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Clinical Validity of Markerless Motion Capture for Biomechanical Decisions

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ABSTRACT

Background: Markerless motion capture can extend quantitative assessment beyond specialist laboratories. Its clinical value depends on whether a specified output supports a defined biomechanical decision.

Objective: To examine the evidence linking video-derived measurements to biomechanical interpretation and propose a practical framework for evaluating clinical use.

Methods: A critical narrative synthesis combined targeted scholarly searches, reference verification, accessible primary studies, and methodological guidance. Searches were updated on 10 November 2026. Evidence was organised by measurement construct, reference, population, task, and intended interpretation. Selection was purposive; no exhaustive search, pooled accuracy estimate, or formal certainty grade was undertaken.

Synthesis: Validation results depend on joint, rotation plane, task, recording conditions, and reference model. Clinical studies show that agreement in average sagittal waveforms can coexist with missed individual abnormalities or gait-classification disagreement. Multicentre group differentiation establishes a different property from individual measurement agreement. Kinetic estimation introduces additional dependence on external forces, contact assumptions, and musculoskeletal model structure. Correlation, repeatability, detectable change, clinically important change, and improved decisions require separate evidence.

Conclusion: Markerless motion capture should be evaluated as linked measurements and interpretations. Credible use requires explicit anatomical definitions, reference-aware agreement analysis, representative external evaluation, transparent treatment of failed recordings, and version-controlled longitudinal assessment. The proposed framework identifies evidence requirements without prescribing universal angular-error thresholds or presenting video-derived outputs as direct tissue-load measurements.

Keywords: markerless motion capture; clinical biomechanics; gait analysis; measurement validity; musculoskeletal modelling; uncertainty

HIGHLIGHTS

- Validity belongs to a specified output, population, task, setting and decision — not to a device.
- Favourable average waveform agreement can coexist with missed individual abnormalities and gait-classification disagreement.
- Kinematic agreement does not establish kinetic validity; net joint moments are not tissue forces.
- Detectable change, clinically important change and improved decisions each require separate evidence.

1. INTRODUCTION

Quantitative movement analysis can help explain how a person completes a task, how mechanical demands change after treatment, and whether a performance improvement conceals redistribution between joints. Conventional laboratory assessment can be difficult to repeat in community settings because it requires equipment, preparation, and specialist interpretation. Markerless motion capture addresses some of these acquisition barriers by deriving body motion from images. Its development has progressed from geometric reconstruction of body shape to contemporary pose estimation and model-based analysis [1,2]. However, easier acquisition does not itself establish the validity of a clinical interpretation.

The relevant question is whether an output is sufficiently accurate and informative for the decision being considered. A system suitable for estimating walking speed may be unsuitable for identifying a small change in axial knee rotation. An algorithm that reproduces a group-average waveform may miss an unusual pattern in the individual who most needs assess-

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ment. OpenCap demonstrates how smartphone videos can be combined with musculoskeletal simulation to estimate kinematics and dynamics [3]. The broader clinical literature nevertheless identifies unresolved questions about anatomical correspondence, pathological movement, and the transfer of validation between settings [4].

This review examines the chain from recorded images to clinical decisions. It contributes three practical elements: a distinction between the quantities being measured or estimated; an evidence map separating laboratory agreement, clinical applicability, and multicentre group differentiation; and a proposed validation pathway with explicit decision consequences. The emphasis is on lower-limb movement and gait because these tasks dominate the selected empirical examples. The principles also apply to other movements, but the empirical results cannot be generalised automatically to upper-limb tasks, wheelchair use, or all sporting actions. The proposed pathway is a methodological synthesis, not a consensus standard, validated score, or clinical clearance rule.

2. EVIDENCE IDENTIFICATION AND CRITICAL APPRAISAL

Targeted web searches of scholarly records indexed in PubMed, accessible PubMed Central articles, publisher websites, and institutional repositories were updated on 10 November 2026. Search combinations addressed markerless motion capture, pose estimation, clinical gait, concurrent validity, repeatability, cerebral palsy, knee osteoarthritis, OpenCap, functional tasks, clothing, joint coordinate systems, and musculoskeletal validation. Exact-title and DOI searches checked the supplied manuscript's references and identified relevant newer studies. Methodological searches addressed agreement with repeated observations, reliability, measurement error, and the evaluation of clinical prediction and decision-support systems.

Sources were selected for their direct contribution to a specific interpretive question. Human validation studies received priority for empirical accuracy claims; original methodological publications and professional recommendations supported measurement and reporting principles. Earlier reviews supplied historical context and helped identify limitations, but they were not used to substitute pooled conclusions for direct examination of pivotal studies. The selection included studies with clinically relevant disagreement as well as encouraging results. Pure computer-vision benchmark performance without a biomechanical interpretation was outside the main scope.

The appraisal considered the target population and task, acquisition conditions, anatomical definitions, reference-system limitations, independence of development and evaluation data, repeated-measure structure, and relevance of the reported outcome to an individual decision. Available full texts were examined for pivotal methodological details. When only an abstract or indexed record was accessible, statements were restricted to information explicitly reported there; unreported exclusions, model settings, and software versions were not inferred. The accompanying source register documents the accessed source and access level. Table 2 identifies the access boundary for representative empirical studies.

This was a purposive critical narrative review. It did not use a registered systematic-review protocol, duplicate screening, an exhaustive database search, or a formal risk-of-bias score. It therefore cannot estimate the prevalence of favourable results, rank commercial products, or provide a comprehensive certainty assessment. Observed findings, deductions from mechanics, and proposed research procedures are distinguished throughout. Numerical errors from different studies were not pooled because their tasks, joint definitions, comparators, metrics, and aggregation levels differ.

3. SPECIFY THE QUANTITY BEFORE EVALUATING ITS VALIDITY

Markerless systems may produce image coordinates, reconstructed landmarks, segment orientations, joint angles, gait events, net joint moments, or estimated tissue forces. These quantities occupy different positions in the measurement chain and require different evidence. A projected two-dimensional angle is not a three-dimensional anatomical rotation. Nor is a learned image landmark necessarily equivalent to a functional joint centre or a palpated bony landmark. Joint coordinate

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conventions, rotation sequence, sign, and segment definition should be reported; the International Society of Biomechanics recommendations provide an established starting point for relevant joints [5].

Needham and colleagues compared several pose-estimation approaches with marker-based capture during walking, running, and jumping. Their findings showed systematic hip and knee centre differences and smaller discrepancies at some other joints [6]. The implication is that a coordinate can be repeatable while referring to a different anatomical location. An error expressed in millimetres also depends on the reference frame and any alignment performed before comparison. Removing a participant-specific offset or optimising the alignment against test data may answer a different question from evaluating an independently acquired clinical recording.

Stenum and colleagues evaluated temporal, spatial, and sagittal kinematic gait measures from two-dimensional video [7]. Their variable-specific findings illustrate why successful event timing cannot validate joint rotation or loading by extension. A pipeline may perform well at identifying foot contact while estimating the pelvis less accurately. Investigators should specify the measurement unit and aggregation rule before reporting accuracy: frame, stride, trial, session, participant mean, or individual change. A favourable mean across many degrees of freedom can conceal the error in the one variable used to select treatment.

An intended-use statement should therefore name the population, task, output, environment, operator, and decision. For example, estimating average walking speed during unassisted overground gait in adults with knee osteoarthritis is narrower and testable. Describing a device simply as clinically validated leaves these conditions unspecified. Acceptable error should be justified prospectively in relation to the consequence of misinterpretation. When no clinically anchored tolerance exists, the study should report precision and applicability without inventing a universal pass threshold. Table 1 summarises the evidence required along the measurement chain.

Table 1. Measurement quantities and the evidence required for interpretation

Output	Required comparison or check	Boundary of interpretation
Image landmarks	Annotation definitions, image visibility, detection and association errors	Pixel agreement does not establish an anatomical joint centre
Three-dimensional landmarks	Calibration, synchronization, reconstruction, reference-frame alignment	Position agreement does not establish accurate segment orientation
Joint angles and gait events	Comparable anatomical models, rotation conventions, event definitions, waveform and timing errors	Validation applies to the tested joint, plane, task, and population
Net joint moments and powers	Motion, external forces or wrenches, model properties, synchronization, residuals	An estimated net moment is not an individual muscle or ligament force
Tissue-level estimates	Anatomical and physiological assumptions, independent converging validation, sensitivity analysis	Plausible model output is not direct measurement of internal loading
Clinical interpretation	Longitudinal or diagnostic evidence appropriate to the claim and intended decision	Measurement agreement alone does not demonstrate improved outcomes

This table is an interpretive synthesis informed by the anatomical, empirical, and modelling literature [3,5–7]. It is not a ratified standard.

Figure 1 maps the measurement chain and distinguishes the additional information required to estimate net joint moments.

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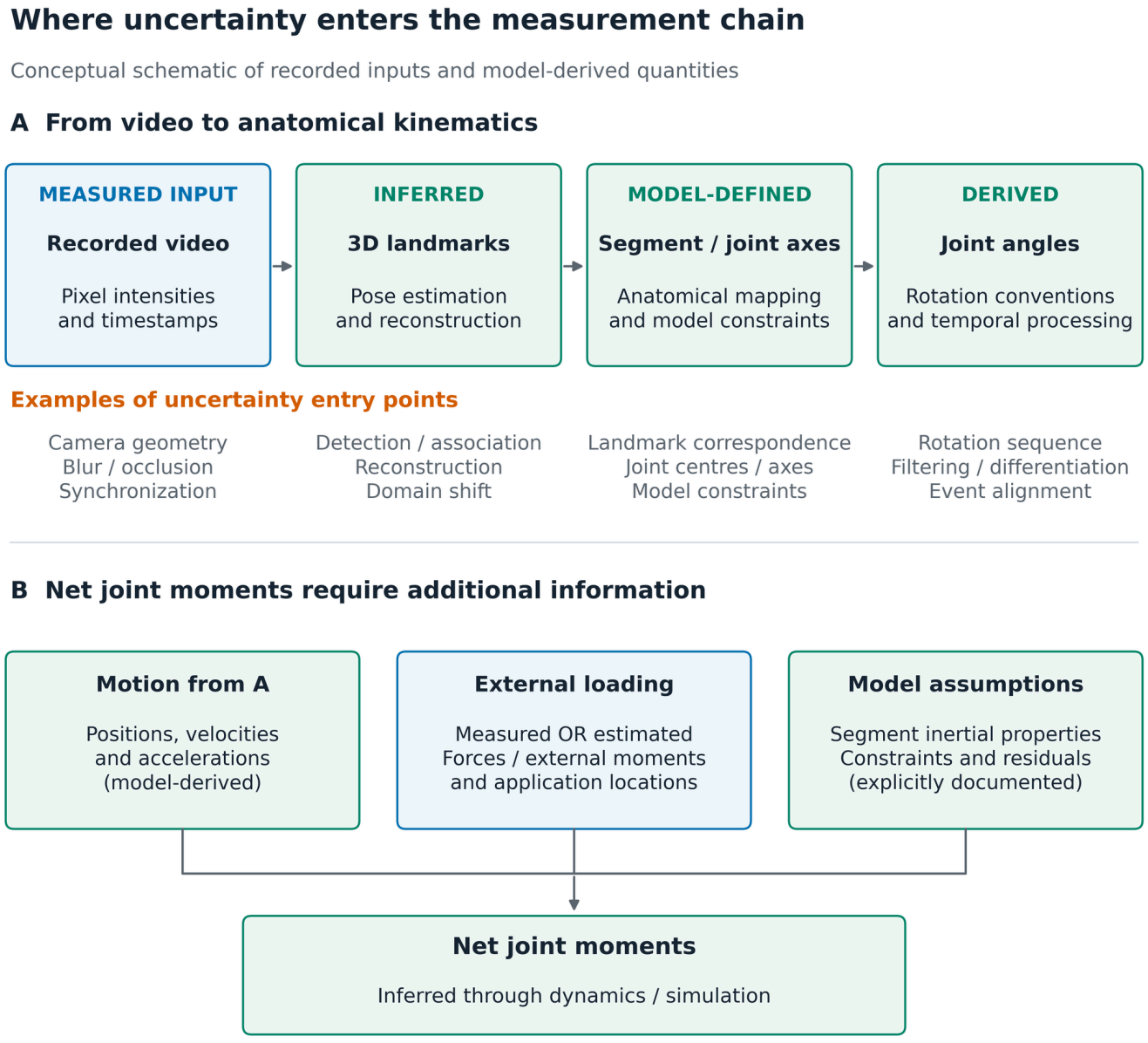


Figure 1. Where uncertainty enters the measurement chain. Conceptual map of the measurement chain and its uncertainty entry points. A, recorded video provides pixel intensities and timestamps. Pose estimation and reconstruction infer landmarks; anatomical mapping and model constraints define segment and joint axes; rotation conventions and temporal processing produce joint angles. Listed uncertainty sources are examples, not a quantified or exhaustive error budget. B, estimating net joint moments additionally requires motion derivatives, external loading that is measured or explicitly estimated, and documented inertial and model assumptions. Contact or support forces and their points of application must be considered. Agreement in joint angles therefore does not, by itself, establish kinetic validity. Net moments represent resultant demand and do not uniquely identify individual muscle, ligament or joint-contact forces. This original schematic synthesises references [3,5,6,18,19].

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4. WHAT EMPIRICAL VALIDATION ESTABLISHES

4.1 Laboratory agreement and repeatability

Kanko and colleagues compared synchronized markerless and marker-based gait measurements in 30 healthy adults and found agreement that varied across joint centres and rotation axes [8]. A separate study of eight adults across three sessions evaluated intersession repeatability [9]. These results address complementary questions: correspondence between systems and stability across repeated acquisition. Neither establishes responsiveness to treatment in a clinical population. Repeatability can remain high when a systematic anatomical offset is present.

Errors are not exclusively small, approximately Gaussian perturbations. In an OpenPose study involving two healthy volunteers, Nakano and colleagues observed failures including incorrect assignment of body segments [10]. Such events are qualitatively different from ordinary coordinate noise. A validation report should show both the error distribution among successful recordings and the proportion of intended recordings producing usable output. Manual repair may be legitimate, but the frequency, rules, operator effort, and agreement between operators should accompany the repaired results.

The original OpenCap evaluation included ten healthy participants performing walking, squatting, rising from a chair, and drop jumps. It reported pooled mean absolute errors of 4.5° for joint angles, 6.2% of bodyweight for ground reaction forces, and 1.2% of bodyweight multiplied by height for joint moments [3]. Those normalised quantities and aggregation choices must remain explicit. An independent evaluation by Svetek and colleagues in 20 participants found task- and joint-dependent discrepancies, including comparatively large sagittal hip errors in gait trials [11]. The studies test different protocols; their results should not be compressed into a single platform accuracy claim.

Task-specific errors from the original OpenCap validation illustrate why performance should be reported separately for each output and activity (Figure 2).

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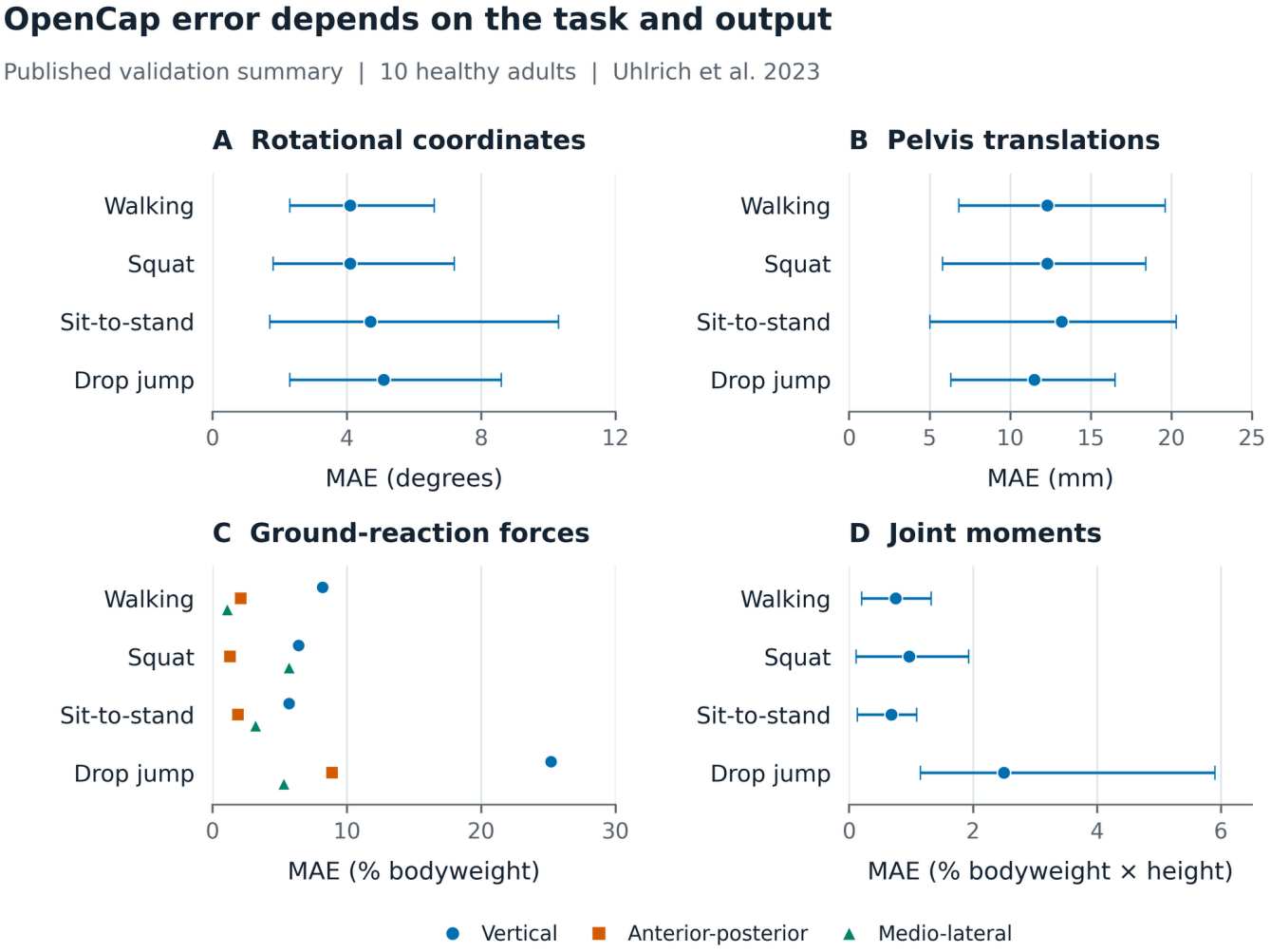


Figure 2. OpenCap error depends on the task and output. Task-specific mean absolute errors (MAEs) for OpenCap in ten healthy adults. A, rotational coordinates; B, pelvis translations; C, the three ground-reaction-force components; D, joint moments. Points show the published MAEs. Horizontal segments in A, B and D span the minimum and maximum MAE across model degrees of freedom after averaging trials and participants. They are descriptive ranges across coordinates, not confidence intervals or participant standard deviations. Force-component points in C have no uncertainty interval reported in the source table. BW denotes bodyweight; moment error is expressed as 100 × moment MAE/(BW × participant height). The reference combines marker-based motion capture, force plates and inverse dynamics. Values are replotted in a new layout from Uhlrich et al. (2023), Table 1 [3]. The results apply to the reported acquisition and processing configuration and do not establish an individual clinical tolerance.

4.2 Clinical populations and consequential disagreement

Clinical validation must examine the abnormal patterns that the intended service needs to recognise. Wishaupt and colleagues studied 30 children with spastic cerebral palsy and 15 typically developing children [12]. Although several angular patterns were reproduced, individual differences in pelvic tilt and transverse hip rotation were not captured to the same extent by the markerless system. Their full text also identifies differences between the biomechanical models, including foot segmentation and knee degrees of freedom. Thus disagreement cannot automatically be assigned entirely to image tracking.

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Individual differences between marker-based and markerless gait outputs vary by anatomical variable and extend beyond the group mean (Figure 3).

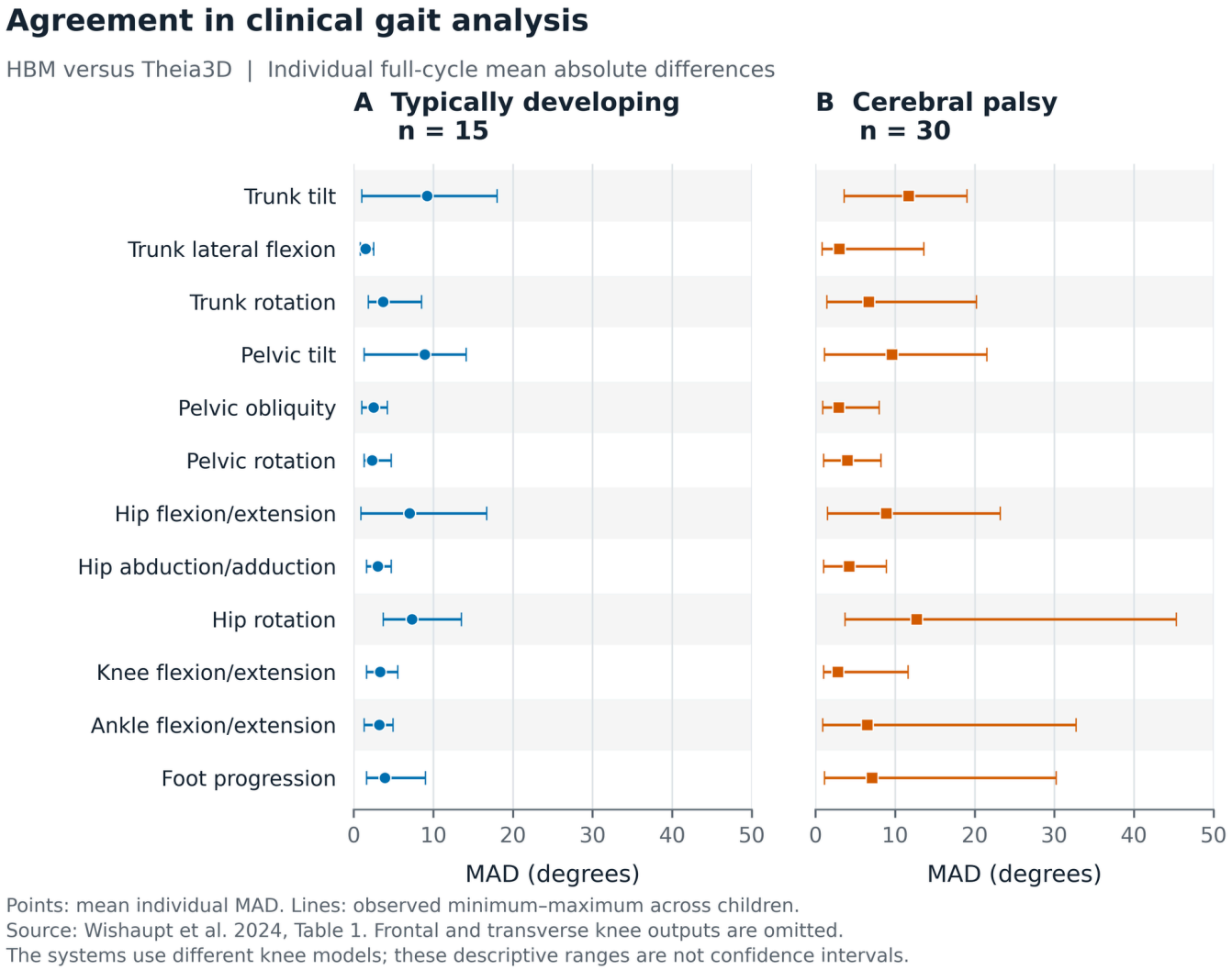


Figure 3. Agreement in clinical gait analysis. Agreement between the marker-based Human Body Model (HBM) and Theia3D markerless measurements during comfortable-speed barefoot walking. A, typically developing children (n = 15); B, children with spastic cerebral palsy (n = 30). Points show the group mean of individual full-cycle mean absolute differences (MADs), and horizontal segments show the observed minimum and maximum across children. These segments are not confidence intervals or Bland-Altman limits of agreement. Five cycles were averaged per child; the source used the right leg for typically developing children and the clinically more affected leg for cerebral palsy, with the right leg selected when impairment was equal. The same angle scale is used in both panels. Frontal and transverse knee outputs are omitted because HBM and Theia3D use different knee degrees of freedom; their published values remain in the source-data file. Values are replotted in a new layout from Wishaupt et al. (2024), Table 1 [12]. Differences include anatomical-model and coordinate-definition effects as well as tracking disagreement.

A larger 2026 study by Chafetz and colleagues compared simultaneous recordings in 202 children with complex gait patterns [13]. The published abstract reports sagittal knee and ankle mean root-mean-square errors below 5°, but only 67% overall concordance for a specified sagittal gait classification, with $\kappa = 0.60$. Errors increased with body mass index and walker use. This is particularly relevant to interpretation: apparently favourable angular summaries do not guarantee equivalent classification. The result does not determine which system gave the correct classification, and it should not be extrapolated to every clinical diagnosis.

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Outerleys and colleagues provide a different type of evidence. Their three-centre study included 486 participants, of whom 351 had knee osteoarthritis, and identified broadly consistent disease-associated gait differences across sites [14]. This supports the feasibility of studying group patterns in a multicentre deployment. Because group differentiation is not the same experiment as paired comparison with an independent reference, it does not establish individual interchangeability, diagnostic sensitivity, or treatment responsiveness. Appropriate recognition of this distinction strengthens the study's contribution without overstating it.

Single-camera three-dimensional inference warrants its own evaluation. Vellucci and colleagues trained a pipeline against a 17-sensor inertial system and examined camera location, clothing, anthropometry, and gait strategy in healthy participants [15]. The best-performing viewpoint yielded reported landmark and angle root-mean-square errors of 2.97 cm and 4.24°, with gait-profile-score ICC of 0.65. These are comparisons with inertial estimates, not direct skeletal measurements. They demonstrate a development signal under tested conditions; they do not justify transferring the values to neurological or orthopaedic patients.

Table 2. Selected empirical studies and their interpretive limits

Study and context	Evaluated property	Main contribution	Interpretive limit and access
Kanko et al. 2021 [8]; 30 healthy adults	Concurrent gait kinematics	Agreement varies by joint and rotation axis	Healthy treadmill gait; abstract checked
Kanko et al. 2021 [9]; eight healthy adults	Three-session repeatability	Demonstrates feasibility of repeated markerless gait acquisition	Small sample; repeatability is separate from accuracy; abstract checked
Uhlrich et al. 2023 [3]; ten healthy adults	Kinematics and dynamics across four activities	Links smartphone acquisition to model-based dynamics	Pooled errors do not validate every output or patient group; full text checked
Svetek et al. 2025 [11]; 20 participants	OpenCap during gait and functional tasks	Reveals important task and joint dependence	Laboratory apparatus and viewing conditions matter; full text checked
Wishaupt et al. 2024 [12]; cerebral palsy and comparison groups	Reproduction of clinical gait deviations	Identifies missed individual pelvic and hip variation	Anatomical model differences complicate attribution; full text checked
Chafetz et al. 2026 [13]; 202 children	Complex gait and classification agreement	Extends evidence to a larger clinical cohort	Published abstract only; classification agreement is not clinical outcome benefit
Outerleys et al. 2025 [14]; 486 participants at three centres	Group patterns and site consistency	Supports multicentre observational applications	No claim of individual criterion validity; abstract checked
Vellucci et al. 2026 [15]; healthy walking	Single-camera inference against an inertial system	Examines viewpoint and ecological perturbations	Reference is itself estimated; published abstract checked

The table is a purposive evidence map, not a systematic inventory or comparative ranking. Different error metrics and aggregation schemes preclude direct numerical ranking.

5. REFERENCE METHODS HAVE THEIR OWN UNCERTAINTY

Optical marker-based capture is an established comparison method, but skin-mounted markers do not provide direct bone motion. In a bone-pin comparison during running, Reinschmidt and colleagues demonstrated that errors from skin movement differed by rotation plane and participant [16]. Agreement with an optical system therefore means agreement with that implementation, including its marker placement, anatomical model, and processing. Inertial systems introduce different alignment, drift-correction, and modelling assumptions. Neither comparator should be described as error-free skeletal truth.

The comparison should first establish whether both pipelines target the same quantity. A multisegment foot and a single rigid foot model can produce different ankle interpretations even when both track their assigned features consistently. Similarly, a constrained knee model cannot independently express all the rotations produced by a less constrained model. Matching Cardan sequences is helpful but does not remove differences in segment axes or anatomical definitions. Model-level disagreement, tracking error, and reference error should be considered separately whenever the data permit.

A stronger study can combine synchronized comparison, independent calibration checks, repeated reference measurements, and sensitivity analyses. Where bone imaging is feasible and justified, it can answer particular anatomical questions, but its availability and field of view limit its role as a universal reference. Where direct observation of an internal quantity is unavailable, the correct response is to delimit the claim and seek converging evidence. Agreement between two algorithms sharing training data or anatomical assumptions offers less independent confirmation than agreement between genuinely independent measurement pathways.

6. FROM MOTION TO NET JOINT DEMAND AND INTERNAL LOADING

Kinematic agreement cannot be carried forward automatically to kinetic validity. For a specified rigid-body model expressed in independent generalised coordinates, a schematic balance is:

$$M(q)\ddot{q} + h(q, \dot{q}) = Q + J_c(q)^{\mathsf{T}} W_{\text{ext}}.$$

Here q denotes configuration, $\dot{q}$ and $\ddot{q}$ its time derivatives, M the mass matrix, and h the motion-dependent and gravitational terms. Q contains generalised actuation, while J_c maps generalised motion to external contact motion and W_{ext} contains the corresponding external forces and moments. The exact formulation depends on the model and constraints. OpenSim provides a computational framework for constructing and solving such models [17,18]. Software availability does not establish that a particular model represents a particular patient's anatomy or movement.

In inverse dynamics, prescribed motion and external loading determine net generalised demand under the selected assumptions. When external forces are not measured, a video-based pipeline must estimate them or introduce additional contact and physiological constraints. Unknown hand support, a carried object, or a walking aid can alter external loading without a uniquely recoverable signature in the visible joint angles. Investigators should state which forces were measured, inferred, assumed absent, or replaced by residual terms. A system's label should not obscure these different sources of information.

Net joint moments do not identify individual muscle forces. Multiple agonist–antagonist combinations can produce the same moment, while co-contraction can change joint contact loading without a corresponding increase in the net moment. Tissue-level estimates additionally depend on muscle paths, moment arms, strength parameters, tendon behaviour, and the criterion used to resolve force sharing. Hicks and colleagues recommend verification, output-specific validation, and sensitivity analysis as linked components of credible simulation [19]. Agreement with activation timing or another indirect signal can strengthen plausibility, but it cannot convert an underdetermined internal-force estimate into a direct measurement.

The mechanical importance of geometric error can be illustrated without assigning an empirical error to a device. For a fixed force F, the first-order change in moment due to a position error δr is $\delta\tau = \delta r \times F$. A 1,000 N force and a 0.01 m error perpendicular to its line of action produce a 10 N·m discrepancy. If force and position are both uncertain, both contributions and their dependence matter. This example is an analytical illustration, not an observed performance result or a universal error budget.

Filtering, differentiation, synchronization, and event alignment also affect kinetic estimates. Differentiating noisy trajectories amplifies high-frequency components; smoothing can attenuate genuine peaks. A time offset between kinematics and exter-

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nal forces can alter an estimated moment even if each signal appears plausible in isolation. Report sample rates, resampling, filter family and cutoff, phase handling, and synchronization procedures. Sensitivity analysis should vary plausible inputs jointly when they are dependent and show whether uncertainty changes the interpretation of the endpoint. The most relevant output may be uncertainty in a decision, rather than uncertainty in a visually averaged waveform.

7. AGREEMENT AND LONGITUDINAL CHANGE REQUIRE DIFFERENT ANALYSES

Correlation describes covariation and does not establish interchangeability. For independent paired differences with approximately normal, stable variance, conventional limits of agreement are estimated as the mean difference plus or minus 1.96 times the standard deviation of the differences [20]. These limits describe the distribution of disagreement, not the confidence interval around the mean bias. Confidence intervals for the limits should also be reported. Proportional bias, skewness, or changing variance may require modelling or transformation, with the interpretation retained in clinically meaningful units.

Movement data are clustered: frames form strides, strides form trials, and repeated trials belong to people and sessions. Bland and Altman describe extensions for repeated observations [21]. Investigators should use a model or resampling scheme consistent with this hierarchy and the intended prediction target. Treating thousands of frames from a few participants as independent observations yields misleading precision. Participant-level averages answer an average-performance question; they do not establish frame-level or single-stride agreement. A study should define the primary unit before selecting its statistical analysis.

Reliability reporting should identify the intraclass correlation coefficient model, agreement or consistency definition, single-measure or averaged-measure target, and uncertainty interval [22]. An ICC can be high in a heterogeneous sample despite substantial absolute error. It is therefore insufficient for assessing whether a small change in one individual is distinguishable from measurement variability. Report absolute within-person error and inspect whether it varies by baseline value, task difficulty, or impairment severity.

Under a conventional model with approximately normal, independent, equally variable errors at two occasions and no unaccounted systematic shift, a 95% minimal detectable change is $1.96\sqrt{2}\,\mathrm{SEM}$, where SEM is the standard error of measurement for the relevant protocol [23]. If the reported score averages repetitions, its SEM must correspond to that same score. Correlated occasion errors, learning effects, heteroscedasticity, or altered acquisition can invalidate the simple expression. Detectable change describes measurement uncertainty; it does not establish the magnitude that matters to a patient or athlete.

Clinically important change requires a separate justification linked to the intended outcome. A statistically distinguishable angle change may be clinically trivial, while a clinically valuable improvement may remain uncertain in a noisy instrument. Schwartz and colleagues demonstrated an approach to separating sources of gait variability and incorporating measurement uncertainty into interpretation [24]. For markerless monitoring, analogous repeat-session designs should retain realistic variation in operators and setup. Standardising chair height, walking speed, footwear, and instructions can improve interpretability, but a study must also establish whether those constraints are achievable where the method will be used.

Waveform and event analyses should complement discrete peaks when the interpretation depends on timing or movement strategy. A low average waveform error can conceal a biased impact peak, while aggressive temporal alignment can remove a real timing difference. Conversely, testing every time point independently without addressing multiplicity can exaggerate evidence of difference. Report the event-detection procedure, alignment rule, and prespecified waveform question. Neither a statistically significant difference nor a nonsignificant comparison establishes clinical non-equivalence or equivalence without a justified tolerance and adequate precision.

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8. GENERALISATION AND MEASUREMENT COVERAGE

Acquisition conditions are part of the instrument. Camera placement, lens properties, illumination, motion blur, synchronization, image resolution, clothing, and occlusion should be described sufficiently for replication. Kanko and colleagues explicitly compared running measurements under different clothing conditions [25]. Such work provides a precedent for manipulating deployment factors, but successful performance with particular garments does not cover every fabric, fit, or movement. Studies should first isolate important factors, then evaluate realistic combinations in an independent setting.

Neural networks depend on training annotations and the range of appearances and movements represented during development [26]. Smooth trajectories can reflect a strong learned prior rather than faithful recovery of an unusual movement. Evaluation should therefore examine not only average error but also the preservation of clinically relevant extremes and between-person variation. The clinical cerebral palsy findings illustrate this concern without proving a single computational cause [12,13]. Investigators should avoid attributing every subgroup difference to a demographic characteristic when visibility, task execution, body dimensions, and model structure may contribute.

Machine-learning guidance in movement biomechanics emphasises separating development from evaluation and avoiding information leakage [27]. Adjacent frames, repeated trials, or both limbs from the same person should not be distributed across training and test sets when the claim concerns generalisation to new people. Tuning on an external site's data makes that site part of model development unless a separate untouched evaluation is retained. Report what was pretrained, retrained, calibrated, and selected, including any human corrections informed by the reference outcome.

Measurement coverage should accompany conditional accuracy. A proposed reporting structure is the number of eligible visits, attempted acquisitions, technically adequate recordings, successfully processed outputs, and interpretable clinical reports, with reasons for each loss. Failures may be associated with the most severe impairment or the greatest need for assistance. Complete-case error alone can therefore give an unduly favourable picture. If no output can be produced, reporting the failure is more informative than silently treating it as normal movement or imputing a plausible curve.

Equity claims require adequate data, explicit comparisons, and uncertainty. It is useful to examine performance across relevant body dimensions, age ranges, skin appearance under the actual lighting, clothing practices, and use of assistive devices. However, a small study cannot establish demographic fairness through nonsignificant subgroup tests. Multicentre collaborations can retain a common core protocol while testing locally relevant conditions. They should report site-specific coverage and uncertainty, as a pooled average can obscure systematic failure at a less-resourced site. Identifiable video also requires appropriate consent, access control, and a reproducible plan for sharing derived data when unrestricted video sharing is unsuitable.

9. A PROPOSED PATHWAY FROM MEASUREMENT TO CLINICAL USE

The V3 framework distinguishes verification, analytical validation, and clinical validation for biometric monitoring technologies [28]. For movement analysis, these categories can be operationalised by specifying the anatomical quantity and decision at each stage. Verification concerns correct implementation and acquisition. Analytical validation concerns the measurement or estimate against an appropriate reference. Clinical validation concerns the interpretation in the intended population and context. Table 3 adds external evaluation, longitudinal use, and prospective workflow assessment as practical components of this pathway.

Table 3. Proposed validation pathway and minimum reporting requirements

Stage	Evidence to collect	Reporting requirement
Intended use	Population, task, output, setting, operator, intended action	State the decision and consequences of error before choosing tolerances

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Stage	Evidence to collect	Reporting requirement
Technical verification	Calibration, synchronization, unit tests, anatomical conventions, processing reproducibility	Identify hardware, model, software version, and all manual interventions
Analytical validation	Paired reference data, bias, absolute error, agreement limits, events and waveforms	Describe reference uncertainty and use the appropriate clustered analysis
External evaluation	New people, operators, sites, and realistic perturbations	Report coverage, subgroup uncertainty, and whether any local tuning occurred
Longitudinal evaluation	Repeat sessions, practical SEM, version bridging, clinically relevant anchors	Separate detectable change from important change and individual from group inference
Decision evaluation	Interpretation, management changes, overrides, burden, unintended consequences	Test whether using the measurement improves an outcome relevant to the intended use
Ongoing monitoring	Acquisition failures, distribution changes, software updates, reproducibility	Define when a result is withheld, reviewed, reacquired, or reprocessed

This proposed pathway combines established measurement principles with the present critical synthesis [19–24,27,28]. It requires empirical evaluation and is not a validated checklist score.

A feasible prospective validation study should designate one primary output and the precision needed for its intended use. Participants should represent the clinical service, including relevant impairment and assistance rather than only convenient healthy volunteers. Repeated sessions should reproduce the complete setup process, and operator variation should be included when the service relies on multiple operators. A synchronized reference subset can test analytical agreement while the larger deployment cohort tests coverage and feasibility. Sample size should follow the required precision of agreement limits, failure rates, or decision performance; a generic minimum number of participants is not defensible for every question.

Before analysis, investigators should specify exclusion rules, minimum recording quality, treatment of partial trials, missing-output handling, primary aggregation, and the boundary between calibration and evaluation. A protocol should retain unsuccessful visits in the feasibility denominator even when they cannot contribute numerical agreement data. Data-driven threshold selection should occur within development data and be evaluated independently. Reporting the full sequence prevents a flexible processing pipeline from turning repeated post hoc choices into an apparently prespecified clinical test.

The pathway branches when the output becomes a clinical prediction model. TRIPOD+AI supports transparent reporting of prediction-model development and evaluation [29], while PROBAST+AI addresses quality, risk of bias, and applicability [30]. These tools do not replace instrument-agreement methods for angle estimation. If markerless outputs feed a diagnostic or prognostic rule, both the measurement stage and the downstream rule require evaluation. A model that predicts future injury additionally needs longitudinal outcomes, representative exposure, calibration, and independent validation; a movement feature's mechanical plausibility is insufficient.

Clinical usefulness also depends on how people act on the report. DECIDE-AI provides reporting guidance for early clinical evaluation of AI-supported decisions [31]. An analogous movement-analysis evaluation should record whether clinicians understand uncertainty, when they disagree with or override outputs, and whether the information changes management. Changes in management are intermediate outcomes. Demonstrating benefit may require a comparative design assessing function, symptoms, participation, resource use, or another prespecified outcome. The size and design of that evaluation should follow the proposed clinical claim.

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10. LONGITUDINAL INTERPRETATION AND SOFTWARE CHANGE

Every result should remain linked to its raw recording, acquisition configuration, processing settings, and model version. Software updates can change estimated joint locations, segment definitions, or derived scores without any biological change. A bridge evaluation should process representative recordings through both versions and examine agreement in the endpoints used for follow-up. A fixed numerical correction is inadequate when the change is task dependent or nonlinear. Reprocessing all visits may be justified, but original and revised outputs should remain distinguishable.

Figure 4 separates measurement variability, detectable change and patient-important change while showing the version-comparison step needed for longitudinal interpretation.

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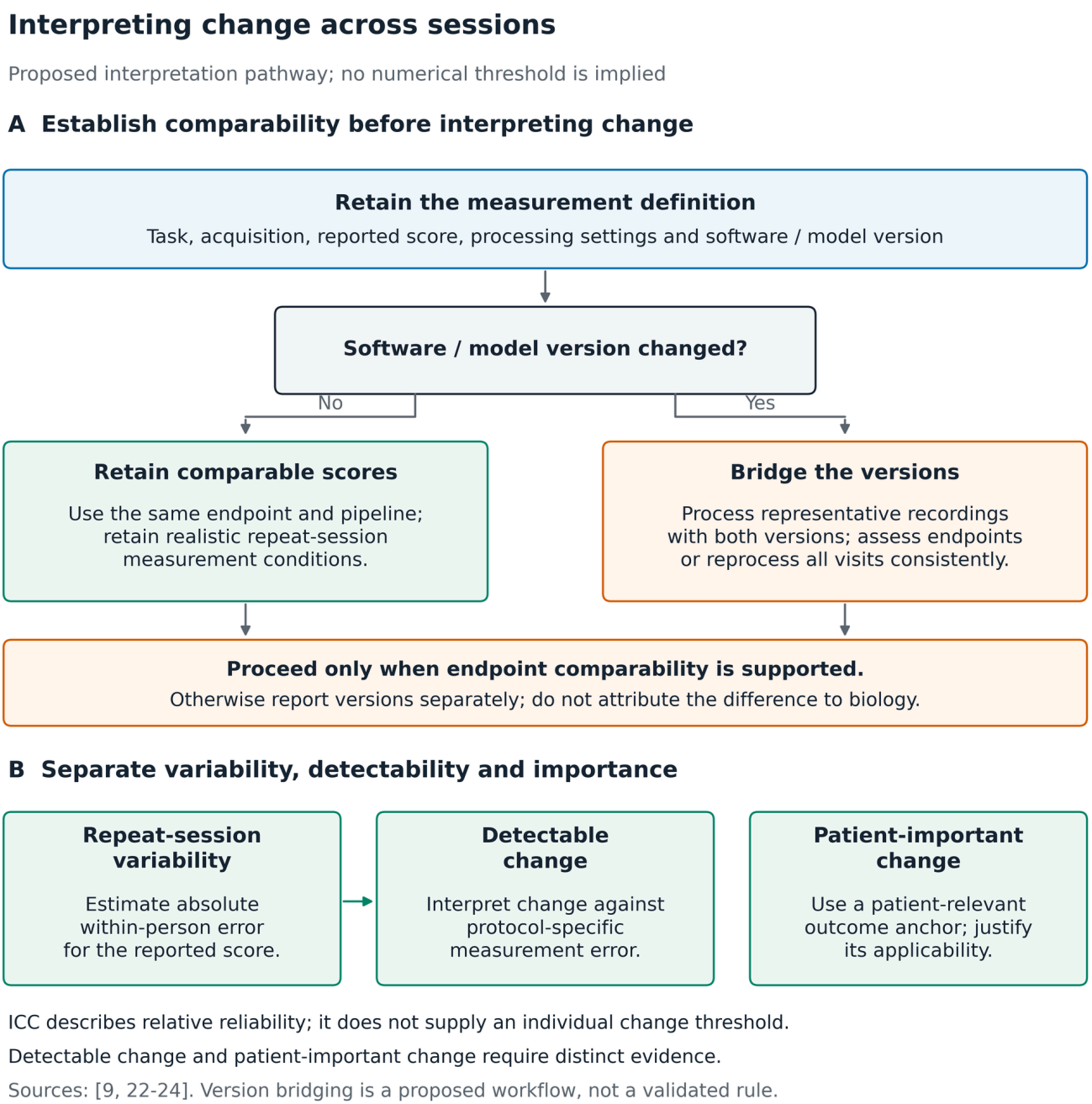


Figure 4. Interpreting change across sessions. Proposed workflow for interpreting longitudinal markerless measurements. A, retain the task, acquisition conditions, score definition, processing settings and software or model version. After a version change, bridge representative recordings across versions or reprocess visits consistently, then establish comparability in the endpoints used for follow-up. If comparability remains unresolved, report versions separately rather than attributing the difference to biological change. B, repeated-session measurements characterise absolute within-person error for the actual reported score. Detectable change concerns that measurement uncertainty; patient-important change requires a separate relevant outcome anchor. An intraclass correlation coefficient (ICC) alone supplies neither an individual change threshold nor a patient-important difference. The schematic provides no numerical cutoff or simulated trajectory. Measurement principles draw on references [9,22-24]; the version-management branch is the review authors' methodological proposal.

Consider a proposed service monitoring peak knee flexion during repeated sit-to-stand. The service would define the chair height, foot placement, instructions, aggregation rule, camera setup, and handling of hand support. Repeat visits would estimate the uncertainty of the actual reported score, and a separate outcome anchor would address clinical relevance. If the

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recording excludes the feet or the participant uses an unmodelled support, the report may need to withhold the affected interpretation. This example specifies a study and quality-control architecture; it does not establish that knee flexion alone represents recovery or supply a validated threshold.

Quality control should communicate a concrete action. A report can provide an estimate with its applicable measurement uncertainty, identify the relevant task and version, and state why an output is unavailable. Automated abstention may reduce misleading interpretations, but it also lowers coverage and can inconvenience patients. Its performance should therefore be measured rather than assumed beneficial. Confidence supplied by a pose-estimation network should not be relabelled as a calibrated clinical probability unless that calibration has been demonstrated for the intended outcome.

11. LIMITATIONS AND RESEARCH PRIORITIES

This review's purposive selection and unequal access to full texts limit its completeness. The evidence map concentrates on lower-limb gait and selected functional tasks, includes both developer-associated and independent studies, and does not establish a hierarchy of systems. Several recent clinical results were available only at abstract level. The proposed pathway expresses a reasoned methodological position rather than a validated intervention, professional consensus, or regulatory standard.

The most useful next studies would connect upstream image and anatomical uncertainty to downstream decision error; evaluate clinically unusual movements in independent settings; compare failures and successful outputs using transparent denominators; and establish longitudinal measurement properties under actual service conditions. Prospective evaluations should investigate whether the measurement adds useful information beyond existing assessment. Studies with accessible cameras should also measure staff time, repeat visits, computing requirements, and interpretation burden, because low acquisition cost alone does not establish a sustainable clinical service.

12. CONCLUSION

Markerless motion capture can broaden access to quantitative movement analysis, but validation belongs to a specified output, population, task, setting, and decision. Empirical studies support selected applications while revealing clinically relevant limits that are obscured by pooled accuracy or repeatability statistics. Credible translation requires anatomical clarity, reference-aware agreement analysis, appropriate treatment of clustered data, transparent failure reporting, and model-specific kinetic validation. Separating measurement performance from meaningful change and clinical usefulness allows researchers to make precise claims and clinicians to interpret estimates within their demonstrated limits.

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MULTILINGUAL ABSTRACTS · RESUMEN · الملخص · सार

Español

Validez clínica de la captura de movimiento sin marcadores para decisiones biomecánicas

Antecedentes: La captura de movimiento sin marcadores puede extender la evaluación cuantitativa más allá de los laboratorios especializados. Su valor clínico depende de si una salida específica respalda una decisión biomecánica definida.

Objetivo: Examinar la evidencia que vincula las mediciones derivadas de vídeo con la interpretación biomecánica y proponer un marco práctico para evaluar su uso clínico.

Métodos: Una síntesis narrativa crítica combinó búsquedas académicas dirigidas, verificación de referencias, estudios primarios accesibles y guías metodológicas. Las búsquedas se actualizaron el 10 de noviembre de 2026. La evidencia se organizó por constructo de medición, referencia, población, tarea e interpretación prevista. La selección fue intencional; no se realizó una búsqueda exhaustiva, una estimación agrupada de la precisión ni una graduación formal de la certeza.

Síntesis: Los resultados de validación dependen de la articulación, el plano de rotación, la tarea, las condiciones de registro y el modelo de referencia. Los estudios clínicos muestran que la concordancia en las curvas sagitales medias puede coexistir con anomalías individuales no detectadas o con desacuerdos en la clasificación de la marcha. La diferenciación de grupos en estudios multicéntricos establece una propiedad distinta de la concordancia de la medición individual. La estimación cinética introduce una dependencia adicional de las fuerzas externas, de los supuestos de contacto y de la estructura del modelo musculoesquelético. La correlación, la repetibilidad, el cambio detectable, el cambio clínicamente importante y la mejora de las decisiones requieren evidencia independiente.

Conclusión: La captura de movimiento sin marcadores debe evaluarse como un conjunto de mediciones e interpretaciones vinculadas. Un uso creíble requiere definiciones anatómicas explícitas, análisis de concordancia que tenga en cuenta la referencia, evaluación externa representativa, tratamiento transparente de los registros fallidos y evaluación longitudinal con control de versiones. El marco propuesto identifica los requisitos de evidencia sin prescribir umbrales universales de error angular ni presentar las salidas derivadas de vídeo como mediciones directas de la carga tisular.

Palabras clave: captura de movimiento sin marcadores; biomecánica clínica; análisis de la marcha; validez de la medición; modelado musculoesquelético; incertidumbre

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العربية

الصلاحية السريرية لالتقاط الحركة دون علامات في اتخاذ القرارات البيوميكانيكية

الخلفية: يمكن لالتقاط الحركة دون علامات أن يوسّع التقييم الكمّي إلى ما وراء المختبرات المتخصصة. وتعتمد قيمته السريرية على ما إذا كان مُخرَج محدّد يدعم قرارًا بيوميكانيكيًا معرّفًا.

الهدف: فحص الأدلة التي تربط القياسات المستخلصة من الفيديو بالتفسير البيوميكانيكي، واقتراح إطار عملي لتقييم الاستخدام السريري.

المنهجية: جمع تركيب سردي نقدي بين عمليات بحث علمية موجّهة، والتحقق من المراجع، والدراسات الأولية المتاحة، والإرشادات المنهجية. حُدّثت عمليات البحث في 10 نوفمبر 2026. ونُظّمت الأدلة حسب بنية القياس والمرجع والفئة السكانية والمهمة والتفسير المقصود. كان الاختيار هادفًا؛ ولم يُجرَ بحث شامل أو تقدير مُجمَّع للدقة أو تصنيف رسمي لدرجة اليقين.

التركيب: تعتمد نتائج التحقق على المفصل ومستوى الدوران والمهمة وظروف التسجيل والنموذج المرجعي. وتُظهر الدراسات السريرية أن التوافق في متوسط المنحنيات السهمية قد يتعايش مع تفويت حالات شاذة فردية أو مع اختلاف في تصنيف المشي. ويُثبت التمييز بين المجموعات في الدراسات متعددة المراكز خاصيةً مختلفة عن توافق القياس الفردي. ويُدخل تقدير الكينتيكا اعتمادًا إضافيًا على القوى الخارجية وافتراضات التلامس وبنية النموذج العضلي الهيكلي. ويتطلب كلٌّ من الارتباط والتكرارية والتغير القابل للكشف والتغير المهم سريريًا وتحسين القرارات أدلةً مستقلة.

الخلاصة: ينبغي تقييم التقاط الحركة دون علامات بوصفه منظومة من القياسات والتفسيرات المترابطة. ويتطلب الاستخدام الموثوق تعريفات تشريحية صريحة، وتحليل توافق يراعي المرجع، وتقييمًا خارجيًا تمثيليًا، ومعالجة شفافة للتسجيلات الفاشلة، وتقييمًا طوليًا مضبوط الإصدارات. ويحدّد الإطار المقترح متطلبات الأدلة دون فرض عتبات عامة للخطأ الزاوي، ودون تقديم مخرجات الفيديو بوصفها قياسات مباشرة لأحمال الأنسجة.

الكلمات المفتاحية: التقاط الحركة دون علامات؛ البيوميكانيكا السريرية؛ تحليل المشي؛ صلاحية القياس؛ النمذجة العضلية الهيكلية؛ عدم اليقين

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हिन्दी

बायोमैकेनिकल निर्णयों के लिए मार्करलेस मोशन कैप्चर की नैदानिक वैधता

पृष्ठभूमि: मार्करलेस मोशन कैप्चर मात्रात्मक मूल्यांकन को विशेषज्ञ प्रयोगशालाओं से आगे ले जा सकता है। इसका नैदानिक मूल्य इस पर निर्भर करता है कि कोई निर्दिष्ट आउटपुट किसी परिभाषित बायोमैकेनिकल निर्णय का समर्थन करता है या नहीं।

उद्देश्य: वीडियो-आधारित मापों को बायोमैकेनिकल व्याख्या से जोड़ने वाले साक्ष्य की जाँच करना और नैदानिक उपयोग के मूल्यांकन हेतु एक व्यावहारिक ढाँचा प्रस्तावित करना।

विधियाँ: एक समालोचनात्मक वर्णनात्मक संश्लेषण में लक्षित विद्वत खोजों, संदर्भ-सत्यापन, उपलब्ध प्राथमिक अध्ययनों और पद्धतिगत दिशानिर्देशों को सम्मिलित किया गया। खोजें 10 नवंबर 2026 को अद्यतन की गईं। साक्ष्य को माप-संरचना, संदर्भ, जनसंख्या, कार्य और अभिप्रेत व्याख्या के अनुसार व्यवस्थित किया गया। चयन उद्देश्यपूर्ण था; कोई संपूर्ण खोज, संयुक्त सटीकता-अनुमान या औपचारिक निश्चितता-ग्रेडिंग नहीं की गई।

संश्लेषण: सत्यापन के परिणाम जोड़, घूर्णन-तल, कार्य, रिकॉर्डिंग-परिस्थितियों और संदर्भ मॉडल पर निर्भर करते हैं। नैदानिक अध्ययन दर्शाते हैं कि औसत सैजिटल तरंग-रूपों में सहमति के साथ-साथ व्यक्तिगत असामान्यताएँ छूट सकती हैं या चाल-वर्गीकरण में असहमति हो सकती है। बहुकेंद्रीय समूह-भेदन व्यक्तिगत माप-सहमति से भिन्न गुण स्थापित करता है। काइनेटिक अनुमान बाहरी बलों, संपर्क-मान्यताओं और मस्कुलोस्केलेटल मॉडल-संरचना पर अतिरिक्त निर्भरता लाता है। सहसंबंध, पुनरावर्तनीयता, पता लगाने योग्य परिवर्तन, नैदानिक रूप से महत्वपूर्ण परिवर्तन और बेहतर निर्णय — प्रत्येक के लिए अलग साक्ष्य आवश्यक है।

निष्कर्ष: मार्करलेस मोशन कैप्चर का मूल्यांकन परस्पर जुड़े मापों और व्याख्याओं के रूप में किया जाना चाहिए। विश्वसनीय उपयोग के लिए स्पष्ट शारीरिक परिभाषाएँ, संदर्भ-सचेत सहमति-विश्लेषण, प्रतिनिधिक बाहरी मूल्यांकन, विफल रिकॉर्डिंग का पारदर्शी प्रबंधन और संस्करण-नियंत्रित अनुदैर्ध्य मूल्यांकन आवश्यक हैं। प्रस्तावित ढाँचा सार्वभौमिक कोणीय-त्रुटि सीमाएँ निर्धारित किए बिना, और वीडियो-आधारित आउटपुट को ऊतक-भार के प्रत्यक्ष माप के रूप में प्रस्तुत किए बिना, साक्ष्य संबंधी आवश्यकताओं की पहचान करता है।

मुख्य शब्द: मार्करलेस मोशन कैप्चर; नैदानिक बायोमैकेनिक्स; चाल विश्लेषण; माप वैधता; मस्कुलोस्केलेटल मॉडलिंग; अनिश्चितता