Response to the Reviewer's Comments

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We thank the reviewers for identifying the weaknesses in our paper and providing us the opportunity to strengthen our research prior to publication.

The comments of the reviewers have been very useful, for they made us aware that some major points we were trying to convey remained obscure even to well-informed readers.

Following up on the reviewers' suggestions, we have restructured the paper by changing the order of some sections, by putting more emphasis on motivation, and by adding clarifications at crucial spots.

In addition, we have also corrected all factual errors and minor infelicities of presentation that the reviewers brought to our attention. Below we indicate for each individual comment how we have dealt with it.

Reviewer 1

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> 1)English and equations in the current paper are terrible. The authors need
> to improve the spelling, structure and presentation of the paper.

> 2) The authors used many abbreviation words without providing any explanation.
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For all abbreviation we add the corresponding meaning. For example, Skinned Multi-Person Linear (SMPL),

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> 3) There are several typo errors.
> For example:
> pg 3 :
> The SCAPE model[3] opened wide possibilities in the field. ??? which field ?
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We added the clarification: "in the field of human shape estimation".

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> has been strongly influence --> influenced
> no human body dimensions are computed of estimated.--> from the estimated
> model
> do not require that level of detail. --> such level of detail
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We have corrected these errors.

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> 4) The authors used the SMPL generative human body model, developed in 2015
> to synthesize 3D human meshes.
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Yes, we do. Here we are not able to grasp the intention of the reviewer.

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> 5) pg 13, it is not accurate to say "The automatic annotation process took
> approximately 1 h 47 min in an enhanced modern personal computer." What are
> the characteristics of the computer ?
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> 6) The description of the bloc diagram of the CNN Calvis-Net depicted in fig
> 6, page 12, is missed; The proposed CNN architecture is not convincing. No
> detail has been provided about the relation and the functioning of the
> weight, the convolutional and the FC layers.

> 7) The results are far from being convincible. Experimental results are with
> limited 3D articulated meshes. It would be better if some quantified
> indicators are used (Accuracy assessment).

We take the point, and we think our new version is much clearer.

Reviewer 2

> 1). It is not clear how 8 images can be used to train CNN with ground trut
> of 3 measures.

> 2). It is not clear how novel the proposed measurement calculation is.

> 3). there is no comparison with existing methods

We read the objection of the reviewer as a complaint about the story line. Our restructuring should have remedied this.

We now emphasize the real reason why we want the reduction axioms: as a tool for *compositional analysis* of the epistemic effects of many different types of information-carrying event. It is actually surprising that this is possible. We see this compositional analysis as the heart of dynamic epistemic logic, not just a negotiable 'luxury' for some cases. As a side-effect, such axioms 'reduce' a complete dynamic epistemic language to its static epistemic base – but this is a consequence of the analysis, not the main motivation. Of course, as we now also stress, this reduction does have another interesting interpretation by itself: the static language must be rich enough to 'pre-encode' the effects of the relevenat class of informational events. This is a requirement on optimal language design which is also known from other areas of logic, such as conditional logic vis-a-vis belief revision. Finally, and we may have given the wrong impression that *this* was our main point: the reduction analysis tends to make for simpler completeness proofs, and some direct borrowing of known properties of the static base into the full dynamic system.

What we also find interesting is how this methodology puts us on the track of new epistemic operators that have not been isolated as important before, even though they were there just below the surface. In particular, relativized common knowledge seems a new notion of independent interest.

Reviewer 3

> Overall, the paper is very well written. However, the main contributions are
> not clear.

> The segmentation method is also too constrained as it requires the
> shapes to be a pre-defined position.

> The paper should also try and compare with other existing segmentation
> algorithms. This is a very well studied topic, see for example:
> https://scholar.google.com.au/scholar?
> q=3d+mesh+segmentation&hl=en&as_sdt=0&as_vis=1&oi=scholart

While we appreciate the reviewer recommendation, it has to be said that we do not focus on a comparative analysis of segmentation algorithms. Nevertheless, we do think that we could perform such as analysis on future works.

Reviewer 4

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> Thank you for sharing the generated database.
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We are glad to contribute and thank the reviewer for this encouraging, motivational and constructive comment.

Reviewer 5

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> The main contribution of this paper is to computing the human body dimensions
> based on the geometric methods and meanwhile to testing the CNN-based
> regression method. However, I have the following concerns:
> 1. The experimental details related to the CNN-based method are missing. The
> detailed hyper-parameters used for CNN training and testing are missing.

> 2. There is no comparisons with other related methods. The error of 12mm
> achieved in this paper has no reference score.

> 3. Overall, I think the problem of 3D human shape measurement is a important
> task, and the geometric method used in this paper has a certain contribution
> to this task.

> 4. The presentation of this paper is not smooth.
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The referee is right: the completeness proof of PDL is not that complicated. More than that: it is very elegant, and we have nothing to add to it.

The new presentation makes our intentions clearer, we think. The completeness proof of the logic of epistemic action models as provided by Baltag, Moss and Solecki is complicated. Our method of reduction axioms allows us to 'lift' the elegant PDL completeness proof to the logic of epistemic action models. And again: the reduction axioms that we find are interesting in their own right as we are after compositional analysis of epistemic effects of events observed in groups of agents, and earlier dynamic epistemic logic were simply unable to provide that methodology for common knowledge.

The 'point' of LCC is to show that 'really' epistemic PDL, contrary to what the designs of intricate logics of communication and knowledge suggest, is all that is needed for a rich logic of communication and change. So if anything, this is a new vindication of PDL, rather than a criticism of it.

It has to be said, though, that we do not provide a knock-down argument why this full system is *needed* for our style of analysis. We have not been able to find anything weaker that works, but our list of open questions at the end does ask if other weaker 'solutions' exist.

Reviewer 7

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> Very interesting work with a recent bibliographic study, nevertheless the
> experimentation part lacks comparative study with the existing one and
> especially in term of algorithmic complexity
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We hope that our new version is much more focussed.

The author is right on one further point. We have chosen to add events that change the world to the standard DEL-framework, since we feel that this increases the scope with little effort, while still operating with the same methodology. But it is clear that this is orthogonal to our main points. We have considered taking it out (but that seemed a pity), or to put it into a separate section: but that would lead to too much duplication. Our solution now is to leave the story combined, while pointing out to the reader who so prefers that a 'change-free' reading of our main notions and results can be obtained by merely omitting all our technical talk about substitutions.

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> As far as style is concerned, the work "perspicuous" appears three
> times on the first page; once in the abstract, and twice in the
> second paragraph. While perspicuous is a delicious word, it is rare
> enough that seeing it three times makes one frown.
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We have rephrased things using a wider repertoire of adjectives.

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> Also, typo on p.3,
> first paragraph of 2.1: "PLaza" instead of "Plaza".
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Corrected.

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>>REFEREE REPORT 2<<
> List of typos, corrections and comments:
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> 1. One of the key definitions ...
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The referee is correct that this definition was wrong.

The following change solves the problem. We now take the transitive closure rather than the *reflexive* transitive closure in the definition. We have adapted the rest of the text including the axioms accordingly, and now also the remark on page 5 is correct.