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Review

Finite element modeling mesh quality, energy balance and validation methods: A review with recommendations associated with the modeling of bone tissue

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ABSTRACT

The use of finite element models as research tools in biomechanics and orthopedics has grown exponentially over the last 20 years. However, the attention to mesh quality, model validation and appropriate energy balance methods and the reporting of these metrics has not kept pace with the general use of finite element modeling. Therefore, the purpose of this review was to summarize the current state of finite element modeling validation practices from the literature in biomechanics and orthopedics and to present specific methods and criteria limits that can be used as guidelines to assess mesh quality, validate simulation results and address energy balance issues.

Of the finite element models reviewed from the literature, approximately 42% of them were not adequately validated, while 95% and 98% of the models did not assess the quality of the mesh or energy balance, respectively. A review of the methods that can be used to assess the quality of a mesh (*e.g.*, aspect ratios, angle idealization and element Jacobians), measure the balance of energies (*e.g.*, hour glass energy and mass scaling), and quantify the accuracy of the simulations (*e.g.*, validation metrics, corridors, statistical techniques) are presented.

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1. Introduction

Experimental testing on human participants and cadaveric specimens provides researchers in biomechanics and orthopedics with valuable insights into how the bones of the human body respond to loading and why they might become injured as a result of different loading scenarios. However, in many instances, experimental testing on humans and cadavers is not always feasible. For example, to ensure the safety of participants, in vivo testing must be limited to sub-maximal loads and non-invasive testing techniques. Furthermore, failure tests on cadaveric specimens are inherently destructive and can become costly (Rogge et al., 2002). In comparison, finite element models provide a feasible alternative for predicting the response of bone under a variety of loading conditions and have become a popular and powerful tool among biomechanics and orthopedics researchers over the last 20 to 30 years (Anderson et al., 2007; Erdemir et al., 2012). As reported by Erdemir et al. (2012), there has been a 6000% increase in the number of finite element modeling papers published between the years 1980 and 2009. However, the attention to mesh quality, model validation and appropriate energy balance methods during this period has not adequately kept pace with the general use of finite element modeling approaches. This is particularly true in the orthopedics and biomechanics literature, where the use of these metrics lags behind that found in the mainstream materials engineering journals (Lund et al., 2012).

Recently, a number of papers that address the issues of model verification, validation (Anderson et al., 2007; Cristofolini et al., 2010; Henninger et al., 2010; Lund et al., 2012) and presentation of modeling results (Erdemir et al., 2012) have been published and provide the reader with an excellent perspective regarding these issues. While these papers present essential definitions and general modeling guidelines, they stop short of recommending specific validation procedures. Rather, they focus only on the validation of simulation results, and do not address the areas of mesh quality and energy balance assessments. As the primary focus of biomechanics and orthopedics finite element models is generally some aspect of human health (*e.g.*, implant failure, injury risk assessment, fracture fixation efficacy), it is imperative that the model represent the physical system with as much accuracy as possible. Therefore, sources of error (mesh quality, energy balance, simulation accuracy) must be considered, quantified and minimized to prevent erroneous findings. Furthermore, the standardization of model assessment criteria would allow for better comparison of models between research groups.

Therefore, the purpose of this paper is threefold: (i) to conduct a literature review to highlight the current state of finite element modeling validation practices in orthopedics and biomechanics; (ii) to present specific methods that should be used to assess mesh quality, validate simulation results and address energy balance issues; and (iii) to recommend criteria limits to be used in assessing the accuracy of a finite element model. While the findings of the literature review and the associated recommendations are applicable to most biomechanical and orthopedic finite element models, the focus of this work is on models representing bone tissue.

2. Review of the literature

2.1. Methods

In response to the work presented by Anderson et al. (2007), Erdemir et al. (2012), and Henninger et al. (2010), a review of the biomechanics and orthopedics literature was conducted to determine the specific methods that are currently being used by researchers to assess their finite element models in terms of mesh quality, energy balance and simulation accuracy. The terms “biomechanics” or “orthopedics” were used in conjunction with “finite element modeling” in varying combinations with “validation”, “mesh quality”, and “energy” when conducting the literature search. Articles were chosen which represented a variety of anatomical locations from a number of different journals and which represented both basic and applied work. However, given the volume of articles dedicated to orthopedics and biomechanics finite element modeling, the review was limited to models that primarily involved bone tissue. Each article was carefully read by one of the authors (T.A.B.) and categorized according to the mesh quality assessment (full=more than two metrics were used; minimal=one metric used; absent=no metrics used), energy balance assessment (full=energy balance assessed; absent=energy balance was not assessed) and the validation of simulation results against experimental work (full=more than two validation methods used; minimal=one validation method was used; absent=no validation methods were used; Note: examples of validation methods can be found in Section 4.0). The articles that included full or minimal validation were read through a second time to classify the specific validation methods as follows: data corridors, statistical techniques (*e.g.*, root mean square errors (RMSE), correlation analysis, percentage errors), qualitative comparisons (*e.g.*, comparing peak values with no statistical basis), and application assessment (*e.g.*, fracture pattern comparison). Finally, to determine the extent to which authors are assessing the size of their meshes, all articles were read a third time to determine whether a mesh convergence/sensitivity analysis was conducted. As part of the final assessment, articles that reported conducting a mesh sensitivity analysis were separated from those that did not. Those that did report performing this analysis were further characterized as either providing sufficient details (*e.g.*, number of mesh sizes tested, output variable) or providing no details (*i.e.*, simply stated that this analysis was completed). Articles that did not report the use of a mesh sensitivity analysis were re-examined to determine if a mesh quality assessment (*e.g.*, element Jacobians, aspect ratios, and orthogonality) was conducted as a substitute for a mesh sensitivity analysis and were categorized appropriately.

2.2. Results

Overall, 39% of the articles reviewed presented no validation data, while 95% did not evaluate the quality of the finite element mesh, and more than 98% did not discuss the energy balance of

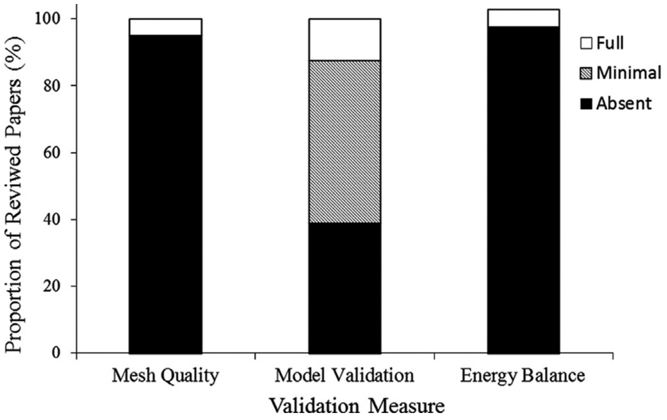

Fig. 1. A summary of the mesh quality, model validation and energy balance methods from previous finite element modeling studies ($N=80$). “Full” indicates that two or more methods were reported; “minimal” indicates that one method was reported; “absent” indicates that no method was reported.

the model (Fig. 1 and Table 1) despite 43% of the reviewed papers presenting dynamic simulation results. Of the papers that included any type of validation procedure, 47% included only a qualitative assessment and 57% validated their model with respect to specimen-specific, original experimental data (*i.e.*, 43% made comparisons to previously reported data in the literature) (Table 1).

Finally, only 33% of all the articles reviewed chose the size of their mesh based on the results of a mesh sensitivity analysis, and of these, only 57% included the details of their analysis (Fig. 2). Of the 67% that did not report performing a mesh sensitivity analysis, almost 90% also did not include any type of mesh quality assessment (Fig. 2).

3. Mesh quality assessment

The accuracy and efficiency of finite element simulations (*i.e.*, the solution to the partial differential equations) is highly predisposed to the quality of the finite element mesh (Knupp, 2007). Knupp (2007), defines the elusive term “mesh quality” as “the characteristics of a mesh that permit a particular numerical partial differential equation simulation to be efficiently performed with fidelity to the underlying physics and with the accuracy required for the problem” (p. 2). Given the complex geometry associated with human bones, elements with large distortions can occur and are potential sources of low accuracy or solution instability (Valle and Ray, 2005). Two primary issues arise when considering the quality of a finite element mesh. The first is the shape of the elements that are chosen to discretize the geometry. When generating a mesh for biological structures, especially bone, there are generally two element shapes that are used: tetrahedral and hexahedral (four node and eight node versions, respectively). In general, hexahedral elements are considered to be more accurate and efficient than their tetrahedral counterparts especially when performing dynamic simulations. Also a consideration is the coarseness of the mesh—in other words, the number of elements from which the mesh is composed. It is generally agreed upon in the literature that an optimal mesh density exists that provides the most accurate solution with the smallest possible number of elements, as commonly determined through the use of a sensitivity analysis. However, as reported previously, this type of analysis is not as common as is perceived, and when it is

Table 1
Summary of the mesh quality, energy balance and model validation methods reported in the studies reviewed from the literature.

Author (year)	Anatomical location	Mesh quality assessment	Model validation	Comparison to experimental	Energy balance	Type of simulation
Gatti et al. (2010)	Glenoid	No	Yes[a]	Yes	Yes	Quasi-Static
Moore et al. (2010)	Glenoid	No	Yes[b]	Yes	No	Quasi-Static
Hopkins et al. (2006)	Glenoid	No	Yes[c]	Yes	No	Dynamic
Debski et al. (2005)	Glenoid	No	No		No	Static
Gupta et al. (2004)	Glenoid	No	Yes[b]	Yes	No	Static
Buchler et al. (2002)	Glenoid	No	No		No	Static
Merz et al. (1997)	Elbow	No	No		No	Static
Clavert et al. (2006)	Humerus	No	No		No	Static
Chennimalai Kumar et al. (2010)	Rat Ulna	No	Yes[a]	No	No	Quasi-Static
Kotha et al. (2004)	Rat Ulna	No	No		No	Dynamic
Lu et al. (2012)	Mouse Forearm	No	Yes	Yes	No	Static
Taylor et al. (2003)	Turkey Ulna	No	Yes[c]	No	No	Dynamic
Chamoret et al. (2011)	Hand	No	No		No	Dynamic
Gislason et al. (2009)	Wrist	Yes	No		No	Static
Guo et al. (2009)	Wrist	No	Yes[c]	No	No	Static
Ledoux et al. (2001)	Wrist	No	No		No	Static
Anderson et al. (2008)	Radius	No	Yes[c,d]	No	No	Static
Boutroy et al. (2008)	Radius	No	No		No	Static
Buchanan and Ural (2010)	Radius	No	No		No	Static
Carrigan et al. (2003)	Radius	No	No		No	Static
Edwards and Troy (2012)	Radius	No	Yes[b]	Yes	Yes	Quasi-Static
Macneil and Boyd (2008)	Radius	No	Yes[b]	Yes	No	Static
Pistoia et al. (2002)	Radius	No	Yes[b]	Yes	No	Static
Pistoia et al. (2003)	Radius	No	Yes[b]	Yes	No	Static
Rogge et al. (2002)	Radius	No	Yes[b]	Yes	No	Static
Troy and Grabiner (2007)	Radius	No	No		No	Dynamic
Ulrich et al. (1999)	Radius	No	No		No	Static
Zhong et al. (2009)	Spine	No	Yes[c]	Yes	No	Static
Clausen et al. (1997)	Spine	No	Yes[c]	No	No	Static
Tadepalli et al. (2011)	Spine	No	No		No	Static
MacNeil et al. (2012)	Spine	No	Yes[b]	No	No	Static
Hussain et al. (2010)	Spine	No	Yes[c]	No	No	Dynamic
Tang and Meng (2011)	Spine	No	Yes[c]	No	No	Static
Chosa et al. (2004)	Spine	No	Yes[c]	No	No	Static
Skalli et al. (1993)	Spine	No	Yes[c]	No	No	Static
Womack et al. (2011)	Spine	No	Yes[a]	Yes	No	Dynamic
Galbusera et al. (2011)	Spine	No	No		No	Static
Massey et al. (2012)	Spine	No	No		No	Dynamic
Guo et al. (2011)	Spine	No	No		No	N/A
Panzer et al. (2011)	Spine	No	Yes[c]	No	No	Dynamic
Noailly et al. (2012)	Spine	No	No		No	Dynamic
Schmidt et al. (2007)	Spine	No	Yes[c]	No	No	Dynamic
Tang and Rebholz (2011)	Spine	No	Yes[c]	No	No	Dynamic
Eichenseer et al. (2011)	Spine	No	Yes[c]	No	No	Static
Ozan et al. (2010)	Femur	No	Yes[b]	Yes	No	Static
Yosibash et al. (2010)	Femur	No	No		No	Static
Anderson et al. (2008)	Femur	No	Yes[c,d]	Yes	No	Dynamic

Table 1 (*continued*)

Author (year)	Anatomical location	Mesh quality assessment	Model validation	Comparison to experimental	Energy balance	Type of simulation
Hamed et al. (2012)	Femur	No	No		No	Static
Budhabhatti et al. (2007)	Foot	No	Yes[c]	Yes	No	Static
Cheung et al. (2005)	Foot	No	Yes[d]	Yes	No	Static
Spyrou and Aravas (2011)	Foot/Leg	No	No		No	Quasi-Static
Qiu et al. (2011)	Foot	No	Yes[d]	No	No	Dynamic
Hsu et al. (2008)	Foot	No	Yes[d]	Yes	No	Static
Chen et al. (2010)	Foot	No	Yes[d]	Yes	No	Static
Jamshidi et al. (2010)	Foot	No	Yes[c]	No	No	Dynamic
Gu et al. (2010)	Foot	No	Yes[d]	Yes	No	Dynamic
Cheng et al. (2008)	Foot	No	Yes[c]	No	No	Static
Yu et al. (2008)	Foot	No	Yes[d]	Yes	No	Static
Goske et al. (2006)	Foot	No	Yes[d]	Yes	No	Static
Wu et al. (2007)	Foot	No	No		No	Static
Sun et al. (2012)	Foot	No	Yes[d]	Yes	No	Static
Torcasio et al. (2012)	Mouse Tibia	No	Yes[b]	Yes	No	Static
Pang et al. (2012)	Tibia	No	Yes[c,d]	Yes	No	Static
Quenneville and Dunning (2011)	Tibia	Yes	Yes[b,d]	Yes	No	Dynamic
Untaroiu et al. (2005)	Tibia	Yes	Yes[c]	No	No	Dynamic
Panzer et al. (2012)	Head	No	No		No	Dynamic
Li et al. (2011)	Head	Yes	Yes[c]	Yes	No	Dynamic
Yan and Pangestu (2011)	Head	No	Yes[c]	No	No	Dynamic
Savoldelli et al. (2012)	Jaw	No	No		No	Dynamic
de Almeida et al. (2011)	Jaw	No	No		No	Static
Jing et al. (2012)	Pelvis	No	No		No	Dynamic
Kunze et al. (2012)	Pelvis	No	No		No	Static
Shim et al. (2012)	Pelvis	No	Yes[c]	Yes	No	Dynamic
Krywonos et al. (2010)	Pelvis	No	Yes[c,d]	Yes	No	Dynamic
Kim et al. (2009)	Pelvis	Yes	Yes[b]	No	No	Dynamic
Shim et al. (2008)	Pelvis	No	Yes[b]	Yes	No	Dynamic
Ellis et al. (2007)	Ligament	No	No		No	Dynamic
Un and Spilker (2006)	Soft tissue	No	No		No	Dynamic
Saidpour (2006)	Fixation plate	No	No		No	Static
Yan and Pangestu (2011)	Pedicle screw	No	Yes[c]	No	No	Dynamic

[a] Corridors.
[b] Statistical Techniques (*i.e.*, RMSE, Correlation analysis, Percentage errors).
[c] Qualitative Comparisons (*e.g.*, comparing peak values with no statistical basis).
[d] Application assessment (*e.g.*, fracture pattern comparison).

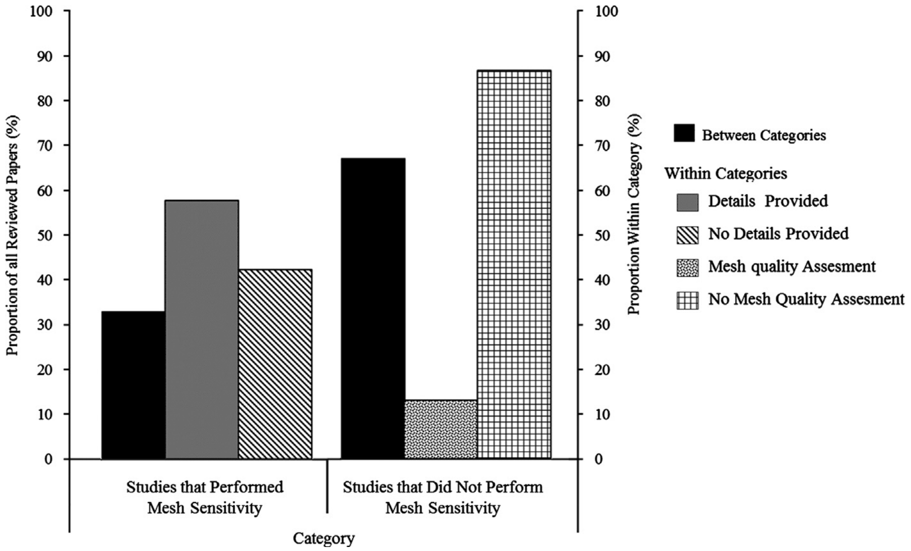

Fig. 2. A comparison of the reviewed articles that reported performing a mesh sensitivity analysis to determine the optimal mesh size. The black bars are read off the left axis and represent an overall comparison of studies that did and did not include a mesh sensitivity analysis. The remaining bars are read off the right axis and are comparisons within each category.

conducted, authors often omit important details. Furthermore, a mesh sensitivity analysis is highly dependent on whether a global (*e.g.*, force) or local (*e.g.*, strain) measure is used to assess the coarseness of the mesh (Erdemir et al., 2012). Even when the mesh is deemed to be valid using this approach, the quality is often still very poor (Knupp, 2000). Finally, when complex meshes are being developed that involve manual adjustments, it is not always feasible to perform a mesh sensitivity study (Gatti et al., 2010). An alternative to a mesh sensitivity analysis, the focus of this section, is to report on the overall quality of the mesh, which is influenced by the shape of each element, partially dependent on the size of the mesh, and completely independent of the output measures. The metrics used to assess mesh quality are based mostly on element geometry (Knupp, 2001), and are concerned with ensuring that an element has a symmetric shape (aspect ratios), internal angles that are idealized, and possess a positive volume (element Jacobians) (Knupp, 2001).

3.1. Mesh shape

While automatic tetrahedral meshing can be much less labor intensive than the manual meshing required of hexahedral elements, tetrahedral meshes are generally assumed to produce less accurate results than those retrieved from a hexahedral mesh (Tadepalli et al. (2011); Benzley et al., 1995; Raut, 2012; Shim et al., 2012). The inaccuracy associated with the implementation of tetrahedral elements is attributed to their high stiffness, incompressibility and predisposition to mesh locking. Benzley et al. (1995) reported that the stiffness matrix eigenvalues were greater for a mesh composed of tetrahedral elements compared to an all hexahedral mesh, leading to significantly greater errors in the calculated displacement and stress results for static bending, torsion and dynamic loading; a result consistent with that of Raut (2012). Similarly, Tadepalli et al. (2011) concluded that tetrahedral elements should only be used under frictionless conditions or when the material incompressibility conditions can be relaxed. While Ramos and Simoes (2006) and Cifuentes and Kalbag (1992) found that the simulation accuracy was comparable between tetrahedral and hexahedral meshes, hexahedral meshes were superior in terms of stability and their sensitivity to changes in mesh refinement. These findings suggest that, when possible, biological structures should be meshed with hexahedral elements, particularly in dynamic modeling scenarios.

3.2. Aspect ratios

Aspect ratios (AR) are relatively simple measurements that quantify the shape of each element in the mesh. For hexahedral elements, the AR is calculated by dividing the longest edge or diagonal of an element by the shortest. Tetrahedral aspect ratios are measured as the ratio of the longest edge length divided by the minimum altitude of the smallest side (Fig. 3). As suggested by Rice (1985), finite elements require, at the very least, moderate aspect ratios to optimize the computational accuracy and the condition of the problem. The most accurate solutions are achieved when the ARs are close to unity (~1); however, many bones contain sections of high curvature and areas of thinness (<1 mm in some sections of cortical bone) that tend to result in unavoidably thin elements (Fellipa, 2012). As a result, it has been suggested that ARs for hexahedral elements be categorized as follows: $1<\mathrm{AR}<3$ are acceptable; $3<\mathrm{AR}<10$ be treated with caution; $\mathrm{AR}>10$ be treated with alarm (Fellipa, 2012). These categories are supported by a recent report where it was noted that 45%, 23% and 19% of finite element modeling practitioners prefer aspect ratios below 3, 5 and 2, respectively (Ray et al., 2008). However, much less has been reported for tetrahedral elements. Tsukerman and Plaks (1998) found that aspect ratios between 1 and 4 produced the smallest errors when compared to reasonably shaped tetrahedral elements. While it is important to consider the magnitudes of the ARs, the total number of alarming elements and their locations are also important factors when assessing the quality of the mesh. Therefore, it is recommended that the percentage of element ARs greater than 3 remain below 5% and that authors report the general locations of the offending elements. For example, although Quenneville and Dunning (2011) reported element aspect ratios between 1.1 and 23.1, those that exceeded 3 only represented a small proportion of all elements and were situated in locations away from the area of highest interest (*i.e.*, the area of interest was the distal aspect of the tibia and the elements with poor aspect ratios were primarily located in the tibial diaphysis).

3.3. Angle idealization

Elements whose interior angles deviate too far from an ideal angle (90° for hexahedral and 60° for tetrahedral elements) can produce unrealistic deformation responses. Consequently, a second mesh quality assessment measure that the authors recommend be included in all papers is the element's Angle Idealization. This measure pertains to the three angles at each of the vertices in

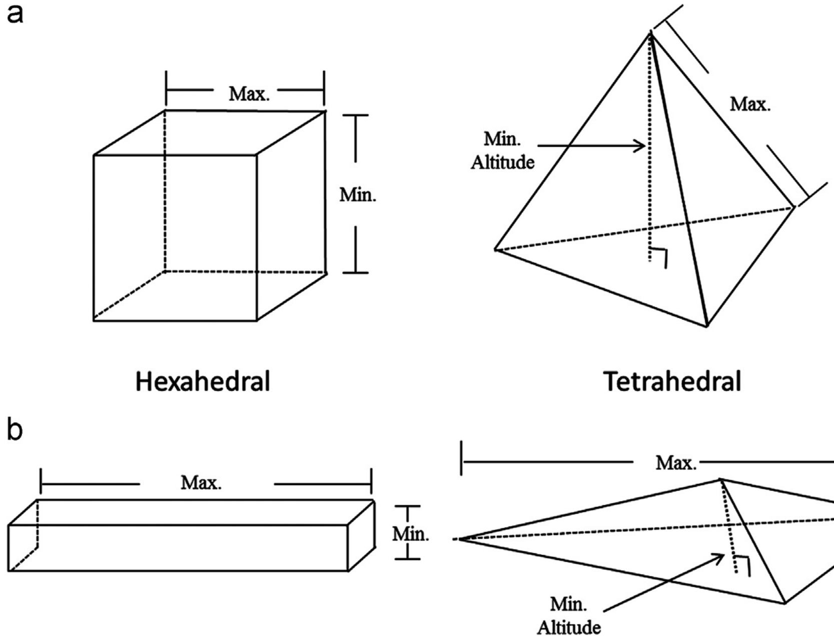

Fig. 3. Illustration showing the general calculation of element aspect ratios. An element with an aspect ratio of approximately one (a) is compared to an element with an aspect ratio of 14 (b).

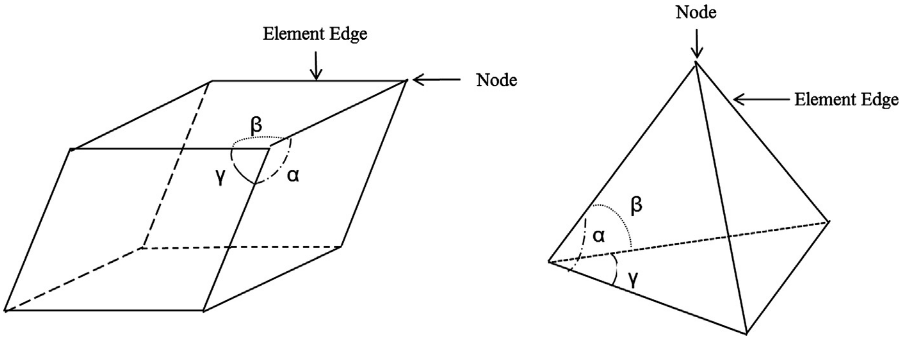

Fig. 4. Illustration showing an example of three element angles that are calculated at each of the eight nodes, resulting in 24 total angle measures.

hexahedral and tetrahedral elements, which are created by the intersection of each pair of edges (24 angle measurements per hexahedral element and 12 per tetrahedral) (Fig. 4). For hexahedral elements, Liu et al. (2007) suggest that the interior angle deviations should not theoretically exceed 90° (absolute angle of 180°), but in practice, should be within 30° of deviation (absolute angle of 120°). Holmes (1994) suggested a similarly conservative criteria noting that mesh quality is primarily related to the number of elements whose interior angles deviate by more than 45° from perpendicular. However, similar to the problem with aspect ratios, modeling bone anatomies with areas of high curvatures will undoubtedly require some misshapen elements that exceed 45° of distortion. Therefore, it is suggested that an element's angle idealization may be considered satisfactory providing that less than 5% of internal angle deviations exceed 70° (absolute angles $<160^\circ$) (El-Hamalawi, 2000; Quenneville and Dunning, 2011; Ray et al., 2008). In terms of tetrahedral elements, there is very little published data on the size of acceptable dihedral angles, but it appears that angles between 30° and 150° will produce a relatively accurate mesh (Klinger and Shewchuck, 2007.)

3.4. Element Jacobians

Element Jacobians provide a measure of volume distortion from an ideally shaped element and represent the determinant of the Jacobian matrix (which itself contains information regarding the volume, shape and orientation of the element) (Knupp, 2001) (Fig. 5). Specifically, the Jacobian matrix defines the mapping of element vertices from the ideally-shaped element to the actual element (Knupp, 2000). The Jacobian of an extremely distorted (*i.e.*, inverted) element is negative and will prevent an analysis from continuing (Zhang et al., 2010; Knupp, 2007). The literature pertaining to element Jacobians suggests that the assessment of tetrahedral and hexahedral elements is the same, and can be described by the following criteria: (i) that they be positive in value (Fellipa, 2012; Knupp, 2007); (ii) preferably greater than 0.2 in magnitude (Quenneville and Dunning, 2011; Untaroiu et al., 2005; Li et al., 2011); and (iii) less than 5% of all Jacobians should fall below a magnitude of 0.7 (Ray et al., 2008).

As all of these mesh quality measures can significantly affect the solution of the finite element analysis (despite a successful mesh sensitivity analysis), it is recommended that authors perform all of these tests on their mesh and report the findings in the following way:

- The type of elements that were chosen to discretize the geometry and the result of a sensitivity analysis, if one was conducted (Erdemir et al., 2012), should be reported.
- The type of mesh quality metrics that were used to assess the mesh should be explicitly stated, as well as the criteria that were used to determine whether the mesh was acceptable.
- The results of the mesh quality assessment should be presented by first indicating if all of the elements in the mesh passed the analysis, followed by the percentage of elements (with respect to the total number of elements in the mesh) that did not meet the assessment criteria. A statement regarding the location of the failed elements should also be included.

4. Model validation and energy assessment

The confidence in a model to accurately predict real world phenomena depends on a critical evaluation of the model's results against experimental data (Oberkampf and Trucano, 2002; Rebba et al., 2006) in the form of numerical validation. While validation is especially important when the goal of the model is a clinical application (Viceconti et al., 2005; Cristofolini et al., 2010), the methods used to validate past finite element models have varied greatly (Anderson et al., 2007).

It is the opinion of the authors of the current review that validating a model should involve an analysis of the energies associated with the model (especially when evaluating the results of dynamic simulations), as well as the engagement of multiple quantitative techniques that compare the model outputs to experimental findings. This section will summarize the measures of energy balance and present a number of commonly used validation techniques.

4.1. Energy balance

Model assurance verification (Ray et al., 2008) involves ensuring that numerical results adhere to the basic physical laws, namely the conservation of energy. At a minimum, the model's global energy must be checked for balance to make certain that there are no major inconsistencies in the energy of the system and should be assessed for both static and dynamic loading conditions. This can be achieved by keeping the sum of internal, kinetic, sliding, hourglass, system damping, and rigid wall energies (Schinkel-Ivy et al., 2012) within an acceptable range (5%) of the total global energy (Ray et al., 2008).

There are special cases of model energies that require further discussion, especially related to dynamic modeling applications. In some finite element simulations (*e.g.*, when implementing constant stress element formulations), a phenomenon known as "hourglassing" can occur, where a hexahedral element undergoes a deformation in the absence of strain (Note: the inherent stiffness of a tetrahedral element prevents it from being prone to hourglassing). While this is especially true for dynamic, high deformation simulations, the effects of hourglassing should be monitored in all situations as it can lead to inaccurate results and, in severe cases, negative volume elements. While hourglass control can be implemented, in which a small elastic stiffness is added (thus generating energy and allowing the elements to resist hourglassing),

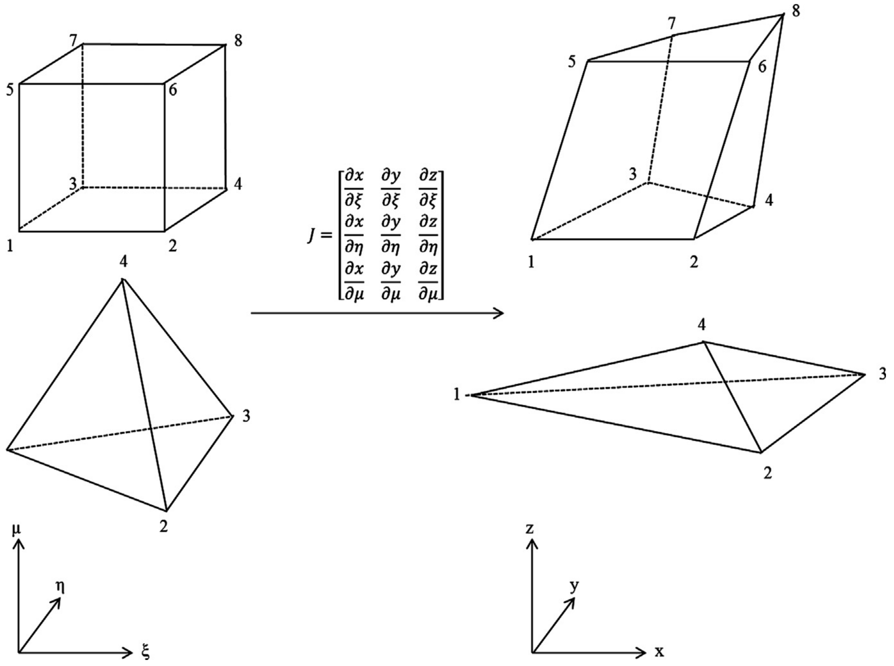

Fig. 5. Illustration representing the calculation of the elemental Jacobians where the Jacobian = det (**J**), such that J is the Jacobian matrix representing the transformation from an ideal unit cube to the hexahedral element.

inaccuracies can result from the addition of too much hourglass energy. When hourglass control is implemented, a specific analysis of the hourglass energy should be conducted to ensure that it did not contribute more than 10% of the total energy (LSTC, 1998; Brewer, 2001; Cheng et al., 2001; LSTC, 2007).

Often it is necessary or desirable to add mass to the system via mass scaling (*e.g.*, to decrease long simulation times), but it is important that the added mass does not alter the physics of the simulation. For example, Langseth et al. (1999) found that the force/deformation response of a model was affected by approximately 20% when 10 times mass scaling was implemented. Akarca et al. (2006) and Langseth et al. (1999) suggest a simple *a priori* comparison of simulations with and without mass scaling to confirm that the kinetic energy is insignificant compared to the strain energy absorbed by the model (Langseth et al., 1999). It is suggested, therefore, that the kinetic energy is less than 5% of the strain energy to confirm that appropriate mass scaling has been implemented (Prior, 1994).

The above energy assessments may not be required for all models, given that these issues are not relevant to all element (tetrahedral vs. hexahedral) or simulation (static vs. dynamic) types. However, it is important to perform an energy balance assessment and present the results if it is relevant to the model using the following recommendations:

- For both static and dynamic analyses an energy balance assessment should be presented.
- It is essential that the type of hourglass control that was implemented be stated and the contribution that hourglass energy makes to the total energy be presented.
- If mass scaling was enforced, the percentage of mass added to the system needs to be reported and a statement as to whether this was determined *a priori* should be included.

4.2. Model/experimental comparison

Validation procedures are used to analyze and quantify the degree of agreement between the numerical results and experimental findings, or as suggested by the ASME V & V10-2006 document, “the process of determining the degree to which a model is an accurate representation of the real world from the perspective of the intended use of the model” (Schwer, 2007, p. 248). The *intended use* of the model is an important component of this definition. For example, if the intended application of the model is to predict the fracture forces of bone in response to impact loading, then the forces calculated from the model should be compared to experimental forces produced from a similar impact loading protocol (Ray et al., 2008; Wheeldon et al., 2006). The quantitative method chosen to perform these comparisons also deserves important consideration and will be the focus of the remainder of this section.

The first method of validation described here, initially proposed by Oberkampf and Trucano (2002), is referred to as the validation metric (VM) and is computed using (Eq. 1):

$$VM = 1 - \frac{1}{N}\sum_{n=0}^{N} \tan h \left| \frac{y(t_n) - Y(t_n)}{Y(t_n)} \right| \tag{1}$$

where, *VM* is the validation metric, *N* represents the total number of samples, tan*h* is the hyperbolic tangent trigonometric function, $y(t_n)$ is the numerical measurement of the dependent variable at time *t*, and $Y(t_n)$ is the experimental measurement of the dependent variable at time *t*. The validation metric produces a value of 1 when there is perfect agreement between experimental and numerical results, and exponentially approaches 0 as the differences increase (Jin et al., 2010) (Fig. 6). The major advantages of this metric are that it measures the agreement between experimental and numerical results over the entire time-course of the signal, as opposed to a single point in time (*i.e.*, most often a single peak value); it guarantees that positive and negative errors cannot cancel each other out; and it provides a normalization of the computational and experimental differences (Oberkampf and Trucano, 2002). However, it should be noted that this validation metric is extremely sensitive to time shifts between the computation and experimental time signals, as well as the time duration over which the metric is computed. For example, the VM decreases from 1 (perfectly aligned signals) to 0.4 when the signals are shifted by only 2 ms (Fig. 7a). While not as drastic, the VM differs by approximately 36% and 18% when the length of time

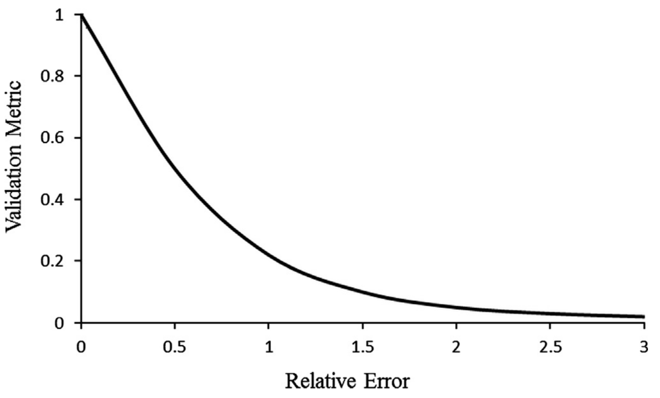

Fig. 6. The validation metric as a function of the relative error between the model and experimental data (Adapted from Oberkampf and Trucano, 2002; Jin et al., 2010).

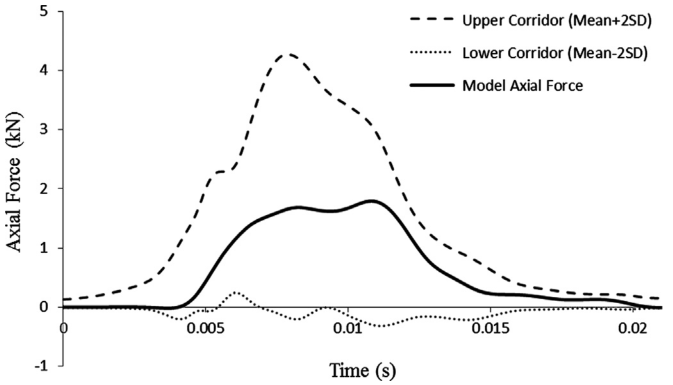

Fig. 8. An example of the model axial force signal (Burkhart et al., 2012) in comparison to the un-scaled force corridors (mean ± 2SD).

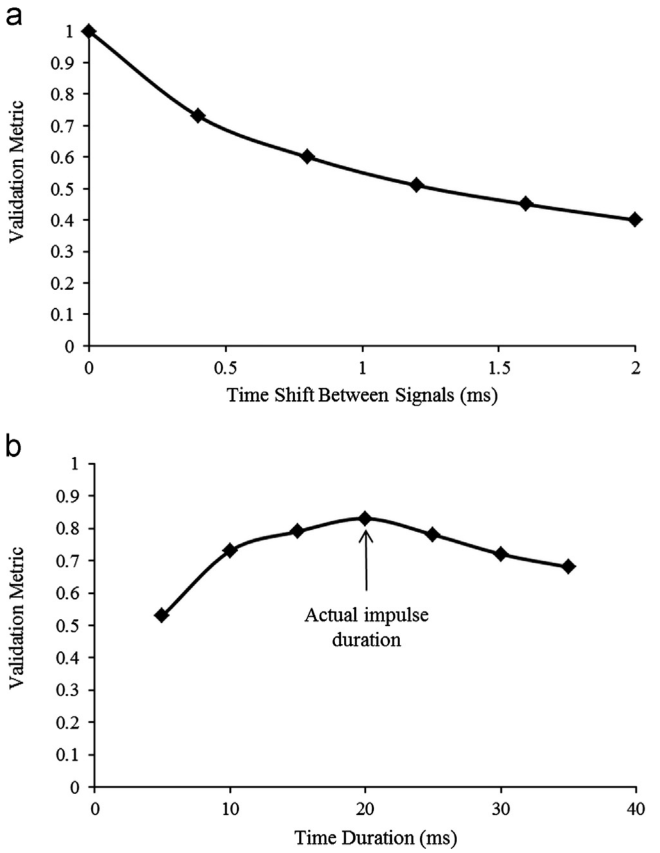

Fig. 7. Sensitivity of the validation metric to signal shifts (a) and the duration over which the validation metric is calculated (b).

over which the metric is calculated is 1.5 times less and greater than the full length of the signal (20 ms as measured from the onset and cessation of the impulse), respectively (Fig. 7b).

Data corridors or ensemble averages are another popular model validation method that produce data boundaries based on the mean and standard deviation of a dataset, and to some extent, describes the level of generalizability of the numerical results (Fig. 8) (Bir et al., 2004; Craig et al., 2008). While a relatively simple procedure, there are a few systematic phases that should be adhered to when generating the most accurate corridors. Depending on the purpose of the model and the variability of the experimental specimens, it may be necessary to scale the data according to mass (*e.g.*, application to a small female). Data scaling allows for the normalization of each specimen's with respect to an individual of a pre-determined size (*e.g.*, normalizing an impact force to the mass of a 5th percentile female). The process of data scaling will not be presented herein, but a thorough description of the procedure can be found in Eppinger et al. (1984), Mertz (1984), Yoganandan and Pintar (2005) and Masson et al. (2005). Once the data have been scaled, it is necessary to ensure that time varying signals are properly aligned, which can be accomplished one of two ways. First, the signals can be aligned according to signal onset (most commonly a force impulse). A second, more elaborate technique, involves iteratively aligning the signals to a pre-determined characteristic signal with the goal of minimizing the cumulative variance (Masson et al., 2005; Yoganandan and Pintar, 2005). Following signal alignment, the upper and lower boundaries of the corridors are calculated by adding and subtracting one or two standard deviations from the mean, respectively. The corridors can now serve as a validation method by ensuring the numerical results fall within the calculated boundaries.

While often overlooked, the level of agreement between model and experimental data can also be handled with statistical techniques, with two of the more common methods being an error assessment and correlation analyses. Error assessments are relatively simple methods that quantify the differences between some value (often the peak value of a signal) measured experimentally and by the model. Two commonly used techniques are percentage errors (Moore et al., 2010; Kim et al., 2009) and root mean squared errors (Torcasio et al., 2012). Correlation analyses are often presented as the Pearson correlation coefficient and provide a measure of the relationship between the experimental and model data. However, it has been noted that while correlation analysis provides a measure of the strength of the relationship, they are not necessarily good measures of agreement (Bland and Altman, 1986; Chan, 2003). Bland and Altman (1986) and Chan (2003) argue that a change in scale will affect the agreement but not the correlation, and variables that have weak agreement often show strong correlation. For example, strains associated with a particular location in a model can be compared to direct measures of strain from an experimental strain gauge (*e.g.*, Burkhart, 2012). It would be logical therefore, that the strains measured by these two different methods would be significantly related, despite the possibility of being in poor agreement. An alternate, more robust measure of agreement is the difference plot, otherwise known as a Bland–Altman plot (Bland and Altman, 1986; Chan, 2003; Myles and Cui, 2007; Motulsky, 2003; Austman et al., 2008). A Bland–Altman plot is constructed by first computing the differences between the experimental and model data points and plotting these against the mean of the same paired values, with limits of agreement determined based on the mean ± 1.96 SD. Finally, the precision of agreement is assessed by calculating the standard errors of the bias (Eq. 2) and upper and lower limits of agreement (Eq. 3) and subsequently constructing 95% confidence intervals (Eqs. 4–6) (Bland and Altman, 1986)

$$\mathrm{SE_{bias}} = \sqrt{\frac{S^2}{n}} \tag{2}$$

$$\mathrm{SE_{limits}} = \sqrt{\frac{3S^2}{n}} \tag{3}$$

$$\text{bias}-(t_{0.05} \times \mathrm{SE_{bias}}) \text{ to bias} + (t_{0.05} \times \mathrm{SE_{bias}}) \tag{4}$$

$$\text{upper limit}-(t_{0.05} \times \mathrm{SE_{upper\ limit}}) \text{ to upper limit} + (t_{0.05} \times \mathrm{SE_{upper\ limit}}) \tag{5}$$

$$\text{lower limit}-(t_{0.05} \times \mathrm{SE_{lower\ limit}}) \text{ to lower limit} + (t_{0.05} \times \mathrm{SE_{lower\ limit}}) \tag{6}$$

Where, SE is the respective standard error, S is the standard deviation and n is the number of samples. Based on these measures, three criteria must be considered: (i) the significance of the bias, which should be addressed in terms of the model's application, and is therefore not a statistical question but a clinical one (Motulsky, 2003; Chan, 2003); (ii) 95% of the values should fall within the limits of agreement (Bland and Altman, 1986), and (iii) the standard errors of the bias and limits of agreement themselves should be small (Bland and Altman, 1986; Chan, 2003).

Finally, model validation should not be considered complete unless the issue of *intended use* is addressed, which should include some comparison that takes into consideration the full application of the model. For example, finite element models of the foot were developed to measure the pressure distribution between the foot and supporting surface (Qiu et al., 2011; Hsu et al., 2008; Chen et al., 2010) and therefore, the pattern of pressure through the foot, predicted by the model, was directly compared to experimental pressures determined by an in-shoe pressure sensing system. Similarly, models developed to predict bone fracture should include a comparison of the location and intensity of fracture within the model to that found experimentally (Quenneville and Dunning, 2011).

With respect to model validation the following should be reported:

- The specific methods that are used to validate the model against experimental results, and from where the experimental data were derived (*i.e.*, previously published data vs. experimental data related to the specimen(s)/participant(s) used to create the model).
- If the validation metric is being used, details regarding the validation period (start and end points of validation), and how the experimental and numerical signals were initially aligned, should be included.
- Specific details related to all statistical methods used need to be reported, similar to the statistical reporting required for an experimental investigation.
- At least one of the validation methods should be a representation of the *intended use* of the model and a summary and interpretation of the validation results (including the details of the measurement method).

5. Summary

Anderson et al. (2007), Cristofolini et al. (2010) and Erdemir et al. (2012), Henninger et al. (2010) and Jones and Wilcox (2008) present an excellent framework regarding validation and verification of computational models and what should be reported when presenting research conducted using this very powerful research tool; readers are encouraged to consult these works when developing and utilizing finite element models for orthopedic and biomechanics research. However, these articles introduce validation and verification procedures in a broad sense, and present general reporting guidelines that do not address mesh quality or energy balance issues. Therefore, this paper presents a review of specific mesh quality and energy balance assessments, as well as model validation methods, as they relate to finite element models of biological structures (specifically bone and soft tissues). While the authors of this paper attempted to include criteria that would indicate a high quality mesh and a valid model, the paucity of work in this area limited the generation of a comprehensive set of generalized modeling criteria. The lack of validation, mesh quality and energy balance evidence provided in the literature to date is unfortunate, particularly given the recent popularity and power of finite element modeling approaches in biomechanics and orthopedics research. Therefore, future work needs to be directed at establishing more definitive validation limits for all modeling applications so that accuracy can be optimized and finite element models more accepted for clinical applications.

Conflict of interest statement

The authors have no conflicts of interest to declare.

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