group is mentioned as 12. However, as noted by the authors, 8 of 12 died prior to starting dialysis. Another 16 in the dialysis group never started dialysis prior to the study completion date. How were these 24 patients managed? We presume that they were given the same treatment that was offered to patients in the conservative arm before starting dialysis. In an as-treated type of analysis, the total number of patients who actually received dialysis would be 52 – 24 = 28, out of which 4 would have died following the initiation of dialysis. Likewise, at initiation, the conservative treatment arm would have started off with a total of 77 + 24 = 101 patients, of which 59 (=51 + 8) would have died while on conservative treatment. Thus, an as-treated analysis may or may not yield results and/or conclusions different from those reached by the authors. It is not that we object to the intent-to-treat approach taken by the authors, it is just that results could vary according to the type of analysis one performs and we wish to make readers aware of such a possibility.

Conflict of interest statement. None declared.

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Reply

Sir,

We agree that the result may vary according to the type of analysis that is performed. However, we chose the intention-to-treat type of analysis to best inform nephrologists and patients at the time when decisions on future treatment are being made. We feel that there is very little data available to help this process. Changing to an as-treated analysis would be less useful for this purpose. Performing the as-treated analysis would focus on the effect of dialytic treatment, which was not the primary purpose of the study. Using an as-treated analysis, where the start point of the study is at eGFR of 15 mL/min, could be flawed, as those patients needing dialysis would generally have survived longer.

Patients who choose dialysis do have a better survival. We are not claiming that this is necessarily due to dialysis treatment. Indeed, as Misra et al. point out, patients who chose dialysis may not have started treatment, either because of death or not requiring dialysis in the study period. We therefore speculate that many factors affect the decision to choose dialysis, including patient’s wishes and advice from physicians. These factors are not readily identified from the medical records. However, the result is that the two groups of patients identified at the time of the dialysis decision have different survival, some of this difference possibly being attributable to dialysis.

Conflict of interest statement. None declared.

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Occurrence of peritonitis in APD versus CAPD: methodologic problems

Sir,

We read with interest the article by Rabindranath et al. ‘Automated vs continuous ambulatory peritoneal dialysis: a systematic review of randomized controlled trials’ [1], especially with reference to the occurrence of peritonitis on continuous ambulatory peritoneal dialysis (CAPD) versus automated peritoneal dialysis (APD). The authors report both the relative risk and the rate ratio for peritonitis based on three randomized controlled trials involving a total of 139 patients.

While the review found a significant reduction in peritonitis rates with APD relative to CAPD, no reduction in the relative risk was found. The authors relate this apparent discrepancy to the fact that some patients may have had more than one episode of peritonitis. It is important to note that comparing peritonitis rates is far more appropriate than determining relative risk, because a peritonitis rate in any given individual incorporates information on the number of episodes of peritonitis as well as the duration of follow-up on peritoneal dialysis (PD). In contrast, a relative risk simply determines if one group is at a higher risk of developing peritonitis than another group, irrespective of the number of episodes per patient or the follow-up time. For example, if one patient has three peritonitis episodes within
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 with respect to peritonitis limits the value of this exercise.

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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.

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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].