Author’s response to reviews

Title: Emergency inguinal hernioplasties in a Tertiary Public Hospital in Athens Greece, during the Economic Crisis

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"Please also find below answers to the questions asked and a point to point response to the comments (out of respect we will keep the order of appearance of the comments in the journal’s website)

Editor Comments

1) According to our data the number of elective hernioplasties did decrease to a significant extent during the crisis period. It is difficult to define the reason(s) for this decrease. As we state in the second paragraph of the Materials and Methods section the referral and the on-call system of the GNHS were practically the same during both examined periods and cannot be responsible for this change. We have no reason to believe that people avoided our hospital for their elective treatment to explain this decrease. This would be in contrast to the increased demand of help in the emergency setting. If people were avoiding our Department they would not seek help from us during our on-call service too. They might as well have addressed to other on-call hospitals. As we analyze in the Discussion Section, we believe that our fellow citizens (for reasons perhaps relevant to their financial situation) simply preferred not to have their hernia electively repaired. They went on with their lives until the appearance of symptoms forced them to the on-call central GNHS hospital. This scenario explains not only the decrease in discussion but most of our other findings as well.
2) The editor correctly underscores that we do not provide data from the private sector. Our study was not a multicentric one; data from other hospitals (private or public) were neither collected nor analyzed. The disproportional increase of emergency cases can only be explained by the recruitment of patients who in other times would have addressed themselves to other hospitals public or private. This is exactly what we state in the sixth paragraph of the Discussion section in the revised manuscript where we also stress out that the present study cannot verify the validity of this scenario. This would require additional research, beyond the scope of the present study.

3) The editor is absolutely right in this composite point and we thank him for that. The previous version of the manuscript created the false impression that the increased number of emergency hernioplasties could possibly be attributed to the increased number of immigrants. We attempted to clear things up in the 7th paragraph of the new discussion section. The difference of the population groups in the two periods of time and the change of the immigration status of the region’s population do not affect our study for many reasons. Nowadays Greece suffers from an immigration crisis. First of all we should keep in mind that a significant proportion of the current immigrants/refugees live in areas away from the capital. Furthermore, a significant population of financial immigrants existed in Athens prior to the financial crisis and the austerity measures as well. These financial immigrants fled towards countries with flourishing economy or returned to their homeland as soon as the crisis has started in Greece. So in the two periods different types of immigrants existed in Greece, or as the editor comments “apples and oranges”. Although we could speculate that financial immigrants (of the first period) and political immigrants/refugees (of the second) may have had a different behavior such a comparison was not our aim and cannot be corroborated by our data.

In contrast in our study we did not discriminate between financial immigrants and political immigrants/refugees. We classified our patients into those of Greek nationality and those of a non-Greek one. The maximum percentage of patients of non-Greek nationality was 5% in total, and 6% in the emergency setting, which is rather low to affect the grand picture. Obviously, the 7 more immigrant/refugees appearing in the second period cannot be responsible for the 46 more emergency hernioplasties conducted in the same time frame.

4) The group of surgeons performing the operations in the two periods was practically the same and this information has been added in the corresponding paragraph of the Materials and Methods section. All surgeons belonging to our department are general surgeons. None of them is hernia affiliated.
5) We strongly and deeply apologize for the error in the manuscript. Somehow it escaped our attention and we are grateful to the editor for taking notice of it. The mistake has been corrected and the whole paragraph was rewritten to make things clear. Indeed our elective workload was less in the second period as our Table reports, in accordance with Question 1 imposed by the Editor.

6) Our study reveals some changes in the practice of a surgical department during the financial crisis. There is limited relevant information in the literature. For this reason we report and discuss our findings offering possible explanations which may, or may not, lie in the field of surgery. On the other hand we feel obliged to present all relevant information – not necessarily scientific evidence – that led us to the formation of our hypothesis. For example population psychology can be affected by mass media articles written in a public-attention attracting manner. Such articles are cited in our manuscript for clarity reasons and obviously do not represent outcomes of scientific research. Taking into consideration our findings, the relevant scientific literature and all other related “information” we formed a hypothesis. We clearly state in the revised manuscript that our study cannot verify the validity of this hypothesis although it offers a reasonable explanation for our findings altogether, and stress out that that further research is needed.

7) A detailed cost analysis would have been a very interesting task. Unfortunately, as we explain in the revised version of our manuscript, reliable data are missing and any attempt would be subject to bias for many reasons: many prices (dispensables, drugs etc) have changed more that once in the studied periods, many of the actual invoices are missing, many companies providing the hospital are currently out of business. Most of the available data from the hospital’s accounting office have been taken into consideration but none of the authors is in position to evaluate and analyze costs, reimbursements and other accounting data reliably. Our aim was to point out changes imposed to clinical practice. Even if we could retrieve all the relevant data (probably impossible) we believe that this task is better be accomplished by someone with an expertise in accounting and on the same time will also be familiar with the ever-changing Greek legislation, otherwise erroneous conclusions will be jumped into.

Reviewer 1

1. The manuscript was re-written in an effort to improve the English language

2. The paragraph with the limitations of the study has been added at the end of the Discussion section.
3. We have no profound explanation for the high percentage of the general anesthesia. The choice of anesthesia is mostly up to the anesthesiologist preference and opinion. We, the surgeons, may have an opinion but usually it is the anesthesiologist who takes the final decision. It is possible, and we inserted a hint in the manuscript, that our anesthesiologists preferred general anesthesia because they were used to it. However we have no solid evidence to support such a suggestion.

4. Unfortunately we are not in a position to perform such an analysis and we added this in the limitations of the study in the Discussion section. We also provided in the manuscript the main reason and we analyzed other reasons in the response to the 7th question of the Editor. It is to our sorrow that we cannot retrieve and analyze such data with a certain degree of reliability.

Reviewer 2

1. We tried to correct the language of the manuscript

2. The word inguinal was repeated in the keyword hernioplasty to discriminate from the rest hernioplasties but it can be omitted

3. We understand that there may be no clinically important difference between 67- and 71-years old but the difference was significant and we believe it has to be reported. More importantly this was the mean age of the sample. Obviously, to have a 4 year decrease in the mean age, the crisis group contained many younger patients.

4. The ambiguity has been clarified in the revised manuscript

5. The tables were reformed and relevant explanations were added in the text of manuscript

6. As explained in the revised manuscript, in the editor’s 7th question and in Reviewer’s 1 4th question we are not in the position to perform a cost analysis

Reviewer 3

1. We fully agree with comment. In the revised manuscript we rephrased this piece, and we also provided some comments in the answer to the 3rd question of the Editor. We took advantage of the Greek language and the peculiarity of the Greek Surnames. For a native Greek speaker identifying a non-Greek nationality by the surname alone is relatively easy. Minor exceptions can be patients from Cyprus and from Northern Epirus which is nowadays part of Albania. By the surname and by the social security data kept in the
electronic records of the hospital we classified our population as those of Greek Nationality and of those of a non-Greek one. Whether the non-Greeks represented legal or illegal immigrants or refugees did not concern our research and they were all collectively classified as of non-Greek Nationality. Even so, they represented a small percentage of the entire cohort unable to change the general picture.

2. The reviewer has a good point with his comment which correlates with the 3rd question of the editor, and we thank him for that. Indeed the non-significant increase may be an observation of limited importance. To the authors’ opinion this observation can be interpreted in 2 aspects. This change may become more significant in time. For example, what if the immigrants are permitted to exit from the remote “hot-spot” areas and become able to move around the country? (This is a question of equally minor importance). On the other hand and more importantly, if what we saw in our study is the outcome of crisis, then the crisis concerns mostly the Greek citizens and the changes we evidenced are not a derivative of the refugee crisis. We are of the same opinion with the reviewer but completeness demands reporting and discussing this finding in order to eliminate doubts concerning the refugee crisis.

3. In the revised manuscript we tried to express more clearly how we associated the necessity of reinforcing district hospitals with our findings and we modified our conclusions accordingly.

4. This paragraph has been added at the end of the discussion section. Indeed the initial intention of the authors was to include both morbidity and readmissions. We realized in this effort that we would have been unreliable. Readmission would have been inaccurate as the patients could have addressed to other hospitals - especially if we were not on call. Morbidity also would have been greatly influenced by the uncontrolled co-morbidities which possibly existed. For these reasons we decided not to include these parameters in our analysis, though the length of postoperative hospitalization is quite informative. Although it would be tempting to say that in a future study we will take appropriate care to investigate these topics, we hope that this will not happen because we hope that this crisis will end soon and we hope that will not evidence a future one.