Letters

Do enforced bicycle helmet laws improve public health?

Determining true effectiveness of safety measures

EDITOR—Hagel et al have not grasped the main point of Robinson's analysis about mandatory bicycle helmet laws.[1 2] Regardless of how effective a particular safety measure might be in theory, or even empirically from case-control studies, failure to demonstrate any real benefits in whole populations over time must necessarily call into question its true effectiveness—particularly when compulsion is involved.

Helmet proponents are often quick to claim any reduction in cycling fatalities as being primarily due to increased helmet use. However, they have been unable to demonstrate that these reductions are not better explained by reduced cycling, due at least partly to the imposition of helmet laws or other more general traffic safety measures, or both—for example, enforcement of lower speed limits, drink driving crackdowns, red light cameras.

Although there is no particular reason to suppose adult pedestrian and bicycling fatalities should necessarily track each other over time, considerable evidence shows that such fatalities have been strongly correlated in juveniles going back to the 1970s, at least in the absence of enforced helmet laws.

During 1979-2004 the correlation between cycling and pedestrian fatalities for children under 16 is an impressive 0.96 in Britain, and an even more impressive 0.99 in the US—strongly suggesting these trends have moved in virtual lockstep over the past 25 or more years.

Despite increasing helmet usage by juvenile cyclists in both countries over the past two decades, from virtually nil to 15-20% in Britain and at least 30-40% in the US, juvenile cycling fatalities declined by a slightly smaller proportion than juvenile pedestrian fatalities—77.3% *v* 80.5% since 1979 in Britain[3] and 73.3% *v* 75.8% in the US.[4]

Even if we were to ignore the data from Australia and elsewhere and agree that bike helmets might still be substantially effective in preventing potentially fatal head injuries, the failure to observe any such beneficial effect in either short or long term time series data must imply that helmeted cyclists are still managing to get themselves killed at roughly similar rates to their formerly unhelmeted counterparts—presumably through some process similar to risk compensation.[5]

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1 Hagel B, Macpherson A, Rivara FP, Pless B. Arguments against helmet legislation are flawed. *BMJ* 2006;332:725-6. (25 March.)
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ED PRITCHARD/STONE/GETTY IMAGES

Evidence is conflicting

EDITOR—Hagel et al cite six, mostly old, "independent" studies that support their views.[1] Two are their own and another, after correction of an agreed mathematical error finds that helmets provide 186% protection—a clear case of major confounding.

Curiously they omit any reference to the many studies that don't support their views. For example, there is no reference to Rodgers' study of 8 million cyclists in the US that found "the bicycle-related fatality rate is positively and significantly correlated with increased helmet use,"[2] in line with the findings of Robinson and others for New Zealand and Australia. Neither is there a reference to recent work by Hewson analysing UK police and hospital statistics.[3 4] Hewson states. "The conclusion cannot be avoided that there is no evidence from the benchmark dataset in the UK that helmets have had a marked safety benefit at the population level for road using pedal cyclists."[3] And, as has been shown in the Netherlands, you can achieve the lowest cyclist head injury rate in the world with helmets being worn by only one in 1000 of your cyclists.

Whatever the truth about helmets, cycling is an extremely safe activity with a lower head injury rate per km than walking. Head injuries form a lower proportion of all child cyclist hospital admissions in England (38%) than for child pedestrian admissions (44%). Cyclists represent just 7% of all hospital head injury admissions exceeded by trips and falls (42%) and even assaults (11%). One has to wonder therefore why cyclists merit singling out for intervention with helmets.

Those who press us down the path of helmet compulsion in the face of the clearly conflicting evidence have failed to learn the lessons of hormone replacement therapy.[5]

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1 Hagel B, Macpherson A, Rivara FP, Pless B. Arguments against helmet legislation are flawed. *BMJ* 2006;332:725-6. (25 March.)
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Start with an assessment of the base problem, rather than assume a solution

EDITOR—In a subject as polarised as the efficacy of cycle helmet research it is hardly surprising that Robinson's work disagrees with a considerable body of the evidence and that the same can be said of pieces suggesting helmet use is highly beneficial.[1 2]

Much controversy still exists about the effectiveness of cycle helmets, and it shows no sign of abating. This uncertain state of affairs is not a reasonable basis for a major piece of public health legislation such as a compulsory helmet law for cyclists.

Before any further calls are made for such measures we must take a much more informed grasp of the true degree of the dangers of cycling. The data (summarised well by Wardlaw[3]) show that cycling is not particularly more dangerous than being a pedestrian and those accidents that do happen are not especially more productive of head injuries. Though a law requiring

helmets for pedestrians might reasonably be assumed to have a similar effect on public health as one for cyclists, we know as a culture happy with the relative safety of being pedestrians that such a law would be absurd. In countries that have retained a cycling culture a similar view is evident for cycling: helmet use among cyclists is low, and so are rates of cyclist head injuries.

When asking why the UK public (including its legislators, civil servants, journalists, and doctors) has lost its confidence in the safety of cycling, a highly plausible answer is the extraordinary amount of time, money, and effort spent telling us that cyclists are in terrible danger so they should wear a helmet. In comparison, pedestrian safety campaigns do not tell us we are always in terrible danger without a piece of armour: they tell us we are typically quite safe if we behave sensibly, which is also true for cyclists.

Helmets are not a sensible answer for pedestrian safety, even though many of them are injured or killed on the roads every year, and cyclist safety should be approached the same way: promote skilled interaction with other traffic, and ensure the other traffic is in turn operating responsibly.

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1 Robinson DL. No clear evidence from countries that have enforced the wearing of helmets. *BMJ* 2006;332:722-5. (25 March.)
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Physical treatments have valuable role in osteoarthritis

Editor—Hunter and Felson deliver a consistent and seemingly thorough clinical review about managing knee osteoarthritis for primary care doctors.[1] We commend them for promoting the role of non-pharmacological interventions and for highlighting the problem of inadequate funding because of "lucrative opportunities for drug development." However, one striking omission in their review was an appraisal of physical treatments, which are widely used in primary care. A cursory search of the literature identified at least 26 randomised placebo controlled trials and six systematic reviews of physical interventions for knee osteoarthritis in Medline indexed journals and the Cochrane Library.

One of the few examples where the efficacy of physical treatments was tested against drugs is a good quality independently funded trial, in which electroacupuncture showed better pain relief than non-steroidal anti-inflammatory drugs (diclofenac).[2] A Cochrane review of transcutaneous electrical nerve stimulation (TENS) calculated the effect size for TENS *v* sham TENS as 0.38, even when inclusion criteria were not restricted to optimal doses.[3]

Attempts are being made to establish optimal doses for TENS, acupuncture, and low level laser treatment. Recent research in animals has established anti-inflammatory dose intervals for low level laser treatment, and this has been confirmed in situ with humans.[4] A meta-analysis of such laser treatment in knee osteoarthritis when inclusion was limited to trials with optimal doses showed an effect size of 0.71.[5] Optimal acupuncture, TENS, and low level laser treatment also seem to give persisting pain relief in knee osteoarthritis for some weeks after the end of treatment.

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Disorders of sodium balance

Iatrogenic hyponatraemia—water overload and not enough potassium

Editor—Reynolds et al dealt with the important, everyday clinical problem of disorders of sodium balance and clearly stated that most cases of hyponatraemia are iatrogenic.[1] However, they did not clarify the reasons for iatrogenic hyponatraemia: usually a combination of water overload and inadequate potassium replacement. Too many doctors believe that a normal 24 hour requirement for water is 3 l or more, so they prescribe that volume to patients receiving intravenous fluids. If the intravenous fluid used is saline, volume overload can occur rapidly; if it is dextrose solution, the dextrose is metabolised, leaving the water to dilute extracellular fluid.

More importantly, this practice is often accompanied by a failure to provide adequate potassium replacement. The nephron is dedicated to the reabsorption of sodium, but only by exchange excretion of either potassium or hydrogen. The potassium is leached from intracellular fluid to be replaced by sodium to maintain the balance between cations and anions. The hydrogen ions are generated by carbonic anhydrase, with the bicarbonate diffusing "back" into the plasma. The overall result is a decrease in plasma sodium and an increase in plasma bicarbonate, with the treatment being proper replacement of potassium.

Arguably, the real failure of the paper was to ignore (like too many clinicians) the basic physiology of water, sodium, and potassium balance. These balances are very closely inter-related and should have been set out at the start of the review. Should they be printed at the top of every fluid balance chart to remind prescribers of the relevant normal physiology?

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1 Reynolds RM, Padfield PL, Seckl JR. Disorders of sodium balance. *BMJ* 2006;332:702-5. (25 March.)

Hyponatraemia and drug use (and abuse)

Editor—In their article on disorders of sodium balance Reynolds et al did not mention the role of prescribed drugs,[1] particularly under the mantle of "iatrogenic causes"—or indeed illicit drug use[2]—despite citing Adrogué and Madias.[3]

Many drugs are implicated in inappropriate secretion of antidiuretic hormone, including most of the selective serotonin reuptake inhibitors (SSRIs, Prozac-like drugs), which can present with life threatening seizures. It would have been useful to gently remind a general readership of this, and the need to take a drug history.

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1 Reynolds RM, Padfield PL, Seckl JR. Disorders of sodium balance. *BMJ* 2006;332:702-5. (25 March.)
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Hyponatraemia can occur during transurethral resection of prostate

Editor—Reynolds et al do not specifically mention the acute hyponatraemia quite commonly seen during transurethral resection of the prostate.[1] The syndrome may present dramatically, with pulmonary oedema, convulsions, and, occasionally, cardiac arrest when the plasma sodium falls below 100 mmol/l.

Sodium must be corrected urgently or the patient may die. I give 200 ml of 8.4% sodium bicarbonate (200 mmol sodium) as soon as I have diagnosed the syndrome and taken a blood sample to confirm the sodium value. I then correct the sodium

value to 120 mmol with further boluses of 8.4% sodium bicarbonate. Once the sodium value is above 120 mmol, there is little clinical urgency and the sodium concentration can be allowed to rise more slowly with the help of normal saline and furosemide 20 mg intravenously.

I have treated 18 patients in this way without any deaths and without any evidence of brain damage. The transient metabolic alkalosis that accompanies this treatment does not seem to cause any problems to the patient.

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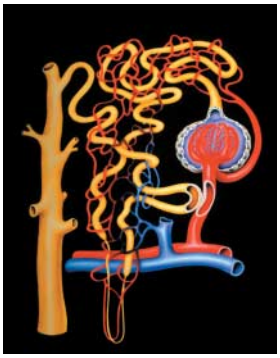
1 Reynolds RM, Padfield PL, Seckl JR. Disorders of sodium balance. *BMJ* 2006;332:702-5. (25 March.)

Hypothyroidism and hyponatraemia: an old wives' tale?



FRANCIS LEROY/BIOCOSMOS/SPL

Editor—Like many other sources, Reynolds et al include hypothyroidism as a secondary cause of hyponatraemia.[1] However, the evidence supporting this association is extremely poor. Studies in babies with severe congenital hypothyroidism have found no change in serum sodium concentrations after treatment with levothyroxine.[2] This has repeatedly been found to be the case in adults too.[3 4] A study performed by our group found no difference in sodium concentrations between 999 patients with a new diagnosis of hypothyroidism from their family doctor and 4875 controls with normal thyroid function.[5] Indeed, none of the hypothyroid patients had a serum sodium concentration <120 mmol/l at diagnosis, while this was present in two of the controls.

Hypothyroidism is a common condition, and so is hyponatraemia in acutely unwell patients admitted to hospital. However, when hyponatraemia and hypothyroidism are found to co-exist, the hyponatraemia is not necessarily a consequence of the hypothyroidism, and so other causes of low sodium concentrations should still be sought. Future guidelines could be helpful in clarifying the misconception of this association.

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Management of hypernatraemic dehydration differs in children

Editor—Reynolds et al recommend rapid correction of plasma sodium when hypernatraemia has developed acutely and the option of administering 5% glucose intravenously or possibly water orally.[1] They have introduced a paediatric link by correctly identifying infants as being a particular risk group for this problem, but they have then not recognised that their recommendation for treatment is the opposite to what is considered in current paediatric literature to be safest practice for management of hypernatraemic dehydration.[2]

In a child patient with hypernatraemic dehydration, once shock if present is treated, the hypernatraemia would actually be corrected slowly using 0.45% sodium chloride intravenously or a commercial oral rehydration solution if possible. Deliberately rapidly correcting hypernatraemia and using a sodium free glucose solution or water would be considered to be potentially dangerous practice, creating an increased risk for the development of cerebral oedema.

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1 Reynolds RM, Padfield PL, Seckl JR. Disorders of sodium balance. *BMJ* 2006;332:702-5. (25 March.)
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The CMA's legitimation crisis

Editor—The Canadian Medical Association (CMA) now faces a legitimation problem over its failure to explain why it fired the *CMAJ* editors. Thousands of medical professionals have signed a petition to reinstate the editors who have brought international recognition to the *CMAJ*. There have been calls from leading medical and science journals, as well as national newspapers, to explain why the editors were fired.

Yet the CMA has offered scant and conflicting information on the issue.[1] It has also lost an opportunity to put the issue to rest, save face and restore confidence among its members, the public, and the international journalistic community. The president of the CMA has noted that the panel formed by the CMA to examine a future governance framework for the *CMAJ* will not investigate circumstances surrounding the editors being fired.[2] Clearly this leaves the issue unresolved. Nor is the CMA bound by the panel's recommendations, as CMA officials have indicated. Against this backdrop it is difficult not to see this governance panel as window dressing. This might help to explain why the bulk of the *CMAJ*'s editorial board and a number of interim editors resigned. It might also help to explain why there has been a call from the academic community to boycott the *CMAJ* as the CMA's flagship sails into uncharted waters with a damaged reputation and skeleton crew.[3]

There are now calls from the academic community to create a new general medical journal in Canada, but with unfettered and guaranteed editorial independence, that would better serve the public interest.[4] The loss of confidence in CMA leadership is growing. The crisis could have been avoided.

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1 Shuchman M, Redelmeier DA. Perspective: politics and independence. The collapse of the Canadian Medical Association Journal. *N Engl J Med* 2006. (Published online 15 March, doi:10.1056/NEJMp068056).
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Guantanamo: a call for action

Good men need to do something

Editor—I agree with Wilks that the declarations of the World Medical Association in relation to force feeding of hunger strikers will have limited effect unless national medical associations are willing to make their members accountable when practice departs from principles.[1] Sadly, it has not been my recent experience that national medical associations and the medical establishment are prepared fully to investigate such breaches.

In response to our letter,[2] Duane Cady (the chair of the American Medical Association, AMA) issued a press statement confirming the AMA's endorsement of the Declaration of Tokyo, yet stating that the AMA is not a regulatory or licensing agency.[3] Although this is factually correct, it is not sufficient for the AMA to avoid this issue when some of its own members have been involved with force feeding in Guantanamo. Clearly, the AMA needs to investigate such serious allegations when the individuals concerned are in danger of bringing the AMA into disrepute.

Secondly, our letter was originally submitted to the *New England Journal of Medicine* in November 2005, but publication was declined, "strictly on editorial considerations," nearly three months later. Finally, Wilks was originally a co-signatory to the letter, and because of concerns within the BMA's legal department, had to withdraw as a co-author. I was not prepared to accept a sanitised version of our letter suggested by the BMA when over 250 coauthors, the *Lancet*, and our own legal advice were all satisfied with the published version.

I therefore fully agree with Wilks that international medical bodies have a basic ethical duty to use their power to enforce their declarations. In relation to Guantanamo, the international medical community could do well to remember the words of the philosopher Edmund Burke: "All that is necessary for the triumph of evil is that good men do nothing."[4]

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1 Wilks M. Guantanamo: a call for action. *BMJ* 2006;332: 560-1. (11 March.)
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Author's reply

EDITOR—I was saddened to see Nicholl's response to my letter since it seems unfortunate that we should risk internal divisions in the face of the major problems that need to be tackled. By implication, he also gives a misleading impression in relation to the BMA's responses to the situation in Guantanamo. The BMA has been in the forefront of lobbying the AMA and other US professional bodies and working with organisations such as Physicians for Human Rights and Amnesty International on this issue.

It is true that I originally intended to be a co-signatory to his letter to the *Lancet* but withdrew following legal advice. I did, however, first ask for a revised version to be considered by the other signatories as the BMA remains committed to the underlying approach. As Nicholl points out, he refused to consider a revised version.

An important part of the reason for my request for the letter to be amended was because the BMA had just finished doing an immense amount of work to revise the World Medical Association's Declaration of Malta. The aim of this work has been to make unambiguously clear the ethical duty of doctors and other health personnel in this kind of scenario. Advice was taken from experts in management of hunger strikes. The BMA is hopeful that the revision—which was submitted to the WMA Council in early March—will generate greater awareness among all national medical associations as well as address the particularly urgent concerns we all have about Guantanamo.

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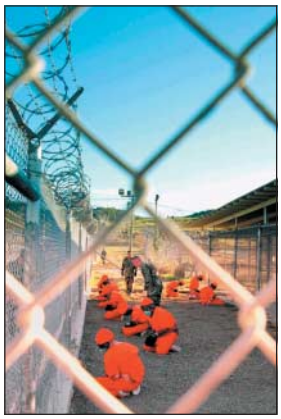
Doctors should not treat detainees only to render them fit for further abuse

EDITOR—Wilks makes a compelling case for doctors to adhere to the ethical parameters set in the Tokyo and Malta declarations.[1–3] Another concern for medical practitioners working in centres where torture is practised must be about treating individuals for moderate to severe medical and surgical conditions, only to render them fit to face further torture and abuse.

Evidence exists that inmates may have various mental health problems in detention centres.[4 5] Furthermore, it can be difficult for the medical practitioners adequately to assess the mental state of their patients when these are in a weak and poor physical condition.

To prevent inmates being exposed to imposed or forced treatments international consensus needs to be achieved on obtaining an independently witnessed (representatives of Red Cross or other such organisations) and explicitly worded written consent. This consent must be reviewed periodically. There should also be an arrangement for legally binding advance directives for catastrophic surgical or medical emergencies.


SIPAREX

UK Prime Minister Tony Blair, in the aftermath of the bombings in London on 7 July 2005, said that the rules of the game have changed. Medical practitioners must not allow politicians to change the ethical basis of medical practice.

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Would GMC dismiss a complaint against Guantanamo doctor?

EDITOR—Recently the Californian Medical Board dismissed a complaint of medical mistreatment brought against Dr E.[1 2] The complaint was dismissed because Dr E was superintendent of medical services at Guantanamo. He works for the military.[1 2]

By dismissing the complaint on these grounds the board has clearly subjugated its authority to the army and legitimised the employment of a modern day state inquisitor. We think that our own General Medical Council acts just as spinelessly, abrogating its responsibilities to unnamed military authorities.

In 2004 one of us (AR) asked the legal adviser to the GMC to consider a report that British doctors regularly supported torture (sorry, "stressing") sessions.[3] He wrote to the British Surgeon General's Office, who "rejected any suggestion of British Forces being involved in torture." More importantly, he informed us that the GMC would not investigate the claim, not because he doubted its seriousness but because the council has a policy of not investigating claims of professional misconduct unless a doctor is named.[4] This stance is clearly absurd. For example, it means that many prisoners who have obviously been abused can never file a complaint because they can never identify the abusing doctor through being hooded, brain damaged, or sedated. Were doctors involved in the recently reported force feeding of detainees by the British in Basra? The British army spokesman refused to give names or other details when requested by one of us (CB-C).

Given the column feet devoted to the abuse of military prisoners, we believe the public and profession could reasonably expect the GMC to issue clear guidelines for doctors working in the military and act when these are not adhered to. We could also expect the GMC to adopt investigative procedures suitable for military situations.

What action will it take over the current military actions against Dr Kendall-Smith?[5] We suggest the GMC does whatever is necessary to remove these Kafkaesque anomalies. It might do well to seek help from Amnesty International, Medact, and Doctors for Human Rights.

We apologise to the GMC if our interpretation is incorrect.

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