Salt wars

Sir,

Two of the four papers that you noted in your Editorial (September 2009 issue, page 2715) had created ‘some confusion among...’ readers were authored by us. You appropriately called attention to an editorial in Kidney International. An additional, highly relevant article is the award-winning ‘The (Political) Science of Salt,’ by Gary Taubes, published in Science in 1998 [1]. The criticism the NDT editorial apparently encountered is consistent with the conclusion of Sanford Miller, former director of FDA’s Center for Food Safety and an earlier supporter of sodium reduction, quoted in the Science 1998 article: ‘The salt controversy is the number one perfect example of why science is a destabilizing force in public policy’. As Taubes noted: ‘many who advocate salt reduction insist publicly that the controversy is due solely...to influences of the salt lobby’, a view again apparent in the criticism of NDT’s decision to publish the four papers in July 2008.

It is noteworthy that virtually all the studies cited, in our articles and likely the other two, were funded by government or foundation sources and published in credible journals. The critics’ focus on the authors’ consultancies, all disclosed, appears to be a calculated tactic to divert attention from the scientific data—data that raise legitimate questions as to whether the dogmatic push to lower salt intake is without significant health risks for many. Evidence supporting that position continues to mount, as exemplified by a recent report documenting increased morbidity and mortality associated with a low sodium diet in patients with compensated congestive heart failure [2].

As research scientists, we are wise to heed the words of Thomas Huxley quoted in the Science 1998 article and paraphrased here ‘...our business is to teach our aspirations to conform themselves to fact, not try and make facts harmonize with our aspirations’.

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About the effect of low-molecular-weight heparin on platelet function in haemodialysis patients

Sir,

We read the recently published study by Gritters et al. that evaluated the role of the extracorporeal circuit (ECC) and of the low-molecular-weight heparin (LMWH) in platelet (PLT) activation. Using the PLT surface marker CD62p, they concluded that various parts of ECC are responsible for PLT activation during the haemodialysis (HD) procedure. However, LMWH had no effect on PLT activation, but it contributes to the HD-induced bioincompatibility by releasing platelet factor 4 by the endothelium [1].

Our team has also evaluated the effect of the HD procedure on PLT function [2]. We used propyl gallate (PG) slide aggregometry. PG-induced platelet aggregation and tyrosine phosphorylation of multiple proteins are substantially abolished by aspirin, apyrase and abciximab, suggesting that PG is associated with activation of platelet cyclooxygenase 1, adenosine phosphate receptors and GpIIb/IIIa, respectively [3]. It thus seems that PG is a suitable reagent for a global evaluation of platelet aggregation. We found an effect of the type of dialysis membrane on PLT function and that in HD patients PLT aggregation is impaired. The last is in accordance with the study by Gritters et al. that, considering the lower levels of serotonin in PLTs of HD patients, suggests that repetitive stimulation by HD results in a state of PLT depletion and exhaustion [1]. However, we found that LMWH affects PLT function in HD patients. A statistically significant negative correlation was almost reached between the dose of tinzaparin sodium, calculated as units/kg of dry body weight and the time needed for platelet aggregation as measured before the start of the HD session ($r = -0.332, P = 0.056$). This negative correlation became much stronger and statistically significant after the HD procedure ($r = -0.428, P = 0.009$), i.e. near tinzaparin administration indicating a short-term effect of this LMWH on platelet aggregation.
Unfractionated heparin enhances platelet aggregation in HD patients [4,5]. Regarding the effect of LMWH, in one study it did not alter ADP- or collagen-induced platelet aggregation during dialysis [6], but in another one no differences between unfractionated heparin and LMWH were observed [7]. The positive impact of LMWH on platelet aggregation, found in our study, could be the result of the reduced microthrombus formation accompanied by less fibrinolytic mechanism activation and fibrinogen fragments production during the HD procedure. Sreedhara et al. showed that platelet dysfunction in chronic HD patients results from decreased GpIIb/IIIa availability due to receptor occupancy by fibrinogen fragments [8]. Additionally, Sobel et al. have detected that unfractionated heparin directly binds to GpIIb/IIIa modulating its function [9]. This aspect has not been extensively evaluated for LMWH.

In our opinion, the effect of LMWH on PLT function in HD patients needs further evaluation.

Conflict of interest statement. None declared.

Editorial Note: Dr Gritters et al. have been invited to reply to this letter but we did not receive a response in time.


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Letter and Reply

Advance Access publication 25 December 2008

Mystery of pentraxin-3 not yet resolved: still a long way to its prime time in surgery

Sir,

With great interest we read the article [1].

Since pentraxin 3 (PTX3) is a leading topic of our own research, we feel that we might add some comments on the authors’ findings. It was their declaration that PTX3 levels increase dramatically after surgery that incited us to open this discussion. On the basis of our current knowledge, we must strongly argue against this unfounded contention. While there is no doubt that plasma concentrations of PTX3 do increase in sepsis and in endotoxic shock, just the same as they do after myocardial infarction, to the best of our knowledge no one has up till now established PTX3 levels after surgery. The authors did not address this issue in their study. The disputable contention of theirs has been extracted from a surgical study performed by Hampel and co-workers [2]. Nevertheless, we must insist that ‘living kidney donation’ represents a tiny and an altogether specific fraction among surgical patients. This specificity, which resides in explanting one kidney from a healthy organ donor, has most probably elicited the increase of PTX3 levels in the quoted study. Moreover, this PTX3 elevation went hand in hand with increase of plasma CRP (C-reactive protein) levels, which also is an unusual finding [3–5]. Of note, the questionable declaration about PTX3 rising dramatically after surgery has been borrowed from a study that included all in all six patients.

As far as PTX3 research is concerned, we must concede that the surgical community seems to stand hesitatingly apart from the scientific mainstream. Three years ago, when we designed our own study that was intended to gain insight into PTX3’s behaviour in cardiac surgical patients, we started by searching the Medline database for similar studies in order to avoid any duplicity. However, using the mesh headings: ‘pentraxin 3’, ‘coronary artery bypass grafting’ (CABG), ‘cardiac surgery’, ‘cardiopulmonary bypass’ (CPB), or quite simply ‘surgery’, we did not find any relevant reference.

Our PTX3 study, realized in the autumn of 2005, searched for an answer to the question: ‘Does cardiac surgery increase plasma levels of PTX3 similarly to or differently from CRP?’ We randomly enrolled 40 CABG patients to be operated either with the use of CPB or avoiding CPB [6]. Study participants were purposely confined to otherwise healthy subjects in whom an uneventful peri- or post-operative course was expected. Since the heart is supposed to represent one of the most important sites of PTX3 production, even if the inflammatory process attacks primarily another organ, e.g. the brain [7], our aim was to set