

Anaesthesia and Pain Management Research Review™

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Issue 15 – 2018

In this issue:

- *Handover of anaesthesia care during major surgery and outcomes*
- *Ultrasound-guided dynamic needle tip positioning vs. palpation for radial arterial cannulation*
- *Neuromuscular blockade in difficult-to-face mask ventilate patients*
- *Proceeding with surgery after resuscitation from intraoperative anaphylaxis*
- *Physical activity level and clinical outcomes in older adults with knee pain*
- *Perineural and intravenous dexamethasone equivalent adjuncts in peripheral nerve block*
- *EAET, CBT and education for fibromyalgia*
- *Chronic insufficient sleep alters pain habituation/sensitisation*
- *Attitudes toward opioids in chronic noncancer pain patients*
- *Rainfall and joint or back pain diagnoses*

Abbreviations used in this issue

BMI = body mass index
CBT = cognitive behavioural therapy
EAET = emotional awareness and expression therapy
OA = osteoarthritis
RCT = randomised clinical trial



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Welcome to the fifteenth issue of Anaesthesia and Pain Management Research Review.

This issue begins with a paper from JAMA assessing the risks associated with complete handover of anaesthesia care during major surgery. Other included research reports equivalent analgesic and safety profiles of adjunctive perineural and intravenous dexamethasone during peripheral nerve blockade. In the pain management section of this issue, there is interesting research looking into the mechanistic relationships between insufficient sleep and pain. We conclude with an analysis of data from a large number of outpatient visits in the US showing no association between rainfall and visits for joint or back pain.

Your comments and suggestions are always welcome, so please keep sending them.

Kind regards,

Dr John Barnard

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Association between handover of anesthesia care and adverse postoperative outcomes among patients undergoing major surgery

Authors: Jones PM et al.

Summary: This retrospective cohort study included 313,066 adults undergoing major surgery lasting >2 hours and requiring >1-night stay; a broad range of specialties were covered. A total of 5941 patients (1.9%) underwent surgery with complete handover of anaesthesia care. Handover versus no handover was associated with a higher risk of all-cause mortality, hospital readmission or major postoperative complications within 30 days of surgery (composite primary outcome; 44% vs. 29%; adjusted risk difference 6.8% [95% CI 4.5, 9.1]) and increases in all-cause mortality (adjusted risk difference 1.2% [0.5, 2]) and major complications (5.8% [3.6, 7.9]) but not hospital readmissions (1.2% [-0.3, 2.7]) within 30 days.

Comment (JB): Anaesthesiology research doesn't get into JAMA all that often, so it is probably worth a look when it does. This research had big numbers (over 300,000 operations), the ultimate primary outcome measure, namely death, and an underlying premise that some of the deaths would have been preventable. Maybe this is what it takes to get in JAMA. The authors were attempting to define the risk associated with an anaesthesiologist handing over care to a second anaesthesiologist partway through an operation, rather than the original anaesthesiologist completing the case. The authors faith in the statistical jiggery-pokery required to adjust for the obvious handover-related associations, like length of surgery, complexity of surgery, multiple operating teams, time of day, etc., exceeded my own by some margin – also the conviction that handover rates should be lower. Some of the most interesting findings were the descriptive statistics rather than the complex inferential analysis. For example, complete handover occurred in 1.9% of cases – is that a lot compared with NZ hospital practice? The average length of surgery was 82 minutes – how does that compare? It is easy to imagine circumstances when handover of care is the sensible and safe option, compared with an increasing exhausted specialist struggling on. Would it be worth considering a written handover, or maybe simply a reasonably standardised template, for the handover conversation?

Reference: JAMA 2018;319:143–53

[Abstract](#)



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Ultrasound-guided dynamic needle tip positioning technique versus palpation technique for radial arterial cannulation in adult surgical patients

Authors: Kiberenge R et al.

Summary: This RCT examined the success rate of a modified ultrasound technique for vascular cannulation (dynamic needle tip positioning; n=132) versus palpation technique (n=128) for radial artery cannulation in 260 adult surgical patients. The first-pass success rate for dynamic needle tip positioning was 83% vs. 48% for palpation (relative risk 2.5 [95% CI 1.7, 3.6]) and the overall 5-minute success rate was 89% vs. 65% (2.4 [1.2, 1.6]). The number of skin punctures was higher in the palpation group (p<0.001). Median cannulation times did not differ between groups (81.5 vs. 76 seconds).

Comment (JB): Ultrasound-guided dynamic needle tip positioning is the phrase used by the authors to describe how they used a short access view to guide the needle to the artery, then in a stepwise fashion moved the probe proximally, lower the angle of the needle then advance the needle tip into the plane of the ultrasound, hoping to see the tip staying in the lumen. Isn't this what most of us do anyway? Maybe with the addition of holding the needle stationary while scanning up and down the region of artery where the needle is. If none of this makes sense to you, the article comes with a helpful video that should help. The study group consisted of anaesthesiologists, trainees and junior residents. The benefit on first-time success rate was evident across all levels of experience. The graph of success against time summed the results up nicely. If the manual palpation technique worked first time, then this was marginally quicker than using the ultrasound, but if more than one pass was needed by palpation, ultrasound-guided was clearly quicker. To add a real-world element, the researchers could have added in a random time element to find the ultrasound machine, and then the other ultrasound machine that had the better high frequency probe.

Reference: *Anesth Analg* 2018;126:120-6

[Abstract](#)

Equivalent analgesic effectiveness between perineural and intravenous dexamethasone as adjuvants for peripheral nerve blockade

Authors: Hussain N et al.

Summary: This systematic review and meta-analysis included 14 RCTs comparing intravenous with perineural dexamethasone for peripheral nerve blockade in adults. Compared with intravenous administration, perineural administration of dexamethasone did not offer any significant incremental benefit with respect to duration of analgesia (ratio of means 1.23 [95% CI 0.85-1.85 {Hartung-Knapp-Sidik-Jonkman}]), motor block duration (1.14 [0.98-1.31]), pain score at 24 hours (standardised mean difference 0.36 [-0.08 to 0.80]) or cumulative opioid consumption (mean difference 5.23mg [p=0.15]). There were no long-term nerve-related complications recorded when perineural dexamethasone was used.

Comment (JB): What does the dose-response curve for dexamethasone look like for the range of effects that are of interest to anaesthetists – for example, the dose-response curve for prevention of postoperative nausea and vomiting, or from this study the dose-response curve for prolongation of analgesia after regional block? For this desired analgesia benefit, does the dose-response curve differ for perineural compared with intravenous routes of administration? Intuitively a smaller dose would be needed if the primary site of action leading to the extended analgesia was at the site of the regional block, e.g. the primary sensory neurons innervating the target tissue. In almost all the studies included in this review, the same dose was given intravenously as given perineurally. Taking a step back from the pharmacodynamics, consider the aim of giving the adjuvant drug – to prolong the analgesia. This is different to a desire to prolong sensory block; in fact sensory blocks persisting at 48 hours and at 30 days were analysed as adverse effects. It is quite a subtle business then expecting a drug to prolong analgesia by influencing the effect of regional anaesthesia yet still have early return of sensation. On the positive side, there was no signal from the data that perineural dexamethasone was dangerous.

Reference: *Can J Anesth* 2018;65:194-206

[Abstract](#)

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Consequences of proceeding with surgery after resuscitation from intra-operative anaphylaxis

Authors: Sadleir PHM et al.

Summary: This retrospective case-control study from Western Australia was conducted to determine whether recovery outcomes after proven acute hypersensitivity reactions (SAFR [Société Française d'Anesthésie et de Réanimation] grade 1 or 2 [25%], grade 3 [56%], grade 4 [17%]) were affected by continuing (n=104) versus abandoning (n=119) the planned surgical procedure. The major hypersensitivity-related complication rate was 0% for grade 1–2 reactions, 4.7% for grade 3 reactions and 12.8% for grade 4 reactions. There was no difference in the frequency of major hypersensitivity-related complications between patients in whom surgery was completed versus abandoned. In patients admitted to the ICU, continuation of surgery was not associated with an increased duration of mechanical ventilation.

Comment (JB): As the authors point out, the question whether or not to proceed with the surgery when anaphylaxis occurs during anaesthesia is unlikely to be answered by an RCT. The best answer is going to come from studies like this one. The authors work in a system where all anaphylaxis episodes are investigated in a single centre for a population of something over 2.5 million. Their data are probably as good as we are going to get. The presentation of the results is excellent, and the discussion balanced and insightful. This is valuable reading for any anaesthetist. If you are unlucky enough to be caught up early in an anaesthetic with managing anaphylaxis, grade 1 or 2, the better cognitive frame to apply may be proceed unless there is a compelling reason not to, rather than abandon unless there is a compelling reason to proceed. Even for higher grade episodes, the evidence presented by this paper is that when you have well-trained clinicians assessing a situation and deciding to proceed rather than abandon, the harm attributable to the anaphylaxis is not obviously worsened. Over the 10-year period studied, the rate of proven anaphylaxis was approximately 1 in 11,000 anaesthetics. Of the 223 cases of anaphylaxis recognised during anaesthesia, 39 were grade 4 (i.e. PEA or impossible to ventilate) and remarkably all of these patients survived. While the lack of a fatality is a notable success, the authors highlight their variability in decision making as a marker that there is room to improve further. With the critical eye of the retrospectroscope, there were cases that were abandoned that should have proceeded, and some that proceeded that should have been abandoned.

Reference: *Anaesthesia* 2018;73:32–9

[Abstract](#)

Independent commentary by Dr John Barnard

Dr John Barnard works as an anaesthetist at Waikato Hospital with a part time academic component. In addition to his role in the operating theatres, four years ago he became the Clinical Director of the Hospital Pharmacy and Chairman of the hospital's Medicines and Therapeutics Committee.



Independent commentary by Gwyn Lewis

Associate Professor Gwyn Lewis is a neurophysiologist based at AUT University's North Shore Campus in Auckland. She obtained a PhD in motor control from the University of Auckland in 2003. Gwyn had an extended post-doctoral experience undertaking research in motor control, rehabilitation and neurophysiology at the Rehabilitation Institute of Chicago. She currently spends half her time teaching in AUT's physiotherapy programme and the other half undertaking pain research in the Health and Rehabilitation Research Institute. Most of her research is in pain neurophysiology and how it relates to persistent pain development, efficacy of pain modulation pathways, and cognitive factors and psychosocial influences.



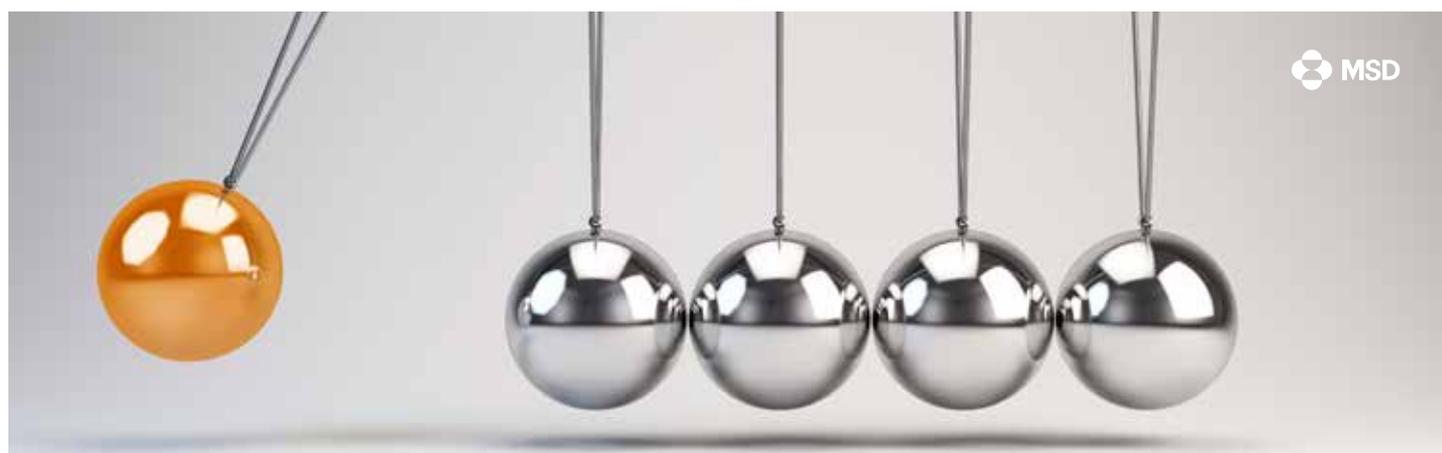
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The effect of neuromuscular blockade on the efficiency of facemask ventilation in patients difficult to facemask ventilate

Authors: Soltész S et al.

Summary: This prospective trial examined the effect of neuromuscular blockade on expiratory tidal volumes in 113 patients with ≥ 3 predictors of mask ventilation difficulty receiving complete neuromuscular blockade with rocuronium 0.6 mg/kg and ventilation via a 'two-handed-jaw-thrust' facemask held and a pressure-controlled mechanical ventilation mode. Median tidal volume increased from 350mL to 517mL 30 seconds after rocuronium administration ($p < 0.001$) and to 600mL ($p < 0.001$) after the onset of complete neuromuscular block. No decrease in tidal volume during the measurements was observed. This included 27 patients with a score of ≥ 4 on the Warters scale, a group of 'difficult to mask-ventilate' patients. Initial tidal volumes were lower in patients who were difficult to mask-ventilate compared with those where ventilation was considered easy. The majority of patients required higher inspiratory pressures to reach the desired tidal volumes, with a mean pressure for all patients of 16.5cm H₂O. Pressures in the 'difficult to ventilate' subgroup were 19.9 vs. 15.5cm H₂O in the 'easy to ventilate' group. Obese patients (BMI > 40 kg/m²) also required higher inspiratory pressures. Mean oxygen saturation increased from 97.5% to 99.2% at 30 seconds after administration of the muscle relaxant.

Comment (JB): Following a standard induction of general anaesthesia, the tidal volume improved by 48% at 30 seconds after the administration of rocuronium. Isn't that a bit like one of these investments that will double you money overnight – i.e. too good to be true. Rocuronium is fast, but is it really that fast? Anyone who has undertaken clinical research will have had 'if only I had thought of' moments at some point in the process. In the discussion, the authors of this paper note that a control group receiving delayed rocuronium would have clarified how much the early improvement in tidal volume was due to the other induction drugs reaching their optimum effect site concentration, or the subject's pre-induction sympathetic nervous system surge dialling down. I wonder at what stage of their research the authors had this thought – hopefully not the via the third reviewer's comments. Regardless, all the patients had features that put them at risk of having a difficult airway, and none of them were more difficult to bag mask once they had been fully paralysed. Of note, a two-handed bag mask technique, with ventilator delivered breaths, and placement of an oropharyngeal airway was routine. No subjects reached the study criteria for a failed airway (ventilation with elevated pressures > 30 cm H₂O, delivered tidal volume < 2 mL/kg ideal bodyweight, no adequate chest rise, no end-tidal CO₂ observed, or oxygen saturation $< 90\%$) with the need to move to a rescue plan. The two patients that were difficult to intubate were both straightforward to bag-mask ventilate using the described technique. Possibly the trickiest part of the research protocol to get right are the factors chosen to predict a difficult airway. For example, what threshold of obesity as a factor would you use? Does a BMI of > 30 kg/m² add the same degree of risk as being a snorer?

Reference: *Anaesthesia* 2017;72:1484–90

[Abstract](#)

Change in physical activity level and clinical outcomes in older adults with knee pain

Authors: Quicke JG et al.

Summary: This was a secondary analysis of data from an RCT investigating the impact of change in physical activity during exercise interventions on future pain and physical function in older adults with knee pain. The trial had consisted of three exercise intervention arms, and 514 primary-care patients aged > 45 years with knee pain attributed to OA had participated. There was no evidence of a significant association between change in physical activity level and future pain, function or treatment response at 3 months or at 6 months across crude and adjusted models. A 10-point increase in self-reported PASE (Physical Activity Scale for the Elderly) score was not associated with pain, physical function or the likelihood of a treatment response at 3 months after adjusting for sociodemographics, clinical covariates and the trial intervention arm; 6-month findings were similar.

Comment (GL): This study is from a well-regarded and -published research group. We know that exercise and physical activity are good for people with knee OA; several studies have shown this and it is included in the guidelines for management. This study took a novel approach to look at the relationship between exercise and outcomes from a previous trial. Although there was an expected increase in physical activity, reduction in pain and improvement in function, there was no relationship between the changes in these variables. Huh! I think this finding was quite a surprise to the authors and I am pleased that they published it (I like a good puzzle). Perhaps you just need to increase physical activity a little bit to benefit, and then anything beyond that doesn't change outcomes? I am certainly intrigued!

Reference: *BMC Musculoskelet Disord* 2018;19:59

[Abstract](#)

Emotional awareness and expression therapy, cognitive behavioral therapy, and education for fibromyalgia

Authors: Lumley MA et al.

Summary: Forty treatment groups, formed from 230 adults with fibromyalgia, were randomised to eight 90-minute sessions of EAET (emotional awareness and expression therapy, developed by the trial's researchers), CBT or education; the retention rate was 90.4%. Compared with education, EAET performed significantly better for overall symptoms, widespread pain, physical functioning, cognitive dysfunction, anxiety, depression, positive affect, life satisfaction and the proportion of participants who reported they were 'much/very much' improved (34.8% vs. 15.4%), but pain severity was not significantly affected. Compared with CBT, EAET was associated with significant reductions in fibromyalgia symptoms and widespread pain and a greater proportion of participants achieving $\geq 50\%$ pain reduction, but with no significant difference for other outcomes.

Comment (GL): While the prevalence is low compared with some other chronic pain conditions, the impact of fibromyalgia is substantial. This study developed and trialled a theoretically-based intervention, which the authors openly acknowledge borrowing and integrating from other techniques. The substantial sample size warrants some mention, but I also liked the active control groups and the fact they evaluated the acceptability of the treatment to the participants. It was great to see this all in one high-quality paper. Another key feature was the extensive effort in training the therapists and maintaining the quality of each intervention arm. So often this is poorly done or poorly reported, yet it is a huge potential source of bias. The statistics are also impeccable. No wonder it ended up in the top pain journal! One finding that is largely obscured until the 'Discussion' is that the average change in pain in the EAET group was $< 1/10$. This is fairly tiny and clinically insignificant; however, the intervention was greatly beneficial for some participants. Like the majority of chronic pain management approaches, it seemed to work well for some but not others, suggesting more targeted application could provide better overall results.

Reference: *Pain* 2017;158:2354–63

[Abstract](#)

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Chronic exposure to insufficient sleep alters processes of pain habituation and sensitization

Authors: Simpson NS et al.

Summary: In this study, 14 healthy adults completed 3-week in-laboratory protocols of restricted sleep with limited recovery (five nights with 4 hours of sleep each night followed by two nights with 8 hours each night versus 8 hours of sleep every night [control]). Compared with the control protocol, the sleep-restriction protocol was associated with mild but significant increases in spontaneous pain. Sleep restriction was also associated with decreased heat-pain thresholds after the first week followed by normalisation with longer exposure to sleep restriction, whereas chronic restricted sleep exposure significantly decreased habituation, and increased temporal summation in response, to cold pain, in weeks 2–3. The authors surmised that such changes may reflect abnormalities in central pain-modulatory processes. Furthermore, these alterations did not resolve completely with limited recovery, suggesting that a greater extent of recovery sleep is required.

Comment (GL): I'm not completely convinced about the novelty of this study as there has been a lot of work looking at the effect of sleep deprivation on nociceptive processing. It turns out this study is an analysis of additional data from a trial investigating the physiological and subjective impact of sleep deprivation, so it wasn't the main goal of the original design. It would have been great to combine the two papers, covering both the physiological and nociceptive system responses in one to provide more insightful information on how stress and pain are linked. One of the conclusions was that different aspects of nociceptive processing (spontaneous pain, habituation, temporal summation) are impacted differently by the hormonal and immune systems. If they had put the data together, this would have been possible to explore! Currently, the article just seems to confirm that sleep deprivation is not great for nociceptive processing and pain, which we already knew.

Reference: *Pain* 2018;159:33–40

[Abstract](#)

Attitudes toward opioids and risk of misuse/abuse in patients with chronic noncancer pain receiving long-term opioid therapy

Authors: Vargas-Schaffer G et al.

Summary: Patients' attitudes regarding long-term opioid therapy and the potential risk of misuse/abuse in patients with chronic noncancer pain were explored and reported in this prospective epidemiological study involving patients who had attended ≥ 1 visit at a tertiary-care pain centre in Montreal during the prior year. Four questionnaires, completed by 370 respondents (response rate ~80%), were used to gather the data. The respondents reported that they had been treated with long-term opioid therapy for an average of 6.31 years, and the median daily dose was 48.21mg morphine equivalents. Responses to the Drug Attitude Inventory Modified questionnaire indicated positive, neutral and negative attitudes toward opioids for 32.16%, 22.16% and 39.73% of respondents, respectively. Opioid Risk Tool questionnaire responses revealed that 86.2%, 13.2% and 0.54% of respondents were at low, moderate and high risk of abuse/misuse, respectively. A low risk of opioid abuse/misuse was evident for 4.2% of respondents according to the Screening Tool for Addiction Risk questionnaire.

Comment (GL): I was a bit sceptical of this paper when I saw the introduction promoted long-term use of opioids for chronic noncancer pain, and indicates that the intent of the paper is to "provide treating physicians with the confidence to deliver the most appropriate care for patients living with chronic pain". Are opioids the most appropriate care? The current (2018) position statement from the IASP on opioid use states caution in prescribing for chronic pain, and that they **may** have a role in carefully selected patients. Only one-third of the participants in the current study had a positive attitude about taking drugs, even though they had been taking opioids for 6 years on average. Interestingly, almost three-quarters felt they were dependent on opioids, but 86% were rated as being unlikely to abuse opioids. It would be interesting to see what those who are not taking opioids think.

Reference: *Pain Med* 2018;19:319–27

[Abstract](#)

Association between rainfall and diagnoses of joint or back pain

Authors: Jena AB et al.

Summary: Using retrospective claims data from 1,552,842 adults aged ≥ 65 years, these researchers explored the relationship between rainfall and outpatient visits for joint or back pain. Of the >11 million visits made by the participants, 18.0% occurred on rainy days. It was found that the proportions of patients with joint or back pain was lower on rainy days than on nonrainy days (6.23% vs. 6.42% [$p<0.001$] and 6.35% vs. 6.39% [$p=0.05$] for unadjusted and adjusted analyses, respectively), which the authors noted was in the opposite direction that they had anticipated, and they also felt that the effect size was likely too small to be clinically meaningful. No significant relationship was seen between the proportion of joint or back pain claims and the number of rainy days in the week of the outpatient visit, or when the analyses were limited to patients with rheumatoid arthritis.

Comment (GL): The title of this study caught my attention. I had visions of underpaid research assistants running around with umbrellas in the rain chasing people with back pain. It turns out it was rather more about data dredging than dodging puddles. In fact, it was a rather superficial look at the relationship between rainfall and joint- or back pain-related visits to primary-care doctors as an attempt to explore the influence of weather on joint pain. After compiling information from over 11 million patient visits, the authors found no relationship between rain and doctor visits. Astoundingly, the authors suggested a relationship may still exist and that larger studies should be conducted. If you need more than 11 million participants, there is something up with your study design!

Reference: *BMJ* 2017;359:j5326

[Abstract](#)



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