I concur (as best I can with the indirect evidence) that the authors did carve up time correctly. It is unfortunate that the record had to be corrected this way. It would have been avoided had key details (or even just the one key phrase ‘time-dependent’) not been editorially excised from the manuscript submitted to *Journal of American Society of Nephrology (JASN).* Post-publication, my co-author raised her concern about immortal time with the editor of *JASN.* She was told that the journal did not have a correspondence section. Her e-mail, which the editor said would be forwarded to the authors, was apparently not received by them.

When the authors contacted me and informed me of my incorrect assumptions about their paper, I asked to see some SAS code, but was told that none could be located. Subsequently, Dr Go shared with me the original draft of Table 1 containing the key phrase ‘adjusted hazard ratio comparing time-dependent receipt vs. non-receipt of renal allograft nephrectomy on death from any cause’. This was replaced in the published version by ‘adjusted HR for death for nephrectomy versus non-nephrectomy’. He also told me that the more lengthy description of their modelling strategy was deleted by the journal staff; and he pointed to similar analyses of time-dependent covariates in previous articles he had co-authored.

I agreed to contact the *IJE* staff and tell them that it seemed that the main hazard ratio was indeed based on a proper division of each patient’s follow up time into pre- and (if the allograft was removed) post-removal time. But before doing so, I had two queries for him. One was whether the two denominators behind Figure 2 were numbers of persons or (more appropriately) numbers of person-years: the reported rates were 32 and 36 per 100 person-years, but these did not seem to fit with the reported amounts of follow-up. Since the crude percentages turn out to be 32% and 36%, I wondered if the label in Figure 2 should have read ‘percentage’ rather than deaths ‘per person-year’.

The second (also time-related) query was how follow-up time was dealt with in Figure 3, which reported that 10% of those who did and 4.1% of those who did not receive a transplant nephrectomy received a second transplant—a difference that surprised the authors, but for reasons that ‘cannot be determined from our analysis’. My concern was that the durations of follow-up of these two groups differed substantially. If one corrected for this and used the same (time-dependent?) propensities they computed when addressing the primary outcome, and used a time-Cox model with time-dependent covariates, the difference in the adjusted percentages or rates might be even greater. The authors could have used their data to address this second query, and even assess how much of the better survival was mediated by the second transplant. I did not receive a reply to these two queries.

These queries bear on the quality and strength and interpretation of the evidence behind an article whose title says a procedure ‘improves’ survival. They deserve to be addressed, and in the subject-matter journal with which these ‘time’ issues were first raised.

One lesson from this case is that, although we may not have a lot of control over editorial staff, when we are authors we should insist that key elements are not excised, even if that means removing other less critical material. We do have control over a second aspect: we should keep all computer codes, computer output and data.
References

