Response to “Vasectomy and ovary-sparing spay in dogs: comparison of health and behavior outcomes with gonadectomized and sexually intact dogs” by Zink et al

We came across and read “Vasectomy and ovary-sparing spay in dogs: comparison of health and behavior outcomes with gonadectomized and sexually intact dogs” by Zink et al with great interest. The authors tackle worthwhile questions about evidence for gonadectomy recommendations. Unfortunately, deficiencies in methodology and reporting make it unclear what conclusions may be drawn.

First and foremost, several major limitations are not addressed. Social desirability bias in survey participants aware of public stigma against intact dogs may partially explain lower rates of problematic and nuisance behaviors in intact, vasectomized, and ovary-sparing spay dogs. Additionally, 42% of intact dogs were kept intact for conformation shows. Dogs competing in conformation must be, by nature of the competition, well trained, not dog aggressive, and comfortable with stranger handling. Similarly, survey participants recruited from Good Dog, an organization that promotes responsible breeding, may have greater dedication to the health and training of their dogs compared to the general population of owners of nongonadectomized dogs. These could all strongly confound the relationship between sex status and health and behavioral issues.

Additionally, the authors make several comparisons of unclear biological relevance. Analyses were not stratified by sex. Consequently, the ORs presented compared outcomes in intact females with neutered males and other cross-sex comparisons. These comparisons are unhelpful for clinical decision-making; veterinarians and owners can use data on differences in health risks between intact versus vasectomized versus neutered male dogs to inform surgery decisions, but knowing that neutered males have 2.6 times the odds of orthopedic disease compared to ovary-sparing spay females does not help guide surgical decisions. Additional problems include mislabeled reproductive reference groups (according to the footnote of Table 1, intact males have 1.4 times the odds of developing orthopedic problems compared with neutered males), only reporting 1 OR (instead of 6) for weight groups, using Bonferroni-corrected significance threshold of 0.003 (which is too high for the number of comparisons made), and the use of receiver operating characteristic curves and Hosmer–Lemeshow tests to evaluate models not built as prediction models. We appreciate the authors’ contribution but feel the conclusions made should be further tempered. Given the potential for considerable confounding, lack of sex stratification, and unclear results reporting, we feel as though this paper should not alter anyone’s clinical practice. We echo the authors’ call for further studies, although this paper should not alter anyone’s clinical practice. We echo the authors’ call for further studies, especially trials, to better elucidate the health risks associated with different contraceptive surgeries.

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The authors respond

We appreciate the comments on our manuscript submitted by Drs. Cummings, Price, and Peters. We recognize that there are limitations of our study, including that the population surveyed may not represent the general population of dog owners. In our survey, participants were allowed to choose multiple reasons for the reproductive choices they made for their dog, understanding that groups may differ in other ways aside from their dog’s neuter status. There is limited information available about owners who choose not to spay or neuter their dogs. We know of no sources of data that would lead us to expect that owners of intact dogs are subject to more desirability bias than other populations of dog owners. It is human nature that all dog owners are likely to report that their dogs are better behaved than they are.

As to whether dogs participating in conformation had a different training history, the purpose of our study was not to investigate the impact of training (or training methodology) on behavior. Several studies have now reported the relationship between gonadal hormone exposure and problematic behavior, as we referenced in our manuscript.

Regarding the statistical analyses, the JAVMA reviewers were very familiar with statistical analyses and significantly modified the analyses to what appeared in the published study. Our models compared each reproductive group, and for the sake of transparency, we reported all statistically significant results. We appreciate that although understanding differences in health outcomes for neutered male dogs
compared to female dogs that have had a hysterectomy does not help guide surgical decisions for an individual dog, we think that population-wide comparisons can be useful and may also inform future studies.

The authors of the letter are correct that the reference group is mislabeled in the Table 1 footnote. The first group listed is the group that the OR applies to compared to the second group. We have taken steps with the editor to correct the footnote.

The OR presented for the weight variable represents the increase in odds for each increase in weight group (eg, from the < 4.5-kg group to the 4.5- to 9.1-kg group). Our analyses involved 15 comparisons of 6 reproductive groups, and as such we adjusted \( \alpha = 0.05 / 15 = 0.003 \). Logistic regression, by nature, is predictive, and we included receiver operating characteristic curves and the Hosmer-Lemeshow test results to help evaluate the models.

In our manuscript, we concluded that there may be benefits to additional gonadal exposure time for dogs, additional studies are needed, and veterinarians should make “an informed, case-by-case assessment of each individual patient, taking into account all the potential risks and benefits of spay/neuter.” Further, emerging peer-reviewed evidence supports that universal spay/neuter at a young age is not necessarily the best choice for every dog or owner,4,5,6 and we stand by our manuscript’s conclusions.

Criteria for veterinary systematic reviews

We read the recent review article on allergen immunotherapy “A systematic review of allergen immunotherapy, a successful therapy for canine atopic dermatitis and feline atopic skin syndrome.”1 with interest. While the author did an excellent job of summarizing the literature of a complex subject, we feel compelled to point out that the article should not have been referred to as a “systematic review.” Instead, it was a narrative review.

Narrative and systematic reviews have substantive differences; the difference is not semantic. While narrative reviews can be exceptionally useful in providing a topic overview by a domain expert—and this article certainly meets this intent—narrative reviews do not employ formal techniques to identify bias in included articles or to reduce biased presentation of summary results. Narrative reviews summarize and present available research; however, they are not rigorous or systematic.

Systematic reviews are conducted using a systematic process to identify research studies worldwide, with the primary aim of recommending the best practice on a certain topic. Systematic reviews use transparent search and data extraction methodology to avoid selective bias in study selection and information bias in data extraction from studies. Additionally, there is typically an attempt to grade evidence sources and the risk of bias in those sources in a systematic review.

When the methodological process involved in researching a topic is absent, important theoretical details may be overlooked. While the author may have searched diligently for articles to use in this review, that is a different process from a systematic review. As a result, narrative reviews are more likely to be biased because they rely on the author’s background knowledge on the topic.

JAVMA’s instructions to authors summarize some of the key steps in conducting and writing a systematic review, including a link to Preferred Reporting Items for Systematic Reviews and Meta-Analyses guidelines.2 We suggest that this article, while useful, does not meet the journal’s criteria or other published descriptions of veterinary systematic reviews.3,4

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The author responds

I wrote this article as a review for practitioners and originally submitted it with the title: “Allergen immunotherapy for dogs and cats.” One of the reviewers asked for a more descriptive title and suggested something much more detailed. In the end, we agreed on “A systematic review of allergen immunotherapy, a successful therapy for canine atopic dermatitis and feline atopic skin syndrome.” However, when I read your comments I had to tell myself I should have known better. You are completely right, and I can only apologize for the oversight.

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From the Editor-in-Chief

Mea culpa—I take full responsibility for publishing this manuscript as a Systematic Review. Dr. Mueller and I agree: Dr. Ramey and colleagues were right! Our Publications team was actively revising our Instructions for Authors when I received the Letter to the Editor. The serendipitous result was, first, a collaboration between JAVMA/AJVR and the Evidence-Based Veterinary Medicine Association (EBVMA) in revising our instructions for authors for Systematic Reviews and, second, an extension of our reviewer database to include members of the EBVMA. Anyone who knows me knows I often say, “Everyday is a school day,” and this was a very positive learning experience for me and our journals. I am grateful to Dr. Mueller, the authors of the Letter to the Editor, and the broader EBVMA for this opportunity to improve the integrity of our journals.

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