Accounting for Compute in Multi-View Identity Conditioning

Caleb Richardson (SO), Kyle Peterson (MU), Ethan Parker (SI)

Abstract

A multi-view conditioning method may improve its outputs while increasing preprocessing, generation, or selection effort. Comparing only the final clip hides that resource tradeoff. This article proposes a compute accounting design for subject-preserving video, using ARGUS as the motivating reference-based system. It records no new timings or hardware measurements. The contribution is a stage-aware evaluation plan that connects identity and task outcomes to the resources actually consumed, while keeping measured performance distinct from assumptions about architectural efficiency.

1. The resource boundary extends beyond generation

A reference-conditioned workflow can include image preparation, reference selection, model loading, subject adaptation, generation, retries, and final review. Some costs occur once per subject, others once per clip, and others once per candidate. A single reported runtime may therefore hide a substantial part of the evaluated workflow. Resource accounting should begin with a declared boundary and a use pattern, since the same upfront cost has different implications for one clip and for many clips of the same person.

ARGUS uses structured identity information in video generation (Meng et al., 2026b). Its setting motivates a comparison of reference use and resource demand, but this article does not benchmark the method's speed or memory. It proposes a measurement design. Claims about efficiency would require direct observations under declared hardware and software conditions, not merely an inference that a particular reference organization must be cheap or expensive because of its conceptual structure.

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.

2. Measuring stages under a declared workload

A proposed experiment would record hardware, model versions, numerical precision, output size, duration, sampling settings, and concurrency. It would separate cold-start and repeated-use conditions where those distinctions matter. Preprocessing and reference-management stages would receive their own measurements, including external model calls if they form part of the workflow. Any unavailable cost would remain explicitly unmeasured rather than being silently treated as zero in the total.

The workload would specify numbers of subjects, references, requested clips, and generated candidates. Costs could be summarized per subject and per accepted output, with the acceptance rule declared in advance. A method that obtains one good clip after many attempts should carry the cost of those attempts when the reported workflow includes selection. A controlled single-attempt comparison can answer another useful question, but it should not be confused with the effort required by a curated final output.

Memory reporting would distinguish relevant notions such as peak accelerator allocation, persistent model storage, and retained reference artifacts. These quantities are not interchangeable, and measurement tools can observe different boundaries. The protocol should identify the instrument and any limitations. Similarly, elapsed time can differ from active device computation because of loading, communication, and scheduling. Recording the measurement conditions permits another reader to judge which part of an observed difference is likely to transfer to another environment.

Precision and identification are separate properties of a result. A narrow interval can surround an association whose scientific target is unclear. Robust covariance calculations allow particular departures from simple error assumptions, including unequal variance (White, 1980) and dependence within declared clusters (Cameron and Miller, 2015). Their role is to improve uncertainty calculations under those assumptions. They do not establish a causal assignment, guarantee a correct measurement, or expand coverage to conditions missing from the data. The analyst should therefore explain both the averaging population and the source of replication. An evaluation repeated many times on one history may provide information about stochastic variability while supplying little evidence about transfer to another history. Those two sources of uncertainty should remain distinguishable in the final interpretation.

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.

A validation split defines which form of transfer has been observed. Related observations can share subjects, scenes, prompts, or acquisition conditions, so randomly splitting rows may leave important information on both sides of the boundary. Structured cross-validation addresses these dependencies (Roberts et al., 2017). Model selection introduces another boundary: choosing a representation, threshold, or tuning parameter using the final assessment data makes the resulting error estimate optimistic (Varma and Simon, 2006). The appropriate held-out unit follows the intended future use. Generalization to a new frame, a new trajectory, and a new subject are not equivalent claims. A study should record those units before fitting or choosing a scoring rule, and preserve an assessment set that has not become part of iterative development.

3. Connecting resources to preservation outcomes

Quality should be assessed under the same workflow whose costs are reported. If the final identity score uses selected outputs while runtime describes only one unselected generation, the two quantities do not form a valid cost-performance pair. The proposed report would link every accepted clip to its attempted candidates and resource record. Identity, instruction execution, visual quality, and measurement coverage would remain separate outcomes before any application-specific tradeoff is calculated.

Comparisons could use matched resource budgets or show outcomes across a range of declared budgets. These designs answer different questions. Matching the number of references alone does not match

compute, and matching runtime alone may not match information access. A useful report should state which constraint is primary and preserve the remaining differences. It should avoid presenting a favorable point on one workload as universal dominance across users with different subject reuse, hardware, and latency requirements.

Uncertainty also matters for resource observations. Repeated timing measurements should identify whether they vary over runs, devices, subjects, or workload composition. A very precise estimate on one machine does not establish portability. The study can still provide useful local evidence when those conditions are clear. Its principal contribution would be an observed tradeoff under a specified use pattern, with enough stage detail to explain which costs might be amortized and which recur whenever a new subject or clip is requested.

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.

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.

An automated measurement should not inherit the authority of its training labels without a direct assessment of use. The relevant checks include systematic disagreement with the intended procedure (Bland and Altman, 1986) and calibration of any probability claims (Guo et al., 2017). Ranking performance, absolute accuracy, and confidence reliability are distinct outcomes. A study can improve one while leaving the others unchanged. Keeping the measurements separate permits examination of those tradeoffs and avoids implying that one favorable statistic establishes all aspects of validity. When independent judgments are unavailable, the limitation should remain part of the result. The absence of a target observation cannot be repaired by combining several correlated proxies into a more elaborate total score.

4. Designing an interpretable preservation study

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.

Reproducibility begins with preserving which scientific objects are being linked. Stable identifiers and reusable metadata help connect inputs, reference observations, model versions, and reported outcomes (Wilkinson et al., 2016). The scoring pipeline also has dependencies that can change the interpretation of a result: preprocessing, representation checkpoints, thresholds, and aggregation code can all matter (Sculley et al., 2015). A compact release should retain those decisions rather than only the final mean. Where source data cannot be redistributed, an access description and hashes can still identify the analyzed inputs. This record allows later users to distinguish a change in the underlying system from a change in how the system was measured. Reproducibility is therefore part of interpretation, not simply a final packaging step.

5. Conclusion

Compute accounting should follow the complete evaluated workflow and its intended reuse pattern. The proposed design links stage costs, attempts, accepted outputs, and independent preservation outcomes. It reports no new efficiency measurement. A future comparison can use these records to identify useful resource tradeoffs without allowing hidden preprocessing, omitted retries, or mismatched workload assumptions to make a subject-conditioning method appear more efficient than the observed evidence supports.

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