Is Fidaxomicin Worth the Cost? The Verdict Is Still Out!

TO THE EDITOR—We read with interest the article entitled “Is Fidaxomicin Worth the Cost? An Economic Analysis,” by Bartsch et al [1]. The analytical methods used by the authors seem appropriate for the research question posed. However, several assumptions are not appropriate. The authors assumed that a second course of therapy is curative, with no chance of recurrence. That means that a relatively less effective first-line therapy could actually result in a better outcome than a better first-line treatment, because a second round of therapy for the less effective treatment would have a 100% cure rate. This assumption is inappropriate. Furthermore, we were not able to verify the data used in the parameter settings. There seems to be a discrepancy between the quoted literature from which the data were supposedly taken and the data used in economic modeling. To identify what the cause may have been, we verified if the literature was out of sequence, but this did not seem to be the case. We also verified if the data may have been abstracted from other published literature, but we were not able to identify these alternative sources. Below are the data points in the article we believe are erroneous:

1. The costs of hospitalization for different age groups as used in the analysis are not consistent with the numbers in the quoted reference “HCUP [Healthcare Cost and Utilization Project] facts and figures: statistics on hospital-based care in the United States, 2009” [2]. We made an attempt to adjust the cost into 2012 US dollars using the discount rate of 3% used in Bartsch et al’s article [1], but were not able to get the same results. Some numbers are larger and some are smaller. We considered medical inflation as well. However, taking this approach, which was not mentioned in their article, we were not able to replicate the data.

2. Regarding the probabilities of NAP1/B1/027 strain, the authors used
0.517 in the baseline analysis; however, the authors cited several references for this parameter, among which were several studies with large sample sizes that suggested this probability to be around 0.3. For example, the study done by Louie et al suggested this probability as 0.381 with a sample size of 596 (Table 1) [3]. In the study done by Cornely et al, 125 of the 509 strains were assayed as NAP1/B1/027, suggesting a probability of 0.332 (Table 1) [4]. A larger study with a sample size of 1005 isolate samples conducted by Miller et al showed a probability of NAP1 type as 0.31 (Table 2) [5]. The probability as 0.517 cannot be found in any of the references cited. If the authors would have pooled the results together, the number would be closer to 0.3 than 0.517. The method the authors used to pool the data, if they did so, was not explained in the article.

3. In the article, Bartsch et al said that the recurrence rate of using metronidazole to treat nonsevere disease is 0.136. However, according to the data provided by the citation, 0.136 is the recurrence rate for all disease severities (9/66), including severe patients. For the nonseverity group, this should be 3 of 37 = 0.081 (Table 3) [6].

4. The Xpert specificity should be 98.36% according to the reference cited, whereas the authors used 93.5% as the specificity, which cannot be found anywhere in the reference [7].

5. The length of stays used in the article cannot be confirmed from “HCUP facts and figures: statistics on hospital-based care in the United States” [2]. The source of these data is unclear.

6. The exact numbers of quality-adjusted life-year decrements used in the article cannot be verified from the references quoted [8–10]. If the authors have pooled the results from several sources to arrive at this number, it would be appropriate to explain the method they used and include the standard error.

On the basis of the data we presented, the findings as suggested by the authors do not hold.

### Note

**Potential conflicts of interest.** Both authors: No reported conflicts.

Both authors have submitted the ICMJE Form for Disclosure of Potential Conflicts of Interest. Conflicts that the editors consider relevant to the content of the manuscript have been disclosed.

**Abraham G. Hartzema and Chao Chen**
Department of Pharmaceutical Outcomes and Policy, College of Pharmacy, University of Florida, Gainesville, FL 32610-0496 (hartzema@ufl.edu).

**References**


Correspondence: Abraham G. Hartzema, PharmD, MSPH, PhD, FISPE, Department of Pharmaceutical Outcomes and Policy, College of Pharmacy, University of Florida, Gainesville, Florida. FL 32610-0496 (hartzema@ufl.edu).

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