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Author's response

We would like to thank Dr. Baker for his kind words expressed in his letter to the editor. This work indeed was the result of hard work and intensive coordination efforts of many investigators from across the globe.

We welcome Dr. Baker's comments, as it was exactly our intention to start a public discussion on this matter. In fact, the discussion has started at the last ISB meeting in Zurich in a specialized session.

Dr. Baker accepts the use of the Grood and Suntay convention (GSC) as the basis for the ISB standard for describing joint kinematics but he is concerned with the application of this convention to certain joints. He believes that the GSC violates, in some cases, conventional clinical terminology. Therefore, proper response requires answering concerns both at the general level as well as at the specific joint-by-joint level.

We must bear in mind that anatomical and clinical terminology, which was developed in the 19th century, was primarily descriptive and did not require rigorous quantitative motion description. As a consequence, modern clinical and anatomical terminology, which follows this historical development, may often be self-conflicting, ambiguous and non-rigorous. The contradictory anatomical and clinical terminology used to describe the motion at the ankle is a case in point. To date, we are unaware of any public document on 'established clinical terminology', 'conventional clinical descriptions', or 'conventional clinical understanding'. It was therefore left to our team to resolve these conflicts and reach a compromise that provides a rigorous, non-conflicting and complete definition.

The conventions that we finally propose were cross checked with individuals from various disciplines including: anatomists; zoologists (morphologists); orthopedic surgeons; radiologists; and biomechanists. Individuals in each of these groups expressed some dissatisfaction with the proposed definitions but accepted them as a necessary compromise. Indeed, the resulting convention represents a rigorous compromise that can serve, hopefully, to bridge the gap between disciplines in describing joint motion.

1. General

We agree with Dr. Baker that the rotations in the GSC correspond to a particular sequence of Cardan angle rotations. Specifically, the first rotation is about an axis fixed in the proximal segment, the second rotation is about the floating axis (often referred to as the "line of nodes") and the third is a rotation about the distal segment. This sequence coincides with the "Nutation", "Precession" and "Spin" convention described in detail in Dynamics textbooks such as Greenwood (1965). However, the GSC extends this convention beyond rotations to include translations as well. These translations are along the three GSC axes, which are non-orthogonal. Together with the rotations they provide a complete and unambiguous description of finite rigid body displacement. We strongly disagree with Dr. Baker's comment that such a description does not make sense. It is precisely this non-orthogonality which renders the GSC convention so attractive and clinically relevant. Moreover, joint motion is in reality continuous, consisting of six degrees of freedom, not six distinct components. The six components are a helpful way to define the motion in a unique and unambiguous way (e.g. no gimbal-lock) relative to an anatomically based reference. In many applications it is the overall motion, and the implications for structures like ligaments that are important. If so, the sequence of the rotations and translations, and hence the values of the individual components of the motion, are not important.

Dr. Baker's definition for the second rotation as "rotation out of the sagittal plane" is ambiguous since it can be either rotation in the frontal plane or in the transverse plane. It is therefore simpler and clearer to define rotations in the rigorous traditional way as rotations about specified axes rather than using planes.

2. Specific response relative to GSC implementation for the ankle complex

Dr. Baker disagrees with the GSC definition of axes for the ankle complex and recommends that the second fixed axis be defined as the "long axis of the foot". In that case inversion/eversion becomes the rotation about the third axis and abduction/ adduction corresponds to the rotation about the floating axis ($e2$). The requirement for consistency and reproducibility excludes this definition for the following reasons. First, the ankle complex includes the tibia/fibula, talus and calcaneus but excludes the forefoot. Second, it is difficult if not impossible to define and identify in a rigorous reproducible manner the long axis of the foot. The foot is an irregularly shaped flexible structure consisting of several bones continuously changing shape during functional activities such as walking and running.

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In the section entitled Tibia, Dr. Baker congratulates the ankle subcommittee for being the only one to define standard anatomical planes for the tibia. The necessity for such a definition is precisely because conventional anatomical and clinical conventions are too vague and ambiguous. For a rigorous, consistent and reproducible description, the definition of these planes must be based on identifiable bony landmarks on the tibia which is what the committee attempted to do. Dr. Baker then continues and expresses a concern that the GSC axes are not aligned with these planes. We assume Dr. Baker refers to the *e1* axis only, since the other two axes are not fixed to the tibia. Our definition of the *e1* axis is the intermalleolar axis and rotation about it is defined as dorsiflexion/plantarflexion. We believe that most clinicians, anatomists and biomechanists will agree with this definition. Finally, the fact that the oblique axis of the tibia, used to define ankle complex motion, cannot be used for the definition used in knee kinematics description is irrelevant. There is no necessity that the ankle and knee reference frames share common axes.

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