

Decreasing the level of hemicelluloses in sow's lactation diet affects the milk composition and post-weaning performance of low birthweight piglets.

Palombo et al

Dear editor,

This study touches an interesting subject in today's pig industry, dealing with piglet health and performance and how they can be improved via sow nutrition and milk composition. The experiment is well designed. However, some clarifications are needed on the rationale for the study, on some variables and results. I also recommend revising some parts of the discussion to make them easier to read.

Introduction

The effects of hemicelluloses described by the authors are positive effects: stimulation of growth and activity of 'beneficial' bacteria, which in turn increases VFA production, VFA that can be used for milk synthesis. Therefore, why did the authors investigate the effects of decreasing levels of hemicelluloses in sow diet? I would have tested the effects of increased levels. I have two hypotheses. 1/ I missed something in the reasoning (that reducing hemicellulose levels would lead to changes in other fibrous constituents which would have even more beneficial effects?). 2/ The authors intended to test the effects of an increase in hemicellulose supply but the effects were opposite to the expected effects. If so, it could be described as is. This would show that the effects of dietary fibers are complex and linked to interactions between the different fibrous constituents. In any case, I suggest that the authors clarify the rationale.

Ln 43. I suggest that the authors do not use their abbreviation L-BtW here since low-birth weight piglets weighed between 0.6 and 0.9 kg in Loisel et al and between 0.8 and 1.2 kg in the present study.

Ln 46. I suggest being more specific about "sow nutrition" since the present study and the cited one focus on the effect of polysaccharides.

Materials and methods

Ln 62. I suggest adding 'lactation' before diet.

Ln 63. The unit kg is missing.

Ln 68. How were piglets weighing less than 800 g excluded from the experiment? Were they adopted by a non-experimental sow or were they kept in their litter and excluded from the datasets? If they were kept with their littermates, could this have an impact on some criteria (e.g., piglet feed intake)? Moreover, it is not clear whether these piglets were counted in litter sizes or not (Table 2).

Ln 81. Sows were provided with straw bedding. Could the consumption of straw by sows have attenuated the potential impact of nutritional variations in maternal diet?

Ln 81. I suggest providing feed allowance during the last week of gestation.

Ln 105. On the day of farrowing instead of during farrowing?

Ln 107. BCS is usually determined visually. It is not always easy to differentiate between two score levels, so even less when intermediate scores are included. May the authors describe a bit more the method (visual approach only?).

Ln 110. Piglets were divided into two BW groups. I suggest adding piglet numbers in Table 4 and Suppl. Table 1.

Ln 117. Why did the authors record the time between the onset of farrowing and the first piglet suckling? Time from birth to first suckling is interesting to assess piglet vitality and, when recorded on all piglets of the litter, to have an idea of the progress of farrowing. I don't know if this data measured on the first piglet only is useful. I suggest the authors explain the meaning of this variable and, possibly, provide a reference.

Ln 132. 'Feed intake per pen was recorded'. Was feed waste taken into account?

Ln. 133. The authors should explain how diarrhoea percentage was calculated.

Ln 139. My guess is that oxytocin was injected intramuscularly.

Ln 177. ...as previously described. Please provide a reference.

Results

Ln 220 and Table 2. It seems to me that there is a mistake in backfat thickness. T7 sows gained on average 3 mm of backfat between day 110 of gestation and farrowing, which is unlikely. It may change backfat thickness loss during lactation.

Ln 249-255. This part could be written more clearly. In its present form, it describes L-BtW piglets first, then N-BtW, then again L-BtW. BW development= BW gain? Suggestion: Although the increase in HC level... did not affect BW from birth up to week 1 post-weaning, it decreased the BW of L-BtW...

Ln 217 and Table 5. Please check milk yield on day 17 of lactation (maybe 10.85 kg instead of 1.85?).

Discussion

Ln 323-337. I am a little confused. First, the authors compared their results (on feeding strategies during the last week of gestation and lactation) with studies of nutritional strategies applied during gestation. Second, some words are too vague. I suggest that the authors clarify 'sow performance' (Ln 329), this setup (Ln 336), these traits (Ln 337). Moreover, please check the reference Renteria-Flores et al (2008); I am not sure they reported increased BW loss during lactation (or on BW gain during gestation).

Ln 335. A word missing? Please check.

Ln 340-352. I am confused in this part too. If I correctly understand, hemicellulose might provide glucose or glucose precursors that could be used by the sow to synthesize lactose? Ln 346. A 3:1 ratio of IDF to SDF increases the fermentability... As compared to what? Ln 348-350. The discussion is first about decreasing hemicellulose levels and then about a higher level of hemicellulose. Ln 349. I don't get the point here.

Ln 353. I suggest citing Costa et al after 'the osmotic power of lactose'.

Ln 371. I would say that Zhao et al showed a negative correlation between VFA concentration in pig's ileum and HC level in pig's diet. Was the rationale for the present study partly based on results observed by Zhao et al?

Ln 401. I suggest deleting "Therefore" and specifying beneficial effects on performance of the L-BtW piglets.

Ln 406. I suggest moving "piglets are highly susceptible to intestinal bacterial disorders" above (e.g., Ln 403) or rephrasing in two sentences. Threonine did not accelerate the gut maturation because piglets are susceptible to bacteria.

Ln 419-421. I am wondering whether diet composition may have influenced oligosaccharide content or profile in colostrum and milk. I am just curious; it is probably too speculative to be discussed here.

Ln 422-456. This part describes the variation of milk composition during lactation, which is not the focus of the study. Moreover, it is a bit difficult to follow. I suggest keeping it short and simple.

Ln 423-432. I do not agree with the suggestion to explain the lack of variation in dry matter, fat and energy content of milk between day 3 and 17 of lactation (too short time between the 2 sampling days). A decrease in dry matter and fat content between day 3 and 10 or 20 was reported by Csapo et al (1996), as well as by Hurley (2015) or Theil et al. (2014). Is it possible that overtime variations in milk composition differ between studies according to sow genotype, sow management, litter size?

Ln 437-441. Please clarify 'these changes'. No changes were described and line 438 deals with calcium and phosphorus while line 441 deals with calcium, potassium and zinc.

Ln 452-456. A positive correlation between calcium and C16:0 was reported by Hu et al. In the present study, calcium content decreased from day 3 to day 17 whereas C16:0 proportion increased, which is not in favor of a positive correlation. Indeed (ln 453)? I suggest deleting this part.

Conclusion

I suggest deleting 'as shown in the present study', offspring (ln 459).