6 months of initiating PD and another has one peritonitis episode 2 years after starting PD, a relative risk analysis would consider these patients to be equivalent with respect to the outcome because the occurrence of peritonitis is registered only as a ‘yes/no’ event (despite the fact that most clinicians would agree that these patients are quite different from one another). Based on these concerns, we would argue that relative risk is not a meaningful way to assess the association of any variable with peritonitis.

With regard to the authors’ conclusion that APD is associated with a lower peritonitis rate than CAPD, we note that this conclusion is based on only two randomized controlled trials. Of these trials, the study by Bro et al. [2] reported only three total episodes of peritonitis (one on APD, two on CAPD), likely related to the relatively short (6 month) follow-up time in this study. Since so few peritonitis episodes occurred in this trial, the results of the ‘review’ of the effect CAPD versus APD on peritonitis are almost entirely driven by the findings of the other trial by De Fijter et al. from 1994, showing a lower peritonitis rate with APD. The conclusions of the review with respect to peritonitis should therefore be interpreted with caution. While we commend the authors for trying to synthesize the existing literature on CAPD versus APD into a systematic review, the paucity of data available from randomized trials limits the value of this exercise.

Conflict of interest statement. Dr. Bargman has received speaker honoraria from Baxter, and Dr. Nessim received educational fellowship funding from Baxter in 2006.

1Division of Nephrology, St. Michael’s Hospital, 30 Bond Street, 8CC Toronto, ON M5B 1W8
2Division of Nephrology, Toronto General Hospital, University Health Network, 200 Elizabeth Street, 8NU-840 Toronto, ON M5G 2C4, Canada

E-mail: nessims@smh.toronto.on.ca


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Reply

Sir,

We agree with Nessim and Bargman’s observation that when determining clinical outcomes with respect to peritoneal dialysis (PD) catheter-related infections rates are more appropriate than relative risk, for reasons we have already stated in our paper [1]. Whilst it is encouraging to note that APD significantly reduces peritonitis rates by almost 40% according to our paper, we agree with Nessim and Bargman that this result is based mainly on the results of the trial by De Fijter et al. [2]. In the Discussion section, we clearly stated the inherent limitations of the trials (very few trials with small patient populations, variability in their design and high dropout rates) included in this review and advised caution in interpreting the results due to those limitations. Given the impact peritonitis has on clinical outcomes for PD patients, we feel the results from this review should provide the impetus for large-scale trials comparing APD and CAPD to provide us with more robust and reliable data to inform clinical practice.

Conflict of interest statement. None declared.

Renal Unit, Churchill Hospital, Oxford OX3 7LJ, UK
E-mail: ksrabi@yahoo.co.uk

E-mail: nessims@smh.toronto.on.ca


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Relative mortality risk in chronic kidney disease and end-stage renal disease: the effect of age, sex and diabetes

Sir,

We read with great interest the article by Raymond et al., published in the November issue of the journal [1]. In a large population-based study in the UK, the authors explored the excess mortality in patients with chronic kidney disease (CKD) against a reference population with an estimated glomerular filtration rate (eGFR) over 60 mL/min, by CKD stage and by age-band. One of the main results was that excess mortality decreased when subject age increased in each CKD stage.

This expected result is consistent with the findings of our team in a regional end-stage renal disease (ESRD) population in France [2]. This result is expected, because in the adult general population, mortality rates are lower in younger subjects and increase with age (as shown in Table 3 of the paper [1]). But the health gap and the absolute mortality risk gap between a population with a given chronic disease and the general population narrow down when age increases. This leads to a ‘natural’ decrease in a mortality ratio when age increases, as shown for an example in the obese population [3].
Moreover, assessment of excess mortality is usually performed with standardized mortality ratios (SMR) developed by Breslow and Day [4]. Ratios should be at least standardized for age and for gender [4], and should take into account actual lengths of subject observation [2,4].

Beyond the methodological issue, adjustment for gender is clinically relevant because mortality rates are lower in females in the general population in each age-band [5]. As in younger patients, SMR were significantly higher in ESRD female patients than in males [2]: ESRD cancelled out the female survival advantage observed in the general population.

Excess mortality in diabetics with CKD is another key issue. In the ESRD population, age- and gender-SMR were higher in patients with diabetic nephropathy than in patients with other nephropathies [2]. SMR gap between female and male was also higher in patients with diabetic nephropathy [2]. This interaction between gender and diabetes was confirmed in the Australia and New Zealand ESRD population [6]. The question raised by these results is the comparison of the effect of ESRD and CKD in diabetes and in non-diabetes populations with computation of age-, gender- and diabetes status-SMR.

In Raymond et al.’s population-based cohort, age, gender, diabetes status, eGFR and mortality rates are available data [1]. Additional analyses by gender and by diabetes status could therefore usefully improve the knowledge of interactions between age groups, genders and diabetes in CKD population mortality.

Conflict of interest statement. None declared.

Hospices Civils de Lyon, Emmanuel Villar
Department of Nephrology and Michel Labeeuw
Renal Transplantation, Lyon-Sud
Academic Hospital, Pierre-Benite, France

E-mail: emmanuel.villar@chu-lyon.fr


Editorial Note: Dr Raymond et al were invited to respond to this letter but we did not receive their reply.

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Conflict of interest in clinical guidelines should be avoided

Sir,

Responding to your request to generate a lively discussion on the nuantial Editorial Comment on the latest US KDOQI Anaemia Guidelines update [1], I will attempt to be extremely critical of several of the comments made by the authors. Firstly, allow me to state that their conflict of interest declarations show all three of them to be heavily involved with the EPO companies. The comment would have had far more credibility had your editorial policy followed recommendations published in the Lancet in 2006 by Richard Steinbrook, in which he stated that ‘Given the billions of dollars at stake for the drug and dialysis industries, such guidance is likely to receive the broadest acceptance if developed without industry support, and by experts without relevant financial associations’ [2]. The extremely rapid acceptance of the comment (1 day after reception) suggests that the authors’ position in this ongoing debate was already accepted prior to reception. Their strident criticism of current guidelines invoked by Strippoli et al. in the Lancet [3], in which the authors had no conflict of interest, confirms my suspicions that their commentary must be viewed more critically.

Their pleas to reconsider quality of life data are scientifically unacceptable, as to date there is no evidence that meets the minimal requirements of objectively collected data. Similarly, the levels of haemoglobin stated are given without scientific evidence of benefit and the overswings undoubtedly put the patients at risk. The argument that one can avoid blood transfusions is not relevant, as clearly it has been possible to maintain patients on haemodialysis indefinitely without blood transfusions [4,5], before the availability of EPO and with no apparent detriment to their survival. Indeed, the world renowned centre at Tassin, La Demi-Lune, has for many years published the best survival data on dialysis in the world with the least consumption of erythropoietin [6].

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Fontvieille, Monaco

E-mail: shaldon@libello.com

3. Strippoli GFM, Tognoni G, Navaneethan SD. Haemoglobin targets: we were wrong, time to move on. Lancet 2007; 369: 346–350

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