Ruptured tubal pregnancy co-existing with acute appendicitis: A rare occurrence.

Sir,

The authors reported a case of ruptured tubal pregnancy and a simultaneously acutely inflamed appendix without any detailed description of the gross appearance of the Vermiform Appendix, and the adjacent viscera and structures which is necessary to document their case as described word for word in the introductions; “Probably not been reported so far in the literature”, with the word “probably” meaning that they are not totally sure if this is the first reported case. This to me indicates that they have not searched the literature thorough enough!

The histopathology report of the vermiform appendix states that the appendix is apparently inflamed with severe transmural inflammation.

Finding a grossly inflamed appendix in intra-abdominal sepsis is a relatively common occurrence especially when there is delay between the onset of the acute abdomen and laparotomy, as in this case where laparotomy was performed 2 days after the onset of acute lower abdomen. The appendix is grossly acutely inflamed, and histopathological examination will show acute inflammation, but the acute inflammation is not primary, as it occurred secondary to an intraabdominal inflammatory process where the acute inflammation extended from the primary focus, commonly gynecological to the near by viscera producing secondarily acute appendicitis which is an established entity known as “Secondary Appendicitis.” The histopathology report describing no fecolith, kink, adhesion, or foreign body in the lumen of the vermiform appendix is also consistent with the case being secondary appendicitis with the primary focus of infection being commonly gynecological due to these organs neighboring the vermiform appendix. Even systemic infectious processes may cause secondary appendicitis.

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Reply from author

We welcome the comments from the reader in respect of the above title and we shall try and clarify the points raised. Yes, we have searched the literature extensively and we did not come across a case like ours. We don’t think that it is out of place to use the phrase (probably has not so far been reported in literature). The probability here meant that almost certainly it has not been reported before. We agree to the fact that acute appendicitis could
occur in association with intra abdominal sepsis and other infections as the reader points out. We are aware that the fallopian tube can be affected by sepsis as a result of spreading infection from the appendix. However, this case was a ruptured ectopic tubal pregnancy co-existing acute appendicitis; not acute salpingitis co-existing with acute appendicitis. The diagnosis of the acute appendicitis was made in the operating room and the histopathological report of the appendix. We feel that this should suffice for the diagnosis of acute appendicitis. Our patient presented with bizarre signs and symptoms of both acute appendicitis and ectopic pregnancy. It was because of this that we invited the general surgeon to the operating room.

We stress again that the findings are casual and encourage our colleagues to inspect the appendix where possible during gynecological operations.

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Obesity and overweight in type II diabetes mellitus patients in Saudi Arabia

Sir,

I read with great interest the article by El-Hazmi and Warsy regarding the prevalence of obesity in Saudi Arubians with type II diabetes.\(^1\) Diabetes mellitus is a very common disease in the Kingdom and efforts made to study the disease and its risk factors in the local population should be warmly welcomed.

The main finding reported by the authors was that the prevalence of obesity (based on BMI measurement) was significantly higher in type 2 diabetic patients compared to that of a matched nondiabetic control group. On the face of it, such a finding would be consistent with that of population studies carried out elsewhere. However, an inspection of the data by El-Hazmi and Warsy (Table 1 of the published paper) reveals that the comparison in question (controls vs. diabetics) showed the two groups were not properly matched, as follows: while young subjects (< 20 years of age) made up 41% of the control population, the corresponding figure for those within the diabetic group was only 3% (as shown in Table 1, there were 796 [219+177+198+202] young subject out of a total of 1922 subjects in the control group versus only 30 subjects [5+5+6+14] out of 912 diabetic subjects, thus, giving the respective values of 41% vs. 3%). Should it surprise us at all therefore, to find that the control group (with a high percentage of children and teenagers) had a lower BMI than that of the middle aged diabetic group?

Another figure to draw your attention to is the low BMI value (20 kg/m\(^2\)) reported for normal women, aged 36-40 years (Table 1). This figure seems surprising for Saudi women of that age group. More importantly, a figure such as this could have easily biased the results further in favor of the diabetic group.

The above points might affect the conclusion of the authors.

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Reply from author

We appreciate the letter of Dr. Aus A. Alzaid and a very relevant observation made by him. The table certainly confirms the assessment made by Dr. Alzaid. However, this observation does not in any way alter the main findings of this paper. As indicated in the methods and material section, the total number of diabetic and non-diabetic individuals that we studied were 1419 and 13241 and data from all these individuals is not used to prepare the Table 1, which was mainly included to show the difference in the BMI of diabetic and non-diabetic subjects of different ages. It also showed that BMI varies with age in both groups. The two groups compared were closely matched in their ages and no bias was introduced due to this factor.

We appreciate that Dr. Alzaid has pointed out a topographical error that we had somehow overlooked i.e. the BMI of females in the age group 36-40 years.