Annotation Disagreement in Subject-Preservation Evaluation

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Abstract

Disagreement about whether a video preserves its subject may reveal ambiguous evidence, different interpretations, or an unreliable annotation procedure. Collapsing all judgments into a majority label removes this distinction. This article proposes retaining reviewer responses and linking disagreement to reference availability and visible events. ARGUS provides a motivating multi-view generation task. No annotations are collected here. The resulting research design treats disagreement as evidence about the assessment process while keeping it separate from an established failure of the generator.

1. What a disputed identity label contains

Two observers can agree about the visible content of a frame yet disagree about whether it shows the same person. They may assign different importance to face structure, expression, clothing, or contextual cues. Alternatively, one may have missed a brief event during playback. A disputed label therefore does not identify its own cause. An evaluation needs the original judgments and enough procedural detail to investigate plausible explanations without treating every difference as random noise.

ARGUS organizes reference evidence for subject-preserving video (Meng et al., 2026b). Its setting makes disagreement especially relevant when generated views reveal information that is sparse or absent in the references. This article develops a conceptual annotation study; it neither labels ARGUS outputs nor estimates its human agreement. The proposed question is which parts of the assessment remain stable across observers under a declared reference and viewing protocol, and which parts require a narrower scientific claim.

A benchmark's observation count should be accompanied by a description of what an observation is. The LFW pair protocol separates identities from their images and from the comparisons used for verification (Huang et al., 2008). Its documented software access routes also distinguish the ten-fold assessment from development subsets (Pedregosa et al., 2011). Dataset documentation connects those files to their collection and intended purpose (Gebru et al., 2021). A generation benchmark needs an equally explicit account of how a referenced subject becomes a set of output clips and scored frames. Without it, repeated observations may receive unintended weight or cross a supposed assessment boundary. A metadata analysis can establish whether those links are consistent. It should still avoid claiming that consistency of filenames proves recognizability, fair representation, or robustness under viewpoint change. Those are further outcomes requiring appropriate measurements.

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.

2. Preserving the evidence behind a label

A proposed annotation record would link each response to the subject, references, clip version, reviewer, task question, and assessment time. Reviewers should be able to identify an uncertain interval or state that the available evidence is inadequate. Recording only a clip-level winner makes it difficult to distinguish a brief substitution from a generally unfamiliar appearance. Time localization is useful, although it adds a task burden that should be evaluated in a pilot.

The procedure should first provide common instructions and examples that explain the intended notion of preservation. It should avoid training reviewers to imitate one automated metric, since that would make agreement partly a consequence of shared scoring conventions. Independent initial judgments can be followed by a separately documented adjudication stage. The final adjudicated label and the original disagreement answer different questions and should both remain available for analysis.

Analysis could describe response distributions, uncertain cases, and agreement by observed condition. Any summary should account for the assignment of clips to reviewers and the repeated use of subjects. An overall agreement measure may change with the prevalence of obvious failures even if reviewer behavior remains similar. Reporting the underlying response frequencies helps interpret that dependence. A benchmark should not choose the agreement statistic after observing which one makes its labeling process appear most reliable.

The recognition literature provides both scoring methods and warnings about incomplete evaluation populations. FaceNet's learned embedding (Schroff et al., 2015), ArcFace's angular-margin formulation (Deng et al., 2018), and CosFace's margin-based discrimination (Wang et al., 2018) are alternative measurement foundations rather than interchangeable proof of personal identity. VGGFace2 introduces substantial pose and age variation (Cao et al., 2017), MegaFace studies recognition at large scale (Kemelmacher-Shlizerman et al., 2015), and FairFace emphasizes representation in annotated face datasets (Kärkkäinen and Joo, 2019). An identity-preserving generator should be assessed with the relevant dimensions recorded separately. A result from familiar frontal images cannot establish reliability under a missing viewpoint. Nor can a frequency-balanced set establish demographic balance without the required labels. The evaluation should retain failures of detection or measurement, identify what threshold was used, and explain whether the endpoint concerns verification, identification among distractors, or human recognizability over a moving sequence.

An association can suggest a research direction without identifying an intervention. Causal diagrams make it possible to state which variables precede the proposed change, which are consequences, and which selection processes remain uncontrolled (Greenland et al., 1999). Their value lies in exposing assumptions rather than decorating a regression with a causal label. A second issue arises when many outcomes or subgroups are explored: selecting the most favorable comparison can exaggerate the impression of support. False-discovery procedures address a defined family of inferential tests (Benjamini and Hochberg, 1995). They do not turn an exploratory pattern into a replicated mechanism. A careful report distinguishes prespecified comparisons, descriptive sensitivities, and hypotheses requiring new observations. This distinction is especially important when an evaluation offers many plausible metrics.

The protected assessment population should be defined before investigators inspect detailed failures. Once a collection of cases guides repeated revisions, it has become development information even if its filenames still carry an evaluation label. Cross-validation for dependent data treats the grouping of observations as a substantive design decision (Roberts et al., 2017). Research on model selection likewise shows why tuning and final error estimation should not reuse the same information (Varma and Simon, 2006). A practical record therefore distinguishes fitting data, diagnostic examples, validation data, and the

final external assessment. Where only a fixed public table is available, a descriptive analysis remains legitimate, but it should not be renamed a held-out prediction experiment without generating and assessing predictions under a protected split.

3. Using disagreement without overinterpreting it

A disagreement audit could compare disputed cases with matched cases that receive consistent judgments. The comparison would examine reference coverage, visibility, playback, and transformation demands using recorded information. Such associations would help identify priorities for a redesigned study. They would not show that a single visual feature caused disagreement unless the relevant intervention and independent observations were available. An exploratory audit should be labeled accordingly rather than presented as a completed causal explanation.

Disagreement also affects method comparisons. If one generator produces more clips that are difficult to judge, removing them can change both the evaluation population and the apparent ranking. Retaining their frequency as a coverage outcome avoids silently rewarding ambiguity. At the same time, assigning every disputed case to failure can penalize a method for limitations of the assessor. Sensitivity analyses can show the consequences of plausible label treatments while acknowledging that the underlying uncertainty is unresolved.

The desired response is not necessarily to eliminate disagreement. Some transformations make identity genuinely difficult to infer from the available reference set. A useful benchmark can state that limit and distinguish it from preventable procedural inconsistency. Additional reference views, clearer questions, or repeated playback may improve the evidence in a later version. Those changes should receive new version records so that improvements in annotation are not mistaken for improvements in the generator evaluated under the revised procedure.

Different reference-conditioning mechanisms can receive different amounts of information. DreamBooth adapts a generation model around a subject (Ruiz et al., 2022), while IP-Adapter offers a compatible image-conditioning route (Ye et al., 2023). A fair comparison should preserve how many views, which resolutions, and which adaptation opportunities are supplied. CLIP's language-supervised visual representation (Radford et al., 2021) and DINO's self-supervised features (Caron et al., 2021) can provide useful descriptions of outputs, but their training objectives do not establish a person-recognition standard. The endpoint should specify whether success means matching reference appearance, retaining a recognizable subject, or satisfying a requested scene. Those properties can disagree. A useful analysis would keep the disagreement visible and ask which source of evidence each method uses. The relevant advantage is a measured improvement under a declared information budget, not an unqualified increase in similarity to whatever reference happened to be available.

A substitute measurement needs evidence about its relationship to the intended judgment. Two scores may correlate strongly while differing systematically in particular conditions. Method-comparison analysis distinguishes association from agreement (Bland and Altman, 1986). When a system additionally reports confidence, calibration asks whether those probabilities correspond to observed outcome frequencies (Guo et al., 2017). These are separate properties: a metric can rank examples usefully while having a distorted numerical scale or unreliable confidence. An evaluation should therefore retain the original target observations when possible, describe the scale of each endpoint, and examine disagreements instead of averaging them away. A convenient automated score becomes more credible when its limitations are measured in the conditions that motivate its use.

4. Scope, uncertainty, and independent assessment

A moving subject introduces quality dimensions that a still-image identity score cannot exhaust. VBench decomposes video-generation evaluation into multiple aspects (Huang et al., 2023). Structural similarity (Wang et al., 2004), learned perceptual correspondence (Zhang et al., 2018), and video-distribution metrics (Unterthiner et al., 2018) provide additional observations under their respective assumptions. They should not be treated as equivalent measurements of the same latent property. A generated clip can have coherent motion and attractive appearance while changing its subject; it can also preserve recognizable identity while containing visible temporal artifacts. Reporting separate outcomes allows those tradeoffs to be examined. The assessment should specify the frames, viewpoints, occlusion states, and durations included in each score. A difficult case that defeats the identity evaluator should remain part of the coverage record rather than disappearing silently. The resulting profile can support a bounded claim about the observed conditions without implying that every aspect of subject consistency has been solved.

A system's reliable operating region may be narrower than the collection of inputs an interface accepts. Selective prediction separates performance on accepted cases from the fraction of cases accepted (Geifman and El-Yaniv, 2017). Reporting only the former can hide a substantial loss of coverage. Uncertainty estimates may also change under distribution shift (Ovadia et al., 2019), so familiar-condition confidence is not automatically evidence about new subjects, scenes, or control histories. A useful evaluation records rejected, ambiguous, and unsupported cases alongside successful ones. This makes it possible to distinguish limited but honest capability from a system that answers broadly with unreliable certainty. Any proposed deferral rule still requires assessment on its intended input population; it is not validated merely by being described.

The unit of provenance should be fine enough to reconstruct an important disagreement. Reusable identifiers can connect a reported score to its input evidence and the relevant model and evaluator versions (Wilkinson et al., 2016). Pipeline dependencies should be treated as part of the experiment because apparently minor changes can alter output eligibility or metric behavior (Sculley et al., 2015). A report that preserves only aggregate numbers prevents this reconstruction. Keeping per-case results and documented transformations enables alternative weighting and error analysis without repeating expensive generation. Such a record also helps identify whether two papers compare the same scientific objects or merely use similar names for different releases. Versioned evidence makes progress easier to assess across successive evaluations.

5. Conclusion

Disputed annotations deserve a structured record before they become a single reference label. The proposed design retains independent judgments, localizes ambiguity where feasible, and reports how label treatment changes a comparison. It offers no new agreement estimate. Its contribution is to keep uncertainty in the assessment process visible, allowing future subject-preservation studies to separate observed identity failures from disagreements whose evidence or interpretation remains incomplete.

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