

**ABSTRACT:**

The abstract does summarize the article, however the conclusions that they make in the abstract does not seem to be the same that they make in the discussion portion of the paper. In the abstract they noted that “..emergency department location, high risk antibiotics, probiotics and statins were independent predictors of CDI. “ However, in the discussion portion of the paper they specifically say that :coadministration of probiotics with high risk antibiotics did not lower the odds of contracting CDI. That it's only the coadministration of moderate risk antibiotics and probiotics at appeared to decrease the odds of CDI.” So, is that difference in interpretation between the discussion and the abstract intentional? Or is it phrase in incorrect way?

**INTRODUCTION:**

The introduction of this article was clear, but they should have been more background on the use of probiotics and their supposed interactions with CDI. Because, it was really focus on in the discussion, but there was not a lot of background knowledge that was provided. They should have been more detail an how do use of anti-bacterial agents could promote the growth of CDI and patients.

**METHODS:**

The methods was sound using a retrospective cohort, so there was no need to worry about sampling biases because of the access to electronic medical records. Having a chart with a stratum of different risk in bacterial agents was a good idea, but no singular agent was mentioned so it was not necessary to put that in the paper.

**RESULTS:**

The results were presented pretty clearly, I had minimal problems with the results section because they had very good tables that show specifically what the results were. The only table I had difficulty with was with the multi variable predicted logistic regression model but that's just because it's a little bit outside my educational limits.

**DISCUSSION/ CONCLUSION:**

My primary problem with the discussion and conclusion is that they focused heavily on the use of probiotics and its interaction with CDI. Even though, in their table, the odds ratio of CDI and probiotics is not significant, so I don't think that probiotics should have been focus on as much. I do appreciate that they found something different then, so the other research says about probiotics, but it is not the only predicted value that they looked at. I appreciate that they emphasize that their model does not demonstrate a causal relationship between CDI and these predictive factors, that they only present the odds of CDI. I think that the only topic in this paper that warrants further research is the link between probiotic use and moderate risk anti-bacterial agents, because that's the only relationship that showed a decrease in odds of CDI. I recommend this paper for a minor revision, if some of the concerns that I stated we change during the editing process Or is further explanations were given then a resubmission would be warranted.