Magnesium sulphate and ischaemic heart disease

Editor—We read with interest Wadhwa and colleagues’

paper about the use of magnesium sulphate to reduce the
shivering threshold. We question whether there is any poten-
tial ‘real world’ use for this proposed therapy of high-dose
magnesium in patients with myocardial ischaemia, either
peri-infarction, periangioplasty or perioperative coronary
bypass graft surgery.

We agree that shivering is undesirable due to the
increased oxygen consumption and can increase the risk
of myocardial ischaemia. However, putting such a frail
population, who are already in a compromised cardiovas-
cular state, at the perils of high-dose magnesium infusion
seems in contradiction to their best interests.

Magnesium can cause heart block, the risk being higher in
patients who are already on calcium or beta receptor
antagonists. Treatment with magnesium by continuous
infusion can cause severe muscle weakness. This effect is
significant when serum levels >2 mg litre\(^{-1}\), especially
patients who are in renal failure. Specifically in the post-
operative cardiac surgery population, there are other detri-
mental effects such as profound recurarization in patients
who were treated with magnesium sulphate even at lower
doses (60 mg kg\(^{-1}\)) after 1 h of recovery of vecuronium
block. Magnesium can also increase the incidence of post-
surgical bleeding by inhibition of platelet function and
antagonizing calcium function on the clotting cascade.

We use magnesium, given as a single 2–5 g slow i.v.
infusion to reduce cardiac irritability after cardiac surgery
in most of our patients. In a recent prospective audit of 110
postoperative cardiac surgery patients, we found 25% of
postoperative patients were shivering with a mean core tem-
perature of 36.3 (range 34.6–38°C), despite sedation with
morphine and low dose propofol. We treat shivering patients
with meperidine, and warm them appropriately as required.
However, we are describing a population that is routinely
mechanically ventilated after operation.

Maintaining magnesium levels at twice the normal level
may not cause any detrimental effects in young, healthy
volunteers. However, in sick patients the use of high-dose
magnesium for the prevention of shivering is not
practical. We wish the authors all the best with future
research, which we suspect may be focused more on the
patient at risk of cerebral ischaemia, rather than myocardial
ischaemia.

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Editor—We would like to thank Drs Thanthulage and Stace-
cy for their interest in our work. It is true that a relatively
high, although not supra-clinical, dose of magnesium
sulphate was used in our trial, which included young healthy
volunteers. However, the purpose of this study was not to
propose a high-dose magnesium therapy in patients with
myocardial ischaemia, but to evaluate the potential of mag-
nesium for inhibiting the normal thermoregulatory defenses
to hypothermia in humans. In this type of study, usually
done in healthy volunteers, it is common to adopt an esca-
lating dose scheme or high-dose infusion in order to ade-
quately characterize the dose–response relationship and
identify the magnitude of the effect for the agent under
evaluation. In addition, since the central nervous system
(CNS) is the major site for thermoregulatory effect and
magnesium demonstrates a low CNS bioavailability, a
‘high-dose’ approach was essential in the present experi-
ment. Although various patient populations may respond
differently to a particular agent or dosing strategy, it is
worth noticing that the serum magnesium concentrations
achieved in our study (1.89–2.21 mmol litre\(^{-1}\)) were
lower than those suggested for the treatment of eclampsia
(2.0–4.0 mmol litre\(^{-1}\)) and much lower than those
producing respiratory (5.0–6.5 mmol litre\(^{-1}\)) or cardiac
(>7.5 mmol litre\(^{-1}\)) toxicity. Interestingly, similar serum
magnesium target concentrations (1–2 mmol litre\(^{-1}\)) are
currently used in clinical trials evaluating the neuroprotec-
tive properties of the agent in patients with subarachnoidal
haemorrhage.

Because of the excellent neuroprotective record of mag-
nesium in animals, a corollary of our investigation is the
potential use of magnesium to facilitate the application of
therapeutic hypothermia in patients who suffer from neu-
rological or myocardial ischemic injury. We have managed
to exclude the possibility of magnesium use, as a sole agent,
for that purpose. Nonetheless, a window remains open for the
potential supplementary role of the agent to facilitate
induced hypothermia. We certainly recognize the diverse
and potentially serious complications of i.v. magnesium
administration in sick patients. However, the range of
dose and method of administration, as well as potential
viable combinations with other agents are the objectives
of ongoing and future trials in volunteers and various patient
populations.

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Antiarrhythmic therapy and ECG

Editor—We read with interest Mueller and colleagues’ paper on simulators in teaching antiarrhythmic therapy to undergraduates. The authors conclude that their ‘study provides justification for the use of simulators in education programmes designed for undergraduate medical students’ but we have some reservations.

There did not seem to be any pre-intervention testing. This means that the tendency in the simulator group to use DC cardioversion for the patient with ventricular tachycardia (VT) could have been present at baseline. These are third year students and it is not inconceivable that they could have some knowledge about the treatment of VT from their previous years of medical school. Additionally, this teaching was incorporated as part of a 6-week course, and the authors do not state when during this time the seminars were given. If the students taught with the simulator received their teaching nearer the end of their course than the control group, it would not be surprising if they performed better when tested. Moreover, a repeat test at 3–6 months may help to determine whether the perceived gain in the simulator group is sustained, particularly as skills learnt using simulators cannot be compared with our study in this context. We would surely not expect the same results when investigating the use of a part task trainer in pharmacological presentations! A similar positive effect may have been seen if a third group had been studied that was exposed to a more entertaining teaching method than PowerPoint lectures; for example, videos of ‘real life’ patient treatment or computer-aided learning (CAL). There is evidence to suggest that simulators do not confer any greater educational value than videos and CAL has been demonstrated to be an effective teaching aid for undergraduates.

The authors also talk enthusiastically about their interdisciplinary approach and how they believe that this led to the overall good results. Although it is an attractive suggestion that incorporating anaesthetists in interdisciplinary teaching improves students’ learning experience, it could simply be the ability and enthusiasm of the teachers that was the reason for success and that their individual specializations made no difference. Mueller’s paper does not provide evidence to support the idea that interdisciplinary teaching is more effective as there was no group which had single disciplinary teaching.

Simulators are fun to use but have limitations and are expensive. Costs that need to be considered include the initial cost of purchasing simulators, maintenance costs and the cost of training instructors. Studies have tried to show their effectiveness in teaching but most have concluded only that they are ‘at least as effective’ as other entertaining teaching methods. If expense is no problem, then simulators can be a useful teaching alternative, but more research is needed before their expense can be fully justified for teaching in this area.

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Editor—We thank Drs Brown and Kessell for their comments on our article.

During the 6 week course ‘Basics of Drug Therapy’ one lecture is held on antiarrhythmic drugs. All students took part in our course ‘Antiarrhythmic Therapy and ECG’ after that lecture, there are no additional lectures or seminars on antiarrhythmic drugs. To avoid a bias in pre-intervention knowledge between both groups, we randomized students into control and simulator group. A repeat test would have been helpful to evaluate long-term retention of knowledge on antiarrhythmic therapy but was not done. However, the treatment of arrhythmia is complex and cannot be compared with the retention of basic life support skills as mentioned by Brown and Kessell. A previous study showed good retention of pharmacology knowledge in third year medical students.

The study cited by Brown and Kessell which had previously shown good linking of theory and practice using simulators cannot be compared with our study in this context. We would surely not expect the same results when investigating the use of a part task trainer in pharmacological

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**References**


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