# Evaluating gesture-generation in a large-scale open challenge: The GENEA Challenge 2022

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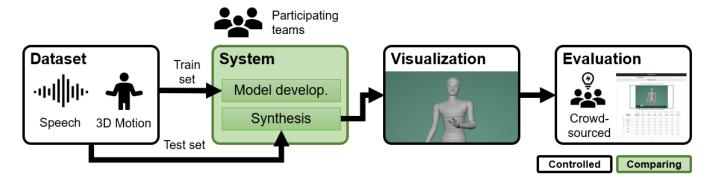


Fig. 1. Overview of the GENEA Challenge. We controlled the dataset, visualisation, and evaluation in order to compare different gesture-generation approaches in a fair and systematic way. The dataset includes speech audio, time-aligned speech text transcription, and speaker identity as input modalities and 3D body motion as the output modality. For the synthesised motion from the participating teams, video stimuli were rendered by a shared visualisation pipeline and evaluated jointly in crowdsourced user studies.

This paper reports on the second GENEA Challenge to benchmark data-driven automatic co-speech gesture generation. Participating teams used the same speech and motion dataset to build gesture-generation systems. Motion generated by all these systems was rendered to video using a standardised visualisation pipeline and evaluated in several large, crowdsourced user studies. Unlike when comparing different research papers, differences in results are here only due to differences between methods, enabling direct comparison between systems. The dataset was based on 18 hours of full-body motion capture, including fingers, of different persons engaging in a dyadic conversation. Ten teams participated in the challenge across two tiers: full-body and upper-body gesticulation. For each tier, we evaluated both the human-likeness of the gesture motion and its appropriateness for the specific speech signal. Our evaluations decouple human-likeness from gesture appropriateness, which has been a difficult problem in the field.

The evaluation results are a revolution, and a revelation. Some synthetic conditions are rated as significantly more human-like than human motion capture. To the best of our knowledge, this has never been shown before on

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a high-fidelity avatar. On the other hand, all synthetic motion is found to be vastly less appropriate for the speech than the original motion-capture recordings. We also find that conventional objective metrics do not correlate well with subjective human-likeness ratings in this large evaluation. The one exception is the Fréchet gesture distance (FGD), which achieves a Kendall's tau rank correlation of around -0.5. Based on the challenge results we formulate numerous recommendations for system building and evaluation.

#### CCS Concepts: • Human-centered computing $\rightarrow$ Human computer interaction (HCI).

Additional Key Words and Phrases: animation, gesture generation, embodied conversational agents, evaluation paradigms

## 1 INTRODUCTION

This paper is concerned with systems for automatic generation of nonverbal behaviour, and how these can be compared in a fair and systematic way in order to advance the state-of-the-art. This is of importance as nonverbal behaviour plays a key role in conveying a message in human communication [McNeill 1992]. A large part of nonverbal behaviour consists of so called co-speech gestures, spontaneous hand and body gestures that relate closely to the content of the speech [Bergmann et al. 2011], and that have been shown to improve understanding [Holler et al. 2018]. Embodied conversational agents (ECAs) benefit from gesticulation, as it improves interaction with social robots [Salem et al. 2011] and willingness to

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cooperate with an ECA [Salem et al. 2013]. Knowledge of how and when to gesture is also needed. This can for example be learnt from interaction data; see, e.g., Jonell et al. [2020a].

Synthetic gestures used to be based on rule-based systems, e.g., Cassell et al. [2001]; Salvi et al. [2009]; see Wagner et al. [2014] for a review. These are gradually being supplanted by data-driven approaches, e.g., Bergmann and Kopp [2009]; Chiu et al. [2015]; Levine et al. [2010], with recent work [Alexanderson et al. 2020; Kucherenko et al. 2020; Yoon et al. 2020, 2019] showing improvements in gesticulation production for ECAs. For more in-depth reviews of recent data-driven approaches see Liu et al. [2021]; Nyatsanga et al. [2023].

Unfortunately, results from different gesture-generation studies are typically not directly comparable [Wolfert et al. 2022]. Studies usually rely on different data sources to train their models. The visualisations of their generated gestures often have different avatars and production values, which can affect the perception of the gestures. On top of this, studies make use of a variety of different methodologies to evaluate the gestures. All these differences are, however, external to the actual methods that drive the gesture generation.

In this paper, we report on the GENEA<sup>1</sup> Challenge 2022. The aim of the challenge is not to select the best team – it is not a contest, nor a competition - but to be able to directly compare different approaches and outcomes. By providing a common dataset for building gesture-generation systems, along with common evaluation standards and a shared visualisation procedure, we control for all other sources of variation except the system-building itself. Our setup is unique to the field of gesture generation, making it possible to reliably assess and advance the state of the art, and to measure the gap between it and natural co-speech gestures. Comparing different methods and their performance also helps identify what matters most in gesture generation, and where the bottlenecks are. In particular, the results make it abundantly clear that natural-looking data-driven gesture motion is achievable today, but that synthetic gestures are much less appropriate for the accompanying speech than the ground-truth motion is. The results also show that most objective metrics are not informative about the perceived humanlikeness of the generated gestures.

Challenge participants benefit by working on the same problem together with researchers interested in the same topic, strengthening the research community, and get an opportunity to compare their systems to other competitive systems in a large and carefully-executed joint evaluation. They also work on and contribute towards a standardised evaluation setup which encourages future benchmarking and reproduction of results. Participants are required to write down their methods, results and experience in a system paper to be presented in conjunction with the challenge itself, giving them a chance to publish their work at the ACM International Conference on Multimodal Interaction (ICMI). Our concrete contributions are:

 Four large-scale user studies that jointly evaluate a large number of gesture-generation models on a common dataset using a common 3D model and rendering method.

- (2) A subjective evaluation that successfully disentangles motion human-likeness from its appropriateness for the associated speech.
- (3) To the best of our knowledge, the first results that identify synthetic gesture motion that surpasses the human-likeness of good motion-capture data on a high-fidelity avatar.
- (4) The first clear evidence that synthetic gestures are much less appropriate for the specific speech than natural motion is, even when controlling for the human-likeness of the motion.
- (5) A validation study of many objective metrics for predicting motion human-likeness, finding that all metrics except the Fréchet gesture distance (FGD) are statistically indistinguishable from chance prediction.
- (6) Providing open code and high-quality data in the spirit of open source and reproducible research. This includes preprocessed multimodal training, validation, and test datasets; the standardised visualisation; submitted motion and video stimuli; a large number of subjective responses from the studies; and evaluation and analysis code.
- (7) Bringing researchers together in order to advance the stateof-the-art in gesture generation, and enabling future research to compare and benchmark against systems and stimuli from the challenge.

Materials derived from the challenge are publicly available at young woo-yoon.github.io/GENEAchallenge2022.

This paper is an extension of a previously published conference paper on the challenge [Yoon et al. 2022], adding more comprehensive information and analysis, experiments on objective metrics, and a more in-depth discussion of challenge findings, recommendations, and limitations. The remainder of this paper first (in Sec. 2) briefly discusses current gesture-evaluation practices and how challenges can help overcome their shortcomings. We then describe the challenge task and dataset in Sec. 3, followed by a breakdown of the challenge tiers and participating teams in Sec. 4. In Sec. 5 we describe the design of the challenge evaluation, with results of the various evaluations recounted in Sec. 6 and discussed in Sec. 7. Each of these three sections detail both the core subjective evaluation as well as the objective metrics we computed, in that order. We round off by discussing limitations of the challenge (in Sec. 8) and summarising its conclusions and implications (in Sec. 9).

## 2 RELATED WORK

2.1.1 Issues with prior evaluations and evaluation practices. Most works that propose new gesture-generation methods incorporate an evaluation to support the merits of their method. Human gesture perception is highly subjective, and there are currently no widely accepted objective measures of gesture perception. Instead, human assessment via careful user studies is the gold standard in the field. However, previous subjective evaluations have several drawbacks, as reviewed in Wolfert et al. [2022]. Some major issues are the coverage of systems being compared and the scale of the studies. Like in Alexanderson et al. [2020]; Kucherenko et al. [2021a, 2020]; Sadoughi and Busso [2019], proposed models are at most compared to one or two prior approaches (often a highly similar baseline) or possibly only to ablated versions of the same model. A large number

 $<sup>^1</sup>$ GENEA stands for "Generation and Evaluation of Non-verbal Behaviour for Embodied Agents".

of studies do not compare their outcomes with other methods at all, let alone other systems trained on the same data. This creates an insular landscape where particular model families are only applied to particular datasets, and never contrasted against one another.

As for scale, large evaluations are expensive, and studies may not be able to recruit enough participants, thus leaving the differences between many pairs of studied systems unresolved and not statistically significant (cf. Yoon et al. [2020, 2019]). Questionnaires, which are one popular evaluation methodology (cf. Bergmann et al. [2010]; Ishi et al. [2018]; Ishii et al. [2018]; Salem et al. [2012]; Shimazu et al. [2018]; Yoon et al. [2019]) demand a lot of time and cognitive effort from test participants, and may be infeasible to scale up to bigger studies. In addition, items used in questionnaires differ across studies and the set of questions used is often not standardised. Evaluations sometimes also fail to anchor system performance against natural ("ground truth") motion from test data held out from training (e.g., Ishii et al. [2018]; Le and Pelachaud [2012]; Salem et al. [2012]).

Studies also differ in the dataset they train on (e.g., Ishii et al. [2018]; Le and Pelachaud [2012]; Salem et al. [2012]) and in how the motion is visualised. For the latter, some prior work displays motion through stick figures (e.g., Kucherenko et al. [2019]; Wolfert et al. [2019]), or applies it to a physical agent (e.g., Ishi et al. [2018]; Salem et al. [2012]). Neither of these may allow the same expressiveness or range of motion as a high-quality 3D-rendered avatar as seen in, e.g., Alexanderson et al. [2020]; Kucherenko et al. [2020]).

2.1.2 Benefits of challenges in other fields. Other fields have done well using challenges to standardise evaluation techniques, establish benchmarks, and track and evolve the state of the art. For example, the Blizzard Challenges have since their inception in 2005 (see Black and Tokuda [2005]) helped advance our sister field of text-to-speech (TTS) technology and identified important trends in the specific strengths and weaknesses of different speech-synthesis paradigms [King 2014]. These challenges are defined by the use of common data and evaluation, and their open participation to both academia and industry. More specifically, participants are provided a common dataset of speech audio and associated text transcriptions, which they use to build a system that generates synthetic speech audio. After the participants submit their systems, the resulting generated speech is subsequently evaluated in a large, joint evaluation, the results of which are provided to the teams. Submitted entries are identified by anonymised labels in official Blizzard Challenge results, but in practice the vast majority of teams identify which label represents their entry in their paper at the Blizzard Challenge Workshop describing the system that they submitted. Data, evaluation stimuli, and subjective ratings remain available after these challenges, and have been widely used both for benchmarking subsequent TTS systems, e.g., Charfuelan and Steiner [2013]; Székely et al. [2012], and in research on the perception of natural and artificial speech, e.g., Govender et al. [2019]; Mittag and Möller [2020]; Möller et al. [2010]; Saratxaga et al. [2016]; Yoshimura et al. [2016]. This has led to the development of new and novel methods, driven by past results, and since participants had access to the same data, significant advances have been made.

Challenges are also actively used in the computer-vision community, for instance for benchmarking purposes. Recent CLIC [Toderici et al. 2020] and NTIRE [NTIRE Challenge organisers 2020] challenges, for example, compared systems for image compression and super-resolution respectively, also incorporating subjective human assessments similar to the challenge described in this paper (although they used a MOS-like setup, which has been found to be less efficient than the side-by-side evaluation methodology we employ [Ribeiro et al. 2015]). This addresses the over-reliance on objective metrics in computer-vision evaluation, which, just like in speech quality and gesture generation, do not always align with human perception. The GENEA Challenge is inspired by these successes of challenges in other fields of study.

In 2020 we organised the first gesture-generation challenge, the GENEA Challenge 2020 [Kucherenko et al. 2021b]. In addition to being an exercise in benchmarking both new [Korzun et al. 2021; Lu et al. 2021; Thangthai et al. 2021] and previously-published [Alexanderson et al. 2020; Kucherenko et al. 2019; Yoon et al. 2019] gesture-generation methods, the results of that challenge have since helped improve gesture-generation benchmarking in other ways as well. Researchers have, for example, used the 2020 visualisation [Teshima et al. 2022; Wang et al. 2021; Zhang et al. 2023], and the objective [Bhattacharya et al. 2021] and subjective [Yoon et al. 2021] evaluation methodologies, as a basis for future research. The data has also been used to benchmark subsequent gesture-generation models [Ferstl et al. 2021; Yazdian et al. 2022], and even for automatic quality assessment [He 2022]. In this paper, we follow up on the 2020 challenge by reporting on the second gesture-generation challenge, the GENEA Challenge 2022. This challenge expands the dataset, the range of behaviour considered, and the number of participating teams, and also improves the visualisation and the evaluation practises, in addition to considering objective metrics together with a large subjective evaluation.

2.1.3 Objective metrics. Given that subjective evaluations are labour intensive, time-consuming, and costly, a large number of different objective metrics have been proposed as automated indicators of gesture-generation performance. Some of these, such as the commonly used [Nyatsanga et al. 2023; Wolfert et al. 2022] average position error (APE) and mean-squared position error (MSE), as well as the "probability of correct keypoints" (PCK) and its extension to semantic relevance gesture recall (SRGR) [Liu et al. 2022b], are used to score the similarity of generated poses to a corresponding recording of human motion. Alternatively, canonical correlation analysis (CCA) can be used [Bozkurt et al. 2015; Lu et al. 2021; Sadoughi and Busso 2019] to quantify the linear (Pearson) correlations between generated and reference poses. These methods are likely to struggle with the stochastic, one-to-many nature of human gestures (there is no single "correct" way to move), leading to high variance.

To accommodate the stochastic nature of motion, statistics such as the average magnitude of motion acceleration and jerk, and distances between motion speed histograms have been used to quantify how similar generated motion is to the distribution of human motion [Kucherenko et al. 2021a]. More recent developments have built on the Freéchet inception distance (FID) from image generation [Heusel et al. 2017] to propose [Ahuja et al. 2020; Yoon et al. 2020] new methods for comparing gesture-motion distributions. These methods were later used by, e.g., Ahuja et al. [2022]; Ao et al. [2022]; Liu et al. [2022b,a]. Beat consistency, which was first proposed for dance motion [Li et al. 2021], has also been used to assess gesture generation [Liu et al. 2022a]. However, few of these works experimentally validate their metrics. In this paper, we use the many conditions and ratings gathered in our user studies to compute and validate five of the above objective metrics for gesture generation.

## 3 TASK AND DATA

The GENEA Challenge 2022 focused on data-driven automatic cospeech gesture generation. Specifically, given a sequence s of input features that describe human speech – which could involve any combination of an audio waveform, a time-aligned text transcription, and a speaker ID – the task is to generate a corresponding sequence  $\hat{g}$  of 3D poses describing gesture motion that an agent might perform while uttering this speech (facial expression is not considered). To enable direct comparison of different data-driven gesture-generation methods, all methods evaluated in the challenge were trained on the same gesture-speech dataset and their motion visualised using the same virtual avatar and rendering pipeline. This is the same core task as in the 2020 challenge, whilst at the same time we changed the dataset (as described below) and refined the evaluation (as detailed in Sec. 5).

#### 3.1 Data

Compared to 2020, we wanted to expand the dataset to include finger motion, lower-body motion, and material from multiple speakers in dyadic interactions. The latter may provide more natural and interesting gestures than the Trinity Speech-Gesture Dataset [Ferstl and McDonnell 2018] used in 2020. We based our challenge on the Talking With Hands 16.2M gesture dataset [Lee et al. 2019], which comprises 50 hours of audio (captured by close-talking directional microphones) and motion-capture recordings of several pairs of people having a conversation freely on a variety of topics, recorded in distinct takes each about 10 minutes long. At the time of the challenge, this was likely the largest dataset of parallel speech and 3D motion (in joint-angle space) publicly available in the English language. We removed parts of the dataset (46 out of 116 takes) that lacked audio or had low motion-capture quality, especially for the fingers. Note that despite the dataset being dyadic by design, the challenge focused on generating one side of the conversation at a time, without awareness of the interaction partner. The data from the interaction partner in each dyad was typically also included in the challenge material, but as a separate recording without providing links between the two. This was the case for both the gesture synthesis and for the subsequent evaluation.

3.1.1 Speech audio and text. Speech data was shared with participants as WAV audio with no additional processing beyond the anonymisation applied by Lee et al. [2019], which replaced many proper nouns with silence. We also provided text transcriptions of the speech, in tab-separated value (TSV) files, and a metadata file with unique anonymous labels for each speaker. The TSV files were created by first applying Google Cloud automatic speech recognition to transcribe the audio recordings, followed by manual review

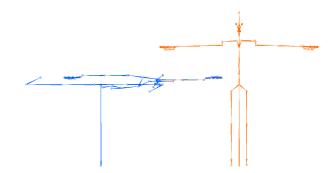


Fig. 2. Visualisations of the default skeletal pose of the data before and after processing. On the left (blue) is the original skeletal pose, as found in the Talking With Hands 16.2M dataset shared by Lee et al. [2019]. On the right (orange) is the transformed skeletal pose (i.e., T-pose) used for the GENEA Challenge 2022.

to correct recognition errors and add punctuation for all parts of the dataset (training, validation, and test).

- 3.1.2 Motion data. Motion data was downsampled from 120 to 30 frames per second and further transformed in two ways:
  - (1) We updated the default skeletal definition relative to which all motion data is defined, from what appeared to be a contorted and arbitrary definition, to a standard "T-pose" shown in Fig. 2. The T-pose is an animation industry standard, whereby all joint rotation values are described in relation to a T-shaped skeleton. This standard is recognised across the animation industry and is widely adopted by existing 3D digital content creation software like Blender and Maya. This is further demonstrated by it often being the required pose for a 3D skeleton, in order to transfer the motion of one character onto another during motion re-targeting. Furthermore, the T-pose is expected to have better mathematical properties due to its symmetry and shape. In particular, the pose more closely resembles the poses found in the motion-capture data. As a result of this, most of the joint rotation values are expected to be closely distributed around 0. Consequently, this would reduce the risk of phase wrapping and gimbal locking across the skeleton, lending itself to smoother behaviour and interpolation in the Euler-angle space. This in turn leads to data that is more numerically stable, making it more practical for training machine-learning models. The data was recomputed to match a T-pose using motion re-targeting inside Motion-Builder, retaining as much of the original visual quality as possible, whilst ensuring that the data had no discontinuities (e.g., at rotations near 180°). We found that this transformation substantially improved the output of the baseline system UBA in Sec. 4.2.
  - (2) We standardised the position and orientation of speakers in all takes. Originally, each take would have the two speakers occupy two locations and face each other. We standardised this on a per-take basis such that all speakers, on average, face the same direction, and occupy the same location. More technically, in a right-hand xyz Cartesian coordinate system (y-up, z-forward), each speaker is on average positioned at

world origin ([x = 0, y = 0, z = 0]), and on average facing the positive *z*-axis (a directional vector [x = 0, y = 0, z = 1]). Averaging was done for each take separately after taking 250 equidistant samples of the hips position and orientation, and then using linear-algebra operations to correct for the difference between the original values and the standardised ones. This change was made to streamline data visualisation and to remove potential confusion due to different absolute positions and orientations across different takes.

The transformed motion data was shared with participants in the Biovision Hierarchy (BVH) format.

3.1.3 Data splits. The challenge data was split into a training set (18 h), a validation set (40 min), and a test set (40 min), with only the training and validation sets initially shared with participating teams. All these data subsets are publicly available via the Zenodo data release at doi.org/10.5281/zenodo.6998230. The validation and test data each comprised 40 chunks (contiguous excerpts approximately one minute long), to promote generation methods that are stable over long segments of speech, and was restricted to takes with finger-motion tracking for the chosen speaker. Some recordings with finger-capture data were excluded from consideration due to poor motion-capture quality, based on visual inspection of a short sample from each recording. The validation data was intended for internal benchmarking during development, so participants were allowed to train their final submitted models on both training and validation data if they wished.

3.1.4 Usage rules. Teams were allowed to only train on a subset of the data and were allowed to enhance the data they trained on however they liked, for instance by manual annotation or by postprocessing the speech and the motion. They were also allowed to make use of additional speech data (audio and text) from other sources, and models derived from such data, e.g., BERT Devlin et al. [2018] and wav2vec Baevski et al. [2020]. However, it was not permitted to use any other motion data, nor any pre-trained motion models, other than what the organisers provided for the challenge. Otherwise, the result would be likely to strongly depend on how much data each team can get access to (as has been the case in many Blizzard Challenges in speech synthesis), which is not an interesting scientific conclusion to replicate.

#### 4 SETUP AND PARTICIPATION

The challenge began on May 16, 2022, when speech-motion training data was released to participating teams. Test inputs (WAV, TSV, and speaker ID, but no motion output) were released to the teams on June 20, with teams required to submit BVH files with their generated gesture motion for these inputs by June 27, in the same format as that used by the challenge training data. Manual tweaking of test inputs or the output motion was not allowed, since the idea was to evaluate synthesis performance in an unattended setting. As a precondition for participating in the evaluation, teams agreed to submit a companion paper describing their system for review and possible publication at ACM ICMI. Evaluations took place only after the generated motion was submitted by all teams.

#### 4.1 Tiers

The challenge evaluation was divided into two tiers, one for fullbody motion and one for upper-body motion only. Each tier had its own reasons for being included. On the one hand, the data comprises recorded full-body motion from conversational interactions. It can furthermore be argued that human embodied conversation uses the full body. Also, generating full-body behaviour seems like a harder problem, since it represents a higher-dimensional probability distribution which is more difficult to learn from a statistical perspective. Therefore, if full-body generation is solved, restricted versions of the problem can be expected to be solved as well. On the other hand, it is debatable to what extent the motion of the lower body whilst speaking constitutes co-speech gestures that depend on the speech, over other aspects such as proxemics and stance in response to the other parties in a conversation (which is data that was not provided to challenge participants this time). As a result, including lower-body motion may add unnecessary complexity to the gesture generation problem, and act as a distraction when evaluating the quality of generated gestures. Focusing on the upper body also is more consistent with earlier evaluations of co-speech gesture generation, such as the GENEA Challenge 2020 [Kucherenko et al. 2021b]. Because it is not clear which perspective to apply, our evaluation included one tier each for full-body and upper-body motion. Teams could enter motion into either tier, or into both, but could only make one submission per tier. Teams that entered into both tiers were allowed to submit different motion (BVH files) to each tier, if they wished. Both tiers used the same training data but differed in which parts of the avatar that were allowed to move, and in the camera angle used for the video stimuli in the evaluation, as follows:

Full-body tier In this tier, the entire virtual character was free to move, including moving around in space relative to the fixed camera. Motion was visualised from an angle facing the character that showed most of the legs, but not where the feet touched the ground. This perspective was chosen to show as much as possible of the character, whilst obscuring foot penetration or foot sliding artefacts from view, since these artefacts arguably do not relate to co-speech gestures, yet they may influence ratings if visible. An example of this camera perspective can be seen in Fig. 3a.

Upper-body tier In this tier, the virtual character used a fixed position and a fixed pose from the hips down, with only the upper body free to move. Motion was visualised from a camera angle facing the character, cropped slightly below the hips, such that the hands always should remain in view. Any motion of the lower-body joints in submitted BVH files was ignored by the visualisation. This camera perspective is shown in Fig. 3b.

## 4.2 Baselines and participating teams

The challenge evaluation featured three types of motion sources: natural motion capture from the speakers in the database, baseline systems based on open code, and submissions by teams participating in the challenge. We call each source of motion in a tier a condition (not a "system", since not all conditions represent motion synthesised by an artificial system). Each condition was assigned a unique

Table 1. Conditions participating in the evaluation. Conditions are ordered based on their median human-likeness scores from higher to lower (see Table 2). For teams (but not for baselines) we cite the respective system-description paper, if available. The following abbreviations were used: AR for "Auto-regression", CNN for "Convolutional Neural Network", RNN for "Recurrent Neural Network", SA for "Neural self-attention" (e.g., Transformers), GANs for "Generative adversarial networks or adversarial loss terms", VAEs for "Variational auto-encoders", MG for "Motion graphs", Frame-wise for "Generating output frame-by-frame", Stoch. output for "Stochastic output" (different output possible even if the inputs are the same), and Smoothed for "Smoothing was applied".

	Pe	r-tier	In	puts u	sed	Hands			7	Гесhnic	lues used	Frame-	Stoch.	Smoo-
Baseline or team name	1	abel	Aud.	Text	Sp. ID	Fixed	AR	RNNs	SA	VAEs	Other	wise	output	thed
GestureMaster [Zhou et al.]	FSA	USQ	1	1	/						Hand-crafted rules, MG			<b>√</b>
Forgerons [Ghorbani et al.]	FSC	USO	1			1	1	1		✓		1	✓	
DeepMotion [Lu and Feng]	FSI	USJ	1	✓		1	1		1	/	CNNs	1	✓	
DSI [Saleh]	FSF		1				1	1	1					
UEA Digital Humans [Windle et al.]	FSG	USM	1	✓	✓			1				1		
ReprGesture [Yang et al.]		USN	1	✓	1	1	1	1	1	✓	CNNs, GANs			1
IVI Lab [Chang et al.]	FSH	USK	1	✓	✓	1	1	1				1	✓	1
FineMotion [Korzun et al.]	FSD		1	✓			1	1				1		1
Murple AI lab (no paper submitted)	Not	revealed	1				1	1			Normalising flows	1	✓	
Text-only baseline	FBT	UBT		✓		1	1	1				1		1
Audio-only baseline		UBA	1					1				1		1
TransGesture [Kaneko et al.]		USL	1			✓	1	✓				1		✓

three-letter label or condition ID, where the first character signifies the tier, with F for the full-body tier and U for the upper-body tier.

Natural motion was labelled FNA in the full-body tier and UNA in the upper-body tier (NA for "natural"). These conditions can be seen as a top line, and surpassing their performance essentially means outperforming the dataset itself, subject to limitations due to the motion capture and visualisation (discussed in Secs. 7.1 and 8).

The natural top line can be contrasted against the two baseline systems included in the challenge, which represent previously published gesture-generation methods with free and open code, adapted to run on the 2022 challenge training data. These two baselines were:

Text-based baseline (FBT/UBT) This motion was generated by the gesture-synthesis approach from Yoon et al. [2019] (which takes text transcriptions with word-level timestamps as the input) but adapted to joint rotations as described in [Kucherenko et al. 2021b]. A neural sequence-to-sequence architecture is used, where an encoder processes a sequence of speech words and a decoder outputs a sequence of human poses. Motion from this baseline used a fixed lower body but was included in both tiers, as conditions FBT and UBT (B for "baseline" and T for "text"). The code is publicly available on-

Audio-based baseline (UBA) This motion was generated by the Audio2Repr2Pose motion-synthesis approach of Kucherenko et al. [2019], which only takes speech audio into account when generating output, adapted to joint rotations as described in [Kucherenko et al. 2021b]. This model uses a chain of two neural networks: one maps from speech to pose representation and another decodes the representation to a pose, generating motion frame-by-frame by sliding a window over the speech input. Motion from this baseline was only included in the upper-body tier, as condition UBA (A for "audio"). The code is publicly available online at github.com/geneaworkshop.

These are the same baselines as in the GENEA Challenge 2020. They were included to track the progress of the field and to provide continuity between different years of the challenge.

Separate from top lines and baselines, a total of 10 teams participated in the GENEA evaluation, with 8 entries (a.k.a. submissions) to the full-body tier and 8 entries to the upper-body tier. Submissions were labelled with the prefix FS and US (S for "submission") depending on the tier, followed by a single character to distinguish between different submissions in the same tier. In particular, challenge entries to the full-body tier were labelled FSA-FSI, and entries to the upper-body tier were labelled USJ-USQ. Condition FSE was withdrawn before the evaluation. These labels are anonymous and have no relationship to team identities, but teams were free to reveal their label(s) in papers describing their systems, if they wished.

Table 1 lists the baselines and participating teams, with basic information about their respective approaches and references to their system-description papers. All teams but one published a paper about their system, and all of the published papers chose to reveal the label(s) of their submitted systems. We have therefore included that label information from their papers in Table 1.

## 5 EVALUATION

line at github.com/youngwoo-yoon/Co-Speech\_Gesture\_GenerationWe conducted a large-scale, crowdsourced, joint evaluation of gesture motion from the 10 full-body conditions and 11 upper-body conditions (listed in Table 1) in parallel using a within-subject design (i.e., every rater was exposed to and evaluated all conditions in each tier). The systems were evaluated in terms of the human-likeness of the gesture motion itself, as well as the appropriateness (a.k.a. specificity) of the gestures for the given input speech. The central difference from other gesture-generation evaluations is that all systems in our evaluation used the same motion data, the same visualisation/embodiment, and were rated together using the same evaluation methodology; only the motion-generation systems differed between the different entries that were compared. This allows the performance of systems to be compared directly, and the design

aspects that influence performance can be traced more efficiently than in most previous publications. The subjective evaluation used an entirely crowdsourced approach, with attention checks used to exclude participants that were not paying attention, as detailed in Sec. 5.5. The remainder of this section describes the experiments we performed. Results of the subjective evaluation are subsequently presented in Sec. 6 and discussed in Sec. 7.

Although the aim of the challenge is to quantify how natural and appropriate motion appears to human observers, we have also seized the opportunity to compute a number of objective metrics of motion quality on the motion materials in the evaluation. The design of that experiment is described in Sec. 5.6, with results reported in Sec. 6.4 and discussed in Sec. 7.3. We see this primarily as an evaluation of the metrics themselves, and not as an evaluation of the different conditions in the challenge.

## Subjective evaluation structure

For each tier, two orthogonal aspects of the generated gestures were evaluated (with one study per aspect and tier):

Human-likeness Whether the motion of the virtual character looks like the motion of a real human, controlling for the effect of the speech. We sometimes use "motion quality" as a synonym for this.

**Appropriateness** (a.k.a. "specificity") Whether the motion of the virtual character is appropriate for the given speech, controlling for the human-likeness of the motion.

More details about these evaluations are provided in Sections 5.3 and 5.4 below, respectively.

Although an interesting question for a multispeaker dataset, we did not attempt to evaluate the appropriateness/specificity of the gesture motion style to different individuals in the database, since the data is too imbalanced to allow such an evaluation. Additionally, even though the speech and motion in the challenge comes from joint full-body motion capture of dyadic interactions with separate close-talking microphones for each speaker, the challenge only considered generating one side of the conversation, without awareness of the other party in the interaction (neither for the synthesis, not for the evaluation), in order to reduce problem complexity.

## 5.2 Stimuli

- 5.2.1 Speech segment selection. The test set was deliberately made large to make it difficult to overfit to specific speech being evaluated. Like the GENEA Challenge 2020 and the Blizzard Challenges, not all test-set motion was included in the subjective evaluation. From the 40 test-set chunks we selected 48 short segments of test speech and corresponding test motion to be used in the subjective evaluations, based on the following criteria:
  - (1) Segments should be around 8 to 10 seconds long, and ideally not shorter than 6 seconds.
  - (2) The character should only be speaking, not passively listening, in the segments. (No turn-taking, but backchannels from the interlocutor were OK.)
  - (3) Segments should not contain any parts where Lee et al. [2019] had replaced the speech by silence for anonymisation.

- (4) Segments should be more or less complete phrases, starting at the start of a word and ending at the end of a word, and not end on a "cliffhanger". A small margin was permitted towards the end of segments.
- (5) Finally, recorded motion capture in the segments (i.e., the FNA motion) should not contain any significant artefacts such as whole-body vibration or hands flicking open and closed due to poor finger tracking.

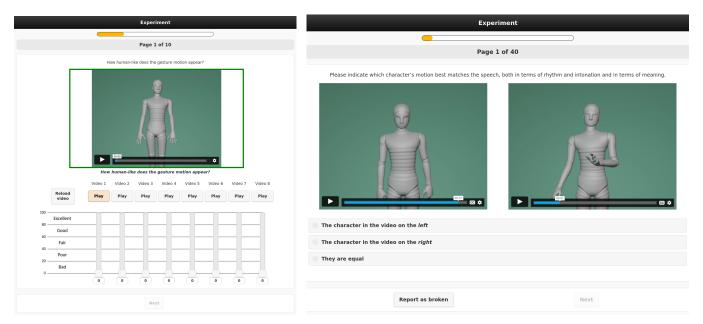
The last item does not imply that the motion capture was perfect or completely natural for all segments in the evaluation, since the finger-tracking quality throughout the database does not allow our evaluations to reach that standard. It merely means that the level of finger-tracking quality in the stimuli was consistent with the better parts of the source material from Lee et al. [2019].

The 48 segments we selected were between 5.6 and 12.1 seconds in duration and on average 9.5 seconds long. Audio was loudness normalised to -23 dB LUFS following EBU R128 [European Broadcasting Union 2020] to maintain a consistent listening volume in the user studies.

5.2.2 Visualisation. We used the same virtual avatar (shown in Fig. 3) in all rendered videos during the challenge and the evaluation. The avatar had 56 joints (full body including fingers) and was designed to be gender neutral and omit eyes or mouth to help evaluators focus on the rest of the body instead. All teams had access to the official visualisation and rendering pipeline during the system-building phase, in the form of code, a portable Docker container, and a web server to which BVH files could be submitted to be rendered as video. Participants could send a 30-fps BVH file to the visualisation server, and these files would then be processed as quickly as possible into videos visualising the motion on the avatar. The visualisation server code is open sourced and publicly available at github.com/TeoNikolov/genea\_visualizer/, whilst the rendered stimulus videos are available at doi.org/10.5281/zenodo.6997925. The visualiser code was available to the participants during the challenge, and they were free to use it to host their own servers if they wished. The final rendered stimuli used a resolution of 1440×1080.

## 5.3 Human-likeness evaluation

The human-likeness evaluation of the GENEA Challenge 2022 closely followed the human-likeness evaluation in the GENEA Challenge 2020 [Kucherenko et al. 2021b], by presenting multiple motion examples in parallel and asking the subject to provide a rating for each one. The human-likeness evaluation was based on the HEMVIP (Human Evaluation of Multiple Videos in Parallel) methodology [Jonell et al. 2021], where multiple motion examples are presented in parallel and the subject is asked to provide a rating for each one. All stimulus videos on the same page (a.k.a. screen) of the evaluation corresponded to the same speech segment but different conditions. The advantage of the HEMVIP method is that differences in rating between the different conditions can be analysed using pairwise statistical tests, which helps control for variation between different subjects and different input speech segments (as seen in the results in Sec. 6.1). For a detailed explanation of the evaluation interface we refer the reader to Jonell et al. [2021], which introduced and



(a) Human-likeness interface (HEMVIP) and full-body video

(b) Appropriateness interface and upper-body videos

Fig. 3. Screenshots of the evaluation interfaces used in the studies, also showing the camera perspectives used by the tiers.

validated the evaluation paradigm for gesture-motion stimuli. Code is provided at github.com/jonepatr/hemvip/tree/genea2022/.

Each evaluation page asked participants "How human-like does the gesture motion appear?" and presented eight video stimuli to be rated on a scale from 0 (worst) to 100 (best) by adjusting an individual GUI slider for each video. An example of the evaluation interface can be seen in Fig. 3a. Like in [Jonell et al. 2021; Kucherenko et al. 2021b], the 100-point rating scale was anchored by dividing it into successive 20-point intervals with labels (from best to worst) "Excellent", "Good", "Fair", "Poor", and "Bad". These labels were based on those associated with the 5-point scale used in from the Mean Opinion Score ITU standard [International Telecommunication Union, Telecommunication Standardisation Sector 1996] for audio quality evaluation. Since it has been found that speech and gesture perception influence each other [Bosker and Peeters 2021] and can confound motion evaluations [Jonell et al. 2020a], the videos seen by participants in these human-likeness evaluations (although they all corresponded to the same speech input and had the same length) were completely silent and did not include any audio. This way, ratings can only depend on the motion seen in the videos, and not on their appropriateness for the speech.

The test was preceded by a screen with instructions, which the participants would read. Then, each subject completed one training page showing a fixed set of videos with different motion, to familiarise participants with the task and what the stimuli would look like, before starting the study in earnest. The training phase was followed by 10 pages of ratings for the evaluation. Responses given on the training page were not included in the analysis. The evaluation was balanced in exactly the same way as in [Kucherenko et al. 2021b], such that each segment appeared on pages 1 through 10

with approximately equal frequency across all participants (segment order), and each condition was associated with each slider with approximately equal frequency across all pages (condition order). For any given participant and study, each of the 10 pages would use a different speech segment. Every page in the evaluation contained one stimulus video from condition FNA/UNA. This was used to help calibrate evaluators' ratings and keep them consistent throughout the test. Since motion-capture data projected onto a virtual character may not necessarily be perceived as perfectly natural, there was no requirement to rate the best motion as 100. After completing the rating pages, but before submitting the study, participants filled in a short questionnaire to gather broad, anonymous demographic information, the results of which are presented in Sec. 5.5.

## 5.4 Appropriateness evaluation

The appropriateness evaluation was designed to assess the link between the motion and the input speech, separate from the intrinsic human-likeness of the motion. In the previous GENEA Challenge, appropriateness was evaluated using a HEMVIP-based rating study very similar to that for human-likeness, except that speech audio was included in the videos. Test takers were asked to ignore the motion quality and only rate the appropriateness of the motion for the speech [Kucherenko et al. 2021b]. Unfortunately, that evaluation was not altogether successful, since their *mismatched* condition M – which paired natural motion segments with unrelated speech segments, intended as a bottom line – attained the second-highest appropriateness rating, above all synthetic systems. This suggests a significant interaction between the perceived human-likeness of a motion segment and its perceived appropriateness for speech. That interaction acted as a confounder in their study, with the result that

all systems ranked below natural-looking motion unrelated to the speech, intended as a bottom line in terms of appropriateness.

For the GENEA Challenge 2022, we decided to evaluate motion appropriateness for speech in a different way. Our design goal for the 2022 challenge was to assess appropriateness whilst controlling for the human-likeness of the motion in an effective way. To do so, we took the idea of motion mismatching like in Jonell et al. [2020a] and used it within every condition (not just for the recorded motioncapture data FNA/UNA): On each page, subjects were presented with a pair of videos containing the same speech audio. Both videos contained motion from the same condition and thus had the same overall motion quality, but one was matched to the speech audio and the other mismatched, belonging to unrelated speech. Whether the left or the right video was mismatched was randomised. Subjects were then asked to "Please indicate which character's motion best matches the speech, both in terms of rhythm and intonation and in terms of meaning." In response, they could choose the character on the left, on the right, or indicate that the two were equally well matched ("They are equal", also referred to as equal or a tie). We asked for preferences rather than ratings since there is evidence [Wolfert et al. 2021] that this is more efficient in pairwise comparisons like these. A screenshot of the evaluation interface used for the appropriateness studies is presented in Fig. 3b.

The extent to which test-takers prefer the character with the matched motion reveals how specific the gesture motion is to the given speech: Random motion will result in a 50-50 split, whereas conditions whose motion is more specifically appropriate to the input speech are expected to elicit a higher relative preference for the matched motion. In this type of evaluation, condition M (the mismatched condition) from the 2020 challenge will perform at chance rate, rather than being tied for second highest as in 2020.

To our knowledge, this approach to control for motion quality in subjective evaluations was first piloted in Jonell et al. [2020a], specifically looking at facial gestures. Rebol et al. [2021] used a similar methodology with preference tests to quantify the correlation (essentially, appropriateness) between generated hand and body gestures and their associated speech, which we were not aware of until after conducting our challenge. However, they asked a different question of the users, did not quantify the appropriateness of real human motion, and only used the approach to evaluate a single gesture-generation method.

Concretely, we created the mismatched stimuli by taking the 48 existing speech and motion segments from the evaluation, and permuted the motion in between them such that no motion segment ever remained in its original place. As the 48 different segments did not all have the same length, a longer or shorter segment of motion generally had to be excerpted from the motion chunks (original or generated), so as to match the new speech duration. The starting point of the motion video was always the same as in the respective matched stimulus video (i.e., corresponding to the start of a phrase).

After an instruction page and a training page, each subject evaluated 40 pages with one pair of videos each. This means that subjects watched 80 videos total in each study, the same number of videos as was evaluated in the human-likeness studies (ignoring the training pages in all cases). Each study was balanced such that each

speech segment, condition, and order of the two videos appeared approximately equally many times.

#### Test takers and attention checks

It has recently been found that crowdsourced evaluations are not significantly different from in-lab evaluations in terms of results and consistency [Jonell et al. 2020b]. The challenge therefore adopted an entirely crowdsourced approach, as opposed to, for example, the Blizzard Challenge, which has used a mixed approach. Attention checks were used to exclude participants that were not paying attention. Test takers (a.k.a. subjects) were recruited through the crowdsourcing platform Prolific. We used Prolific's built-in prescreening tools to restrict the pool of test-takers in two ways: i) subjects were required to reside in any of six English-speaking countries, namely Australia, Canada, Ireland, New Zealand, the United Kingdom, and the USA, and ii) subjects were required to have English as their first language.

We conducted four user studies, two for human-likeness and two for appropriateness. A subject could take one or more studies, but could only participate in each study at most once, and could not use a phone or tablet to take the test.

Each study incorporated four attention checks per person, to make sure that subjects were paying attention to the task and remove insincere test-takers. For the human-likeness studies, these attention checks took the form of a text message "Attention! You must rate this video NN" superimposed on the video. "NN" would be a number from 5 to 95, and the subject had to set the corresponding slider to the requested value, plus or minus 3, to pass that attention check. Which sliders on which pages that were used for attention checks was uniformly random, except that no page had more than one attention check, and the natural motion (condition FNA and UNA) was never replaced by an attention check. For the appropriateness studies, the attention checks either displayed a brief text message over the gesticulating character, reading "Attention! Please report this video as broken", or they temporarily replaced the audio with a synthetic voice speaking the same message. Subjects were exposed to two attention checks of each kind. To pass the attention check, participants had to click a button marked "Report as broken" seen in Fig. 3b, forwarding them to the next pair of videos in the evaluation. Since reporting a video as broken avoids having to give a response, it can in theory be used to quickly skip through the test. To help prevent this, we implemented the button such that it becomes clickable after a 5-second delay, after the page is loaded. However, as this does not fully prevent skipping through the test, subjects who used that button more than three times on pages without attention checks were also removed without pay. In all studies, the attention-check messages did not appear until a few seconds into each attention-check video, so that participants who only watched the first seconds would be unlikely to pass the checks.

Subjects who failed two or more attention checks were removed from the respective study without being paid, since Prolific's policies do not allow rejecting a subject on the basis of a single failed attention check. Only the subjects who failed zero or one attention check for a study have been included in our analyses below. Responses to videos used for attention checks were not included in our analyses. Right before submitting their results, subjects also filled in a short questionnaire to gather broad, anonymous demographic information about the population taking the test.

A design goal of the human-likeness studies was that every combination of two distinct conditions should appear on the pages approximately equally often, and at least 600 times (not counting FNA/UNA, which appeared on every page). To meet this goal, we recruited 121 test takers that successfully passed the attention checks and completed the full-body study, and 150 test takers that successfully passed the attention checks and completed the upper-body study. Since the latter study compared 11 conditions instead of only 10, it required more participants to reach the desired number of ratings pairs. Of the 121 test takers in the full-body study, 60 identified as female, 60 as male, and 1 did not want to disclose their gender. The same numbers for the 150 upper-body test takers were 74, 75, and 1, respectively. For the full-body test takers, 2 resided in Australia, 2 in Canada, 3 in Ireland, 110 in the United Kingdom, and 4 in the USA. The upper-body study had 1 participant residing in Australia, 4 in Ireland, 134 in the United Kingdom, and 11 in the USA. The mean age of the test takers was 38 years with a standard deviation of 12 in the full-body study and 40 years with a standard deviation of 13 in the upper-body study. All these participants passed all attention checks, except for one subject in the upper body study, who failed one attention check.

For the appropriateness studies, our design goal was for each condition to receive as many responses per condition as the number of ratings that each condition (aside from FNA/UNA) received in the corresponding human-likeness evaluation. This works out to 880 responses per condition in the full-body studies and 990 responses per condition in the upper-body studies. Because a subject in these studies provided half as many responses as in a human-likeness study (40 vs. 80), the appropriateness studies needed to recruit approximately twice as many test takers. In the end, 247 test takers successfully passed the attention checks in the full-body study, while 304 passed the attention checks in the upper-body study.

Of the 247 participants in the full-body study, 137 identified as female, 107 as male, and 3 did not want to disclose their gender. The same numbers for the 304 upper-body test takers were 127, 173, and 4, respectively. For the full-body test takers, 3 resided in Australia, 13 in Canada, 10 in Ireland, 2 in New Zeeland, 211 in the United Kingdom, and 8 in the USA. The upper-body study had 2 participants residing in Australia, 10 in Canada, 1 in Ireland, 256 in the United Kingdom, and 35 in the USA. The mean age of the test takers was 38 years in both studies, with a standard deviation of 14 for the full-body study and 13 for the upper-body study. All of these passed all attention checks, except for 10 participants in the full-body study and 14 participants in the upper-body study, who each failed one attention check. Each subject in a study contributed 36 ratings to the analyses after removing attention checks, unless they had to skip a page in the rare case of a video failing to load (which occurred approximately 1.6 times per 1000 videos presented).

Test takers were remunerated 6 GBP for each successfully completed human-likeness study. Since the median completion time was 28 minutes each, this corresponds to a median compensation just above 12 GBP per hour. Similarly, the appropriateness studies took a median of 24 or 25 minutes to complete, and earned a reward of

5.5 GBP each, amounting to around 13 GBP per hour. These compensation levels all exceed the UK national living wage and also exceeds the highest living wage quoted by the Living Wage Foundation in the UK. All numbers are as measured by Prolific, which uses the median rather than the mean for these calculations to prevent extreme completion times from skewing the data. Response data from the evaluation and statistical analysis code are provided at doi.org/10.5281/zenodo.6939888

## 5.6 Objective metrics

The main goal of the GENEA challenge is to compare human subjective impressions of the outputs of different gesture-generation systems. We therefore we discourage using the results of automated performance metrics as indicators of the perceptual impressions of different systems. However, since subjective evaluation is costly and time-consuming, it would be beneficial for the field to identify meaningful objective evaluation methods, especially for use during system development. As a step in this direction we therefore considered five objective measures previously used to evaluate cospeech gestures, namely average jerk, average acceleration, distance between gesture speed (i.e., absolute velocity) histograms, canonical correlation analysis, and the Fréchet distance between motion feature distributions. We computed these metrics for each condition in each tier using the complete test sequences, i.e., not only on the motion segments featured in the subjective evaluation. Details on each metric are provided below.

To compare and validate these metrics against our subjective evaluation, we provide results on the rank correlations between subjective and objective metrics in Sec. 6.4.

5.6.1 Average acceleration and jerk. The third time derivative of the joint positions is called *jerk* and can be formulated mathematically as jerk(x) = x'''(t). The average value of the absolute magnitude of the jerk is commonly used to quantify motion smoothness [Kucherenko et al. 2019; Morasso 1981; Uno et al. 1989]. We report average values of absolute jerk (defined using finite differences) averaged across all test motion segments. A perfectly natural system should have average jerk very similar to natural motion.

We also evaluated the same measure, but computed using the absolute value of the acceleration acc.(x) = x''(t) instead of the jerk. Again, we expect natural-looking motion to have similar average acceleration as in the reference data.

5.6.2 Comparing speed histograms. The distance between speed histograms has also been used to evaluate gesture quality [Kucherenko et al. 2019, 2020], since well-trained models should produce motion with similar properties to that of the actor it was trained on. In particular, it should have a similar motion-speed profile for any given joint. This metric is based on the assumption that synthesised motion should follow a similar speed distribution as the ground truth motion. To evaluate this similarity we calculate speed-distribution histograms for all systems and compare them to the speed distribution of natural motion (condition N) by computing

Table 2. Summary statistics of responses from all user studies, with 95% confidence intervals. "M." stands for "matched" and "Mism." for "mismatched". "Percent matched" identifies how often subjects preferred matched over mismatched motion.

(a) Full-body

		Median	Appropriateness					
	]	human-	Nun	n. res	ponses	Percent matched		
ID	]	likeness		Tie	Mism.	(splitting ties)		
FNA	70	∈ [69,71]	590	138	163	$74.0 \in [70.9, 76.9]$		
FBT	27.5	$5 \in [25, 30]$	278	362	250	$51.6 \in [48.2, 55.0]$		
FSA	71	$\in [70, 73]$	393	216	269	$57.1 \in [53.7, 60.4]$		
FSB	30	∈ [28, 31]	397	163	330	53.8 ∈ [50.4, 57.1]		
FSC	53	∈ [51,55]	347	237	295	53.0 ∈ [49.5, 56.3]		
FSD	34	∈ [32, 36]	329	256	302	$51.5 \in [48.1, 54.9]$		
FSF	38	∈ [35, 40]	388	130	359	$51.7 \in [48.2, 55.1]$		
FSG	38	∈ [35, 40]	406	184	319	54.8 ∈ [51.4, 58.1]		
FSH	36	∈ [33, 38]	445	166	262	$60.5 \in [57.1, 63.8]$		
FSI	46	$\in [45, 48]$	403	178	312	$55.1 \in [51.7, 58.4]$		

the Hellinger distance [Nikulin 2001],

$$H(\boldsymbol{h}^{(1)}, \boldsymbol{h}^{(2)}) = \sqrt{1 - \sum_{i} \sqrt{h_{i}^{(1)} \cdot h_{i}^{(2)}}}, \tag{1}$$

between the histograms  $h^{(1)}$  and  $h^{(2)}$ . Lower distance is better.

For both of the objective evaluations above the motion was first converted from joint angles to 3D coordinates. The code for the numerical evaluations has been made publicly available to enhance reproducibility.

5.6.3 Canonical correlation analysis. Canonical correlation analysis (CCA) [Thompson 1984] is a form of linear subspace analysis, and involves the projection of two sets of vectors (here the generated poses and those from FNA/UNA, respectively) onto a joint subspace. CCA has been used to evaluate gesture-generation models in previous work [Bozkurt et al. 2015; Lu et al. 2021; Sadoughi and Busso 2019].

The goal of CCA is to find a sequence of linear transformations of each variable set, such that the Pearson correlation between the transformed variables is maximised. This correlation is what we use as a similarity measure, and we report it as global CCA values in our results. A high value is considered better.

5.6.4 Fréchet gesture distance. Recent work by Yoon et al. [2020] proposed the Fréchet gesture distance (FGD) to quantify the quality of generated gestures. This metric is based on the FID metric used in image-generation studies [Heusel et al. 2017] and can be written

$$\text{FGD}(X, \, \hat{X}) = ||\mu_r - \mu_q||^2 + \text{tr}(\Sigma_r + \Sigma_g - 2(\Sigma_r \Sigma_g)^{1/2}). \tag{2}$$

Here,  $\mu_r$  and  $\Sigma_r$  are the first and second moments of the latentfeature distribution  $Z_r$  of the human motion-capture data X, whereas  $\mu_q$  and  $\Sigma_q$  are the first and second moments of the latent-feature distribution  $Z_q$  of the generated gestures  $\hat{X}$ .  $Z_r$  and  $Z_q$  were extracted by the same feature extractor, which was obtained as the encoder part of a motion-reconstructing autoencoder. We used a CNN-based autoencoder trained on the challenge dataset following the implementation in Yoon et al. [2020]). Lower values are better.

(b) Upper-body

	1	Median	Appropriateness						
	ŀ	numan-	Num. responses			Percent matched			
ID	1	ikeness	M.	Tie	Mism.	(splitting ties)			
UNA	63	∈ [61, 65]	691	107	189	$ 75.4 \in [72.5, 78.1]$			
UBA	33	∈ [31, 34]	424	264	303	56.1 ∈ [52.9, 59.3]			
UBT	36	∈ [34, 39]	341	367	287	52.7 ∈ [49.5, 55.9]			
USJ	53	∈ [52, 55]	461	164	365	54.8 ∈ [51.6, 58.0]			
USK	41	$\in [40, 44]$	454	185	353	55.1 ∈ [51.9, 58.3]			
USL	22	$\in [20, 25]$	282	548	159	56.2 ∈ [53.0, 59.4]			
USM	41	$\in [40, 42]$	503	175	328	58.7 ∈ [55.5, 61.8]			
USN	44	∈ [41, 45]	443	190	352	54.6 ∈ [51.4, 57.8]			
USO	48	∈ [47, 50]	439	209	335	55.3 ∈ [52.1, 58.5]			
USP	29.5	∈ [28, 31]	440	180	376	53.2 ∈ [50.0, 56.4]			
USQ	69	∈ [68, 70]	504	182	310	$59.7 \in [56.6, 62.9]$			

5.6.5 System ranking comparison. A good objective metric might help in evaluating the performance of a system, especially when such a metric correlates with a subjective measure. To get more insight into whether the objective metrics in our study may be used as a proxy for subjective evaluation results, we calculated the correlation between the ranking of the conditions on median humanlikeness, and the result on the objective metrics listed above. For this, we used Kendall's  $\tau$  rank correlation coefficient, and associated statistical tests [Kendall 1970].

Of the objective metrics we studied, only CCA compares output poses directly to the corresponding reference motion-capture poses. All other metrics are invariant to permutation, in the sense that changing the order of the different sequences (mismatching them with other speech/reference motion) will not change the value. They thus cannot measure appropriateness, which is why we only consider how those metrics correlate with human-likeness scores.

## **RESULTS**

The results of the challenge are revolution and a revelation, for the first time finding performance that exceeds the ground-truth data in human-likeness, whilst simultaneously laying bare the true extent of the gap between natural and synthetic motion in terms of speech appropriateness. We furthermore find that all objective metrics except for the FGD correlate so poorly with subjective scores as to be statistically indistinguishable from chance correlation. More detail is provided in the sections below, first reporting the results of the subjective evaluation and thereafter the objective metrics. Discussion of the various findings is reserved for Sec. 7.

## Analysis and results of human-likeness studies

Each test taker in the human-likeness studies contributed 76 ratings to the analyses after removing attention checks, giving a total of 9,196 ratings for the full-body study and 11,400 ratings for the upper-body study. The results are visualised in Fig. 4, with summary statistics (sample median and sample mean) for the ratings of all conditions in each of the two human-likeness studies given

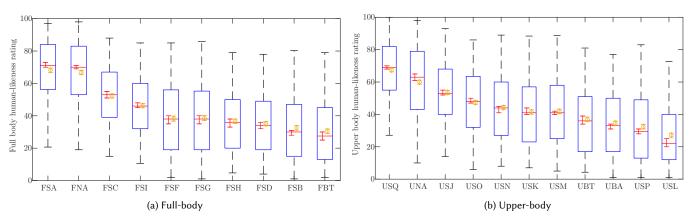


Fig. 4. Box plots visualising the ratings distribution in the human-likeness studies. Red bars are medians and yellow diamonds are means, each with a 0.05 confidence interval and a Gaussian assumption for the means. Box edges are at 25 and 75 percentiles, while whiskers cover 95% of all ratings for each condition. Conditions are ordered descending by sample median for each tier.

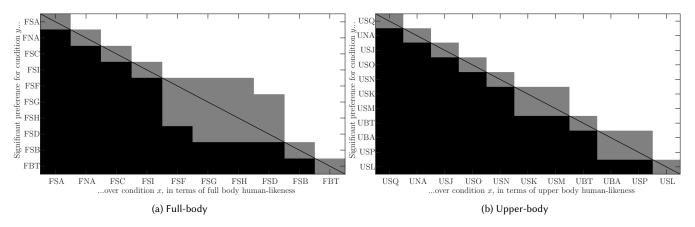


Fig. 5. Significant differences in human-likeness. White means the condition listed on the y-axis rated significantly above the condition on the x-axis, black means the opposite (y rated below x), and grey means no statistically significant difference at level  $\alpha = 0.05$  after Holm-Bonferroni correction. Conditions use the same order as the corresponding subfigure in Figure 4.

in the first half of Table 2, together with 95% confidence intervals for the true median. These confidence intervals were computed using order statistics, leveraging the binomial distribution cdf, while those for the mean used a Gaussian assumption (i.e., using Student's *t*-distribution cdf, rounded outward to ensure sufficient coverage); see Hahn and Meeker [1991]. We note that statistics regarding the mean should be interpreted with caution, since responses should be seen as ordinal rather than numerical, and it is therefore improper from a perceptual perspective to perform averaging on the ratings.

The distributions in Fig. 4 are seen to be quite broad. This is common in evaluations like HEMVIP [Jonell et al. 2021], since the range of the responses not only reflects differences between conditions, but also extraneous variation, e.g., between stimuli, in individual preferences, and in how critical different raters are in their judgements. In contrast, the plotted confidence intervals are seen to be quite narrow, since the statistical analysis can mitigate the effects of much of this variation.

Despite the wide range of the distributions, the fact that the conditions were rated in parallel on each page enables using pairwise

statistical tests to factor out many of the above sources of variation. To analyse the significance of differences in median rating between different conditions, we applied two-sided pairwise Wilcoxon signed-rank tests to all unordered pairs of distinct conditions in each study. (This is the same methodology as in the GENEA Challenge 2020 [Kucherenko et al. 2021b].) This closely follows the analysis methodology used throughout recent Blizzard Challenges and, unlike Student's t-test (which assumes that rating differences follow a Gaussian distribution), this analysis is valid also for ordinal response scales, like those we have here. For each condition pair, only cases where both conditions appeared on the same page and were assigned valid ratings were included in the analysis of significant differences. (Recall that not all conditions were rated on all pages due to the limited number of sliders and the presence of attention checks.) This meant that every statistical significance test was based on at least 615 pairs of valid ratings in the full-body study, and 603 pairs of valid ratings in the upper-body study. Because this analysis is based on pairwise statistical tests, it can potentially resolve differences between conditions that are smaller than the

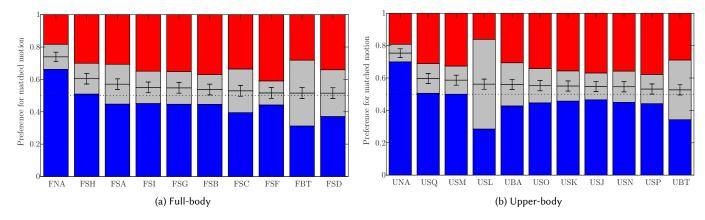


Fig. 6. Bar plots visualising the response distribution in the appropriateness studies. The blue bar (bottom) represents responses where subjects preferred the matched motion, the light grey bar (middle) represents tied ("They are equal") responses, and the red bar (top) represents responses preferring mismatched motion, with the height of each bar being proportional to the fraction of responses in each category. The black horizontal lines bisecting the light grey bars represent the proportion of matched responses after splitting ties, each with a 0.05 confidence interval. The dotted black line indicates chance-level performance. Conditions are ordered by descending preference for matched motion after splitting ties.

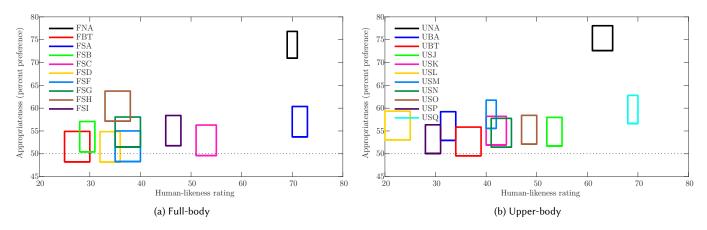


Fig. 7. Joint visualisation of the evaluation results for each tier. Box widths show 95% confidence intervals for the median human-likeness rating and box heights show 95% confidence intervals for the preference for matched motion in percent, indicating appropriateness.

width of the confidence intervals for the median in Fig. 4, since those confidence intervals are inflated by variation that the statistical test controls for. The p-values computed in the significance tests were adjusted for multiple comparisons on a per-study basis using the Holm-Bonferroni method [Holm 1979], which is uniformly more powerful than conventional Bonferroni correction at keeping the family-wise error rate (FWER), often referred to as alpha-level, at or below  $\alpha = 0.05$ 

Our statistical analysis found all but 5 out of 45 condition pairs to be significantly different in the full-body study and all but 2 out of 55 condition pairs to be significantly different in the upper-body study, all at the level  $\alpha = 0.05$  after Holm-Bonferroni correction. The significant differences we identified in the two studies are visualised in Fig. 5 which uses the same condition order as the box plot and shows which conditions were found to be rated significantly above or below which other conditions.

## 6.2 Analysis and results of appropriateness studies

We gathered a total of 8,867 responses for the full-body study and 10,910 responses from the upper-body study that were included in the analysis. Every condition received at least 873 responses in the full-body study and 983 in the upper-body study. Raw response statistics for all conditions in each of the two studies are shown in the second half of Table 2, together with 95% Clopper-Pearson confidence intervals for the fraction of time that the matched video was preferred over the mismatched, after dividing ties equally between the two groups (rounding up in case of non-integer counts). The confidence intervals were rounded outward to ensure sufficient coverage. The response distributions in the two studies are further visualised through bar plots in Fig. 6, while Fig. 7 visualises the results of the entire challenge in a single coordinate system per tier.

Table 3. Objective evaluation results. The word "acceleration" has been abbreviated to "accel."; ± shows the standard deviation per sequence. The best two or three numbers in each column, i.e., those closest to the numbers from the held-out motion-capture data (FNA/UNA, first row of values), are bold. Except for FNA/UNA, conditions (rows) are ordered by decreasing median human-likeness rating. Numbers have generally been rounded to three significant digits.

(a)	FII	II-b	odv

Condition	Average jerk	Average accel.	Global CCA	Hellinger distance	FGD
FNA	31300 ± 6590	798 ± 208	1	0	0
FSA	14600 ± 2970	668 ± 161	0.849	0.041	3.18
FSC	$5130 \pm 2120$	$332 \pm 129$	0.818	0.125	16.4
FSI	$7370 \pm 1710$	$345 \pm 98$	0.789	0.111	4.87
FSF	22600 ± 6240	$666 \pm 223$	0.916	0.195	7.49
FSG	$5560 \pm 2380$	$282 \pm 127$	0.992	0.060	10.1
FSH	$8630 \pm 2440$	$313 \pm 92$	0.968	0.104	4.02
FSD	8690 ± 8320	$405 \pm 257$	0.886	0.132	43.4
FSB	$27200 \pm 4680$	$628 \pm 116$	0.782	0.050	16.3
FBT	$3510 \pm 1090$	177 ± 56	0.738	0.267	28.6

(b) Upper-body

Condition	Avera jerk	0	Average accel.	Global CCA	Hellinger distance	FGD
UNA	33000 ±	7030	842 ± 222	1	0	0
USQ	15400 ±	3190	710 ± 173	0.685	0.043	2.84
USJ	8280 ±	1460	$375 \pm 81$	0.640	0.197	4.83
USO	5450 ±	2260	$353\pm138$	0.812	0.129	16.4
USN	7510 ±	3400	$384 \pm 127$	0.789	0.092	194
USK	8180 ±	2450	$311 \pm 99$	0.962	0.137	15.5
USM	6840 ±	3200	$385 \pm 172$	0.991	0.039	2.17
UBT	3760 ±	1170	$190 \pm 60$	0.707	0.248	18.2
UBA	18000 ±	14900	$513 \pm 326$	0.964	0.244	17.0
USP	28500 ±	4960	661 ± 123	0.769	0.051	18.0
USL	7730 ±	5420	$258\pm157$	0.849	0.306	28.4

Table 4. Rank correlations (Kendall's  $\tau$ ) between the "error" in the objective metrics (how much each objective value differed from the reference FNA/UNA) and median human-likeness scores (here abbreviated "Hum.") or – only for CCA – the preference for matched motion after splitting ties (abbreviated "App."). A strong predictor of human scores will exhibit a  $\tau$ -value close to negative unity combined with a low p-value.

(a) Full-body

Metric	Average	Average	Glo	bal	Hellinger	FGD
	jerk	accel.	Global CCA Hum. App.		distance	
Versus	Hum.	Hum.	Hum.	App.	Hum.	Hum.
$\overline{\tau}$	-0.09	-0.36	-0.36	-0.38	-0.36	-0.49
p-value	0.72	0.15	0.16	0.15	-0.36 0.15	0.048

(b) Upper-body

Metric	Average	Average	verage Glo		Hellinger	FGD
	jerk	accel.	Global CCA Hum. App.		distance	
Versus	Hum.	Hum.	Hum.	App.	Hum.	Hum.
τ	-0.11	-0.26	0.11	-0.49	-0.40 0.085	-0.51
<i>p</i> -value	0.64	0.27	0.64	0.041	0.085	0.029

Unlike the human-likeness studies, the responses in the appropriateness studies are restricted to three categories and do not necessarily come in pairs for statistical testing in the same way as for the parallel sliders in HEMVIP. A different method for identifying significant differences therefore needs to be adopted. We used Barnard's test [Barnard 1945] to identify statistically significant differences at the level  $\alpha=0.05$  between all pairs of distinct conditions, applying the Holm-Bonferroni method [Holm 1979] to correct for multiple comparisons as before. (Here and forthwith, we only consider the relative preference in the sample after dividing ties equally.) Barnard's test is considered more appropriate than Fisher's exact test for a product of two independent binomial distributions [Lydersen et al. 2009], as here.

Our statistical analysis found 13 of 45 condition pairs to be significantly different in the full-body study and 10 out of 55 condition pairs to be significantly different in the upper-body study. Specifically, FNA/UNA were significantly more appropriate for the specific speech signal compared to all other, synthetic conditions. In addition, FSH was significantly more appropriate than FBT, FSC, FSD, and FSF in the full-body study. No other pairwise differences were statistically significant in either study.

Instead of comparing the appropriateness of different synthesis approaches against one another, one may instead compare to a random baseline (50/50 performance), and test if the observed effect

size is statistically significantly different from zero. We can assess this at the 0.05 level by checking whether or not the confidence interval on the effect size overlaps with chance performance. From this perspective, FSA, FSB, FSG, FSH, FSI are significantly more appropriate than chance in the full-body study, and all systems except UBT are more appropriate than chance in the upper-body study. Unlike other significance tests in the subjective evaluation, these assessments do not include a correction for multiple comparisons.

#### 6.3 User comments

As part of the post-evaluation questionnaire, we asked study participants to comment on the user studies, including positive and negative aspects they perceived. 97% of the respondents in the user studies responded positively on whether the compensation was adequate. Additionally, they often commented positively on how interesting and engaging the study was.

We also asked participants regarding any negative aspects of the study. Here, 15% of the participants answered that they found repetitiveness a negative aspect of the study. Some users pointed at the lack of a proper human face on the humanoid, and suggested incorporating that in future work. Others commented on the lack of real conversation, and proposed to have the humanoid be part of an actual conversation. All responses to these questions can be found in our data release.

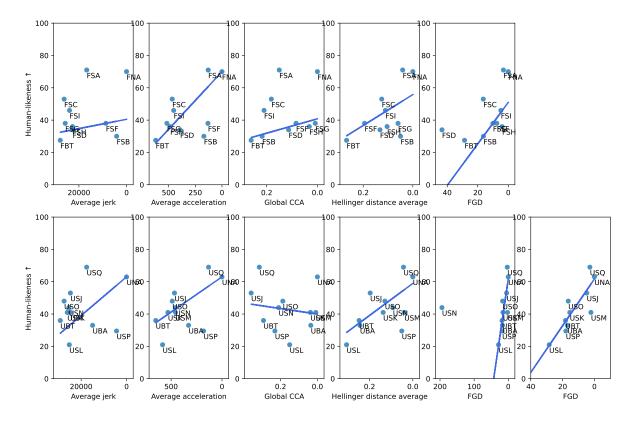


Fig. 8. Scatterplots comparing objective metrics and human-likeness ratings. The first row is for the full-body tier and the second row is for the upper-body tier. The x-axis shows the absolute magnitude of the difference between the objective value for each system and the corresponding value for the reference motion FNA/UNA, with the scale reversed such that the systems most similar to the reference are on the right. Regression lines (from the Theil-Sen regressor [Sen 1968; Theil 1992], which is robust to outliers) are also shown. The last plot in the second row is for FGD but with a narrower x-axis range for a better view.

## Objective evaluation results

The values of the objective metrics we computed are listed in Table 3. For each number in the table, we also calculated how much it differed from the corresponding value for the reference system (FNA/UNA), and then computed the rank correlation between the absolute value of these differences and the median human-likeness scores from the subjective evaluation. The idea is that systems exhibiting values closer to FNA/UNA should appear more human-like. The resulting rank correlations and *p*-values can be found in Table 4. For median human-likeness, we only found a statistically significant (p < 0.05) rank correlation with FGD, for both the full and upper-body tier (Kendall's  $\tau = -0.49$  and -0.51, respectively). The negative sign is expected, since a smaller difference from FNA/UNA should be associated with better-looking motion and higher human-likeness scores. Fig. 8 visually compares the subjective human-likeness ratings and objective metric results.

CCA is the only metric we computed that can indicate appropriateness, since it directly compares each generated sequence with the corresponding reference motion-capture poses. We therefore computed its rank correlations with the appropriateness data as

well. Here we found a statistically significant effect ( $\tau = -0.49$ ) for the upper-body tier, but not for the full body.

#### DISCUSSION

We now discuss our results and how they may be interpreted, first for human-likeness (in Sec. 7.1), then for appropriateness (in Sec. 7.2), and then for the objective metrics (in Sec. 7.3). We connect our discussion of each part to the other evaluations we performed and to previous literature. Based on our findings, we then formulate a number of take-home messages regarding what matters most in gesture generation (in Sec. 7.4) and give examples of how materials from the challenge can be used by the field (in Sec. 7.5).

#### 7.1 Discussion of human-likeness results

Generating convincingly human-like gestures is a difficult problem, and nearly all conditions rated significantly below natural motion capture. However, each tier contains an entry which is rated significantly above the motion from the motion-capture recordings in terms of human-likeness. This is a leap forwards compared to GENEA 2020, and we believe it represents a motion quality not before seen in large-scale evaluations. Although there has been work, specifically Rebol et al. [2021], that reported a proposed motion-generation method as being statistically not significantly different from natural motion, they only evaluated a single method and their study was not based on motion-capture data but on 3D pose estimation from monocular video. We think that that choice of data source restricted the motion quality of their natural-motion condition to be less convincing (and thus a weaker baseline) than our reference-motion conditions FNA/UNA. Furthermore, all differences between natural and synthetic conditions are significant in our study.

7.1.1 Interpreting the high scores of FSA and USQ. Despite conditions FSA and USQ being rated above the corresponding natural reference motion, we caution that this does not mean that the motion is "superhuman", or even completely human-like - indeed, the median rating is much below 100, which would constitute "completely human-like" as per our explicit instructions to test takers. What the result means is rather that the visualised motion in the majority of cases was perceived as more human-like than the motion-capture data used for FNA/UNA in the subjective evaluation. In making this distinction, it is important to keep in mind that our human-likeness evaluation is constrained by several factors. Most notably, the nominally natural motion is constrained by our ability to accurately capture the entire range of human motion, especially the fingers, using the technology we used. Finger motion capture is very difficult, and dataset limitations meant that the finger motion could not be chosen so as to look completely natural in all test segments evaluated, potentially degrading the ratings of FNA/UNA as a result. An artificial system might have its training data cleaned of problematic instances, so as to prevent it from generating such motion, giving it an edge over FNA/UNA. This is in fact what was done for systems FSA and USO, which only used selected training-data segments, manually chosen to have high motion quality, in generating their output gestures [Zhou et al. 2022].

Our ability to visualise human characters and their motion also plays a role in our findings. The use of a deliberately neutral 3D avatar lacking potentially distracting human features such as gaze and lip motion significantly reduces the bandwidth of the communication channel to the user, which lowers the threshold for what needs to be achieved in order to match human motion ratings in the evaluation. If the challenge had involved generating additional modalities such as gaze and facial expression, the shortcomings of artificial systems may have become more clear, at the expense of increased complexity when running and taking part in the challenge.

7.1.2 On the differences between the two tiers. There are fewer significant differences in the full-body evaluation than in the upper-body evaluation, perhaps meaning that full-body motion is more difficult to rate consistently. Although the difference is not substantial, we would naively expect the opposite, due to the correction for multiple comparisons being more conservative in the upper-body evaluation. There are many possible explanations for this finding, beyond the fact that the different teams did not all participate in both tiers. For example, our finding is consistent with an interpretation that full-body motion is a more difficult machine-learning problem, for instance due to increased dimensionality of the output space and the increased number of behaviours that need to be learnt. This

could explain why the best entry in the upper-body evaluation more clearly outperformed UNA, compared to the margin between the best entry in the full-body evaluation and FNA.

Another possible explanation for the same result is that the process of imposing full-body motion from a walking and talking human onto an avatar with a fixed lower body may not always yield completely natural results, and could sometimes give rise to incongruous motion. This could also explain the wider span (greater interquartile range) of ratings of UNA compared to FNA. Future GENEA challenges intend to only consider full-body motion.

## 7.2 Discussion of appropriateness results

We find the results of the appropriateness evaluation both thoughtprovoking and revealing about the state of the field. To begin with, the greatest relative preference, a 75% preference for matched motion, was observed for natural motion capture, i.e., FNA/UNA. This +25% effect size over the 50/50 bottom line validates that our methodology can well identify when gestures are appropriate for the speech and is about half the theoretical maximum value of +50% (a 100/0 split). A +25% effect size should be considered a good result, since previous studies that have incorporated mismatched stimuli, e.g., Jonell et al. [2020a]; Rebol et al. [2021], have found that they sometimes are difficult for participants to distinguish from matched ones, especially if they - like here - both correspond to segments where the character is speaking (and do not, say, match audio of active speaking with a segment of motion corresponding to the character listening without speaking). Furthermore, both matched and mismatched motion stimuli here have their starting points aligned to the start of a phrase in the speech, meaning that the motion in the stimulus videos might initially be more similar to each other than if the mismatched motion had been excerpted completely at random and not aligned to the start of phrase boundaries. It is therefore not surprising to find that the preference for matched motion over mismatched motion is not larger for FNA/UNA.

In line with expectations, no system has a relative preference for matched motion below 50%, which is the theoretical bottom line, attained by a system whose motion has no relation to the speech. However, the synthetic conditions are all far behind natural human motion in terms of appropriateness. The measured effect sizes over the 50/50 bottom line range from +10% and down to 1.5% for all these conditions, compared to +25% for FNA/UNA, and all differences compared to FNA/UNA are highly statistically significant. This is a very substantial gap, and it is clear that generating meaningful and appropriate gestures is still far from a solved problem.

One other interesting trend is that a few conditions with relatively poor human-likeness, specifically FBT, UBT, and USL, show a noticeably larger fraction of tied responses, compared to other conditions. We hypothesise that this could be due to underarticulated motion, noting that a hypothetical, extremely underarticulated system that does not move at all should receive the response "They are equal" all the time. This hypothesis is consistent with the fact that these conditions all had the three lowest average acceleration values in Table 3, indicating little motion overall.

7.2.1 Comparison to the human-likeness studies. Compared to the results for the human-likeness studies, we did not find as many

differences between the submissions in terms of appropriateness. We can envision four factors that could contribute to this, which we list below, along with thoughts regarding potential mitigations:

- Responses are confined to much fewer categories, meaning that each response provides less information in an informationtheoretic sense. This could potentially be addressed by having test-takers complement their response with an indication of the strength of their preference. We recommend that future developments in evaluation consider using a preference scale with more response options, e.g., five or seven possible responses.
- Unlike the HEMVIP-based human-likeness studies, the responses to the appropriateness studies were not analysed using pairwise statistical tests to control for variation between subjects and stimuli. This might have led to reduced resolving power. It might be possible to improve on the statistical analysis using, e.g., log-linear mixed effects models to account for the effects of different test takers and different videos, or by changing the study setup to allow for pairwise statistical testing. One can furthermore gather more responses per condition, which we recommend in case the same testing methodology is used.
- Assessing appropriateness may be a more difficult task for humans than assessing human-likeness, meaning that there is more random variation in the responses relative to the humanlikeness studies. In a signal-to-noise analogy, this means that the noise is higher. Mitigating this would probably require changing the evaluation and its task. For example, differences might become more obvious if segments were mismatched completely randomly, such that speech sometimes would be paired with motion from a segment where the character is not actively speaking, and vice versa, although doing so would essentially change the type of appropriateness that is being assessed.
- It may simply be that current artificial systems struggle to generate motion that is particularly appropriate to any specific input speech. In other words, in a signal-to-noise analogy, the signal is weaker. Consequently, there is less of a difference to be uncovered in the first place.

Although all of these factors may contribute to the results we observe, the big gap in effect size between natural motion capture and synthetic motion, and the fact that FNA/UNA were very significantly better than all other conditions, shows that our methodology is sufficiently accurate to clearly resolve important differences between conditions. From Fig. 7b, we can furthermore see that there is no strong correlation between the human-likeness ratings and the appropriateness ratings in the evaluation. This supports a conclusion that our evaluation successfully disentangled these two aspects of gesture motion.

In addition to its strong ability to control for the effect of motion quality, our method for assessing appropriateness only requires comparing a system to itself. We believe this feature may enable direct comparison between different studies on the same data, without having to include the various other synthetic baseline conditions in the new user study. Seeing that creating appropriate baseline systems is one of the sticking points both for carrying out research and for its

subsequent assessment in peer review, this can be a major simplification compared to parallel methodologies like HEMVIP [Jonell et al. 2021] that involve simultaneously comparing and evaluating many different conditions against each other. Since responses in those studies are affected by what other videos are shown on the same page, their results thus cannot be directly compared unless stimuli or implementations of previous synthetic baseline conditions are included in the new study. Our recommendation for future research that uses the same methodology in this paper is to report effect size and  $\alpha = 0.05$  Clopper-Pearson confidence intervals similar to Table 2, to enable easy and accurate comparison between studies.

7.2.2 Comparison to other gesture-appropriateness assessments. Overall, the distribution in Fig. 6 of the three different responses across the different conditions is similar to that seen in the mismatching study reported in Jonell et al. [2020a], which used a similar methodology. On the other hand, we see fewer statistical differences compared to the appropriateness study in GENEA 2020 [Kucherenko et al. 2021b], which asked participants to rate the appropriateness of the stimuli on an absolute scale using HEMVIP. However, the ratings in that study were strongly biased towards conditions with high human-likeness, as discussed in Sec. 5.4, evidenced by the fact that mismatched natural motion (M) scored second best in terms of appropriateness there. In effect, we have traded the high-resolution, high-bias method from GENEA 2020 for a reduced-resolution, lowbias method. We think this is a step forward, since most prior evaluations of gesture appropriateness for speech have been highly confounded by motion quality, whereas our methodology is not.

## 7.3 Discussion of objective metrics

The values of each of the the six objective metrics in Table 3 span a wide range. From the acceleration and jerk values, we can observe that some systems, e.g., the text-based baselines from Yoon et al. [2019], exhibit much less movement than others. Unfortunately, most objective metrics are not well aligned with subjective humanlikeness scores. In the full-body tier, one of the least human-like systems, FSB, received some of the best scores in terms of average absolute jerk, acceleration, and Hellinger distance. At the same time, one of the most human-like systems, FSC, is not in the top three according to any of the objective metrics used. In the upperbody tier, one of the least human-like systems, USP, was in the top three systems according to average jerk, acceleration, and Hellinger distance while one of the most human-like systems, USO, is not in the top three according to any of the objective metrics. The rank correlations in Table 4 make these observations more precise, by showing that most correlations are not statistically significantly different from zero. The one exception is the FGD. Although the correlations we found there are moderate (around -0.5) and system USN shows an outlying value, this metric nonetheless might have some potential as an objective evaluation metric useful for faster evaluation in the development phase, although it is not clear how well it will resolve smaller differences between systems.

As for speech appropriateness, only the CCA metric takes reference motion into account and thus has any possibility to measure this aspect. The CCA results are not clear-cut, but nonetheless

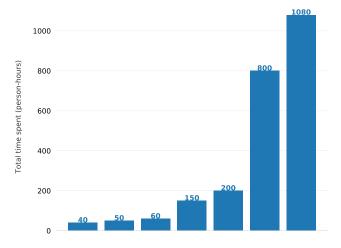


Fig. 9. The number of person-hours different responding teams reported spending on the GENEA Challenge 2022, sorted in ascending order.

somewhat encouraging, seeing that the systems with the best appropriateness (namely FSH, USQ, and USM) also exhibit some of the highest CCA values, of 0.96 and above, and we found a statistically significant correlation for one of the tiers.

All in all, we want to emphasise that objective evaluation of generated gestures is still an open problem. Subjective evaluation, as used by this challenge, remains the gold standard for comparing gesture-generation models [Wolfert et al. 2022], and none of the objective evaluation metrics can replace subjective user studies.

## 7.4 Take-home messages

In this section we combine salient points from our results and discussion with information that the teams provided about their challenge entries, in order to see what we can learn about what aspects matters most in gesture-generation methods, data processing, and evaluation.

7.4.1 What have we learnt about successful gesture generation methods? Table 1 contains all the submissions with the corresponding system properties, sorted according to their human-likeness scores. We can note that all systems except the text-based baseline used audio as an input modality, fewer systems used text, and even fewer used speaker IDs. There seems to be no clear indication that using any given combination of modalities necessarily gives better results than others, as some systems using only audio are on the top and others on the bottom of the list. However, the fact that so many of them did use audio input suggests a perception among teams that taking audio into account is important.

When it comes to the techniques used, RNNs were the most popular choice and used almost by all the systems, followed closely by auto-regression. Again, for most of these there seems to be no strong indication that certain choices are necessarily better than others. Our main, perhaps surprising, observation is that the state-of-the-art in human-likeness is not to use deep learning for everything (or at least not to generate the gesture poses), seeing that the most human-like system, GestureMaster [Zhou et al. 2022], is based on

motion graphs [Arikan and Forsyth 2002; Kovar et al. 2002; Lee et al. 2002] and a library of carefully selected high-quality motion segments.

7.4.2 What have we learnt about gesture-data processing? Fig. 9 shows how much time different teams spent on their submissions. We can see a very high variation, with some teams spending between 40 and 60 person hours whilst some others spent 800 hours or more. The two teams who spent 800 or more hours on their submissions reported devoting a large amount of time on data pre-processing, which other teams did not. One of the former teams is the topperforming team in terms of gesture human-likeness scores. This suggests that spending time on data preparation is likely to pay off in better model performance. Data processing tasks included cropping the recordings into shorter segments, annotating those short segments for, e.g., motion quality, and similar. Some teams found it important to remove segments where the character was listening rather than talking, since the character exhibits little gesture motion in these segments, which can make deterministic gesture-generation approaches regress towards the mean pose and thus produce less vivid movement.

Another important aspect when it comes to the data is post-processing, such as hip-centering or smoothing (cf. Kucherenko et al. [2021a]). As seen in Table 1, most of the systems (good and bad performance alike) applied motion smoothing in some form. This suggests that they found smoothing to be beneficial for gesture generation, although the user studies do not allow us to make a statistical conclusion about the importance of smoothing the output motion.

Finally, modelling the motion of the fingers or having them fixed emerged as another important decision. Roughly half of the systems in the evaluation used fixed fingers. Some of these systems achieved good performance whilst others did not. This does not allow us to make strong statistical conclusions about the importance of modelling fingers. However, we may surmise that finger motion may be especially difficult to make natural, otherwise all teams would presumably have included finger motion in their submissions.

7.4.3 What have we learnt about evaluating gesture generation? Previous experience shows that it is not easy to disentangle perceived human-likeness from appropriateness as more human-like systems are often ranked as more appropriate [Kucherenko et al. 2021b]. In this challenge we made a concerted effort to disentangle these two aspects. Specifically, we (1) muted the audio tracks during the human-likeness evaluation, to remove any influence speech may exert on perceived appropriateness (cf. Jonell et al. [2020a]), and (2) compared each model with a mismatched version of itself (having the same human-likeness), to control for the effect of human-likeness when evaluating appropriateness. This effort paid off, since the two metrics are essentially uncorrelated for the synthetic conditions in Fig. 7b. However, it would be beneficial to improve the statistical resolution of the evaluation procedure.

## 7.5 How materials from the challenge can be used

All materials derived from the Challenge are publicly available at youngwoo-yoon.github.io/GENEAchallenge2022. We believe these

materials have many benefits for gesture-generation research. To illustrate this, we provide a list of possible use cases, often with references to how the data from the previous GENEA Challenge has been used in subsequent research. One may, for instance...

- Benchmark/compare new models to the state of the art using our public data and existing motion or video stimuli, like Ferstl et al. [2021]; Yazdian et al. [2022] did with the materials from the GENEA Challenge 2020.
- Evaluate models using our open-sourced code for the evaluation interface and analyses, similar to how the HEMVIP methodology and code from Jonell et al. [2021]; Kucherenko et al. [2021b] was reused by Wolfert et al. [2021]; Yoon et al. [2021].
- Use our questions and evaluation structure for evaluating new proposed methods, similar to how Teshima et al. [2022] re-used the questions from GENEA 2020 in their subjective evaluations.
- Use our public visualisation code to simplify development and obtain more standardised and comparable visuals, similar to prior re-use of GENEA visualisations in Teshima et al. [2022]; Wang et al. [2021]; Zhang et al. [2023].
- Evaluate new models objectively using the same metrics that showed the most promise here, similar to how Ahuja et al. [2022]; Liang et al. [2022]; Ye et al. [2022] re-used metrics and sometimes code from Ahuja et al. [2020]; Yoon et al. [2020].
- Use our large dataset of subjective evaluation responses to build and/or validate new automatic quality-assessment methods, like how He [2022] used the GENEA 2020 ratings data and stimuli, or perform in-depth analyses of human preferences using the individual response data, perhaps linking these to the time taken by study participants, their questionnaire responses, etc.
- Use our materials and those released by participating teams to probe reproducibility in the field.

#### 8 LIMITATIONS

Despite being a large evaluation with many conditions and raters, there are inevitable limitations to the challenge and its results, imposed by scope, systems, data, visualisation, and evaluation choices. We discuss some of these limitations below.

- 8.1.1 Scope and scale. The ten teams participating in the 2022 challenge do not represent the full spectrum of all gesture-generation approaches available today. While ten teams (plus the top line and baselines) are more systems than considered in any other joint comparison of gesture-generation systems we are aware of, it is still not large enough to, e.g., make strong conclusions regarding which system architectures to prefer. We hope to attract more teams to participate in the challenge in future years.
- 8.1.2 Data. Motion capture is a remarkable technology, but does not yet perfectly capture every aspect of human pose and figure. There are hardware issues such as calibration, and software challenges in estimating humanoid skeletons of various dimensions whilst dealing with problems like reflective marker displacements, occlusion, and markers in close proximity. Together, these issues

may lead to problems with the data, commonly seen as artefacts in the produced motions (e.g., twitching or unnatural bone rotations), which may be especially pronounced in the fingers. Although we have worked to exclude low-quality parts of the data and process it to make it more amenable to deep learning, some artefacts are still present. We suspect that this is an important reason why generated motion could surpass the notionally natural motion capture in terms of human-likeness. More, and more high-quality, motion data might allow for generating better gesture motion and comparison to a stronger top line.

Some useful information is also missing from the current data. On the verbal side, this includes speech information removed for anonymisation. On the non-verbal side, one prominent missing aspect is facial data, which is an important communication channel but was not recorded in the current dataset. Neither was body form, such as muscle mass, body fat, skin, nor how these deform when muscles flex and extend, since the data has abstracted the humanoid form down to only a skeletal hierarchy. Future challenges should maintain awareness of new datasets being published, and their data quality and modalities captured. One modality worth investigating further is face motion, as it may help systems learn more appropriate gestures that relate to facial expressions and emotions.

8.1.3 Visualisation. The gesture visualisation used in the challenge has several limitations. Some are dictated by the data, and some are deliberate choices to, e.g., reduce complexity. The result is a virtual character that, whilst representative of typical gesture-generation visualisations, lacks both skin deformations and many human communication channels, such as gaze, facial expression, and lip motion. Whilst the absence of such features can help focus attention on the body motion currently being studied, it does also lead to a less human-like overall impression for the character. Our evaluation also deliberately obscured some aspects of motion, e.g., by clipping the view so as to not show potential foot sliding and (for the upperbody tier) fixing the legs of the virtual character, which is innately unnatural. We think future challenges should consider incorporating additional communication channels, e.g., facial features on a 3D mesh, to improve the realism of the virtual characters and the produced gestures.

Aside from limitations on what agent behaviours are visualised and how, the interlocutor from the recorded conversations is missing entirely in both modelling and visualisation. This was a deliberate choice to not increase the complexity of the challenge too much, but the absence of such information prevents us from assessing interlocutor-dependent aspects of motion such as proxemics and behavioural alignment. (We deliberately excluded turn taking, back channels, and listening behaviour from the subjective evaluation, since these are likely to look odd without seeing both sides of the conversation.) Future challenges may opt to include information about both conversation parties in the evaluation, so that study participants can be interlocutor-aware in their responses. However, any increases in complexity, whether due to adding additional inputs or output modalities, should be performed one step at a time, so that it is more clear which findings relate to which aspect of the complex problem that is gesture generation.

8.1.4 Evaluation. Our core evaluation only sought to quantify two performance measures, namely subjective human-likeness and perceived appropriateness for the given speech. Aspects such as gesture diversity, or generation speed and latency, were not measured. Furthermore, we only studied the overall appropriateness of the gestures for the speech, but there is value in evaluating appropriateness with respect to the speech rhythm and speech meaning separately, since these are distinct aspects. We will consider doing that in future challenges, for example by performing two user studies, each focused on a separate type of appropriateness: semantic appropriateness and rhythmic/temporal appropriateness.

There are also many other kinds of appropriateness that can be assessed, e.g., appropriateness for the given speaker, and for the interlocutor behaviour as discussed above. (See the discussion of grounding in Nyatsanga et al. [2023] for a more extensive list.) None of these were considered in the present challenge, either due to dataset limitations or to keep the complexity to a manageable level. A difficult but important long term goal is to pursue a more "ecologically valid" evaluation, to eventually compare different gesturegeneration methods in human interaction, similar to He et al. [2022].

#### 9 CONCLUSIONS AND IMPLICATIONS

We have hosted the GENEA Challenge 2022 to compare many different gesture-generation methods and assess the state of the art in data-driven co-speech gesture generation for full-body and upperbody avatars. The central design goals of the challenge were (1) to enable direct comparison between many different gesture-generation methods whilst controlling for factors of variation external to the model, namely data, embodiment, and evaluation methodology, and (2) to disentangle the effects of motion human-likeness and motion appropriateness in the evaluations.

Our evaluation results show that, with the right approach, synthetic motion can attain human-likeness ratings equal or better than the underlying motion-capture data. This is a big step forward, although most systems did not come close to this level of performance. The results also suggest that the field is advancing measurably, since most submissions performed significantly better than the previously published baseline methods. However, using a careful evaluation paradigm, we find that synthetic gestures are much less appropriate for the speech than human gestures, also when controlling for differences in human-likeness. We are thus only at the beginning of the road when it comes to generating cospeech motion that is appropriate for the specific speech. Finally, most objective metrics we computed did not exhibit any statistically significant correlations with our subjective human-likeness ratings, with the Fréchet gesture distance being the lone exception to the rule. Objective metrics should thus only be used with great caution.

#### 9.1 Implications

The challenge findings have implications for both research and practice. We summarise our perspectives below.

9.1.1 Implications for practical systems. If you are building a gesture-generation system and want to reach top-of-the-line human-likeness, you should currently consider using "playback-based" methods like motion graphs [Arikan and Forsyth 2002; Kovar et al. 2002; Lee

et al. 2002] as demonstrated by GestureMaster [Zhou et al. 2022] to generate the pose sequences, instead of relying solely on deep learning to go all the way from input features to motion. Playback-based systems need less data, and the quality of the motion material is then a higher priority than database size, in contrast to current deep-learning trends. Machine-learning is still useful for deciding which gestures to generate (e.g., which motion clips to concatenate). In all cases, it appears important to spend time on data processing.

9.1.2 Implications for research and evaluation. We believe the challenge adds value to the research community in several ways. A lot can doubtlessly be learnt from the system-description papers by the participating teams. The materials we release from the challenge (e.g., time-aligned splits of audio, text, and gesture data; visualisation; code; and evaluation stimuli and responses) have broad utility for future research, system building, and benchmarking in gesture generation, similar to the community uptake of the resources from the GENEA Challenge 2020. Furthermore, the methodology we demonstrate for assessing motion appropriateness for speech is much more accurate at controlling for the effect of subjective motion quality and does not involve subjects making any direct comparisons between videos generated by different conditions, which is beneficial for efficient benchmarking against previous publications (see Sec. 7.2.1 for details and recommendations).

9.1.3 Implications for future developments in the field. Based on the fact that one condition in each tier managed to achieve excellent human-likeness, we expect that, in the medium-term future, gesturegeneration systems (at least ones based on motion playback) should be able to advance to more consistently match, or possibly even exceed, motion capture in terms of human-likeness. Systems that generate poses directly from deep learning are likely to improve in human-likeness as well, as larger datasets with more accurate motion become available (e.g., Liu et al. [2022b]). This would be similar to recent developments in verbal behaviour generation, where neural language models [Brown et al. 2020] and speech synthesisers [Li et al. 2019; Shen et al. 2018] trained on large datasets are approaching the text and speech produced by humans in terms of surface quality (but not necessarily appropriateness). Gesture generation may be lagging behind due to the relative scarcity of high-quality motion data, compared to text and audio, since accurate motion estimation from monocular in-the-wild video remains a challenging problem.

As the evolution runs its course, we believe that research into appropriate rather than human-like motion is poised to become the new frontier in gesture generation. There is already evidence that existing deep-learning methods in principle can predict appropriate gestures to generate, even for the hard case of semantically motivated, communicative gestures from speech [Ao et al. 2022; Kucherenko et al. 2021c, 2022]. We also believe that there is great potential for devising better objective metrics, using challenge materials to validate these, and that the adoption of meaningful metrics may further accelerate progress in the field.

9.1.4 Implications for future challenges. We think that future challenges should study more difficult scenarios that are farther from being solved, for example full-body motion in dyadic interaction.

That can also provide interesting opportunities for exploring other types of appropriateness, e.g., with respect to the interlocutor stance and behaviour, as studied in Jonell et al. [2020a]; Woo [2021]. Generating interlocutor-aware full-body gestures might therefore be a focus of the next GENEA Challenge. This should be coupled with further method development to obtain methodologies for conducting and analysing appropriateness tests with increased resolving power whilst still controlling for motion human-likeness. In general, challenges like the one described here can play an important part in identifying key factors for generating convincing co-speech gestures in practice, and help drive and validate future progress towards endowing embodied agents with natural and appropriate gesture motion.

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