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Barking up the wrong tree: mechanical alignment’s surgical errors should not be confused with the kinematic alignment technique.

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Dear Chief Editor,

You recently published a note by David A. Parker (1) “A Cautionary Note for Advocates of Constitutional or kinematic Alignment”. In this commentary, the author voices his concern over the Kinematic alignment (KA) technique for implanting total knee arthroplasty (TKA) components by referring to the article from Van Hamersveld et al. (2) “The Effect of Coronal Alignment on Tibial Component Migration Following Total Knee Arthroplasty” that was released by your journal. Van Hamersveld et al. (2) conducted a radiostereometric analysis (RSA) study and found that patients with radiographic (long-leg radiograph) varus lower limb alignment after mechanically aligned (MA) TKA were at increased risk of excessive migration and loosening of the tibial component. There are however, in our opinion, some imprecisions in the way this commentary has been written, which may confuse our orthopaedic community, and therefore deserve clarification.

Firstly, a post-operative varus hip-knee-ankle angle (HKA) alignment after a MA TKA does not equate to replicating a patient’s constitutional varus alignment with KA. Most residual varus HKAs measured on long leg X-ray after MA TKA are linked to surgical errors (e.g reducing the lateral distal femoral angle [LDFA] below 90 degrees) and/or inaccuracy of radiographic measurements (3–8). With that being said, Parker et al. maintain the confusion between the KA technique and the new variant of MA surgical technique, namely the adjusted MA technique (9, 10). The adjusted MA technique consists of preserving some limb varus by cutting the distal femur in slight varus (relative to the femoral mechanical axis) when performing MA TKA (11). With this technique, in most cases, both the femoral and tibial anatomies are modified (12). On the other hand, the goal of the KA technique is to restore the native knee joint line orientation and collateral ligament laxities (13). It also aims to reproduce the individual knee kinematics during knee flexion-extension, under both loading and non-loading conditions. As pointed out in a recent editorial, “…it is important to unambiguously describe and define the current implant orientation and alignment options. Only when knee surgeons use the same definitions to discuss alignment, progress can be made and misinterpretation be limited” (14). It is indeed of the utmost importance to understand that findings generated with a certain surgical technique for implanting TKA components (MA, adjusted MA, KA, etc.) are not transferable to another surgical technique. We regret that such a cautionary note for advocates of KA has been published, despite warnings from Van Hamersveld et al. (2) against any extrapolation of their data to this specific topic: “all included patients were managed with the intent to achieve neutral mechanical alignment, and thus the constitutional alignment analysis should not be interpreted as being the result of kinematically aligned TKAs, which may produce different outcomes”.

Secondly, Hamersveld et al. concluded that “subgroup analysis revealed that within each constitutional alignment category, mean migration values were higher in components that were placed in mechanical varus compared with mechanically in-range TKAs”. Therefore, the correlation between varus and loosening or migration is mainly present in the cases where the patient’s anatomy was modified to a varus alignment when it was not their constitutional anatomy. Again, Parker (1) misled the readers by suggesting that KA equates to surgical errors (putting lower limb in varus [>3°] when they were not in varus constitutionally). We believe that the real question is to estimate the effects of anatomy modifications during TKA and implant migration or loosening.

Thirdly, the author states “The concern remains…with these less-conventional alignment strategies, particularly in implants designed with the intention of neutral mechanical alignment”. One should realize that many commercialized TKA implants are adapted to reproduce the individual native knee joint line tri-dimensional orientation (15), and have shown good early to mid-term safety (acceptable failure rate) when kinematically aligned (16). Most TKA implant designs therefore seem suitable for KA implantation, and probably do not add extra risk of component loosening for the patients when positioned as such. On the other hand, we agree that improvements could be made, especially on the prosthetic trochlea design (17), and that the future of TKA may be KA custom implants.

Fourthly, the author states “there is concern regarding whether or not the good results from 1 or 2 enthusiastic groups are generalizable across the general arthroplasty community”. It should be noted that,

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as far as we are aware, 11 teams [3 from USA (18–20), 1 from UK (21), 1 from Germany (22), 1 from NZ (23), 2 from Japan (24, 25), 1 from AUS (4), and 2 from CAN (26,27)] have assessed the value of KA TKA and published encouraging clinical early- to mid-term outcomes. Ten of those studies were comparative study designs, and found similar (21, 23, 27) or better (4, 18, 19, 22, 24, 25, 28) clinical outcomes for KA patients over MA patients.

Fifthly, the author states that "A randomized RSA study comparing kinematic or conventional alignment strategy may be impractical, and therefore the current study does provide useful information to guide both further study and clinical practice". We agree that performing such a study is challenging and we should applaud Laende et al. who published the results of such a study (27). Comparing navigated-MA TKA and PSI-KA TKA, the authors found similar tibial component migration between groups. We agree with Parker that more knowledge is needed to confirm the limits and benefits of KA TKA, but on the other hand, we disagree about the incapacities of modern research to produce the needed data.

Finally, KA and MA are 2 different surgical techniques, with differences involving component positioning in the 3 planes (9, 29). While the KA technique intends to prevent biomechanically deleterious TKA imbalance, MA TKA with measured resection often inherently generates it (30, 31). Compared to MA patients, KA patients have a more natural gait and dynamic HKA that tends to lower the dynamic adduction moment (28, 32, 33), and have more physiological soft-tissue balance that is likely to reduce the frequency and intensity of lift off (4, 34). These biomechanical advantages over MA TKA may mitigate the potential flaw that increased adduction moment could occur on knees with severe standing constitutional varus deformity, and explain the acceptable in-vivo walking biomechanics (28, 32, 33), tibial component 2-year migration (27), and 11-year implant survival (20) that have been reported for KA patients. Other perspectives to consider include the fact that total (KA-TKA) and partial (UKA) knee replacement procedures that aim to restore the dynamic constitutional limb alignment and the physiological soft-tissue balance, have both shown evidence of good mid- [KA TKA (20)] to long- [UKA (35)] term component lifespan that is independent of post-operative limb alignment (20, 35), and acceptable implant migration on RSA (27, 36). In addition, high BMI and high weight, which probably increase joint load much more than few degrees of residual varus in the limb, have been shown to only slightly (approximately 20%) increase the risk of tibial component late-loosening (37); the 'at risk BMI' threshold was found to be 35 (38).

Taking all of the above in to consideration, we continue to have concerns regarding the compatibility of Kinematic positioning of TKA implants with extreme patients' anatomies; the future challenge lies in determining if all patients' anatomies should be restored with KA-TKA (29). We agree that some knee anatomies may be inherently biomechanically inferior and considered pathological, and thus may not be compatible with current TKA prostheses and fixation methods. Currently, there is a lack of evidence to help determine which native knee anatomies or biomechanics should not be fully restored when performing KA TKA. Acknowledging the lack of data on these extreme cases, Vendittoli et al. proposed the Restricted KA (rKA) protocol. With rKA, the independent tibial and femoral cuts must be within ± 5° of the mechanical axis of the respective bone and the overall resulting HKA must fall within ± 3° of neutral. By applying the rKA protocol, approximately 50%, 30%, and 20% of the cases will have no (pure KA), minimal (less than or equal to 1°), and more important (>1°) anatomy modification, respectively. By adjusting KA bone cuts (50% of cases), outlier frontal knee/limb anatomies are reduced (32). By adhering to these rKA boundaries, it is possible to reliably produce a prosthetic knee with component/knee/limb alignments that always fall within an evidence-based safe alignment range (32, 39).

Instead of being alarmist and following dogmas, the scientific community should focus on determining the limits of KA. With the frequent unsatisfactory results of MA TKA, we prefer to be enthusiastic and try to optimize the preservation of patients’ anatomy (12), reduce ligament imbalances (39), improve patient knee kinematics (28), and improve clinical outcomes estimated by validated scores (16, 28), without compromising implant survivorship by using the rKA protocol.

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Conflict of Interest:
Charles Rivière declares being a paid-consultant for Medacta and Pascal André Vendittoli declares being a paid-consultant for Microport, Medacta, Stryker, Ethicon, and Johnson and Johnson.

Article Author Response

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Article Author(s) to Letter Writer(s)

Not “barking”, just advocating for rational, evidence based discussion

Dear Editor,

Many thanks for providing the very detailed response from Dr Charles Riviere to my recent commentary on the article by Van Hammersveld et al. And many thanks to Dr Riviere for taking the time to construct such a detailed itemized response. I should start by noting that my commentary was in response to a request from the editorial staff of JBJS-A, having been a reviewer for the article that was subsequently published. Overall my impression on reviewing the article was that it had a number of limitations, but enough scientific merit and relevance to the current discussion around implant alignment to merit publication, and to be of interest to the readership of the journal. It is pleasing that a commentary, which by its nature is an opinion piece, and was intended to be somewhat provocative, has undoubtedly achieved that goal in stimulating this ongoing discussion. Having read other similar replies from Dr Riviere to other

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journals expressing concern at the suggestion of possible concerns with kinematic alignment (KA) I can see that this is clearly an area about which he is quite passionate (1).

In the title of his response Dr Riviere suggests that I am “barking up the wrong tree” and later says that I am being “alarmist”, which I would humbly suggest is somewhat unnecessary hyperbole. Whilst Dr Riviere goes to great lengths to detail why this paper is not a study on KA and therefore commentary that in any way tries to relate it to KA is inappropriate, I believe that the entire premise of his response is somewhat misguided, as I don’t believe that in my commentary I have said that the paper is a valid study on KA, and in fact I have suggested quite the opposite. I have merely suggested that some of the findings of the paper may have some relevance to some aspects of KA. For me to then suggest that this may provide reason to be “cautious” is I think a reasonable concept. To suggest, as Dr Riviere does, that to advise caution is being “alarmist” is a somewhat aggressive response. I would suggest (again most humbly) that if I wished to be “alarmist” then I would have used the findings of this paper to advocate abandonment of the KA technique, rather than advising caution and a balanced approach to interpretation of evidence.

The first two points Dr Riviere makes are based around the premise that I have ”confused” the goals of the surgery in this study with the goals of KA. Having again read my commentary, I don’t believe that in any section of it have I said that this is a valid study of KA, but that the analysis of the achieved alignments in this study may have some implications for intended alignments when KA is actually performed. Although I would unreservedly agree that unintended overall varus malalignment is not relevant to intended KA, and is often the result of surgical inaccuracy, it is interesting to note that this study found (as mentioned in my commentary) that a sub-analysis showed that matching the patient’s constitutional alignment also did not preclude high implant migration. Again this is not the same as intended KA, but to suggest that this has absolutely no relevance to “constitutional alignment” or KA is rather disingenuous.

In his third point Dr Riviere states that “many commercialized TKA implants are adapted to reproduce the individual native knee joint line tri- dimensional orientation”, then uses this to justify that “Most TKA implant designs therefore seem suitable for KA implantation”, but somewhat paradoxically then concludes this paragraph with “we agree that improvements could be made” and that “the future of TKA may be KA custom implants”. There is quite a dichotomy between on one hand stating that most current designs are suitable for KA, and then saying that the future is custom implants. It would appear that in initially disagreeing with my commentary in this area, he then decides to somewhat reluctantly agree with at least part of it. Again, my commentary was not suggesting that KA could not be done with standard implants, as there are many studies examining this as he has quoted, but again that caution should be exercised and consideration given to the possible limitations of standard implants with these techniques.

In his fourth point Dr Riviere provides a list of references from teams in 7 countries that have "published encouraging clinical early- to mid-term outcomes" . I would firstly respond by saying that the paper I have provided the commentary on is examining component migration, which is more relevant to longer term outcomes, and indeed the possible concern raised by this study is for these longer term outcomes and not "early to mid-term outcomes". These references are therefore somewhat redundant for this discussion. The relevant concern in this instance is clearly that the proposed benefit that may be seen with improved early to mid-term outcomes may be negated by increased longer term failure, particularly in more extreme alignments.

Whilst I do acknowledge Dr Riviere's point that this topic has been studied in several centres around the world, it is an interesting exercise to look in a bit more detail at the actual findings of the studies he has referenced as having published these "encouraging" outcomes. There are 11 studies referenced, from 7 countries. Of these studies, one from the USA (2) is simply a questionnaire survey, and another from Canada (3) is a surgical technique description with no comparison group, and are not valid for any comparison of KA with more conventional techniques. Another study from the USA (4) reported good functional outcomes and survival with KA, but again with no comparison group. There are two studies, one from the USA (5), and one from Japan (6), that actually do show improved outcome with KA versus Mechanical Alignment (MA). However, one study from Germany (7) showed "better overall results" with KA but also reported "more outliers with poor outcomes in this group". One study from Japan (8) reported that KA "improved functional activity but not patient satisfaction". The remaining 4 studies, one each from the UK (9), New Zealand (10), Australia (11) and Canada (12) found no difference in clinical outcomes between KA and MA. The authors of the New Zealand (10) study concluded that: " the theoretical advantages of improved pain and function that form the basis of the design rationale of KA were not observed in this study", and expressed concern about impact on longer term survivorship with this technique. Knowing that these are the articles Dr Riviere has personally selected to support his argument, I would assume that he feels that this represents the "best evidence" currently available.

Given that the proposed benefit of KA over MA is improved patient outcomes, and particularly improved patient satisfaction, it would seem a fair conclusion that studies showing no difference in outcome are not particularly supportive of this concept. Of the studies quoted by Dr Riviere, only 2 studies from 2 countries showed improved outcomes with KA vs MA, whereas 5 studies from 5 countries showed either no difference in outcome, or no improvement in patient satisfaction. It would seem therefore again rather disingenuous to make the statement that "11 teams" from 7 different countries have "published encouraging clinical early to mid-term outcomes", unless of course one has a very low threshold for being "encouraged".

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In his fifth point Dr Riviere agrees that "more knowledge is needed to confirm the limits and benefits of KA TKA" but then states "we disagree about the incapacities of modern research to produce the needed data." Having read through my commentary again, I don't believe that at any stage did I state that modern research is "incapable of producing the needed data" to examine this question and provide clearer answers, but again suggested that to do this research in a pure form that has no significant limitations is a major challenge. It would appear that in this way Dr Riviere has created a statement by me purely for the purposes of disagreeing with it, when in fact he is agreeing with my comment about the challenges in doing this type of research. Interestingly the randomised study he provides to support his argument demonstrates these difficulties, with only 24 and 23 patients recruited for the 2 groups. I do believe that as we move forward with the ability to combine large data sets from multiple centres, and have improved techniques of big data analysis, that clearer answers to these types of questions can be provided, hopefully with less need for difficult to execute clinical trials.

I applaud Dr Riviere's passion for this topic, and his repeated efforts to "clarify" this topic for the orthopaedic community. I would again humbly suggest that such clarification is not necessary in this instance. The purpose of this commentary was not to be "alarmist" or "dogmatic" about the merits of KA, but merely to try and relate the relevant paper to the current discussion around alignment strategies, and to suggest caution particularly with more extreme alignments, and encourage further good quality research in this area. Reading Dr Riviere's response, I would (cautiously) suggest that he seems to agree.

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