the analysis of the data. It is interesting to note, however, that also in our study ‘good sleepers’ have Hb values significantly higher than the ‘poor sleepers’, even if they do not reach the values obtained by Iliescu et al.: this confirms that a clinical trend to a better sleep quality exists by raising Hb levels.

In summary, although the data of statistical analysis must be clearly kept in mind, to raise Hb levels above our reported values remains a possible way to challenge the bad quality of sleep and to diminish the clinical risks associated with sleep disorders [2].

Conflict of interest statement. None declared.


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Reply

Sir,

Thank you for the opportunity to comment on the letter of Sabbatini et al. in the current issue of the Journal, regarding the association between haemoglobin level and quality of sleep in haemodialysis patients. The authors measured haemoglobin level and quality of sleep using the Pittsburgh Sleep Quality Index (PSQI) in a cross-sectional study of 249 prevalent haemodialysis patients. The authors compare the results to those of our study of 89 prevalent haemodialysis patients [1], and seek to explain the differences. In both studies the mean haemoglobin level was higher in subjects with PSQI < 5 (good sleep) compared with those with PSQI > 5 (poor sleep). In our study the haemoglobin level was weakly but significantly correlated with PSQI (Spearman r = −0.27, P < 0.01). In the Sabbatini study this correlation was weak, and did not reach statistical significance (r = −0.09, P = 0.17). Sabbatini et al. found that haemoglobin was not an independent predictor of PSQI (≤5 vs > 5) in multiple logistic regression adjusting for dialysis unit and age.

In bivariate analysis, the comparison of mean haemoglobin among categories of PSQI (≤5 vs > 5) is superior to the comparison of continuous variables by correlation because the PSQI is primarily intended as a categorical instrument to identify ‘poor sleepers’ [2], and because PSQI may not be normally distributed. In the categorical analysis the two studies have very similar results. In regards to the multivariate analysis, we agree with the authors that both studies were underpowered to examine the independent influence of haemoglobin on quality of sleep while controlling for the large number of possible confounding variables.

We conclude that the results of the two studies are similar. They support the hypothesis that haemoglobin influences quality of sleep. While there is evidence for biological plausibility [3], this association clearly does not satisfy the criteria for causation at this time. Inclusion of the PSQI and polysomnography as outcome variables in prospective studies that randomize patients to different levels of haemoglobin would bring us closer to the ‘truth’.

The similarity in the prevalence of poor sleep observed in the two studies, despite two culturally different populations, is remarkable and speaks to the validity of the PSQI as a tool for comparing sleep quality among different populations.

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Primary care management of chronic dialysis patients: emerging challenges

Sir,

The article by Zimmerman et al. [1] on the diverging attitudes of patients towards their own primary care from their physicians adequately describes the challenges faced by the physician in an era of scientific uncertainty, with increasing need to see additional patients, delivering evidence-based health care, while being responsive to patients’ individual concerns. However, there could be several additional reasons to explain the variable pattern of physician behaviour (nephrologist vs family physicians) in delivering primary care to dialysis patients.

The widely variable rates of colon and breast cancer screening in patients, among nephrologists and family physicians (table 1) highlights the need for complementary efforts on the part of both group of physicians when it comes to taking care of these patients. Redelmeier et al. [2] have reported that clinicians often overestimate the risks of adverse drug reactions but may underestimate the risks of systemic disorders, and often fail to treat unrelated disorders in patients with chronic disease. There is also a difference in the

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approach of nephrologist and family physicians, as the former tends to feel comfortable with diagnostic certainty while the latter group of physicians often practice under circumstances of uncertainty. Stacey [3], using his certainty-agreement diagram, has masterfully described the response of most individuals and organizations in any complex decision process. Most decisions can be classified as simple (high certainty, high agreement), complex (intermediate certainty, intermediate agreement, one or both) and chaotic (low certainty, low agreement). It is conceivable that most of the speciality care often revolves around making decisions with high certainty and agreement.

Recent developments in medicine provide the physician with newer approaches to understanding the scope of the complex adaptive processes of medical decision making. In general, such decisions involve a collection of individuals free to act in ways that are not totally predictable, yet their actions are interlinked in such a way that one person’s actions change the context for the other [4]. It is widely recognized that using an analytically reductionist approach to medicine may not be easily applicable to several problems faced by the physician. Clinicians should have to be increasingly aware of their own inertia in treating patients with chronic disorders [5]. Among the recommendations to avoid clinical inertia are included continued emphasis on evidence-based guidelines and emphasis on improving clinical care and routine use of computerized or paper flowsheets to follow diagnostic tests and monitor therapy. I totally concur with the authors that better communication among physicians caring for chronic dialysis remains the first step in the co-management of chronic dialysis patients.

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Reply

Sir,

I would like to acknowledge the insightful comments of Ghosh with regards to our study of primary care in dialysis patients. While I agree with many of the comments, I believe a couple of issues require further elaboration. I think some of the ‘inertia’ in the treatment of dialysis patients stems from the lack of evidence to support applying the same guidelines of care to our patient population as the general population. Perhaps the area where this has been explored the most is in the cancer screening literature, but also extends to other therapies such as ASA for stroke prevention. The risk/benefit ratio may be very different in the dialysis population. This lack of evidence inhibits the development of guidelines that are more than just expert opinion. This issue will be highlighted in the next DOQI guidelines on the management of cardiovascular disease in patients with end-stage renal disease. We need well-designed studies to address our lack of evidence in order to improve the overall care of patients with kidney failure.

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