Embedding Shift between Photographs and Generated People

Robert Walker (ME), Blake Mitchell (VA), Dylan Campbell (RO)

Abstract

An identity embedding validated on photographs may behave differently on generated video. A change in its similarity distribution can reflect altered visual content, unfamiliar rendering, or an actual change of subject. This article proposes a transfer assessment that separates those possibilities using independent judgments and recorded acquisition conditions. ARGUS supplies the motivating generation setting. No embeddings are computed here. The resulting design treats photographic validation as relevant background while requiring direct evidence for the interpretation of scores on generated people.

1. The measurement population changes

Recognition systems learn from images produced under particular collection and annotation processes. Generated imagery can introduce other textures, temporal artifacts, and relationships among facial features. A scoring system may respond to those properties even when observers continue to recognize the subject. Conversely, an embedding may remain similar while a generated person changes in a way that matters to the intended task. Neither possibility can be resolved from the name of the embedding alone.

ARGUS uses organized identity references to support subject-preserving generation (Meng et al., 2026b). Its evaluation setting therefore connects reference photographs with synthesized observations. The present article proposes a study of that measurement boundary, without calculating features or assessing ARGUS outputs. The question is whether a numerical relationship established on one image population continues to support the same identity interpretation after the observation process changes to generation and motion.

Replication has several levels: a second measurement, a second trajectory, and a second independently acquired setting are different additions to evidence. Statistical calculations should retain those distinctions. Heteroskedasticity-robust regression inference concerns unequal conditional variances (White, 1980); cluster-robust inference concerns dependencies shared within groups (Cameron and Miller, 2015). The group definition should be justified through the data collection and evaluation process, and not chosen because it produces a preferred significance result. Unequal group sizes also influence what a pooled average represents. Reporting both the group count and observation count permits a reader to see whether the calculation describes common cases, heavily recorded cases, or an explicitly balanced group population. No variance estimator alone resolves that choice of estimand.

Identity evidence has a sampling structure before any recognition score is calculated. LFW defines a particular pair-verification protocol (Huang et al., 2008), and the documented scikit-learn loader makes its prescribed files accessible through a reproducible software interface (Pedregosa et al., 2011). Neither source turns a pair count into a count of independent subjects or images. Dataset documentation should identify how references, identities, comparisons, and splits were assembled (Gebru et al., 2021). For generated video, the analogous record should distinguish a new person from another reference photograph, another trajectory, or another frame. Each adds evidence along a different dimension. A transparent report should preserve those links rather than flattening every observation into one score table. The resulting metadata can support a useful audit even when no images or generated clips are available. Such an audit describes coverage and organization; it does not itself establish recognition accuracy, demographic representativeness, or identity preservation in an unobserved video.

2. A transfer study with distinguishable comparisons

A proposed dataset would include reference photographs, held-out real observations where appropriate, and generated clips associated with declared subjects and instructions. Each item would preserve its source and processing history. The generated portion should span the output conditions relevant to the intended claim rather than only attractive demonstrations. Independent judgments would be collected under a specified information boundary so that the assessment has evidence beyond the automated representation whose transfer is being examined.

The analysis would compare score behavior conditional on those judgments and observed conditions. A distributional difference between real and generated images is a useful descriptive observation, but it is not by itself proof of measurement error. The populations may contain different identity difficulty or visibility. Matched or stratified comparisons can reduce measured differences, while a sensitivity analysis should acknowledge remaining selection. No claim of pure rendering causation follows merely from controlling a few metadata fields.

Threshold selection should be separated from final assessment. A photographic threshold can be retained to measure its transfer, then compared with a threshold selected using independent generated development examples. These are different operating policies and should receive distinct results. Repeated frames of one clip should remain linked. Otherwise a long easy clip can dominate a score distribution and give a misleading impression of the number of independently supported subject comparisons.

A proposed explanation should be distinguishable from alternative explanations using observations the study can actually collect. Diagramming the relationships among selection, exposure, measurement, and outcome makes that requirement explicit (Greenland et al., 1999). The diagram does not establish its own correctness, but it prevents adjustment choices from remaining invisible. The same transparency is needed when investigators inspect many endpoints before choosing a narrative. Controlling false discovery concerns the behavior of a specified testing family (Benjamini and Hochberg, 1995); it is not a general certificate of scientific validity. Primary endpoints, secondary descriptions, and future mechanism tests should be named separately. Readers can then assess whether an attractive pattern was predicted, explored, or merely noticed after the available comparisons were examined.

Discriminative identity representations address a different question from the design of the test population. FaceNet learns an embedding for verification and related recognition tasks (Schroff et al., 2015). ArcFace (Deng et al., 2018) and CosFace (Wang et al., 2018) develop margin-based approaches to discriminative face learning. These methods do not determine which reference views should be supplied to a generator. Dataset choices also alter the challenge: VGGFace2 emphasizes pose and age variation (Cao et al., 2017), MegaFace increases the scale of recognition distractors (Kemelmacher-Shlizerman et al., 2015), and FairFace studies representation across annotated demographic groups (Kärkkäinen and Joo, 2019). Their relevance is to separate distinct kinds of coverage. Many comparisons of a small reference set cannot replace missing pose variation or a different population of people. A study should state which identity measurement it uses, how thresholds are selected, and what the represented cases actually cover. Strong performance on one protocol does not establish universal validity of the embedding across generated or stylized subjects.

3. Interpreting score disagreement under generation

Independent human judgments should not be treated as infallible labels. They need clear instructions, adequate references, and an uncertainty option. Cases in which both the embedding and observers are unsure are especially relevant to coverage. A transfer report should preserve their frequency rather than resolving them through whichever labeling convention improves agreement. Additional evidence can help later, but it changes the assessment setting and should be documented as such.

The analysis should also compare more than one kind of output success. A method can obtain familiar photographic features by limiting the requested motion or view change. That may improve a photographic identity score while undermining the generation task. Recording instruction execution and temporal quality separately makes that tradeoff visible. A decline in one embedding's score during a valid transformation should not automatically be interpreted as a subject substitution without corroborating evidence.

The final claim should identify the tested population, evaluator, operating point, and uncertainty procedure. A favorable result on one generator release does not establish validity on future releases or every rendering style. Maintaining a small versioned transfer set can reveal when the measurement relationship changes, although repeatedly tuning to it would eventually weaken its independence. The proposed study therefore separates development monitoring from a reserved assessment used to substantiate the principal preservation claim.

Subject conditioning is not identical to measuring the resulting subject. DreamBooth studies subject-driven generation through model adaptation (Ruiz et al., 2022), whereas IP-Adapter develops image prompting compatible with text conditioning (Ye et al., 2023). The reference evidence available to those mechanisms should be matched or explicitly described in a comparison. Generic representations also have different training purposes: CLIP connects imagery to language (Radford et al., 2021), while DINO investigates self-supervised visual features (Caron et al., 2021). Either can contribute to a measurement pipeline, but neither automatically defines human identity across generated motion. A study should separate input fidelity, recognizability, and obedience to scene instructions. It should also retain cases in which these objectives conflict. An image-conditioning mechanism can reproduce an accessory or background very well while leaving a more important identity requirement unresolved. Measuring those conflicts directly is more informative than declaring every increase in reference similarity a successful preservation of the person.

The phrase generalization needs an object: another observation of the same source, another source within the same collection, or a different acquisition process. Each requires a different assessment boundary. Structured validation makes explicit how correlated observations are assigned across training and evaluation (Roberts et al., 2017). Selection bias can still arise if the final boundary is repeatedly consulted while choosing a method (Varma and Simon, 2006). Publishing the split identifiers and the information permitted during each stage allows the claimed transfer to be checked. When a study proposes a new architecture but performs only a data audit, it should state that difference directly. Useful evidence about data coverage does not require pretending that a prediction system has already been trained and externally validated.

Difficult cases can disappear at several stages: data eligibility, generation, measurement, and final scoring. Each exclusion changes the population represented by the reported result. Coverage-aware evaluation makes those decisions visible (Geifman and El-Yaniv, 2017). A confidence score should also be checked when the input distribution changes, because uncertainty under shift is an empirical property rather than a guarantee of the model interface (Ovadia et al., 2019). An informative record preserves

failures to produce an output and failures to obtain a valid measurement. Treating either as absent data without explanation can inflate apparent reliability. A proposed method should state its unsupported conditions and test the boundary between accepted and rejected cases as carefully as the quality of its successful outputs.

4. Scope, uncertainty, and independent assessment

Video evaluation should preserve the distinction among identity, temporal behavior, and general visual appearance. VBench offers a multidimensional approach to video assessment (Huang et al., 2023), while structural similarity (Wang et al., 2004), learned perceptual metrics (Zhang et al., 2018), and generative video-distribution evaluation (Unterthiner et al., 2018) provide different kinds of measurements. Their outputs can disagree for scientifically meaningful reasons. A temporal average may dilute a brief subject substitution, and a strong reference match may reward a nearly static output that fails to carry out the requested motion. The test should therefore retain where and when each observation was made. Human judgments or task-specific measurements may be needed to determine whether a disagreement is consequential. Combining metrics is reasonable only when the combination's interpretation is explicit. A total score should not conceal the fact that one component was unavailable or inappropriate for the very cases motivating the new generation method.

A score is useful only relative to the question it is supposed to answer. Correlation measures a relationship between varying quantities, whereas agreement concerns the discrepancies produced when one procedure substitutes for another (Bland and Altman, 1986). Confidence estimates add a further question about calibration (Guo et al., 2017). These distinctions matter when a learned evaluator becomes the principal source of evidence about another learned system. The evaluation should preserve examples with conflicting scores and obtain independent target judgments where feasible. Numerical ranges and normalization choices should also be documented. An apparently small difference may have a different practical meaning near a decision threshold than in an unimportant part of the scale. Reporting the intended consequence gives those comparisons an interpretable purpose.

An evaluation remains comparable only if its inputs and transformations can be identified. The FAIR principles emphasize persistent identification and reusable scientific metadata (Wilkinson et al., 2016). Work on machine-learning technical debt highlights how dependencies and hidden couplings accumulate in a pipeline (Sculley et al., 2015). In this setting, a score should be linked to the exact references, generation settings, evaluator version, and aggregation rule that produced it. A replacement checkpoint or a revised eligibility filter may change the scientific question even when the output column keeps the same name. Preserving such changes in a release record permits later comparisons to be interpreted honestly. The objective is not maximum documentation volume, but a recoverable account of the decisions that affect the result.

5. Conclusion

Photographic validation gives an identity metric a starting point, not a complete interpretation on generated video. The proposed transfer study links score behavior to independent judgments, observed conditions, and generation-task outcomes. It reports no newly measured shift. Its contribution is a way to identify whether changed numerical similarities reflect meaningful subject errors, a changed observation process, or limitations in an evaluator operating beyond the population on which its decisions were established.

References

Meng, Zijie, et al. "Argus: Stacked multi-view identity mosaic injection for subject-preserving video generation." *arXiv preprint arXiv:2606.11670* (2026).

White, Halbert. “A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity.” *Econometrica*, vol. 48, no. 4, 1980, pp. 817-838. https://doi.org/10.2307/1912934.

Cameron, A. Colin, and Douglas L. Miller. “A Practitioner’s Guide to Cluster-Robust Inference.” *Journal of Human Resources*, vol. 50, no. 2, 2015, pp. 317-372. https://doi.org/10.3368/jhr.50.2.317.

Greenland, Sander, Judea Pearl, and James M. Robins. “Causal Diagrams for Epidemiologic Research.” *Epidemiology*, vol. 10, no. 1, 1999, pp. 37-48. https://doi.org/10.1097/00001648-199901000-00008.

Benjamini, Yoav and Yosef Hochberg. “Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing.” *Journal of the Royal Statistical Society Series B: Statistical Methodology*, vol. 57, no. 1, 1995, pp. 289-300. https://doi.org/10.1111/j.2517-6161.1995.tb02031.x.

Roberts, David R., et al. “Cross-validation strategies for data with temporal, spatial, hierarchical, or phylogenetic structure.” *Ecography*, vol. 40, no. 8, 2017, pp. 913-929. https://doi.org/10.1111/ecog.02881.

Varma, Sudhir and Richard Simon. “Bias in error estimation when using cross-validation for model selection.” *BMC Bioinformatics*, vol. 7, no. 1, 2006. https://doi.org/10.1186/1471-2105-7-91.

Wilkinson, Mark D., et al. "The FAIR Guiding Principles for Scientific Data Management and Stewardship." *Scientific Data*, vol. 3, 2016, article 160018. https://doi.org/10.1038/sdata.2016.18.

Sculley, D., et al. "Hidden Technical Debt in Machine Learning Systems." *Advances in Neural Information Processing Systems*, vol. 28, 2015. https://papers.nips.cc/paper/2015/hash/86df7dcfd896fcaf2674f757a2463eba-Abstract.html

Bland, J. Martin, and Douglas G. Altman. "Statistical Methods for Assessing Agreement between Two Methods of Clinical Measurement." *The Lancet*, vol. 327, no. 8476, 1986, pp. 307-310. https://doi.org/10.1016/S0140-6736(86)90837-8.

Guo, Chuan, Geoff Pleiss, Yu Sun, and Kilian Q. Weinberger. “On Calibration of Modern Neural Networks.” *Proceedings of the 34th International Conference on Machine Learning*, PMLR, vol. 70, 2017, pp. 1321-1330. https://proceedings.mlr.press/v70/guo17a.html.

Geifman, Yonatan, and Ran El-Yaniv. “Selective Classification for Deep Neural Networks.” *Advances in Neural Information Processing Systems*, vol. 30, 2017. https://arxiv.org/abs/1705.08500.

Ovadia, Yaniv, et al. “Can You Trust Your Model’s Uncertainty? Evaluating Predictive Uncertainty Under Dataset Shift.” *Advances in Neural Information Processing Systems*, vol. 32, 2019. https://arxiv.org/abs/1906.02530.

Huang, Gary B., Marwan Mattar, Tamara Berg, and Erik Learned-Miller. "Labeled Faces in the Wild: A Database for Studying Face Recognition in Unconstrained Environments." *ECCV Workshop on Faces in Real-Life Images*, 2008. https://cs.umass.edu/~elm/papers/Huang_eccv2008-lfw.pdf.

Schroff, Florian, et al.. "FaceNet: A Unified Embedding for Face Recognition and Clustering" *arXiv preprint arXiv:1503.03832*, 2015. https://arxiv.org/abs/1503.03832.

Deng, Jiankang, et al.. "ArcFace: Additive Angular Margin Loss for Deep Face Recognition" *arXiv preprint arXiv:1801.07698*, 2018. https://arxiv.org/abs/1801.07698.

Wang, Hao, et al.. "CosFace: Large Margin Cosine Loss for Deep Face Recognition" *arXiv preprint arXiv:1801.09414*, 2018. https://arxiv.org/abs/1801.09414.

Cao, Qiong, et al.. "VGGFace2: A dataset for recognising faces across pose and age" *arXiv preprint arXiv:1710.08092*, 2017. https://arxiv.org/abs/1710.08092.

Kemelmacher-Shlizerman, Ira, et al.. "The MegaFace Benchmark: 1 Million Faces for Recognition at Scale" *arXiv preprint arXiv:1512.00596*, 2015. https://arxiv.org/abs/1512.00596.

Kärkkäinen, Kimmo and Jungseock Joo. "FairFace: Face Attribute Dataset for Balanced Race, Gender, and Age" *arXiv preprint arXiv:1908.04913*, 2019. https://arxiv.org/abs/1908.04913.

Ruiz, Nataniel, et al.. "DreamBooth: Fine Tuning Text-to-Image Diffusion Models for Subject-Driven Generation" *arXiv preprint arXiv:2208.12242*, 2022. https://arxiv.org/abs/2208.12242.

Ye, Hu, et al.. "IP-Adapter: Text Compatible Image Prompt Adapter for Text-to-Image Diffusion Models" *arXiv preprint arXiv:2308.06721*, 2023. https://arxiv.org/abs/2308.06721.

Huang, Ziqi, et al.. "VBench: Comprehensive Benchmark Suite for Video Generative Models" *arXiv preprint arXiv:2311.17982*, 2023. https://arxiv.org/abs/2311.17982.

Caron, Mathilde, et al.. "Emerging Properties in Self-Supervised Vision Transformers" *arXiv preprint arXiv:2104.14294*, 2021. https://arxiv.org/abs/2104.14294.

Radford, Alec, et al.. "Learning Transferable Visual Models From Natural Language Supervision" *arXiv preprint arXiv:2103.00020*, 2021. https://arxiv.org/abs/2103.00020.

Zhang, Richard, et al.. "The Unreasonable Effectiveness of Deep Features as a Perceptual Metric" *arXiv preprint arXiv:1801.03924*, 2018. https://arxiv.org/abs/1801.03924.

Unterthiner, Thomas, et al.. "Towards Accurate Generative Models of Video: A New Metric & Challenges" *arXiv preprint arXiv:1812.01717*, 2018. https://arxiv.org/abs/1812.01717.

Wang, Zhou, Alan C. Bovik, Hamid R. Sheikh, and Eero P. Simoncelli. "Image Quality Assessment: From Error Visibility to Structural Similarity." *IEEE Transactions on Image Processing*, vol. 13, no. 4, 2004, pp. 600-612. https://doi.org/10.1109/TIP.2003.819861.

Gebru, Timnit, et al.. "Datasheets for datasets" *Communications of the ACM*, 2021. https://doi.org/10.1145/3458723.

Pedregosa, Fabian, et al. "Scikit-learn: Machine Learning in Python." *Journal of Machine Learning Research*, vol. 12, 2011, pp. 2825-2830. https://jmlr.org/papers/v12/pedregosa11a.html.