Peer Review File

Article Information: https://dx.doi.org/10.21037/jtd-24-943

Reviewer A

The authors have conducted a systematic review on the rare but potentially fatal complication of intrathoracic migration of Kirschner wire, examining the physiology, mechanisms, symptoms, treatment, and outcomes in detail. I commend the authors for their excellent effort and significant contribution. The manuscript is logically and comprehensively written, providing valuable insights for medical professionals involved in the treatment of this complication. However, I have identified several areas for improvement, as detailed below.

Reply: We thank the reviewer for this very detailed and insightful analysis of our manuscript and many excellent ideas to improve it. Thank you very much for the huge amount of time you took for your many useful comments.

Comment 1. There are instances of typos, duplicated sentences, and inconsistencies in articles and plurals. It is strongly recommended to use an English language editing service by a native speaker.

Reply 1: We thank the reviewer for this comment. We have double-checked our manuscript and indeed corrected many linguistic issues and rephrased many sentences.

Changes in the text: All modifications marked in red.

SHORT ABSTRACT

Comment 2. In line 19, "pleura" should be replaced with "pleural space," "thoracic cavity," or something for better clarity.

Reply 2: We agree and have modified our text as advised.

Changes in the text: Page 4, Line 26

Comment 3. In line 23, the phrase "not rare" is used. If there are specific references discussing the incidence, it would be beneficial to cite them.

Reply 3: With more than 110 published cases found we may conclude that this complication is “not rare”.

Changes in the text: Page 4, Line 27-28

ABSTRACT

Comment 4. In the Background and Objectives section, it would be more engaging for readers if the specific discussion points of the paper are highlighted.

Reply 4: Within the word limit of 350 words, we cannot be much more explicit. As written in Line 46-47 our aim was to study the frequency and patho-physiology of intrathoracic migrations.

Changes in the text: none.

Comment 5. The term "type" in line 36 should be elaborated.

Reply 5: We agree with the reviewer and have modified our text as advised.

Changes in the text: Page 5, Line 51-52

Comment 6. In line 36, if "integrity" refers to whether the K-wire is broken or not, "intactness" might be more appropriate for clarity.

Reply 6: We agree and have used the term “intactness”.

Changes in the text: Page 5 Line 52-53

Comment 7. Similar to the comment on the SHORT ABSTRACT, "pleura" in line 42 should be revised.

Reply 7: We agree and have used the term “pleural space” here and later wherever adequate.

Changes in the text: Page 5 Line 58

Comment 8. In line 52, "shorter" is mentioned. Since the comparison is between groups treated with thoracoscopic surgery and open surgery, it would be beneficial to indicate whether the difference was statistically significant.

Reply 8: We agree with the reviewer. We used a “T-Test Calculator for 2 Independent Means” and we found that there was a non significant difference between these two normally distributed groups (T = -1.542, P = 0.071). We added our findings to the manuscript.

Changes in the text: Line 70, 295

RESULTS

Comment 9. In lines 163-164, the terms "linear" and "nonlinear trajectory" are somewhat unclear. Defining each and providing representative clinical images would facilitate better understanding for readers.

Reply 9: We agree and added the definitions for linear and non-linear migration in our material section and adapted the text accordingly. As suggested we added a clinical vignette to illustrate the divisions based on the trajectory and intactness of the OM.

Changes in the text: Line 154-158, 207, Figure 3

Comment 10. In lines 163-164, "10 cases with an intravascular embolization of the material" are discussed parallel to "linear" and "non-linear." Is this because the trajectory in these 10 cases was unpredictable? If so, this should be explained.

Reply 10: We agree and defined linear, non-linear and intravascular embolization as requested in M&M.

Changes in the text: Page 9, Line 157-159.

Comment 11. In line 166, the phrase "if the origin was the clavicle" is used. What is the control group for the clavicle origin cases, and what is the rationale for choosing this control group? This should be elaborated.

Reply 11: We agree that this sentence is not clear. The “odds ratio” of a linear migration for a case with clavicular origin of OM was calculated in comparison to the “odds ratio” of a linear migration in all the other groups. We have modified our text to better clarify this.

Changes in the text: Page 11, Line 211-214

Comment 12. In line 167, similar to the previous comment, the control group for the proximal humerus origin cases should be clarified.

Reply 12: We agree. We removed this passage and refer to our reply 11.

Changes in the text: Page 11 Line 211-214

Comment 13. In lines 176-178, is there a statistical significance in the time to diagnosis between the linear migration and non-linear migration groups? Also, it would be valuable to discuss the clinical implications of this result.

Reply 13: We agree that it would be important to know if there a statistical significance in the time to diagnosis between the linear migration and non-linear migration groups. To calculate statistical significance between two independent “mean values”, the data would need to be normally distributed, which is not the case, as indicated by our P values in line 225 and 227. To be exhaustive and answer your question, we used a “T-Test Calculator for 2 Independent Means” which showed a non-significant difference with a p-value of 0.289498. ($P > 0.05$).

Changes in the text: None.

Comment 14. In lines 181-182, specify which hospital stay is being discussed.

Reply 14: We meant the mean duration of hospital stay for all cases. We have modified our text to better specify this.

Changes in the text: Page 12, Line 240-241

Comment 15. In line 193, providing more details on the fatal cases, such as causes of death, the course to diagnosis, treatment methods, and outcomes, would be more informative for readers.

Reply 15: We agree and will provide more details on the five fatal cases in their respective sections, as advised.

Changes in the text: Line 236, 370-387

Intrapulmonary Migration

Comment 16. In lines 199-201, comparing with the intrapleural migration group should be done in the discussion section to avoid confusion. Additionally, if possible, it would be valuable to discuss how the fact that intact pins migrated more frequently in the intrapulmonary migration group can be applied or utilized clinically.

Reply 16: We agree and have moved these lines into the discussion and have discussed potential consequences.

Changes in the text: sentence moved to discussion: Page 22, Line 472-478

Comment 17. In lines 206-209, it is fascinating that wires and pins around the shoulder sometimes migrate into the opposite lung. It would be beneficial to discuss the routes and mechanisms of this migration in the discussion section.

Reply 17: We agree completely. We found that in the group of intrapulmonary migration, 10 cases had migrated to the lung opposite of the site of their original placement. We have corrected our manuscript in the result section and have extended our discussion on this interesting phenomenon.

Changes in the text: Line 266-268, 442-459

Comment 18. In lines 222-223, mentioning these four cases in line 217 would avoid confusion for readers.

Reply 18: We agree and have moved these cases as suggested.

Changes in the text: Page 14, Line 285-286

Comment 19. In lines 223-224, the minimally invasive nature of thoracoscopic surgery is one of the most noteworthy findings. Statistically comparing with the open surgery group would be beneficial.

Reply 19: We agree and refer to our reply to comment 8.

Changes in the text: Line 70, 295

Mediastinal Migration

Comment 20. In lines 233-234, the case described here is highly dramatic. Providing more details on the diagnosis and treatment course would captivate readers' interest.

Reply 20: We agree and added more details about this case.

Changes in the text: Page 15, Line 300-304

Comment 21. In lines 242-243, detailing the reasons for conversion to open surgery would be very informative for readers.

Reply 21: We agree and have removed this sentence since these cases had already been described in a previous paragraph. We have provided more details about the cases describing their reason for conversion.

Changes in the text: Page 14, Line 286-292

Comment 22. In lines 256-259, this sentence is repetitive with the previous paragraph.

Reply 22: We apologize for this repetition and removed the redundant lines.

Changes in the text: removal of repetitive sentence.

Intracardiac Migration

Comment 23. In lines 278-279, the total number of cases mentioned is only 22, which seems one case short.

Reply 23: We congratulate the reviewer for their astute observation. Indeed, case 23 (ref. 19) was reported further down in this paragraph and therefore is missing in your count. We moved it higher to line 363 for the sake of clarity.

Change in the text: Page 17, Line 363-364

Comment 24. In lines 288-289, similar to comment 15, providing detailed information on fatal cases is expected.

Reply 24: We agree and have added more detailed clinical informations on the fatal cases.

Changes in the text: Page 18, Line 370-387

Intrapleural Migration

Comment 25. In lines 298-299, as mentioned in comment 16, comparisons with other groups should be summarized in the discussion section to avoid confusion.

Reply 25: We agree and have removed these lines from the result section to the discussion section where this was already mentioned anyhow.

Changes in the text: Page 22, Line 487-489

Comment 26. In lines 308-310, similar to comments 16 and 25, discussing in the discussion section would be better.

Reply 26: We agree and have removed this from the result section to the discussion section where this was already mentioned.

Changes in the text: Page 22, Line 472-476

DISCUSSION

Comment 27. In lines 337-341, summarizing the limitations of this study at the end of the discussion would be preferable.

Reply 27: We agree and have moved this limitation passage.

Changes in the text: Page 25, Line 534-539

Comment 28. In lines 341-343, "4.5%" does not seem to align with "numerous." Another expression would be more suitable. Additionally, there is a logical leap between the first part "As shown..." and the second part "every migration...."

Reply 28: We agree that 4.5% is not numerous but still represents a significant problem. We removed the word “numerous”.

Changes in the text: Page 23, Line 500

Comment 29. In lines 352-353, it would be more informative to state the exact percentage that constitutes the majority.

Reply 29: We agree. We have adapted this sentence and added the requested percentage for more clarity.

Changes in the text: Page 23, Line 513-514

Comment 30. In lines 353-355, the authors recommend imaging follow-up at 6 or 12 months post-fracture surgery. How long should follow-up continue? If possible, providing insights on this would be very beneficial for readers.

Reply 30: We agree with the reviewer that we have found no evidence to recommend any specific follow-up scheme. However as a follow-up period of 10 years would be needed to prevent 98 of the 112 cases, such a long period of follow-up does not seem to be realistic.

Changes in the text: Highlight box, Page 23, Line 504-510

Comment 31. In lines 355-356, the meaning is unclear and should be clarified.

Reply 31: We agree and have explained this more clearly.

Changes in the text: Page 24, Line 534-539

Comment 32. In lines 360-362, the advantages of thoracoscopic surgery are mentioned suddenly. Providing supporting references would be helpful.

Reply 32: We agree. It has been largely proven in many fields that VATS is less invasive than open thoracotomy and we have added a reference for this as requested.

Changes in the text: Page 24, Line 526-527

Comment 33. Discussing which cases or situations warrant consideration of conversion from thoracoscopic to open surgery, and the reasons for such conversions in previous reports, would be very informative for readers.

Reply 33: We agree that this is an important subject and it deserves to be mentioned in the discussion. We have provided more details about the cases describing their reason for conversion.

Changes in the text: Page 24, Line 527-531

Comment 34. In lines 368-370, the meaning is unclear and needs to be revised.

Reply 34: We agree and have revised and reorganized our discussion section to meet your request.

Changes in the text: removal of unclear sentences.

Comment 35. In line 370, defining "antegrade migration" would be helpful.

Reply 35: We agree. Antegrade migration was defined in our methods section.

Changes in the text: Page 9, Line 152-153

Comment 36. In lines 373-376, what is the control group compared with the clavicle origin? Stating the reasons for this comparison and the insights gained from the results would make the content more engaging for readers.

Reply 36: We agree and we refer to our reply to your comment 11. We reformulated this for clarity. We discussed our insights more in depth in the discussion section.

Changes in the text: Line 211-214, 476-484

Comment 37. In lines 380-381, the authors mention "Patient age and the duration of presence of the material in the body might also have an influence." Providing supporting data, literature, or reasons for this would be beneficial.

Reply 37: Although we have no specific studies or references on these issues, it seems obvious that patient characteristics play a role. Use of K-wires in pediatric cases will increase lifelong exposure risk to migration, whereas elderly patients might present with bone fragility and thus potentially increased risk of migration. We have added these items to our discussion section.

Changes in the text: Page 21-22, Line 464-469

Comment 38. In lines 383-383, there seems to be a contradiction with the previous statement regarding patient age. Clarification is needed.

Reply 38: We agree and this overlaps with our reply to your comment 37.

Changes in the text: Page 21-22, Line 464-469

Comment 39. In lines 383-384, similar to comment 38, there seems to be a contradiction in interpreting the clinical information of "the duration of the OM in the body." While it may not be conclusive, discussing potential relationships would be beneficial.

Reply 39: We agree that this sentence is unclear. Since this subject will be discussed elsewhere, we have removed this sentence here to avoid confusion.

Changes in the text: removal of redundant sentence

Comment 40. In lines 389-391, deeper exploration of the mechanism by which wires used in the shoulder migrate to the opposite lung would be fascinating for readers. It would be helpful to discuss the conditions that make such migration more likely.

Reply 40: We agree and have added more detailed clinical information on cases describing migration to the contralateral thorax.

Changes in the text: Page 21, Line 443-459

Comment 41. Regarding line 392-393, does "non-broken linear migration" mean "non-broken and linear migration"? If so, it would be beneficial to explain the rationale for focusing on this group. Additionally, please clarify the control group used for comparison.

Reply 41: We agree and thank the reviewer for this important remark. Our findings about “intact” “linear” migration in the “intrapulmonary migration group” were indeed supposed to be discussed separately. We have adapted this sentence to show this distinction more clearly.

Changes in the text: Page 22, Line 472-476

Comment 42. In line 394-397, "These findings suggest a mechanism..." seems inappropriate. It would be more accurate to state that "These findings can be explained by a mechanism....”

Reply 42: We agree and have adapted this sentence as suggested.

Changes in the text: Page 22, Line 476-477

Comment 43. In lines 398-399, the phrase following "could..." is repetitive within the same sentence.

Reply 43: We thank the reviewer. We have removed this redundant part of the sentence.

Changes in the text: removal of redundant part of the sentence.

Comment 44. In lines 401-404, the meaning is unclear and difficult to understand. If discussing odds ratios, the control group should be specified.

Reply 44: We agree and we have adapted this sentence to state more clearly that the intrapleural migration group is compared to the intrapulmonary group.

Changes in the text: Page 22, Line 472-474

Comment 45. In lines 404-408, the logic seems flawed. The statement "the high occurrence of intrapleural migration in the presence of pneumothorax is supported by the fact that pneumothorax is common in cases of intrapleural migration" needs to be clarified and revised.

Reply 45: We agree and have corrected this sentence.

Changes in the text: Page 22, Line 487-489

Comment 46. In lines 409-410, the meaning is unclear and should be revised.

Reply 46: We agree. We have changed this paragraph and moved it to the beginning of the discussion section

Changes in the text: Page 20, Line 441-442

Comment 47. In lines 409-412, the content is more general and should be moved to the beginning of the discussion section.

Reply 47: We agree and refer to out reply to comment 46.

Changes in the text: Page 20, Line 441-443

Comment 48. In lines 413-419, the content lacks coherence. Moving general content to the beginning of the discussion and using a separate paragraph for fatal cases would improve readability and informativeness.

Reply 48: We agree and have moved this paragraph to the beginning of the discussion, where we also expand more on the fatal cases we had described in the result section.

Changes in the text: Page 21, Line 442-450

Comment 49. In lines 422-424, limitations are mentioned. As per comment 27, summarizing limitations together would improve readability.

Reply 49: We agree and have grouped the discussion of the limitations in our study together.

Changes in the text: Page 24, Line 539-541

Comment 50. In lines 426-427, while the content is correct, it appears abruptly. Citing previous reports supporting the benefits of patient education would be advisable.

Reply 50: We agree and have added a relevant reference, supporting our claim about the benefits of patient education.

Changes in the text: Page 25, Line 555-557

CONCLUSION

Comment 51. In lines 438-440, the authors emphasize the utility of VATS. Discussing the characteristics and outcomes of cases completed with VATS in the discussion section would further highlight this assertion's value.

Reply 51: We agree with reviewer that this discussion needs to be more detailed, we have adapted the paragraph about VATS and its possible advantages and differences in comparison to an open/invasive approach.

Changes in the text: Page 24 Line 521-533

Reviewer B

I would like to congratulate the authors on their effort and resulting manuscript which we have also reviewed for EJCTS and whereafter the comments mentioned below remain.

Reply: We thank the reviewer for their analysis of our manuscript. Based on your previous review, we had already adapted this manuscript before submission based on your useful comments.

General comment: please refrain from the use of not commonly used abbreviation and acronyms. Such as AC, HS, K and so on. Please write them in full for the sake of readability.

General reply: Throughout the main text, we have written in full all unusual abbreviations as you requested. We only kept our abbreviation for Kirschner (K-wire) and osteosynthesis material (OM) as these abbreviations were used frequently throughout the text and are the standard used in any orthopedic journals.

Changes in the text: removal of all inadequate abbreviations throughout text.

Comment 1. why didn’t you include observational studies or case series?

Reply 1: Based on your comments, we had already adapted this in our present manuscript. (Line 127-128) Our literature review did not find any other observational studies than those already included.

Changes in the text: none.

Comment 2. retrieving 33 additional inclusions through snowballing adds question marks to the primary search.

Reply 2: We agree with the reviewer that the proportion (32%) of articles found during secondary search may seem high. We initially tested other search strings. But they yielded too large numbers of irrelevant records. Thus our current search strategy appeared as the most efficient one.

Changes in the text: none.

Comment 3. shouldn't neck and intraabdominal migration be an exclusion criterium? It is wrong to exclude studies that late on.

Reply 3: We agree with the reviewer and indeed isolated neck and intraabdominal migration of osteosynthesis material were not included in our review. We had only kept those articles describing a passage through the intrathoracic region or those articles describing multiple pins or fragments of pins migrating to several anatomic regions in a same patient. We heave explained this more clearly in our methods section.

Changes in the text: Page 8, Line 131-135

Comment 4. Apart from the migration I find it even more interesting: what was the indication for osteosynthesis using Kirshner wires? To date, the indication for Kirshner wires is narrowing and narrowing, hence the fact that part of the case reports presented are dated! Please comment. For example why use Kirshner wires for a fractured clavicle.

Reply 4:

We agree with the reviewer that the indications for K-wires have been narrowing. However, it goes far beyond the scope of our review to discuss in detail every indication for the initial choice of osteosynthesis material. It would even be impossible, as most case reports did not clearly report the initial indication and some migrations occurred as late as 30 years after osteosynthesis. Fact is that these complications did occur and their treatment represents the scope of our manuscript. Even if indications may be narrowing, the treatment of late complications still remains relevant. Some recent studies did not find significant outcome differences between plating and Kirschner wire internal fixation for clavicle fractures (references 1 - 2). Furthermore, beyond the fact that the use of Kirschner pins remains a topic in recent orthopedic research, it may be the only material available in many remote places worldwide (developing countries…). We emphasized on these aspects in the discussion.

Changes in the text: Page 25, Line 548-551

Comment 5. What was the position of the osteosynthesis material? As to understand the chances for migration. Where the wires also used for intramedullary fixation?

Reply 5: As mentioned previously, precise information regarding initial trauma, indication and intervention were often missing in the original reports. Therefore, no relevant analysis and statistics were possible. Based on available images, at least for clavicular fractures, intramedullary use of the wires was rather the rule than the exception.

Changes in the text: none.

Comment 6. In addition, given the mean time for both linear and non-linear migration, why weren't the Kirshner wires removed after adequate fracture healing?

In some cases the migration occurred 30!! Years after osteosynthesis.

Reply 6:

We agree with the reviewer that all wires should theoretically be removed. However, based on our observations, and relevant papers (reference 67, Ngarmukos et al.) this was not always the case or may not always be possible (patient compliance, broken pin fragments …).

Changes in the text: Page 25, Line 548-551.

Comment 7. And to what extent should we blame faulty primary osteosynthesis using the Kirshner wires such that migration can occur? Aren't we just reviewing surgeon faults'?

Reply 7:

We do not believe that only faulty usage or surgeons faults were included. We had excluded all perioperative migrations or accidents, since these could not be defined as a migration. (cfr. Line 129-131 and Exclusion criteria 3D, supplementary table S2)

Changes in the text: none.

Comment 8. Apart from a summation of partially dated case reports; what can we learn from this review? Use Kirsher wires only in selected cases and remove them upon fracture healing?

Reply 8:

We believe that we do not report dated case reports, since most cases were reported only during the last 20 years. (Table 2) Only recently, we had to deal with a similar case, where the K-wire had been placed in Northern Africa. We learned that this complication is not rare, is obviously still happening and therefore raising awareness is necessary. We adapted Figure 4 to more clearly represent our data.

Changes in the text: Page 23, Line 542-545 + Table 2

Reviewer C

A good review of literature with key take home messages are useful. Given the paucity of such scenarios I do believe there is merit in the review of case reports.

Reply: We thank the reviewer for their kind comments. We fully agree that our review fills a knowledge gap.

Reviewer D

Dear authors thanks for the interesting manuscript!

Please let me make some comments and encourage you to improve your valuable work.

Reply: We thank the reviewer for their analysis of our manuscript.

Comment 1. Please clarify the role of different types of osteosynthesis. You focused on k wires and Steinmann pins.

Does this mean you rule out sternal (closure) wires? Plates and screws, judet- clamps, any struts, pectins bars etc? Please define and clarify. I'm happy to focuse on the role of k wires (only) but it needs to be clearly defined

Reply 1: Only osteosynthesis material used for fracture related osteosynthesis, was included. We indeed excluded all reports dealing with any material used for chest reconstructions, pectus… because this would have broadened too much the variety of incomparable clinical scenarios (e.g. complications of pectus bars have their own literature…). We have modified our text in our methods section to better clarify this distinction.

Changes in the text: Page 8, Line 135-137 + supplementary table S2

Comment 2: I like your differentiation in a variety of targets the wires moved to.

Vice versa please specify more on the original procedures. What exactly had been fixed in which technique? Orthopedic und trauma surgery mainly abandoned k wires around the clavicle and prox humerus exactly due to the reason you show here. Therefore some strong conclusions should be drawn from your great work!: don't use wires around the clavicle and others!

Reply 2: As mentioned previously in response to reviewer B, precise information regarding initial trauma, indication and intervention were often missing in the original reports. Even if the indication for using K-wires may be narrowing, the treatment of late complications still remains relevant. Furthermore, even if evidence-based literature advises against, K-wires probably still remain the only available option in many developing countries. As advised, we have expanded this subject in our discussion.

Changes in the text: Page 23, Line 542-545, 548-551

Comment 3: Could you please show some short examples / cases

- from each region

- from the type of migration since I try a little hard to get linear and non linear migration from your text and your chest x ray.

Reply 3:

We agree and have added a case image of our own to illustrate the distinctions based on the trajectory and intactness of the OM. (Figure 3) All other images from cases in our review could not be used because of copyright issues.

We added the definitions for linear and non-linear migration in our material section and adapted the text accordingly.

Changes in the text: Page 8, Line 153-158 + Figure 3

Comment 4: Next topic is the timeframe of the incidents: please clarify the included time and show the numbers by time. Did they decrease overcome years? Did any orthotrauma guidelines influence this?

Reply 4: We agree that this is relevant information. To follow your request and better visualize this, we added table 2 showing the evolution of reports on this complication over time. We found that guidelines did not influence the interval to diagnosis. We added table 3 to our article.

Changes in the text: Page 23, Line 542-545, 548-551 + table 2

Comment 5: Once again: did sternal closure, pectus correction etc play a role in your work?

Reply 5: This question has previously been answered, therefore we refer to Reply 1.

Changes in the text: Page 8, Line 135-137 + supplementary table S2

Comment 6: Please comment on the biomechanic rationale of material failure and migration. It could be the mobility of the fixed bones and joints. What is the difference in orif with (locked) plate osteosynthesis? Safer!

Reply 6: We agree that the mobility of the fracture fragments plays a major role in failure of any orthopedic hardware. This is confirmed in the orthopedic literature (reference 1 – 3). We have adapted our discussion accordingly.

Changes in the text: Page 20 Line 439-441

Comment 7: Some diagrams would make it easier to compare the huge amount of data you present. It even would make it easier to cite your work in future.

Reply 7: We appreciate the suggestion of the reviewer and have added some diagrams. As requested we added figure 3, figure 4, figure 5 and table 2. We hope the reviewer can appreciate our effort to better and more completely present our data.

Changes in the text: addition of Figure 4, Figure 5 and Table 2