

Probiotics as medical therapies

I am disappointed that the editors of *CMAJ* published a misleading and biased research letter on the “underuse of probiotics.”¹ I believe that there is some biological plausibility to the potential effectiveness of probiotics as therapies for certain medical conditions.² However, each of these agents must be tested and assessed in proper trials. In fact, I recently completed a study of the effectiveness of *Lactobacillus rhamnosus* GG in preventing *Clostridium difficile*-associated diarrhea (CDAD).

Lindsey Edmunds is incorrect in stating that antibiotic-related diarrhea can sometimes “cause pseudomembranous colitis.” *Pseudomembranous colitis* (or CDAD) is not caused by diarrhea. It is one type of antibiotic-associated diarrhea, accounting for approximately 40% of diarrhea in patients in hospital who are receiving antibiotics.

More importantly, her assertion that “probiotics ... beneficially affect humans by altering their intestinal microbial balance” is unsubstantiated, yet she states it as fact. The mechanisms by which probiotics exert their effects are largely unknown.³ Describing the benefits of probiotics as if they were a single agent is similar to lumping all antihypertensive drugs together as a single drug type and saying that they are beneficial. In fact, even closely related probiotics, such as similar *Lactobacillus* species, have displayed varying effects when tested in humans.⁴ In addition, some “natural” probiotic products have been shown to contain only dead bacteria (owing to improper manufacturing or storage) or to have bacterial contents other than those stated on the label.^{2,5} An authoritative review of probiotics recognized that the “definition of the pharmacodynamic profiles and viability of organisms in many commercially produced probiotic preparations are lacking.”²

I applaud Edmunds’ use of a questionnaire to survey physicians’ attitudes concerning probiotics. However, the author’s assertion that all probiotics are beneficial but underutilized is a biased notion that is inappropriate as a starting point for a study of this type. The research letter should have been entitled “Attitudes of family physicians about probiotic use,” to avoid the assertion in the author’s own title that underuse is bad. Such assertions only add to the myths surrounding such therapies, making it more difficult to test and develop the individual probiotics that would truly benefit humans.

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References

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2. Alvarez-Olmos MI, Oberhelman RA. Probiotic agents and infectious diseases: a modern perspective on a traditional therapy. *Clin Infect Dis* 2001;32:1567-76.
3. Kopp-Hoolihan L. Prophylactic and therapeutic uses of probiotics: a review. *J Am Diet Assoc* 2001;101:229-38.
4. Salminen S, Arvilommi H. Probiotics demonstrating efficacy in clinical settings. *Clin Infect Dis* 2001;32:1577-78.
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[The author responds:]

Mark Miller’s primary criticism of my research letter¹ is that the term “underuse” is misleading and indicates a “biased notion that is inappropriate as a starting point for this type of research.” First, the term accurately reflects the fundamental outcome of the research: only 32% of the physicians who responded to the survey recommend probiotics to patients when prescribing antibiotics and only 15% reported that they do so always or often.

Second, given the reported benefits of probiotics, the rationale for my study was that probiotics should be coprescribed with antibiotics. My premise that probiotics have potential benefits is the same basic premise Miller uses in describing his research into “the effectiveness of *Lactobacillus rhamnosus* GG in preventing *Clostridium difficile*-associated diarrhea.” Finally, Miller’s suggestion that my title should have been “Attitudes of family physicians about probiotic use” indicates a lack of understanding. The purpose of the research was to determine the rates at which physicians recommend probiotic use and their rationales for recommending or not recommending the use of probiotics. The attitudinal finding that physicians want more research and information on probiotics was a complementary, but secondary, result.

Miller is correct that pseudomembranous colitis is not caused by antibiotic-related diarrhea. The word “this” in the second sentence of my research letter should have referred to the destruction of intestinal flora, not to antibiotic-related diarrhea.

Miller says my claim that probiotics beneficially affect humans by altering their intestinal microbial balance is unsubstantiated. However, his argument is primarily based on problems related to the use of poor-quality probiotics, not on the effectiveness of high-quality probiotics. I wholeheartedly agree that “each of these agents must be tested and assessed in proper trials.”

I maintain that the benefits of probiotics for the conditions I described are well documented and should not be negated or ignored because we have not yet proven the effectiveness of *all* probiotics.

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Reference

1. Edmunds L. The underuse of probiotics by family physicians. *CMAJ* 2001;164(11):1577.