Since January 2020 Elsevier has created a COVID-19 resource centre with free information in English and Mandarin on the novel coronavirus COVID-19. The COVID-19 resource centre is hosted on Elsevier Connect, the company's public news and information website.

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Counting stitches does not save time

We appreciate the interesting comments of Chiranjeevi H and Pankaj Kumar about our article.[1]

We agree with them that emergency midline laparotomies are at high risk for the development of incisional hernias (IH), so closure must be very careful in these patients and even the use of prophylactic mesh (PM) should be considered in many of them.[2] For this reason, we consider that it was not the best model on which to study the implementation of the small bites technique (SBT). We have considered that low-risk patients were the best to increase surgeons' confidence in the technique. On the other hand, there are few data on the use of SBT in emergencies,[3] and the only randomized study published[4] documented a high frequency of fascial dehiscence in a group of emergencies in high-risk patients comparing SBT (13.5%) with an onlay PM (0%).

Our article emphasizes the importance of measuring the suture length/wound length ratio and ensuring that it is higher than 4/1 regardless the technique of closure. We have doubts that counting the number of stitches adds greater security to confirm the correct ratio of a running suture and even that the SBT has been properly done. In fact, stitch counting is not part of the suggestions of the pioneers of SBT[5] and is only an indirect method of measuring the ratio. The same happened registering the time for closure. This parameter had already been determined in other studies,[6] and adding it to the collected data could increase the possibilities of low adherence to the protocol. Ours is a study on a real scenario, and that has its advantages and disadvantages; some variables are not fully controlled, and introducing more data to collect makes it more complex and may introduce a greater number of biases in the analysis.

Finally, in our opinion, age as an isolated parameter, without considering the individual and population health status and how this influences the healing of the laparotomy, seems insufficient to consider a patient at high risk of IH and in consequence using a PM. The age ranges from which to consider it a risk factor is also unclear (HR 1.30 for every 10-year increase).[7] In a recent study to determine risk factors and to design a formula to calculate the probability of IH,[8] age was not included in the final formula. In our series, more than 75% of the patients were under 70 years of age; the patients operated on in our hospital in the same period and considered at high risk (2 of more risk factors for IH) had a median age of 73 years, and 63% were over 70 years.

In summary, as our study suggests, there is still a long way to go to achieve a safe closure of all types of laparotomies in all our patients.

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Conflict of interest/Disclosure

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What special considerations should acute appendicitis bring to the clinician in the midst of Coronavirus Disease 2019

To the Editor:

Recently we read with great interest an article in your journal about changes in acute appendicitis presentation and severity of illness during the pandemic.[1] There is no doubt that the treatment of acute appendicitis in this particular period is more challenging, as reported, it subtly affects the structure of the disease. Acute

appendicitis, as a common acute abdominal disease requiring surgical operation or endoscopic surgery, brings more thinking and challenges to clinicians. We should explore whether the cause of acute appendicitis has its own particularity in the context of coronavirus disease2019 (COVID-19). At the same time, acute appendicitis should arouse the attention of clinicians for some new concepts in the midst of COVID-19.

COVID-19 is an acute infectious disease characterized by respiratory symptoms, yet over time it has been demonstrated that more and more other systems are affected, including the digestive system. To the best of our knowledge, acute appendicitis is a digestive tract disease mainly characterized by abdominal symptoms, which are obviously easily confused with the abdominal symptoms of COVID-19. Based on this consideration, we should never ignore acute appendicitis directly induced by novel coronavirus infection of the appendix, so as to prevent the spread of pandemic caused by misdiagnosis and missed diagnosis in the clinical diagnosis and treatment process. It is time for us to come up with new concepts: acute appendicitis-like symptoms of COVID-19; acute appendicitis combined with COVID-19; and novel coronavirus acute appendicitis. It is particularly important in the context of COVID-19 pandemic.

It is easy for us to take their differences literally. Acute appendicitis like symptoms of COVID-19 means that COVID-19 is characterized by abdominal symptoms similar to acute appendicitis. Although acute appendicitis combined with COVID-19 means that the occurrence of the 2 diseases overlap in time but is not cause-and-effect. However, novel coronavirus acute appendicitis indicates that acute appendicitis is caused by novel coronavirus infection. Although no novel coronavirus has been reported to induce acute appendicitis, coronavirus has been isolated from appendix tissue.[2] This means that novel coronavirus has a potential risk of triggering acute appendicitis because viral infection of the appendix is one of the causes of acute appendicitis. From this point of view, there is an urgent need for relevant studies and reports to confirm.

As the pandemic continues to spread and poses more uncertain risks, it is of great importance to accurately judge acute appendicitis in clinical practice. Therefore, it is necessary to determine the pathological features of coronavirus to guide doctors in the diagnosis and treatment of acute appendicitis. After all, in this special time, we do not want to cause missed diagnosis or misdiagnosis of novel coronavirus related disease.

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Assessing neuropsychological symptoms in primary hyperparathyroidism: Further work needed to confirm the findings

To the Editors:

We read with great interest the recently published report by Liu et al[1] investigating the neuropsychologic (NPS) changes before and after parathyroid surgery in two institutions. The authors conclude the NPS score should be a relative indication for consideration of parathyroidectomy, after reporting a drop in scores relative to patients undergoing parathyroid surgery. We highlight several issues that need careful consideration.

First and most important, Liu et al[1] present data with standard deviation values higher than the average values, followed by a significant P value for the statistical analysis. This problem affects most variables reported in this report and is encountered in each paragraph and Table. For example, the authors report that the NPS score was 11.2 ± 11.5 before the operation and 5.1 ± 7.1 after the operation, with alleged statistical significance at $P < .01$ (as presented in Table II). Such data are not normally distributed and therefore can only be presented as median [range]. Instead of being compared using the paired t test, a nonparametric test should have been used. Consequently, the reader must exercise caution with interpretation of the data and conclusions drawn. It might be that the same data set analyzed with appropriate statistical tests might still be able to show significant change in NPS scores before and after parathyroidectomy but this has not been shown in the published paper.

Second, we observe that 10% of patients reported were normocalcemic preoperatively. Significant learning insight would be added if subgroup analysis explored whether NPS symptoms at presentation were different between those with normocalcemic primary hyperparathyroidism (PHPT) and those with “classical” PHPT and whether the postoperative improvement was observed in both subgroups. Patients with normocalcemic PHPT are an increasingly worrying group, and because this diagnosis remains controversial and the indication for surgery is yet to be clearly defined, it would be valuable to learn from the authors how this subgroup of patients benefited from surgery.

Third, it is traditional for cohort studies to detail whether there was any selection bias and to detail the dropout rates from follow-up.[2] Comparing the initial assessments of those who completed the entire study and those who dropped out, one can determine whether there was a risk that patients most satisfied with their outcomes would have remained engaged with the study, and those with minimal or no changes might had felt less motivated to continue.

Finally, Liu et al[1] report a very high incidence of postoperative hypercalcemia (reported as 11.9% in Table I), suggesting that 1:10 patients were not cured. This appears at odds with much higher cure rates expected after first-time surgery for PHPT. For example, the British Association of Endocrine and Thyroid Surgeons registry[3] reported a national average for postoperative hypercalcemia of 5.2%. It would be interesting to know whether the postoperative change in NPS scores was calculated in all patients or only in those cured, as this would add weight to support their conclusion.

In summary, the data from this study has the potential to contribute significantly to this field, but incorrect data analysis has weakened its message. The study protocol could have allowed the establishment of a valuable tool for assessing