



Letter to the Editor

Are we doing right suggesting a non-operative management for suspected appendicitis in children? ☆



Dear Editor,

Recently many authors have been reporting their findings regarding the value of the conservative treatment in cases of non-complicated appendicitis [1–10].

The data from the different trials and the numerous meta-analyses seems to support the theory that a non-operative management might be the correct solution.

However, many doubts arise as to the terminology used, the methodology and the interpretation of the data.

Probably the titles should be changed adding the term “suspected” since the diagnosis is only anatomopathological until there is evidence to the contrary.

Science and progress are based on a simple triad: hypothesis, reasoning, and thesis. From a correct hypothesis, wrong reasoning can lead to an incorrect statement.

This emerges from the data on the non-surgical treatment of appendicitis. What data allow a clinician to diagnose appendicitis correctly? Probably the clinical picture together with the laboratory tests and the radiological results. Starting with the clinical picture, the validity of considering the surgical evaluation homogeneous can be certainly challenged. Literature data report that the patients are evaluated by more surgeons but do not state whether these patients are re-evaluated by the same surgeon at 48 h from the beginning of treatment. This is indeed a bias that causes several false positives and false negatives. Conversely, little can be said on the interpretation of laboratory data since they are objective data. However, regarding the radiological evaluation, several aspects are to be questioned and carefully analyzed. Some patients are studied using CT scan, a test that cannot be performed in all hospitals and does not have an ideal cost–benefit ratio for both the patient and the hospital. Also, a CT scan can be very invasive, especially for pediatric patients.

When abdominal US scan is used instead, the study inclusion criteria do not specify how many radiologists perform such procedure. Ultrasound scan is an operator-dependent procedure with some limitations related to the type of probe used as well as to the position of the appendix inside the abdomen. It is difficult to measure the diameter with certainty; in addition, it is much more difficult to visualize correctly the appendix at each ultrasound scan. Is the appendix always placed in the right iliac ditch? From my experience as a surgeon, many of the uncomplicated appendixes are in a retrocaecal or sub-hepatic position. Are they always and clearly visualized and measured?

How many abdominal US scans clearly show the appendixes? Could the radiologists involved in the studies always and clearly visualize and

describe the appendix even if it was not inflamed, independently from the study? [11–14].

Therefore, it is extremely difficult to be able to obtain comparable data when 2 elements out of 3 are operator-dependent (either surgeon or radiologist).

The failure rate of a non-surgical treatment varies, depending on the study, from 25% to 60% after one year; obviously, it is necessary to consider that any treatment involves expenses in terms of medicinal products and human resources.

Conversely, would it be acceptable for a patient to receive surgery if informed that there is a recurrence or failure rate of between 25% and 60% after a year? Are we sure that the patient would agree to receive such specific surgery?

How many patients receive non-surgical treatment but do not actually have a “true” appendicitis? Patient recruitment is based only on a suspected diagnosis.

Likewise, how many patients receive surgery before their histological examination shows that they did not have appendicitis at all?

Have failure rates between 25% and 45% ever been reported in literature? The only data questioned by to recorded surgical cases are complications, which are mostly wound infections or abdominal abscesses, probably caused by an incorrect post-operative short-term antibiotic therapy, as it is very often reported. Back to the hypothesis that the antibiotics are useful to treat cases of “suspected” appendicitis, this is undoubtedly true, however, studies must be done to clarify when we are dealing with cases of “true” appendicitis instead. The data must be as comparable as possible, with clinical and radiological evaluation always performed by the same clinical operators [10–16].

Therefore, should we rely on US diagnosis only? Consequently, how should we treat abdominal pain when we find high WC values, high PCR, a clinical picture positive for suspected appendicitis but US scans do not show the appendix but only presence of liquid in the right iliac septum?

Unless otherwise proven, the diagnosis is only histological.

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