False Differential Predictions in Lee, Lai, Brodale & Jampolsky (2007)

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This paper from Jampolsky's group reports two results of an MRI study: [1] that the posterior medial rectus muscle sideslips slightly upward relative to the orbit when the eye elevates and slightly downward when it depresses, and [2] that this movement is greater when the eye moves vertically in abduction (1.5 mm at the posterior pole of the eye) than in adduction (0.2 mm). They believe that these findings disprove the notion of EOM pulleys. Actually, they do not bear on the existence of EOM pulleys at all, even if uncritically accepted. Finally, (Lee, Lai, Brodale & Jampolsky, 2007) state that to account for their data they must assume that orbit-relative muscle paths are determined by "distributed resistance from the surrounding tissue", which is exactly the pulley theory of Miller (1989), which they claim to have disproved.

Imaging

Although they used modern imaging facilities, the methodology chosen by Lee et al is problematic. It has been assumed by other workers that elongated muscles surrounded by contrasting fat and connective tissue are best imaged in cross-section (ie, quasi-coronal sections for rectus EOMs), because: [1] muscles in such images have clear, sharp boundaries, and [2] muscle cross-sections vary little through the thickness of a each such image slice, so that slice positioning is not critical, and artifacts due to partial volume averaging are minimal. Lee et al are alone in having attempted to measure paths of horizontal recti in sagittal view, in which muscles tend to move from one image plane to another as the eye rotates or if the head moves, and are seen against the poorly contrasting globe and optic nerve. They dismiss proven methodology with the criticism that muscles were manually outlined on MR images (a reasonable method when borders are clear), rather than being delineated by some automatic technique, or as in their case, in an unspecified way. Image registration algorithms must be used with data such as theirs, in which precise location of image planes drastically alters image contents (see, eg, Lee et al, Figs 2 & 4), but has the side effect of reducing image resolution: the fuzzy indistinctness of Lee et al's images is not an artifact of the publication process. Some close-cropped coronal images are shown (Lee et al., 2007, Figure 3), but they do not seem to have been used in their analysis. It is difficult to accept findings about millimeter-sized muscle movements, when they could easily result from sub-millimeter variations in the positions of image planes relative to a muscle moving against the spheroidal globe. Having acquired images in axial planes, Lee et al must have data for the lateral rectus muscle, as well as the medial rectus, but they do not report it.

In what follows, however, for the sake of argument, we will accept Lee et al's results as presented and ask only whether such results have any bearing on the existence or function of EOM pulleys.

Models

Almost 20 years ago I performed the first MRI study of EOM paths as a function of gaze, a study which Lee et al have essentially repeated using modern equipment. A fundamental problem interpreting the data of both studies is that <u>plausible pulley and non-pulley models do not predict different EOM paths</u>. Thus, in my 1989 paper I could only suggest that a pulley theory might account as well as the traditional model for the muscle paths measured. Subsequent studies, quite different from those of (Miller, 1989) and (Lee et al., 2007), were necessary to distinguish the two types of model, and show that the traditional model was wrong.

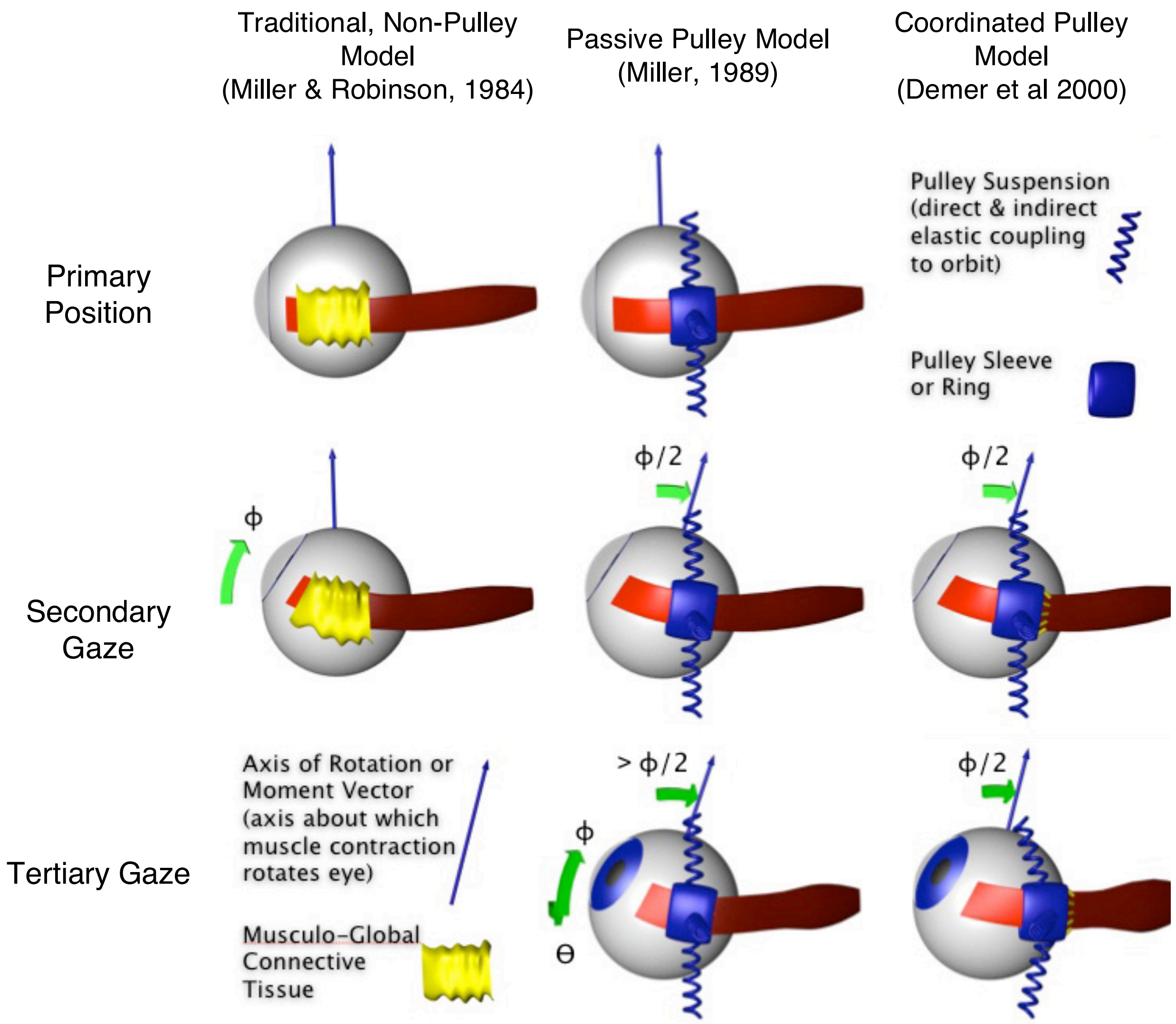

Figure 1: *Muscle paths are the same according to pulley and non-pulley models (compare panels in center row), but the mechanics are critically different. The blue sleeve is a schematic representation of the pulley sleeve, and the blue coils suggest the elastic coupling to the orbital wall (anatomically, both tissues are distributed and, of course, look quite different). All pulley models other than that of Lee et al (2007) have specified that transverse constraints on muscle paths are elastic (not rigid), and are distributed along the muscle's length (not applied at a point). The coordinated pulley model further specifies that EOMs translate their pulleys longitudinally (compare Tertiary Gaze for the passive and coordinated models).*

Non-pulley models have evolved from the "shortest path model" to what we call the "traditional model":

1. The <u>shortest path model</u> supposes EOMs to be constrained only at points of origin and insertion (Boeder, 1962, Krewson, 1950). This oldest model allows the muscle to as-

sume any “angle of departure” from its insertion, predicts catastrophic, uncontrollable muscle “flipping”, and is obviously incorrect (Robinson, 1975).

2. The permitted sideslip model is a modification of the shortest path model that reduces the instabilities of the shortest path model using a mathematical trick, a geometric “cosine” factor that is admittedly non-physiologic (Robinson, 1975).
3. The most viable non-pulley model is the traditional model of Miller and Robinson (1984), which treats the effects of muscle tension on sideslipping and tendon width on muscle action in physiologically plausible ways. It is generally considered to have rendered preceding non-pulley models obsolete.

Standing against these developments, the non-pulley model Lee et al choose to compare with their data is principally a “(restricted) shortest path model” that lies somewhere between models “1” and “2”, above.

Pulley models too have evolved: the original “passive” model (Miller, 1989) and the subsequent “strong differential” model (Demer, Oh & Poukens, 2000) have both yielded to the current, well-supported, “coordinated” model (Demer et al., 2000, Kono, Clark & Demer, 2002, Miller, 2007). In Figure 1 we schematize the original passive pulley model and the currently accepted coordinated pulley model.

The pulley model Lee et al favor for comparison with their data is none of these. Instead they evaluate a rigid, localized pulley model no one had previously proposed.

Which models would have made more reasonable choices? The “traditional model” (Miller & Robinson, 1984), is certainly the most realistic of the non-pulley models. It supposes that muscles are constrained by musculo-global connective tissue bands that function like distributed springs along the muscle’s arc of contact with the globe (Miller & Robinson, 1984). Imagine the forces at play in the left column of Figure 1 as the eye elevates. The anterior path of this horizontal muscle is shaped by elastic connective tissues that tend to hold it to its primary position arc of contact with the globe, and opposing forces related to muscle tension that tend to pull it towards the top of the globe, to where the muscle would follow the shortest path from insertion to origin. The path that results from the balance of these forces may allow the posterior muscle belly to move slightly upwards in the orbit as the eye elevates. Such orbit-relative movement of the muscle belly is called sideslip.

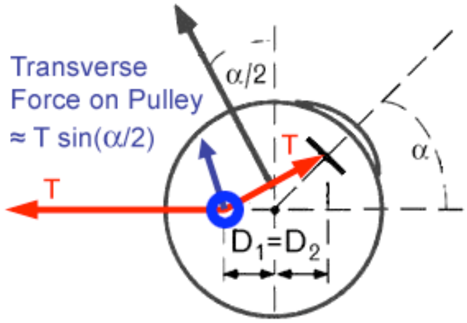

Figure 2: *The angle between anterior and posterior muscle segments determines the force that moves an elastically suspended pulley transversely. The pulley is shown as a small blue circle for diagrammatic purposes.*

Pulley Models, in contrast, suppose musculo-orbital coupling to be dominant, such as would exert transverse forces on the muscles and so determine their paths, while allowing them to contract and relax longitudinally (see center and right columns of Figure 1). The coordinated pulley model (right column) is currently the best supported, and differs from the passive pulley model (center column) in that its muscle origin-determining connective tissues are pulled longitudinally by the EOM passing through them (schematized by the small yellow lines connecting pulley sleeve and EOM in Figure 1). For the rectus muscles, the musculo-orbital coupling is thought to consist of a spatially distributed matrix of elastic tissues: the reflected sleeves of Tenon's capsule (pulley "sleeves") and associated intermuscular and musculo-orbital connective tissues (pulley "suspensions"), stiffened by smooth muscle, and probably supplemented by fascially compartmentalized orbital fat. Muscle paths in a pulley model therefore result from a balance of musculo-orbital elasticities against shortest path sideslipping forces. As with non-pulley models, if stabilizing elasticities are weak, muscle tensions are high, or geometry determines that a large component of muscle tension acts in an appropriate direction (Figure 2), the muscle sideslips towards its shortest path (upward in Figures 1 & 2).

Lee et al (2007) correctly distinguish pulley from non-pulley models when they explain (pg 4532, col 1) that the connective tissue constraints in non-pulley models would determine a muscle's functional insertion, whereas pulley constraints would determine its functional origin. But then, they propose that their muscle path findings both disprove the existence of pulleys, and support exactly the distributed, origin-determining, elastic constraints of the passive pulley model:

> "Vertical sideslips demonstrated in our study must have occurred under distributed resistance from the surrounding tissue, such as the orbital fat and septal meshwork of connective tissue (Lee et al., 2007, pg 4531, last paragraph)."

Lee et al correctly dismiss their localized, rigid pulley model, but then, as though anything called a "pulley" must be like the cartilaginous trochlea of the superior oblique muscle, suppose that they have disproved all pulley models, despite the fact that all but theirs suppose distributed, elastic pulleys. If rectus muscle pulleys were hard and compact, how could they possibly have escaped the notice of ancient anatomists (see discussion in Miller, Demer, Poukens, Pavlovski, Nguyen & Rossi, 2003)?

Predictions

Lee et al compare a version of Robinson's (1975) obsolete "permitted sideslip model" to a trochlea-like localized rigid pulley model. It is apparent from their Figure 1 that their differential predictions of medial rectus sideslip rely on a pulley model that constrains the muscle sharply, at the point labeled "presumed pulley location", and rigidly, allowing no muscle sideslip at all. A realistic, distributed, elastic pulley, would predict muscle paths that could not be distinguished from those in the left, "shortest path", column of their Figure 1. Conversely, paths drawn in the left column of their figure are completely dependent on details of the non-pulley model assumed. In terms of the traditional model (our Figure 1, left column), Lee et al's non-pulley predictions only follow if all of the musculo-global elasticities are bunched up near the insertion, and consequently act, unphysiologically, as an extremely stiff torsional spring. In contrast, non-pulley models that incorporated physiologically possible, distributed musculo-global coupling could be made to predict the paths in the "pulley model" column of their Figure 1. Thus, Lee et al's differential predictions for pulley and non-pulley models with the eye on the midline do not follow from reasonable pulley and non-pulley models, and are therefore unconvincing.

Next consider Lee et al's finding of greater medial rectus sideslip with the eye moving vertically in abduction compared to in adduction. Here, they propose two effects: an insertion movement effect that tends to increase, and a muscle tension effect that tends to decrease medial rectus sideslip in abduction. They then fabricate agreement between their findings and their non-pulley model by applying the first, sideslip increasing effect, only to the non-pulley model, although it actually applies to both, and the second, sideslip reducing effect, only to their pulley model, although it too applies to both.

The muscle tension effect is due to medial rectus muscle tension being lower in abduction than in adduction. Although they otherwise suppose pulleys to be rigid, Lee et al here predict reduced sideslip by allowing that sideslip is determined by the balance of muscle tension, which tends to increase it, against musculo-orbital elasticity, which tends to hold the muscle path fixed. However, they fail to apply the corresponding argument to their non-pulley model, although they otherwise assume sideslip in that model to be elastically restrained. Under both models, then, less muscle tension means less sideslip.

The insertion movement effect, is explained in terms of a claim about Listing's Law that is incorrect (Lee et al., 2007, pg 4527, pp 7). Listing's law determines a plane for each position shown in Lee et al's Figure 1, not an axis line; any destination position determines another plane, and fixed-axis eye movements are made about the linear intersection of two such planes (see Tweed & Vilis, 1990, pg 120, pp 3). To proceed nevertheless, we will take Lee et al to mean that the axes shown are the axes of rotation for their particular vertical movements (although they do not describe the coordinate system of their fixation targets or the vergence state of their subjects, both of which influence eye position). The essence of Lee et al's claim is then simply that the medial rectus wraps further around the globe in abduction than in adduction, which positions its insertion more anteriorly with respect to the axis of rotation, so that globe elevation, for example, moves the insertion higher. Thus, in abduction and elevation, the shortest path (see Robinson, 1975, Figure 4) passes nearer the top of the globe, directing the path of a muscle upwards, against the restraints of musculo-global connective tissues. Such paths are suggested in the left column of Lee et al's Figure 1. However, a similar effect occurs in pulley models, except that the elastically restrained movement is at the pulley rather than at the insertion (our Figure 2). Thus, under both models a higher muscle insertion means more sideslip. If Lee et al wish to avoid pulley movement by supposing that EOM pulleys are fixed and rigid, they should do the same for the angle of departure from the insertion of their non-pulley model, and then neither model would reflect the abduction-elevation effect in their data.

Conclusions

The imaging method used by Lee et al is unreliable, but even accepting their empirical claims as presented, none provide any evidence against EOM pulleys.

Although Lee et al's interpretations are unconvincing, their results themselves may be new. It does not seem that anyone has studied sideslip in tertiary gaze since the study of Miller (1989), who did not find an abduction-adduction difference in sideslip. This issue should probably be revisited using methodology that preserves the exquisite resolution possible with modern imaging equipment.

Jampolsky's group has opposed the notion of EOM pulleys (see also McClung, Allman, Dimitrova & Goldberg, 2006) that has evolved over the past 20 years based on broad evidence of many types (reviewed in Miller, 2007), including recently, the compelling findings of Ghasia & Angelaki (2005) and Klier & Angelaki (2006), which Lee et al misrepresent and dismiss. Here, they use unproven, artifact-vulnerable, poorly specified methodology, where suitable broadly accepted methodology exists, and report only selected data. Lee et al interpret their results with respect to an obsolete non-pulley model and a “straw man” pulley model, the later having trochlea-like, localized, rigid pulleys, unlike anything previously proposed. Even with these biases, Lee et al cannot explain their data without accepting the notion of distributed, elastic, origin-determining EOM pulleys, which they claim to have disproved.

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