LETTERS TO THE EDITOR

Preference is given to letters commenting on contributions published recently in the *JRSM*. They should not exceed 300 words and should be typed double spaced

Surgical correction of refractive error

The article by Mr Brahma and Professor McGhee (March 2000 *JRSM*, pp. 118–123) contains errors of omission and commission. For the past ten years German researchers have studied the effect of using the excimer laser to remove (ablate) portions of the cornea to surgically treat myopia. The procedure can be done either by photorefractive keratectomy (PRK) or by laser *in-situ* keratomileusis (LASIK). They have shown that the majority of patients lose sufficient contrast sensitivity, and therefore night vision, to fail the German visual standard for night driving[1]. As a result of these findings we recalled all of our excimer laser patients and found that 58% had reduced contrast sensitivity making it unsafe for them to drive at night. The excimer laser removes tissue to a very accurate depth by splitting the molecular bonds and vaporizing corneal tissue. Unfortunately, some of the energy disturbs the precise regularity of the perfectly aligned corneal fibrils beneath the ablated area and this reduces contrast sensitivity and therefore night vision.

Your correspondents rightly point out the unfortunate experiences of Professor Sato in Japan[2] when incisions into the posterial corneal surface gradually destroyed the mechanism that controls hydration of the cornea. It took 18 years for 85% of the patients to go blind from corneal oedema and subsequent opacification. These problems have tended to be either ignored or swept under the carpet, but BBC Television's *HealthCheck* programme in July 1999 alerted the public to this complication by documenting some of these findings. Additionally, class actions in the United States are starting to alert the public there to the dangers of using the excimer laser on the human cornea. If we continue to ignore the effect of the excimer laser on the cornea we may also set back refractive surgery by a generation.

Brahma and McGhee dismiss radial keratotomy—now termed corneal microsurgery, to include transverse and arcuate incisions—by quoting the results of the PERK Study[3]. This was a five-year study begun twenty years ago to establish the safety and effectiveness of radial keratotomy for the surgical treatment of myopia; very limited parameters were used and no allowance was made for the patient's age, a major factor affecting the outcome. They seem to ignore the many papers documenting improvements in the safety and predictability of this procedure[4]. No laser study has yet given superior results to corneal microsurgery in both safety and predictability, particularly when four-incision microkeratotomy is performed[5]. Fears of vulnerability to trauma were discounted by Robin[6] who found an incidence of only one in 84 000 ocular ruptures after this operation, similar to that in untreated eyes. This study based on 750 000 cases was ignored. Today corneal microsurgery is still the safest surgical treatment for low to medium degrees of myopia, up to approximately −7.00 D.

Because of concerns about use of the excimer laser some leading refractive surgeons are moving on and treating higher degrees of myopia with lens implants[7].

It is an old saying but one that bears repeating: what we learn from history is that we do not learn from history.

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I should like to draw attention to two omissions in the review by Mr Brahma and Professor McGhee. No mention is made of changing the refraction not by altering the corneal curvature but by modifying its refractive index by the use of polysulfone corneal inlays (polysulfone having a higher refractive index than that of the cornea), which I pioneered in the early 1980s[1]. Although not currently practised because of manufacturing difficulties which arose in providing the inlays with microperforations, this technique is quite likely to be looked at again.

Secondly, when dealing with the complications of LASIK, they do not mention possibly the most serious

complication of all—leaving the patient with a permanently dry eye. In recent years I have built up a substantial medicolegal practice and I have seen in the past twelve months three patients with keratoconjunctivitis sicca consequent upon LASIK (I should, perhaps, call them plaintiffs, not patients). They will probably have to spend the rest of their days instilling artificial tear drops every hour or so—not a pleasant prospect.

For higher myopes the Baikoff angle-supported lenses have given satisfactory results in my hands (96 cases over ten years). I am not the only ophthalmic medicolegal expert in the UK, and if I have seen three such cases, there must surely be many more.

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I know that I am barking up a brick wall but I cannot forbear to cavil at the title of the paper by Mr Brahma and Professor McGhee.

Despite many authoritative dicta over the years it has been clear ever since the work of Sorsby and his associates that the vast majority of refractive conditions including astigmatism are not diseases. They are not caused by visual activity, sexual activity or the 'eating of meat' and are entirely related to the dimensions of the eyeball and its components in a way quite analogous to the factors governing the range of height in a given population.

May one make a final plea, before the descent to the grave, for a concerted effort to replace the word 'errors' with all its connotations of disease and defect with the simple—and truthful—'variations'. For that, if words mean anything, is what they are.

It will be and has been said that 'error' is too widely used and accepted to justify a change. That is no excuse for continuing to tolerate a nomenclature which is the cause of widespread human fear and anxiety.

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The sin of Onan

Dr Evans and Dr Goodman (December 1999 *JRSM*, pp 653–655 and March 2000 *JRSM*, p. 159) are both wrong about the sin of Onan. It was not the act of 'spilling of seed' but his failure to do his duty, that is to impregnate his late brother's wife thereby providing family for her support in old age. In medicine, as in other walks of life, we commonly focus on the superficial defects without seeing the real problem beneath.

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I respectfully write to ask my learned colleagues to examine the evidence again. Careful study of the context (always a good practice when quoting from holy scripture) shows the sin of Onan to have been his persistent refusal to provide his dead brother with an heir. Such an heir would inherit the family property by descent.

God killed Onan for his refusal to submit to this obligation, imposed by the statute of inheritance. This transgression shows a remarkable attitude in the light of the previous death of his older brother, who had similarly refused obedience to the command and likewise expressed contempt for God's Law. This is, I submit, never a healthy attitude.

The mechanism involved in Onan's refusal was only incidental to his real sin. Nothing from this passage should be taken as indicating God's attitude to masturbation or *coitus interruptus*, which trap our authors have again fallen into.

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What Dr Goodman describes (and presumably Onan indulged in or was unable to control) is *ejaculatio ante portam*. *Coitus interruptus* is ejaculation on withdrawal after penetration—or, of course, being caught with one's pants down.

Adrian Landra
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Modified approach to tailgut cyst excision

Costello and others report on a tailgut cyst which, after excision at laparotomy, recurred in the perineum and was subsequently removed via a posterior approach (February 2000 *JRSM*, pp. 85–86). These are the two standard techniques for tailgut cyst excision, with the posterior approach favoured in the largest reported series[1]. We recently removed a tailgut cyst via a modified posterior

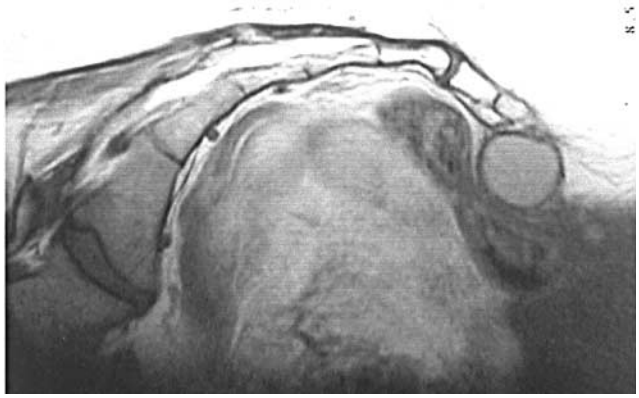

Figure 1 **Sagittal magnetic resonance scan showing cystic mass in precoccygeal space**

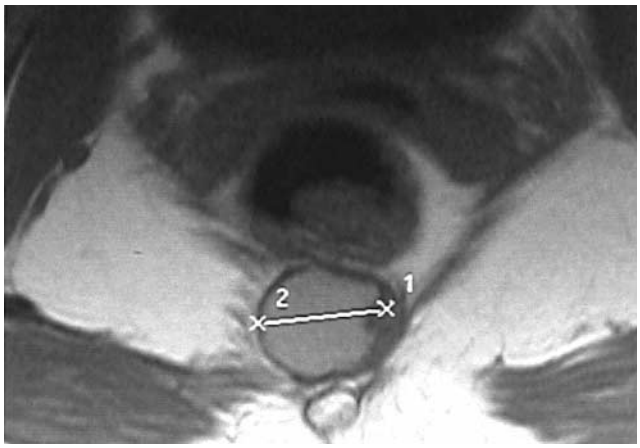

Figure 2 **Axial magnetic resonance scan with cyst measuring 2 cm by 1 cm**

approach involving coccygectomy, a technique that has not previously been reported.

A woman aged 48 complained of sacrococcygeal pain and had a palpable lump *per rectum*. Magnetic resonance imaging confirmed its size and location anterior to the distal coccyx (Figures 1 and 2) and aspiration under ultrasound guidance yielded material of a benign histological nature. To remove it, coccygectomy was performed through a midline incision. This exposed the cyst and nearby structures including the rectum and anal sphincters. As a combined procedure with a colorectal surgeon the cyst was dissected out *in toto* and a sound layered closure was achieved. The patient recovered without complications and was symptom-free two weeks later. The formal histology was that of a retrorectal or tailgut cyst.

We recommend this technique for definitive excision of a tailgut cyst in the precoccygeal space. As a simple and direct approach it has fewer potential complications than a laparotomy. It offers much greater exposure than a standard posterior approach and may thereby prevent the type of recurrence encountered by Costello.

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'Grand mal' in hypotension

Dr Valabhji and colleagues (March 2000 *JRSM*, pp .141–142) have provided an excellent contribution on profound hypotension in occult systemic mastocytosis. However, I would take issue with their sentence, 'A further episode of hypotension and sinus tachycardia occurred in the presence of medical staff, during which initially there was no palpable cardiac output and he had a *grand mal* seizure'. I have spent a long time labouring this point[1–4] but it seems still not to be clear that a '*grand mal*' with no blood pressure or with cardiac asystole is not an epileptic seizure but an anoxic seizure as a consequence of severe syncopal cerebral energy failure. Grossi *et al.*[5] showed in adults that this sort of non-epileptic convulsive syncope could occur with profound hypotension as in the case of Valabhji *et al.* without asystole.

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So you want to write a textbook?

I searched in vain through Philip Gordon's otherwise admirable paper (March 2000 *JRSM*, pp. 150–151) for any mention of the importance of the contract between the author and the publisher. This is a vital omission.

Before any medical author signs a contract with a publisher, that contract should be vetted by an expert. The

best possible course for somebody who has had the outline of a textbook accepted by a publisher is to join the Society of Authors. Members of the Society have access to staff who are expert in all aspects of contract, copyright and publishing, and their advice is free and independent. I am not suggesting that all publishers try to do authors down, but it is better to be on the safe side. This will protect both author and publisher.

In addition to these valuable services the Medical Writers Group of the Society exists to promote good fellowship between medical writers. It organizes meetings, and also awards prizes for several categories of medical textbook one of which, the Asher Prize, is for the best first book. These prizes are being supported this year by the Royal Society of Medicine.

Mary Evans
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I was particularly interested by Dr Gordon's very acute Personal Paper because I was responsible for the RSM publications in the days when the Society used to work with a publishing partner.

While agreeing with almost all the advice he gives, I want to add one perception of my own. To my mind perhaps the most important discussion between would-be author and potential publisher is to make sure that both parties are on the same wavelength over whom the book is intended for. Is it for beginning students, graduate students, fellow clinicians or likely to be bought by libraries only? There is nothing inappropriate about discussing the competition and what the price will have to be if your book is to be bought. The price is often crucial.

I would strongly advise all authors to make sure they have a clear idea of the level for which they are writing and make sure that the publisher is in accord. It is not unreasonable to ask for an addendum to the contract setting out the agreed position. Editors come and go and publishing houses are bought and sold; something firm on paper will make sure that, even if the worst is threatened, the author can withdraw the finished manuscript and offer it elsewhere.

Anthony Watkinson
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It is appropriate that you should have published advice from a surgeon in North America. I have submitted two proposals in past years to different publishers for a book on the early management of spinal and spinal cord injury. There certainly is a gap on the shelves which this book would fill. After initial encouragement, which resulted in considerable preparatory work on my part, both proposals were rejected. The reasons given were similar:

> 'I am not confident that we could sell such a book successfully, particularly in North America. As I am sure you understand, we do depend heavily on this market for sales of our more specialised books.' (Churchill Livingstone, 1986)

> 'Our sales and marketing staff did not feel that there would be sufficient interest in the book outside the UK and this would restrict the print run to an uneconomical level.' (Butterworth Heinemann, 1992)

Sadly it seems that British medical practice must be dictated by the North American market, and therefore by medical practice in North America, this despite the facts that both patient expectations and responses to diseases and their treatment, and the economics of medical practice, differ greatly in that continent from those in the British Isles.

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Dr Gordon provides an excellent summary of the practical issues facing those who set out to write a book. The subject will have a further airing at the RSM's Millennium Members Day on 7 July, when I am to give a short talk on how to get a book published. I hope that existing authors will join us to share their experience and advice.

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Antithrombotic therapy in atrial fibrillation

Dr Adhiyaman and colleagues report underutilization of antithrombotic therapy in atrial fibrillation (March 2000 *JRSM*, pp 138–40). Adjusted-dose warfarin reduces risk for stroke by about 60% compared with placebo, aspirin reduces the risk (primarily for noncardioembolic ischaemic strokes which are less disabling than cardioembolic ischaemic strokes in atrial fibrillation) by about 20% compared with placebo, and warfarin reduces the risk by about 40% compared with aspirin[1]. Contrary to conventional wisdom, elderly patients with paroxysmal (>1 episode) atrial fibrillation seem to have a risk for stroke similar to that of patients with sustained (persistent/permanent) atrial fibrillation[1].

Our hospital participated in the National Sentinel Clinical Audit of Evidence Based Prescribing for Older People conducted by the Royal College of Physicians, London. One arm of the audit was to assess use of

antithrombotics for patients with atrial fibrillation. Prescribing was considered appropriate if aspirin 300 mg was used in patients with contraindication to warfarin. Nationally 50% of the sample of patients in atrial fibrillation were not on warfarin or 300 mg aspirin. As with the paper by Adhiyaman *et al.*, the data pointed to underutilization of antithrombotics[2]. Did they look at aspirin dosage? There is wide variation in the recommended dose (75–300 mg) for patients in atrial fibrillation. Selection of 300 mg as the appropriate dose of aspirin in the National Sentinel Clinical Audit was based on two reviews published subsequent to the Stroke Prevention in Atrial Fibrillation trial. I agree with the authors that if a prescriber judges that a given individual should not receive antithrombotics, that decision should be justified and clearly recorded in the patient's notes.

The key message derived from these audits should be conveyed to junior doctors, nurses and pharmacists; and, if the underutilization of antithrombotics in atrial fibrillation is to be remedied, this education will need to be continuous because of the recurrent changes in junior staff at the frontline.

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We fully agree with Dr Adiyaman and colleagues that guidelines on antithrombotic therapy for atrial fibrillation (AF) are commonly disregarded in clinical practice and that this is most apparent in elderly patients with AF, in whom the benefits are potentially greatest[1]. Indeed, the elderly are underrepresented in the randomized trials of thromboprophylaxis in AF, and in one trial the rate of intracranial haemorrhage on warfarin nearly offset the beneficial reduction in thromboembolic stroke[2]. In view of the concerns over anticoagulation for AF in the elderly, the consensus conference on AF by the Royal College of Physicians of Edinburgh proposed a target international normalized ratio (INR) range of 1.6 to 2.5 for elderly patients with AF aged >75 years, to provide substantial yet partial thromboprophylaxis efficacy with minimal bleeding risk[3]. However, this strategy has not been validated in any prospective randomized controlled trial. In fact, measurement of plasma prothrombin activation fragment F1-2, which provides a better index of *in-vivo* thrombin generation than INR, has shown levels to be significantly higher in older patients with AF than in younger patients despite anticoagulation to equivalent INR values[4]. This study perhaps suggests that elderly patients should be anticoagulated to an even higher INR than younger patients, but at least to a target INR range validated to be beneficial (that is, an INR of 2–3).

The aim must be an acceptable balance between the risk of bleeding and the benefit of preventing thromboembolic strokes. Since attempts at using low-intensity or fixed-minidose warfarin, or even aspirin–warfarin combinations, have been unsuccessful, we are left with a strategy of providing thromboprophylaxis based on risk stratification, so that the highest-risk patients are identified and targeted for anticoagulation[5]. Most risk stratification can be performed on clinical grounds, with some refinement from echocardiography in cases of uncertainty.

Dr Adhiyaman and colleagues also do not fully address the issue of what to do with paroxysmal AF patients, whose risk of stroke is similar to that of patients in chronic AF[6]. The range of stroke risk is likely to be wide, since a patient with a short paroxysm of AF once a year is likely to be at different risk from a patient with prolonged daily paroxysms[7]. Patients aged >75 years with paroxysmal AF who have clinical risk factors or structural heart disease are likely to be at high risk[7].

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Differential diagnosis of varicose veins

Dr Barnett and colleagues (January 2000 *JRSM*, pp. 29–30) correctly point out the difficulty in distinguishing between tricuspid regurgitation and arteriovenous fistula as causes of pulsatile varicose veins.

We recently cared for a patient quite similar to the one reported: 67 years old, she was referred to me with rapidly enlarging varicose veins said to have begun some 4 years previously after the right femoral artery had been used for cardiac catheterization performed in preparation for coronary artery bypass grafting. She had aching discomfort localized to her varicosities and had bled from a varix a few weeks before being seen. Although she was referred with a known diagnosis of tricuspid regurgitation documented by echocardiography, her pulsatile liver edge was not initially appreciated. She had varicosities in her right groin and thigh with pulsations visible as well as palpable. There were smaller varicosities in the left groin and thigh as well. No audible bruit was appreciated over the right femoral artery, and Branham's sign was not present.

Rather than performing arteriography to differentiate between tricuspid regurgitation and arteriovenous fistula, we obtained a blood sample from a thigh varix for blood gas determination which unequivocally documented *venous* blood, obviating the need for the more invasive and costly arteriogram. The patient thereafter underwent excision of her varicosities. Blood gas determination is easily done, and does not require the expertise of a sonographer. Moreover, the cost of this test is much less than that of a duplex ultrasound examination.

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Age-related distortion

Dr Rohde (January 2000 *JRSM*, pp. 35–36) suggests that the phenomenon age-related distortion deserves a more formal name. Could I suggest 'dysjuvemnesia', which, although not lightly tripping off the tongue, encapsulates the elements of memory impairment and its distortion, and also indicates its focus on the recall of youth.

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I had a similar experience to that described by Dr Rohde upon returning to my childhood summer village after an absence of twenty years. My proposed name for this phenomenon: relative eco-diminution.

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The two cultures of medicine

It is stimulating to read Professor Wulff's ideas on the application of 'science and arts' to medical thinking (November 1999 *JRSM*, pp. 549–552). Having run a dental phobia clinic for more than 20 years, involving a rational approach utilizing dentistry, physiology, psychiatry, medicine; surgery, etc., I have found great difficulty in communicating successful treatments to the specialists concerned. Each specialty has its own language and culture. If one does not use the 'correct language or jargon' for each, evidence is dismissed. Specialties also differ in the application of scientific knowledge (e.g. recommendations for the use of heparin/warfarin) country to country, area to area. Will these differences disappear with the acceptance of evidence-based medicine and dentistry by the professions?

How do patients select their medical professionals? In dentistry, patients will travel many miles to a dentist they believe and trust (or is it the devil they know?). It is always useful to be reminded of the human condition and how difficult it is to achieve rapport.

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Tyger tyger

Anand Date cautions that continuing study of tiger injuries will be professionally unrewarding—since the aetiological agent is soon to be eradicated (March 2000 *JRSM*, p. 160). The *International Herald Tribune* of 17 March carries a report of a 3-year-old boy who lost part of his arm upon reaching into the cage of a pet Bengal tiger, in Harris County, Texas. The limb, which was torn off 'between the shoulder and elbow,' was packed in ice and reattached in an 11-hour procedure at Memorial Hermann Children's Hospital. The outcome of the procedure was not stated. Notwithstanding his eloquent poetic allusion, Dr Date's counsel therefore appears premature. Nor, it would seem, can we yet afford

to disregard the advice offered by Hilaire Belloc a century ago when, at the Zoo, Jim 'slipped his hand and ran away' and encountered a related 'aetiological agent':

'He hadn't gone a yard when—BANG!
With open Jaws a Lion sprang . . . '

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REFERENCE

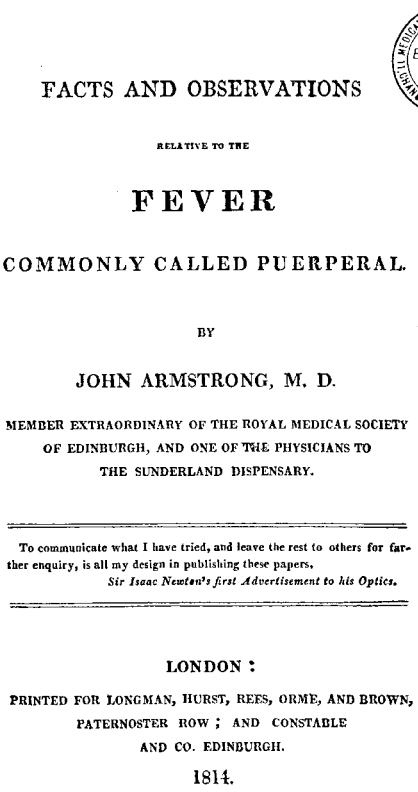
1 Belloc H. Jim, who ran away from his nurse, and was eaten by a lion. *Cautionary Tales for Children* 1896

The cause and prevention of puerperal sepsis

Attribution of the cause/prevention of puerperal sepsis is traditionally given to Ignaz Phillip Semmelweis (1818–65). Working in the obstetric department of the General Hospital, Vienna, he demonstrated (long before the advent of the 'germ theory') that this disease was conveyed by the hands of students and physicians who had previously visited the necropsy room. In 1846, he reduced the mortality rate from this 'infection' from 10–30% to 1% by getting the 'operators' to wash their hands.

However, there are other contenders. Alexander Gordon (1752–1799), in his book *A Treatise on the Epidemic Puerperal Fever of Aberdeen*, which was published in London in 1795, expressed the opinion that attendants carried the infection. Another candidate for the early solution of the cause of this deadly disease was the physician to the Sunderland Dispensary, John Armstrong (1784–1829). In his book *Facts and Observations relative to the Fever commonly called Puerperal*, published in London and Edinburgh in 1814, Armstrong stated: 'I am now well convinced, that when puerperal fever is once generated, there is almost always cause to apprehend its being communicated to other puerperal women, especially by accoucheurs and nurses who have previously waited upon affected persons. This is an important consideration, as it respects the prevention.' There is now ample evidence, therefore, that the underlying cause of puerperal sepsis was well understood many years before Semmelweis' celebrated work.

G C Cook



FACTS AND OBSERVATIONS

RELATIVE TO THE

FEVER

COMMONLY CALLED PUERPERAL.

BY

JOHN ARMSTRONG, M. D.

MEMBER EXTRAORDINARY OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH, AND ONE OF THE PHYSICIANS TO THE SUNDERLAND DISPENSARY.

To communicate what I have tried, and leave the rest to others for farther enquiry, is all my design in publishing these papers.
Sir Isaac Newton's first Advertisement to his Optics.

LONDON:

PRINTED FOR LONGMAN, HURST, REES, ORME, AND BROWN, PATERNOSTER ROW; AND CONSTABLE AND CO. EDINBURGH.

1814.

Abraham Groves

©Venita Jay

This month in history

A general practitioner in rural Canada made history on 10 May 1883, by performing the first appendicectomy in North America. This Canadian pioneer was Dr Abraham Groves (1847–1935), a brilliant and innovative surgeon practising in Fergus, Ontario. In his rural practice with no trained nurses, Groves performed operations on the kitchen table under chloroform anaesthesia. He insisted on absolute cleanliness, using rubber gloves and boiling his instruments. His most famous operation was removal of the appendix of a 12-year-old boy in May 1883. Although taught in medical school that the appendix was a vestigial organ with no function and hence could not become diseased, Groves had noticed the association of abdominal abscesses and perforated appendix. This led him to opt for an appendicectomy, which took place in the kitchen of a farmhouse, with the boy's father administering the anaesthetic. The patient was considerably improved three days later, but the medical establishment did not immediately take to the methods of this 'doctor from the back woods'. Groves was undeterred and had performed some 6000 appendicectomies by 1924. The innovations of this surgeon extended to other arenas of surgery. His book *All in a Day's Work* contains a reprint of his original article on the appendix.

Venita Jay