Sir,
We welcome Dr Montalto’s response to our article and his contribution to this important debate, and we broadly agree with many of the points he raises. However, we strongly reject his assertion that we ‘have presumed that all home-based parenteral therapy is delivered in a similar way’. Our title used the term ‘case study’ to emphasize that it related specifically to the particular model used at our own health service and we explicitly stated that ‘costs will depend on the service model used, the condition treated and a variety of health-system factors’. In framing the results of our analysis, our intention was not to discredit hospital in the home (HITH) models in general. As we state, this model has numerous advantages, including patient safety and satisfaction. Rather, we felt that by having analysed our own system performance (and in doing so brought to light important process issues) we could provide a salient lesson as to the need for ‘a more critical ongoing analysis of the cost-effectiveness of HITH-based management’. We acknowledge that numerous alternative models exist, including the one used by Dr Montalto at his service. However, where is the published evidence that quantifies their cost-effectiveness? We are very concerned that much of the thinking here is based on assumptions that have not been sufficiently held up to evidence-based scrutiny. Indeed, we ourselves would have remained blissfully unaware of the important process issues at our own institution had we not performed this analysis in the first place.

As acknowledged in our discussion, we agree that our dataset was limited by the lack of clinical markers of severity. Although it seems less likely that patients with more severe disease would have been admitted to HITH, we agree that it is theoretically possible that those with more severe disease could have been more likely to have a period of inpatient care followed by HITH. Nonetheless, the 7.4 day average length of stay in this large sub-group still seems excessively long. Our study was not sufficiently powered to compare the HITH-only and mixed inpatient–HITH sub-groups (and this was not part of our a priori analytical plan), but we agree with Dr Montalto’s suggestion that a crucial process issue may be the transition from inpatient ward to HITH prolonging overall treatment duration. However, his suggestion to ‘ensure that all patients with cellulitis are admitted directly to HITH’ may have patient safety implications. We did not include comparison of 1 month readmission rates in our a priori analytical plan due to issues of sample size, but these rates were not significantly different in non-HITH (7/204) versus HITH (9/124) groups (P = 0.18).

Lastly, we agree that health economic analyses can vary in their scope, with broader analyses (beyond the scope of our study) incorporating opportunity costs raising important additional considerations. The idea of increasing bed capacity at a lower capital cost is intuitively very appealing. However, it is important that this is not done in a way that encourages ‘over-treatment’. This not only compromises overall health system cost-effectiveness, but may also increase risks of adverse events such as those due to antibiotics and intravascular devices.

Transparency declarations
None to declare.

References