouse for research on protein metabolism and nutrition and a magnet for students and visiting scientists.

Peter's long-term interest in human paediatric nutrition was rekindled when, in 1987, he took up an appointment at the Children's Nutrition Research Center (CNRC) at Baylor College of Medicine in Houston, Texas. There, with a talented young team including Teresa Davis, Doug Burrin and Marta Fiorotto, he exploited his experience with animal models, including pigs, deep knowledge of protein and amino acid metabolism, and facility with use of stable isotopic tracer techniques, to conduct wide-ranging investigations of factors influencing rates and patterns of juvenile growth. These included studies of the roles of individual tissues and organs in determining nutrient requirements of infants (Fiorotto *et al*. 1991; Davis *et al*. 1993), development of the concept of 'conditionally essential' amino acids (Berthold *et al*. 1991), and the intestinal preference for dietary amino acids to synthesise mucosal proteins (Fuller and Reeds 1998).

Although Peter had clearly returned to the human nutrition and biomedical science community, he did not abandon his connections to animal science. Throughout his tenure at the CNRC, he continued to be a regular attendee at the annual scientific meetings of the American Society of Animal Science and other livestock research fora to present his research in terms of applicability to the livestock industries (e.g. Reeds 1991; Reeds *et al*. 1996).

In 1988, not long after coming to Houston, Peter was diagnosed with bladder cancer. With typical resolve, he quickly decided to have the offending organ removed and replaced with a section of his ileum. This worked out in the long run, but not before he suffered much discomfort from the substitute bladder's continued secretion of mucin and urinary tract blockages. When I visited him at the CNRC several months after his surgery, I was intrigued to see many small bottles containing a pale, powdery substance lining his shelves and bookcases. These turned out to be samples of mucin that he had extracted from his own urine and was labelling with stable isotopes for animal studies of mucin biosynthesis!

One of Peter's endearing but sometimes frustrating characteristics was his self-doubt despite his enormous scientific talents and personal charm. For example, he was a commanding and entertaining speaker, yet tied himself in knots before a presentation. He confided to me that he once was in such a state at the beginning of a presentation to honour Sir John Hammond that he blanked on the great man's name. I was not in the audience, but such was Peter's natural eloquence that I doubt any of his listeners noticed.

Among his many scientific talents, Peter was a superb editor. He was a highly regarded associate editor for the *Journal of Nutrition* for many years and in that capacity I had numerous dealings with him as both an author and a reviewer. Unlike many in his position, he thoroughly read and digested every manuscript that crossed his desk. This, combined with his unusual breadth of knowledge, put him in a strong position to arbitrate disagreement among referees and he was very willing to use his editorial prerogative when the occasion required.

In 1990, Dale Bauman (see this article) and I invited Peter to spend several days at Cornell, lecturing and mentoring graduate students in the Department of Animal Science. This event was enormously successful and Peter became a magnet for his new Cornell devotees at subsequent national meetings. Given his obvious gift for teaching and inspiring young minds, I always felt he would flourish in a university environment, notwithstanding his achievements during a career devoted almost exclusively to research. Therefore, in 2001, when the opportunity came, I strongly encouraged him to take a position in the Department of Animal Science and Division of Nutritional Sciences at the University of Illinois. Tragically, he died only a year after relocating to Urbana–Champaign, from complications of Legionnaire's disease. Sixteen years later, I and his many other friends and colleagues continue to miss him greatly.

For a more detailed appreciation of Peter's professional life and achievements, readers are referred to the biographical article written by his close colleagues at Houston, Teresa Davis, Doug Burrin and Marta Fiorotto (Davis *et al*. 2003). This is a glowing testament to a man who not only was a wonderful scientist but also an unselfish and empathetic mentor and friend.

Robert Lovell Reid, FRSE, FASAP, FAIAS 1921–1996

Bob Reid was born in the Melbourne suburb of Hawthorn, but lived most of his youth in Sydney where he attended Fort Street High School. After completing school, he took a year off before enrolling in agricultural science at the University of Sydney (Cunningham 1999). Encouraged by his father, who was a company secretary, he spent his 'gap year' working in an accounting office, which was not to his liking. He also may have spent some of his year off on his uncle and aunt's property at Hillston in western New South Wales. The year in question, 1940, is consistent with Bob's frequently mentioned memories of a famous mouse plague that occurred at Hillston in the same year. This experience and earlier happy holidays on the same farm presumably helped motivate this city boy towards his illustrious career in support of the agricultural industries.

At Sydney, Bob specialised in agricultural chemistry and graduated (BSc (Hons 1) in Agriculture in 1944, with a University Medal (Cunningham 1999). Soon after graduation, he was recruited by the CSIR (later CSIRO) McMaster Animal Health Research Laboratory at Sydney University. There he worked with the eminent ruminant nutritionist, M. C. Franklin, on mineral nutrition of sheep (Reid *et al*. 1947). Bob's talent and potential for research were recognised early with his 1946 award of the Thomas Lawrence Powlett Scholarship to study for a PhD at the University of Cambridge (Cunningham 1999). At Cambridge, Bob studied under the great Sir Joseph Barcroft and was a student contemporary of another bright young Australian, Bill McClymont (see earlier in this article). Other highly accomplished colleagues at Cambridge included Andrew Phillipson and Sydney Elsden. Bob thrived in this exciting research environment, completing his degree research in 2 years and making important contributions to the laboratory's rapidly growing understanding of volatile fatty acid metabolism in ruminants (Reid 1950*a*, 1950*c*). On his return to Australia in 1948, he was jointly appointed to a lectureship at Sydney University as well as resuming his research position at the McMaster Laboratory.

There followed a remarkable, sustained series of studies that yielded 19 papers with the primary title of 'Carbohydrate metabolism in sheep', most of which were conducted after Bob transferred to the newly established CSIRO Sheep Biology Laboratory (later the Ian Clunies Ross Animal Research Laboratory) at Prospect in 1954. The series began in 1950 with a simple description of the range of blood sugar values under varying conditions (Reid 1950*b*) and ended in 1962 with a detailed study of the metabolism of glucose, free fatty acids and ketones after feeding and during fasting or undernutrition (Reid and Hinks 1962). The major aim of most of this work was to develop an understanding of the aetiology and pathophysiology of ovine pregnancy toxaemia, with a view to the prevention or, where necessary, effective treatment of this often fatal metabolic disease (e.g. Reid 1960). The findings were summarised in Bob's classic review of the subject in which he explained underlying mechanisms and clearly distinguished the metabolic and pathological symptoms of the several naturally occurring syndromes of pregnancy toxaemia from those induced by experimental starvation or undernutrition of pregnant ewes (Reid 1968). The 'carbohydrate metabolism' series of papers published in the *Australian Journal of Agricultural Research* was accompanied by a steady stream of other publications on topics that ranged from fundamental metabolic endocrinology (e.g. Reid *et al*. 1963; Bassett *et al*. 1966) to practical advice on livestock husbandry (e.g. Reid 1958). The excellent quality and practical utility of all this research was recognised by Bob's award, in 1963, of the Australian Medal of Agricultural Science.

In 1964, Bob left CSIRO to take up the position of Associate Director, and then Director of the Hill Farming Research Organisation in Scotland, the land of his paternal ancestors. There, in just 4 years, he succeeded in galvanising what had been a fairly pedestrian institute to focus on the integration of animal, plant and soil sciences, with emphasis on performance of grazing animals in the challenging Scottish hill environment. In recognition of his outstanding scientific leadership and contribution to Scottish agriculture, he was elected a Fellow of the Royal Society of Edinburgh in 1967 (Cunningham 1999). Bob's sojourn in the land of his fathers also enhanced his appreciation of single malt whiskies, of which he made an impressive collection.

The final phase in Bob's career began in 1968 when, to the regret of many of his Scottish colleagues (Cunningham 1999), he accepted the Foundation Chair in Agriculture at the newly established La Trobe University in Melbourne. Three years later, he was appointed Foundation Dean of Agriculture. His published inaugural lecture, entitled 'Animals in agriculture' (Reid 1969), is a very readable treatise on the history of animal domestication, the role of animal products in human nutrition, and reflections on the future of animal production. With the hindsight of almost half a century, some of his predictions were highly prescient.

Bob threw himself into his new role with characteristic gusto and drive, recruiting a mostly young team of teachers and researchers and developing a novel degree program as a distinct alternative to the more traditional Melbourne University degree in agricultural science. Reflecting his recent Scottish experience, the new degree featured a balanced integration of not only the soil, plant and animal sciences but also agricultural economics and farm management. The School of Agriculture was housed in a brand-new, custom-built building on La Trobe's suburban campus at Bundoora in north-eastern Melbourne. It had some access to facilities on campus for field research and teaching but Bob was opposed to the acquisition of a university farm. After years of being challenged by the management of research farms at Cornell University and CSIRO, I can understand, although not necessarily agree with, his position. On the positive side, this lack of facilities forced the academic staff to collaborate extensively outside the university, particularly with the state Department of Agriculture, which had research stations in most regions of Victoria.

I was well aware of Bob Reid's reputation and had met him briefly when he visited the Prospect laboratory while I was working there with his good friend and former colleague, George Alexander (see earlier in this article). However, I did not really get to know him until 1976 when I was on study leave in Australia from the Hannah Institute. I was unashamedly job-hunting and, as luck would have it, Bob informed me of an upcoming lecturer position in his School that seemed to fit my credentials. I sent him my application before returning to Scotland and, in April 1977, was pleased to be offered the job. Apart from the opportunity for me and my family to return to Australia, the major attraction of the position was Bob himself. Although he was no longer actively involved in personal research, his background in and deep knowledge of metabolic physiology, together with his dynamic personality, was very appealing.

After a year in the job, my expectations were more than fulfilled so I was disappointed when, after a health scare, Bob decided to retire in 1978 to spend time with his wife Catherine (known to all as Cath), who had been so supportive throughout his very busy career. Sadly, Cath died suddenly and unexpectedly in 1980, leaving Bob alone in their large, heritage house at Eaglemont in north-eastern Melbourne. Peter Cranwell, a La Trobe friend and colleague, and I were to visit him there often, to discuss science and sample his collection of malt whiskies. However, he began to spend increasing time on his bush property at Whitlands in north-eastern Victoria and we saw him less often as the 1980s progressed.

After 'retiring', Bob continued to teach human nutrition at La Trobe for several years and was actively involved in various publishing endeavours, including his editorship of the 4th and 5th editions of *A Manual of Australian Agriculture* (Reid 1981) and his publication in 1984 of *Healthy Eating in Australia* (Reid 1984). The latter monograph combined his deep knowledge of nutritional principles with down-to-earth practicality and a recently acquired love of cooking. In addition, he employed his famous editorial rigour to assist CSIRO Publishing as a consultant during the 1980s.

Happily, Bob's health remained stable and, in 1988, he was re-married to a fellow nutritionist, Margaret Robinson, from Canberra University, which led him to move to Canberra. There he became immersed in a variety of avocational activities, including studying Latin at the University of the Third Age. He also discovered the Bureau of Statistics library where he was able to indulge his passion for the accurate acquisition and analysis of data. For example, he had long been annoyed by the unwillingness of successive federal governments to acknowledge progressive increases in the levels of taxation, which led him to publish a paper entitled 'Taxation trends 1965–66 to 1988–89' in the prestigious journal *Australian Tax Forum* in 1991 (Reid 1991). I have sometimes wondered how this publication was received by taxation professionals!

According to his wife, Margaret, Bob retained his vitality and joie de vivre despite failing physical health during the early 1990s. During this time, I was living in the USA and greatly regret being unable to catch up with him before his death in 1996.

Brian Peter Setchell, FIB 1931–2018

Brian Setchell grew up in the Sydney suburb of Punchbowl where he attended East Bankstown Primary School and Canterbury Boys High School. With the assistance of a cadetship from the NSW Department of Agriculture, he studied veterinary science at the University of Sydney, graduating in 1953. During these studies, his interest in reproductive physiology was sparked by the teaching of John Biggers and the renowned embryologist, C. R. ('Bunny') Austin. However, Brian was unable to pursue postgraduate studies immediately after graduation because he was bonded to the Department of Agriculture for 5 years (Setchell 2012). Nevertheless, he was fortunate in obtaining a position as research officer in the Nutrition Research Laboratory at the Veterinary Research Station, Glenfield, to work with Bill McClymont (see earlier in this article) on ovine pregnancy toxaemia. Papers documenting this and their related early studies on brain metabolism are cited elsewhere in this article.

Brian's early research promise was rewarded by a Walter and Eliza Hall Scholarship for PhD studies at the University of Cambridge Veterinary School from 1955 to 1957. There he continued to pursue his interest in brain metabolism (Setchell 1959) under the supervision of Alan Wilson. This also brought him into contact with several famous scientists at lectures and seminars, including Sir Rudolph Peters whose kindness to him Brian continued to remember (Setchell 2012). On returning to Australia in 1957, he could not have known that Cambridge would again play an important part in his career.

Back at Glenfield, Brian continued to work on brain metabolism and various other projects addressing livestock health problems (Setchell 2012). During this period, he first met Geoffrey Waites, a young English physiologist who recently had been appointed to the CSIRO Division of Animal Physiology at Prospect to work on male reproduction. They immediately hit it off and started a personal and professional friendship that lasted until Geoff's death in 2005. Their research collaboration really took off after Brian also was appointed to the CSIRO Prospect laboratory in 1962. Although this appointment ostensibly was to investigate metabolic responses of sheep to underfeeding, Brian and Geoff began to work together on testicular metabolism and physiology in the ram. Their early success presumably explains why the Division Chief, Ian McDonald, never insisted that Brian return to his original remit (Setchell 2012).

Brian and Geoff pioneered methods for measuring testicular blood flow and metabolism in conscious rams (Setchell and Waites 1964) and showed that the testis, like the brain in sheep, depends heavily on glucose for its oxidative metabolism (Setchell and Hinks 1967). Years afterward, when I was teaching nutritional physiology at Cornell University, I compared these data with those in Brian's paper with Derek Lindsay (see see this article) on glucose uptake by the sheep brain. I calculated that, despite the much greater mass-specific uptake of glucose by the brain, the total glucose uptake of the testes in a well endowed ram matches that of its brain because of the greater mass of testicular tissue. For some reason, this revelation greatly impressed my students who, regardless of its relevance to specific examination questions, always found a way to include the brain versus testes information in their answers!

Brian and Geoff's technical breakthroughs led to a series of papers on testicular biology and function, including effects of underfeeding on testosterone secretion and heat exposure on testis metabolism (Setchell 2012). However, their most significant achievement was the development, with J. K. (Sepp) Voglmayr, of a technique for cannulation of the rete testis in sheep, which enabled chronic collection of testicular sperm and fluid (Voglmayr *et al*. 1966). This ultimately led them to propose the existence of a blood–testis barrier, somewhat analogous to the blood–brain barrier (Setchell *et al*. 1969), which Brian considers to be his greatest contribution to the field of andrology (Setchell 2012).

In 1966, Brian was granted a study leave for a year to work with Jim Linzell (see this article) at the Agricultural Research Council Institute of Animal Physiology at Babraham near Cambridge. This productive time included their development of an isolated, perfused ram-testis preparation (Linzell and Setchell 1969) and, with Derek Lindsay, a perfused sheep-liver preparation (Linzell *et al*. 1971). Brian also collaborated with Martin Johnson to show that the relative concentration of immunoglobulin in rete testis fluid versus blood plasma was lower than that of total protein (Johnson and Setchell 1968). Brian's second sojourn in Cambridge clearly whetted his appetite for scientific globe-trotting that would include sabbatical leaves and other research stints in Italy, Finland, France and Sweden. After his return to Prospect, in 1968 Brian organised a national meeting of reproductive biologists from which emerged the Australian Society for Reproductive Biology, a successful organisation that soon will celebrate its jubilee.

When I arrived at CSIRO Prospect to work with George Alexander (see earlier in this article) in early 1969, Geoff Waites had just left to take up the Chair of Physiology and Biochemistry at the University of Reading. He was remembered fondly, not only by Brian but also by many others, including technicians and animal house staff. One of the latter told me about an incident he described as 'payback' for Geoff's surgical insults to his experimental rams. The alleys in the animal houses at Prospect were very long and, when Geoff was bending down to attend to a sheep many metres away, one of his Merino rams escaped from its pen, lined him up and charged, causing a broken collarbone.

Brian was to follow Geoff back to England later in 1969. However, in the meantime, he and George Alexander, with my assistance, completed a study of the effects of cold exposure on the distribution of cardiac output in neonatal lambs (Alexander *et al*. 1972*a*). This would be my only direct experience of working with Brian, although our paths were to cross several times in the future. At this time, Brian's interests in comparative aspects of male reproduction were manifested by the collection in his office of preserved testes from many different mammalian species. Some of these came from animals kept at Taronga Park Zoo where he had an understanding with one of the head keepers, and to which his technician, Dennis Quinlan, was despatched on his motor bike whenever samples from interesting animals became available.

In 1969, Brian accepted the offer of a research position at Babraham. While sailing to England with his family, he continued to work on the English translation of two 17th century Latin manuscripts on male and female reproduction by Regnier de Graaf, a project he had started in collaboration with a former school friend and Latin scholar, Professor Harry Jocelyn, from Sydney University. These were published as a *Supplement of the Journal of Reproduction and Fertility* in 1972 (Jocelyn and Setchell 1972*a*, 1972*b*). The success of this project later encouraged Brian to translate from the Italian Enrico Sertoli's description of the cells that bear his name (Setchell 1984), which was the first English version of the original article published in 1865.

While at Babraham, Brian continued to work on the blood–testis barrier and to collaborate with Geoff Waites on the effects of local heating of the rat testis on fluid secretion and spermatogenesis (Setchell 2012). He also resumed his interest in brain metabolism with Derek Lindsay and a visiting scientist, J. R. Pappenheimer from Harvard (Setchell 2012). In 1973, he was appointed to the WHO Task Force on Methods of Regulating Male Fertility. The funding he received from WHO allowed him to employ a series of talented students and postdoctoral fellows who greatly increased the activity of the already busy Setchell laboratory. A most significant achievement was their production of unequivocal evidence for existence of the hitherto elusive, putative hormone, inhibin (Davies *et al*. 1978). Although Brian's team was unable to isolate and purify this novel peptide, their evidence soon spurred others to do so. During this period, he was awarded a DSc by the University of Cambridge and spent much time writing a substantial monograph, *The Mammalian Testis*, which was published in 1978 (Setchell 1978).

As discussed in the pieces on Derek Lindsay and Jim Linzell, when I was a PhD student at the Hannah Institute during the 1970s, I frequently visited the Babraham Institute. This allowed me to renew my acquaintance with Brian and Derek, as well as to get to know Jim. In 1976, after I submitted my thesis to the University of Glasgow, Brian was selected to be my external examiner. In the British academic system, the oral examination that is central to this process can be a scarifying experience. However, Brian was kind to me and I escaped relatively unscathed.

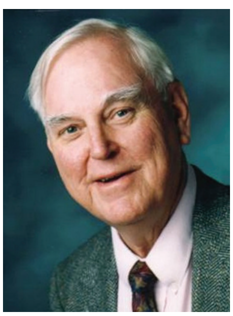
In 1982, Brian was appointed Professor and Chairman of the newly formed Department of Animal Sciences at the University of Adelaide. Although I was at La Trobe University in Melbourne at this time, soon afterwards I spent a year of sabbatical leave in Denver in 1983–1984. Therefore, I was not to see much of him before I moved to Cornell University in late 1985. Despite the administrative challenge of welding together several previously disparate groups and new responsibility for undergraduate teaching, Brian continued to maintain an active research program with the collaboration of Simon Maddocks and contributions from a series of excellent international PhD students (Setchell 2012). Before and after stepping down as Chairman in 1991, Brian undertook a series of productive international study leaves over several years. The last of these, in 1995, included a year in Cambridge working with Professor Azim Surani on the paternal effect on embryo development (Setchell *et al*. 1998). He also resumed his longstanding interest in comparative biology with collaborative studies on testis anatomy and physiology of Australian rodents (Wechalekar *et al*. 2011).

After retiring in 1996, Brian continued to travel and conduct research collaborations on paternal influences on embryo development (Setchell 2012). Most recently, while in his ninth decade, he co-supervised PhD students investigating the effects of obesity on reproduction in males (Bakos *et al*. 2011). An accomplished musician who played several instruments, he also was able to devote more time to chamber and orchestral music, including playing viola with the Burnside Symphony Orchestra.

Brian Setchell had an extraordinary career as an internationally pre-eminent researcher on male reproduction and fertility, particularly testis biology. His work provided the mechanistic foundation for applications in animal agriculture and human health. Accordingly, he received numerous prestigious awards and honours, including Distinguished Andrologist by the American Society of Andrology (1997); Pioneer Award of the Ruminant Reproduction Symposium (2006); organisation of an international symposium on Animal Reproduction in Belo Horizonte, Brazil, to recognise his 75th birthday (2006); and the institution of an annual Brian Setchell Medal by the British Andrology Society (2007).

I am grateful that Brian and his family had the opportunity to read a near-final draft of this appreciation of his life and work before he died in July 2018.

Geoffrey Donald Thorburn, AO, FAAS 1930–1996



Geoff Thorburn was born in the Sydney suburb of Marrickville. His secondary education was obtained at Canterbury Boys High School where his later CSIRO colleague, Brian Setchell (see earlier in this article), would have been a contemporary. He then went on to obtain MB BSc (1954) and BMedSci (1956) degrees at the University of Sydney where, incidentally, his classmates included Gustav Nossal and Jacques Miller (Jenkin *et al*. 2009). After completing his medical residency training in 1958, he joined the laboratory of the eminent physiologist, Paul Korner, at Sydney University before going on to a fellowship (1961–1963) with Clifford Barger at Harvard University. These were formative experiences during which he came to appreciate the essential ingredients of research success, namely hard work and the quality of experimental evidence (Jenkin *et al*. 2009). In both laboratories, he worked on methods for assessing cardiovascular performance, including blood flow to the kidneys and other organs (Thorburn *et al*. 1963).

After returning to Australia in 1963, Geoff was appointed senior lecturer in the newly established Department of Physiology at the University of New South Wales, led by his previous mentor, Paul Korner. His expertise in measurement of organ blood flow led to collaboration with CSIRO scientists, Brian Setchell and Geoff Waites, in studies of testicular blood flow in rams (Setchell *et al*. 1966). This positive experience contributed to his appointment, in 1966, as a Research Scientist in the CSIRO Division of Animal Physiology at Prospect in western Sydney where he saw an opportunity to further his interests in renal function, using the sheep fetus as a model. His timing was propitious because it coincided with the publication by Giacomo Meschia (see earlier in this article) and his colleagues of techniques for studying fetal physiology in relatively normal, conscious ewes (Meschia *et al*. 1965).

Geoff's initial motivation was to use the fetal sheep as a null model for investigating the role of the kidney in the metabolism and elimination of growth hormone. This led to more comprehensive studies of the roles of the kidney and, with his first PhD student, Peter Hopkins, the thyroid gland in regulation of fetal growth, as later reviewed (Thorburn 1974). Geoff also took advantage of the recent development at Prospect of highly sensitive assays for steroid and protein hormones through collaborations with endocrinologists such as John Bassett, Ron Cox and Alan Wallace. This analytical proficiency, combined with Geoff's surgical and experimental skills, led to a plethora of highly original papers on fetal endocrinology (e.g. Bassett and Thorburn 1969, 1971; Bassett *et al*. 1970) and, in particular, corroborative evidence for a fetal role in the initiation of parturition (Challis and Thorburn 1975). His search for the ultimate trigger to parturition was to remain a central theme of his later research at Oxford, Queensland and Monash Universities, as reviewed in two seminal publications by Geoff and his eminent colleague and sometime competitor, Sir Graham (Mont) Liggins (Liggins and Thorburn 1994; Thorburn and Liggins 1994).

In addition to his core research on endocrine regulation of fetal growth and the initiation of parturition, Geoff collaborated widely with other Prospect scientists, including Ron Cox and Phil Mattner, working on regulation of the oestrus cycle in sheep (e.g. Cox *et al*. 1971). The latter work initially was encouraged by Bill Hansel (see earlier in this article) who was at CSIRO Prospect on sabbatical leave from Cornell in 1966–1967, and enabled by a novel surgical technique for direct sampling of utero-ovarian venous blood devised by Geoff and Phil Mattner (Mattner and Thorburn 1970). Another important friend and collaborator was Brian Stacy, who shared Geoff's interest in renal function.

When I arrived at CSIRO Prospect to work with George Alexander (see earlier in this article) in early 1969, Geoff was clearly regarded as one of the leading scientists at the laboratory. Despite my inexperience and his relatively lofty position, he always was ready to chat and offer advice whether in the laboratory or after work at the local pub on Friday nights. His sometimes rather abstracted, professorial manner disguised an element of the likeable larrikin and he could swap yarns with the best or engage in deep scientific discourse according to the company or his mood at the time. As mentioned, Geoff was a talented experimental surgeon who introduced George Alexander and, indirectly, me to fetal catheterisation techniques. In the early 1970s, he still smoked and, during surgeries, would have his non-sterile assistant pull down his face mask and offer him a lit cigarette held by artery forceps. Once, while I was observing an operation, he actually dropped ash into an open laparotomy wound. When his co-surgeon remonstrated he said, 'Don't worry, it's sterile'!

Much of my contact with Geoff's laboratory at Prospect was through social interaction with his students, including Peter Hopkins, Tony Gleeson, and, particularly, Bruce Currie, who would later become a long-term colleague at Cornell University. Bruce's PhD research during the early 1970s focussed on the fetal and maternal endocrine events accompanying parturition in sheep and goats (e.g. Currie and Thorburn 1977) and was an important facet of his supervisor's growing interest in the factors contributing to the initiation of the birth process. My other, more formal association with Geoff was through his collaboration with my boss, George Alexander, on the role of ACTH in accelerating fetal maturation of thermogenic capacity and other systems during late pregnancy (Alexander *et al*. 1972*b*). The latter appeared to include pulmonary function, which was later confirmed and extended by Geoff and colleagues after he left CSIRO Prospect.

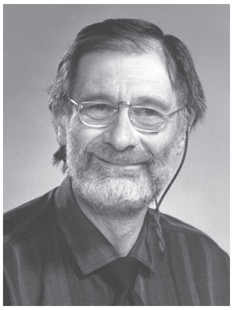
In 1973, Geoff left Prospect to join the Nuffield Institute for Medical Research (NIMR) and the Nuffield Department of Obstetrics and Gynaecology in Oxford, where he also was an external member of the Medical Research Council. His 4 years there overlapped with my time in the UK, at the Hannah Institute, and we crossed paths several times. On one of these occasions, he introduced me to the NIMR Director and renowned prenatal physiologist, Geoffrey Dawes, with whom he had a complex relationship. While at Oxford, Geoff built a research team whose achievements included application of George Alexander's premating caruncleclomy technique to study the underlying causes of intrauterine growth retardation (Robinson *et al*. 1979) and, with John Challis and his group, continued studies of the sequence of endocrine events leading to parturition, especially those involving prostaglandins (e.g. Louis *et al*. 1976). They also investigated aspects of lung maturation that are critical to neonatal survival, including isolation of sheep lung surfactant and elaboration of its physical and physiological properties (Morley *et al*. 1978). This led to the production by others of an artificial surfactant and its highly successful therapeutic application to relieve respiratory distress syndrome in premature human infants.

While at Oxford, Geoff was diagnosed with a debilitating, chronic autoimmune disease that was to plague him for the rest of his life. Despite frequent, crippling episodes that sometimes landed him in hospital, his research drive and curiosity persisted and his reputation continued to grow. According to his biographers, Geoff had long aspired to lead a department of physiology in his home country (Jenkin *et al*. 2009). This ambition was realised when he was appointed Chair of Physiology at The University of Queensland in 1977 and later, in 1981, to a similar position at Monash University where he remained until his retirement. Thus, in the early 1980s, our geographic proximity and overlapping research interests again brought us into contact. This also enabled me to interact with his colleagues, some of whom he had brought with him from Oxford and Queensland, including Graham Jenkin, Richard Harding, David Walker and Ross Young.

My final meeting with Geoff occurred in 1987, after I had joined the Department of Animal Science at Cornell University in Ithaca, New York. To my surprise, Geoff showed up to interview for the chair of Physiology in Cornell's College of Veterinary Medicine. Later, I learned that he had been encouraged to do so by his former student and my colleague, Bruce Currie, who had visited him in Melbourne earlier that year. Geoff was in good form and gave a typically entertaining and insightful interview seminar that highlighted his interests in comparative physiology. However, to me and others the Cornell opportunity seemed no more than a lateral move and I am not sure he would have taken the position if offered. In any case, another candidate was selected and that was that.

Geoff Thorburn was an imaginative thinker, talented experimentalist and forceful academic leader whose achievements were fittingly recognised by many awards, most notable of which were the Marshall Medal of the Society for the Study of Fertility (1989), Fellowship of the Australian Academy of Science (1991) and appointment as an Officer of the Order of Australia (1995). More than 20 years after his death, he is still remembered as a pre-eminent contributor to perinatal and reproductive physiology during the second half of the 20th century.

Peter J Van Soest, FAIC, FAIN 1929–



Peter Van Soest ('Pete' to all who know him) was born in Seattle and grew up on a dairy farm near Snohomish, Washington. He received his BS in 1951 and his MS in 1952, both from Washington State College. His Masters research on calcium and magnesium metabolism was supervised by T. H. Blosser. He then went on to PhD studies at the University of Wisconsin, Madison, where he worked with N. N. Allen on effects of low-fibre diets in dairy cows and goats. Pete's PhD research was summarised in his review on the metabolic aetiology of low milk-fat syndrome, published in 1963 (Van Soest 1963), in which he proposed that a primary cause of the syndrome is reduced availability of acetate for *de novo* synthesis of fatty acids in the udder. Although his theory was eventually disproven, the review was an excellent synthesis of research findings up to that time. It also is interesting to note that Pete is one of four subjects in this compilation who have studied the dietary origins of low milk fat, the others being Annison, Bauman and McClymont.

After receiving his PhD in 1955, Pete spent 2 years at the Walter Reed Army Institute of Research, Washington DC, working on metal chelation. He then joined the Agricultural Research Service, USDA at Beltsville, Maryland, as a research chemist. There, under the direction of Dr Lane Moore, he developed the detergent system of fibre analysis for which he has become universally famous. The early phases of this work were published in a series of methodological papers in the *Journal of the Association of Official Analytical Chemistry*, and summarised in the context of nutrient availability and feeding value of forages (Van Soest 1967). The extent to which Moore influenced the conduct and outcome of Pete's research is not clear, but Pete has always expressed great respect and affection for his mentor to whom his book, *Nutritional Ecology of the Ruminant* (Van Soest 1982, 1994), is dedicated.

Pete's system partitioned the chemical components of forages into two fractions, one that is completely available to all animals, and another, the plant cell wall, that requires microbial fermentation to release energy to the host animal. The latter, extracted with a neutral detergent solution, contains lignin, cellulose and hemicellulose, plus some minor organic and inorganic components. Further extraction with a strongly acidic detergent solution removes the hemicellulose fraction, leaving a residue of mostly lignin and cellulose. The advantages of the detergent system, which have stood the test of time, are its simplicity, repeatability, and, above all, the nutritional relevance of its measures, especially neutral detergent fibre.

In 1968, Pete joined the faculty of the Department of Animal Science at Cornell University where he was to spend the rest of his career. There, with the assistance of Jimmy Robertson, he continued to refine the detergent system, including fractionation of neutral detergent fibre into digestible and indigestible fractions and development of improved techniques for measuring specific non-starch polysaccharide components (Van Soest *et al*. 1991). The latter paper alone has been cited almost 15 000 times since its publication! Pete's single-minded pursuit of feed chemistry brought him criticism from some, including Ron Leng in Australia, who felt that he was ignoring the importance of animal studies in feed evaluation. However, I believe this was unfair because Pete always insisted that accurate and relevant chemical description of feeds was simply the beginning, not the end of the prediction of animal performance.

Pete's research program at Cornell quickly expanded to include animal studies of fibre digestion in domestic ruminants (e.g. Colucci *et al*. 1982) and more than 50 other species, including humans (e.g. Ehle *et al*. 1982; Udén and Van Soest 1982). This work led to his involvement in mathematical modelling of digestive processes to describe the complexity of energy and protein utilisation in ruminants, resulting in evolution and progressive development of the Cornell Net Carbohydrate Protein System (Russell *et al*. 1992; Van Soest *et al*. 2005). Numerous collaborators, including Charlie Sniffen, Danny Fox, Jim Russell and, later, Alice Pell and Mike Van Amburgh, have been important contributors to this endeavour. However, central to the Cornell Net Carbohydrate Protein System approach is appropriate chemical description of feeds in relation to their predicted rates of digestion, and absorption and assimilation of nutrients, for which Peter Van Soest clearly was the linch-pin.

When I arrived at Cornell in late 1985, Pete was well established as a legend throughout the national and international animal nutrition community as well as within the Department of Animal Science. His external reputation rested on the almost universal adoption of his system for fibre analysis. Internally, he was regarded as unconventional, if not eccentric because of his long hair, beard and attire (jeans and jean jacket in winter, ragged cut-offs and T-shirt in summer) and, more particularly, his disregard of the 'expected' behaviour of senior academics. This made him almost a cult figure with students, but, at times, exasperated the Department Chairman, Murray Elliot, who had to deal with missed appointments and often-long, unexplained absences during international travels. However, on the whole, Pete's colleagues tolerated his foibles and greatly respected his astonishing intellect.

Most of Pete's teaching was at the postgraduate level and his course, *Forages*, *Fibre and the Rumen*, which he taught from 1969 to 1995, was regarded as essential even by students with peripheral interests in livestock nutrition. His lecture notes for this iconic course provided the foundation for his book, *Nutritional Ecology of the Ruminant*, the first edition of which was published in 1982 (Van Soest 1982), followed by a second edition in 1994 (Van Soest 1994). Much of Pete's work on the latter was conducted at the Institute of Grassland Research at Hurley, UK, during periods of leave in 1988–1989, supported by funding from the British Agriculture and Food Research Council. When I retired from CSIRO in 2012, I disposed of most of my personal library. However, among the few books I kept is an autographed copy of the second edition, to which I often still refer.

Finally, no account of Peter's life would be complete without reference to his breadth and depth of knowledge of an eclectic range of subjects beyond the natural sciences, including music (classical and other forms), literature, history and gastronomy. His polymathic ability greatly enlivened his teaching and made him wonderful company wherever he went. This included Friday nights at a local watering hole and weekend parties at his cabin in the wooded hills above Dryden, near Ithaca. There, he would lead parties of students through the woods, looking for and explaining the characteristics of unusual edible fungi, as well as expounding on whatever topic was in his mind at the time.

Pete officially retired in 1994, which I must admit was a relief to me when I later became Department Chair! However, he continued to maintain an active research program for another decade, during which he published regularly (e.g. Van Soest 2006) and, in 2001, received the American Society of Animal Science's highest honour, the FB Morrison Award, to add to his numerous previous awards. This accolade was a fitting capstone to a stellar career, especially considering that Morrison, the author of 22 editions of *Feeds and Feeding*, and a former head of Pete's Cornell department, is regarded by many as the father of applied animal nutrition in the USA.

In recognition of Peter Van Soest's outstanding contributions to feed chemistry and evaluation, his colleagues, led by Mike Van Amburgh, recently re-edited and published his laboratory manual (Van Soest 2015).

Final reflections

Not surprisingly, the individual scientists included in this article are mostly animal nutritionists with a sprinkling of reproductive physiologists, reflecting my own research background and experience. This also explains the lack of representation of the third major discipline in the animal sciences, genetics and breeding. However, I have often wished that I had arrived at Cornell in time to have known Chuck Henderson who revolutionised the application of quantitative genetics to livestock breeding during the second half of the 20th century, with an amazing impact on the performance of dairy cattle and other livestock species.

It is perhaps more surprising that all of my scientific 'heroes' are men. However, while there have been eminent female animal scientists throughout the history of the field, their numbers and influence have begun to grow only recently and I never had the good fortune to be even remotely influenced by famous women such as Helen Newton Turner, another illustrious animal geneticist. For example, when I arrived at Cornell in 1985 as a still-impressionable 38-year-old, the Animal Science department faculty of 29 included only one woman, Elizabeth Oltenacu, a geneticist mostly committed to youth extension. Today, of the 15 professors in the department, six are female, including the chair, Pat Johnson.

Several recurring themes are apparent in these semi-biographical pieces. First is the significant influence of early exposure to agricultural practice among those whose careers were devoted to animal production science. This includes not only those who were born into farming families (e.g. Baldwin, Bauman, Black, Foote, Hansel, Van Soest), but also those who gained experience on farms of friends or relatives (e.g. Alexander, McClymont, Reid) or through personal initiative (e.g. Blaxter). Today, less than 2% of people in affluent western societies live on commercial farms and the great majority are at least a couple of generations removed from a family connection with food or fibre production. This highlights the importance of exposing school children to agriculture at an early age and providing meaningful practical experience to university students who, for whatever reason, have been attracted to study the agricultural sciences.

A second theme, not unrelated to the first, is the ability of my subjects to combine the highest-quality science with practical applicability. This characterised not only those whose careers were clearly focussed on the animal production industries, but also biomedical scientists such as Fred Battaglia, Peter Reeds and Geoff Thorburn, each of whom have made great contributions to human health and well-being. The achievements of animal scientists who were able to contribute not only to animal agriculture but also human nutrition and reproductive health, such as Dale Bauman, Bob Foote, Bill Hansel, Brian Setchell and Peter Van Soest, also should be recognised.

A third theme relates to scientific lineage. For example, George Alexander, Fred Battaglia and Giacomo Meschia owe much to their early associations with Donald Barron who was a protégé of Sir Joseph Barcroft. Barcroft's multiple research interests included not only prenatal physiology but also the special features of ruminant digestion and metabolism, which attracted Bill McClymont and Bob Reid to undertake their doctoral research with him at Cambridge. Frank Annison, Derek Lindsay and Brian Setchell also were beneficiaries of this Cambridge legacy. Although not documented in this compilation, the influence of another great Cambridge animal scientist, Sir John Hammond, on the thinking and work of Dale Bauman and John Black also must be acknowledged.

Finally, the individuals in this compilation share the common attribute of enlightened leadership and ability to inspire and enhance the performance of students and colleagues, whether by personal example or by skilful management of large groups of highly intelligent, independent scientists. Perhaps my selection of these individuals was biased by a personal liking, but, in almost every case, their humanity and generosity of spirit is very evident.

Conflicts of interest

The author declares no conflicts of interest.

Acknowledgements

I am very grateful for the input of living subjects, and of family members or former colleagues of deceased subjects, to verify, add to or amend the content of sections of this article. The opportunity to reconnect with colleagues and establish new relationships with family members has been particularly satisfying.

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