Repeatability and Reproducibility of Decisions by Latent Fingerprint Examiners

Bradford T. Ulery1, R. Austin Hicklin1, JoAnn Buscaglia2*, Maria Antonia Roberts3

1 Noblis, Falls Church, Virginia, United States of America, 2 Counterterrorism and Forensic Science Research Unit, Federal Bureau of Investigation Laboratory Division, Quantico, Virginia, United States of America, 3 Latent Print Support Unit, Federal Bureau of Investigation Laboratory Division, Quantico, Virginia, United States of America

Abstract

The interpretation of forensic fingerprint evidence relies on the expertise of latent print examiners. We tested latent print examiners on the extent to which they reached consistent decisions. This study assessed intra-examiner repeatability by retesting 72 examiners on comparisons of latent and exemplar fingerprints, after an interval of approximately seven months; each examiner was reassigned 25 image pairs for comparison, out of total pool of 744 image pairs. We compare these repeatability results with reproducibility (inter-examiner) results derived from our previous study. Examiners repeated 89.1% of their individualization decisions, and 90.1% of their exclusion decisions; most of the changed decisions resulted in inconclusive decisions. Repeatability of comparison decisions (individualization, exclusion, inconclusive) was 90.0% for mated pairs, and 85.9% for nonmated pairs. Repeatability and reproducibility were notably lower for comparisons assessed by the examiners as “difficult” than for “easy” or “moderate” comparisons, indicating that examiners’ assessments of difficulty may be useful for quality assurance. No false positive errors were repeated (n = 4); 30% of false negative errors were repeated. One percent of latent value decisions were completely reversed (no value even for exclusion vs. of value for individualization). Most of the inter- and intra-examiner variability concerned whether the examiners considered the information available to be sufficient to reach a conclusion; this variability was concentrated on specific image pairs such that repeatability and reproducibility were very low on some comparisons and very low on others. Much of the variability appears to be due to making categorical decisions in borderline cases.

Introduction

The forensic use of latent fingerprints and palmprints depends on the analysis, comparison, and evaluation decisions made by expert latent print examiners. An assessment of the accuracy and reliability of those decisions is therefore critical to validating the use of latent prints in forensic science [1]: the recipients of latent print examiners’ decisions must know whether those decisions are correct, and whether they would get the same decisions on a different occasion.

This study measures repeatability and reproducibility of latent print examiners’ decisions: we use the term reproducibility to refer to inter-examiner agreement (whether two examiners reach the same decision on the same fingerprints) and repeatability to refer to intra-examiner agreement (whether one examiner consistently reaches the same decision on the same fingerprints).

To date, there have been several studies demonstrating that examiner decisions are not always in agreement [2,3,4,5] and that individual examiners sometimes change their decisions [4,6,7]. Prior work on repeatability has demonstrated that changed decisions occur under both biasing and non-biasing circumstances; some recent discussion has focused on contextual bias as a potential source of erroneous identifications [8,9,6]. In this study, we investigate the repeatability and reproducibility of decisions under test conditions designed to minimize the effects of bias and other contextual influences.

Our previous study [5] evaluated the accuracy and reproducibility of examiners’ decisions. Subsequent to that initial test, we retested the original participants to observe whether examiners would repeat their decisions after an interval of seven months. Here we present repeatability data from the retest, and further analyses of the reproducibility data from the initial test, to more completely characterize the accuracy and reliability of latent print examiners.

The results of this study strengthen the understanding of latent examiners’ decisions, contributing to the scientific basis for fingerprint examination. This serves the needs of the forensic science community by clarifying the value of forensic evidence with respect to legal questions of admissibility; by helping to identify where to focus training, certification, and standardization; and by providing data to assist agencies in managing finite resources and improving procedures to ensure the quality of results.

Background

Latent prints (“latent”s) are friction ridge impressions (fingerprints, palmprints, or footprints) left unintentionally on items such...
as those found at crime scenes. Exemplar prints ("exemplars"),
generally of higher quality, are collected under controlled
conditions from a known subject using ink on paper or digitally
with a livescan device. Latent print examiners use their expertise
rather than a quantitative standard to determine if the information
content is sufficient to support a given decision. During analysis of a
print, latent print examiners must determine the value of the image
before proceeding to comparison: value for individualization (VID),
value for exclusion only (VEO), or no value (X). After a
comparison of two prints, the examiner makes an evaluation
decision of individualization, exclusion, or inconclusive. The VEO
category is used operationally by a minority of participating latent
print examiners (see Information S10). Many agencies combine the
VID and VEO categories as "value for comparison" [10].

Latent-exemplar image pairs collected under controlled condi-
tions for research are known to be mated (from the same source) or
nonmated (from different sources). An individualization decision
based on mated prints is a true positive, but if based on nonmated
prints, it is a false positive (error); an exclusion decision based on
mated prints is a true negative (error), but is a true negative if based on
nonmated prints. The term "error" is used in this paper only in
reference to false positive and false negative conclusions when they
contradict known ground truth. No such absolute criterion exists
for judging whether the evidence supports reaching a conclusion
as opposed to making an inconclusive decision. The failure to
make an individualization decision on mated prints includes
inconclusive decisions as well as false negative errors: such missed
individualizations may or may not be considered appropriate based
on the sufficiency of information available. The best information
we have to evaluate the appropriateness of reaching a conclusion is
the collective judgments of the experts. Operationally, the
reproducibility of a decision by another examiner therefore serves
as a surrogate for ground truth regarding the appropriateness of a
decision.

This retest was motivated in part by the inter-examiner
disagreements and error rates on the initial test [5], summarized
here. The overall false positive rate for VID comparisons of
nonmated pairs (FPRVID) was 0.1%; the false negative rate for all
comparisons of mated pairs (FNRCMP) was 7.5%. (We use VID
and VEO to qualify comparison decisions to indicate that we are
referencing specific subsets of the data based on latent value. For
example, "VID comparisons" include comparison decisions based
on latents assessed as VID, and not those decisions based on
latents assessed as VEO.) No two examiners made false positive
errors on the same comparison. However, examiners frequently
made false negative errors on the same comparison: 85% of
examiners committed false negative errors; these were distributed
across half of the mated image pairs. False negative rates and
conclusion rates varied by individual examiner and by image pair.
Inter-examiner agreement at the 90% level (at least 90% of
examiners agreeing) was achieved on 66% of latents (deciding
whether VID or Not VID); 73% of mated pairs (deciding
individualization vs. inconclusive); and 56% of nonmated pairs
(deciding exclusion vs. inconclusive). These descriptive statistics
pertain specifically to the mix of data and participating examiners
included in the initial study. The individual examiners did not (and
will not) know how they performed individually on the initial test;
the retest was conducted before any results were reported from the
initial test.

The initial study demonstrated (consistent with prior expecta-
tions) that reproducibility of decisions is highly image dependent.
The overall level of reproducibility on a test such as this, or in any
specific operational environment, can be expected to reflect the
mix of data encountered (image characteristics and the proportion
of mated to nonmated pairs) and the mix of examiners (skills).
Based on the results of the initial test, we were interested to
determine whether erroneous decisions were any less repeatable
than correct decisions, which would have operational implications
for quality assurance. We were also interested in repeatability from
the perspective of the recipient of a decision (posterior probabil-
ities): are certain decisions more or less repeatable than other
decisions? We therefore designed the retest and focused the
analyses to address these several questions. The rates measured in
this study provide useful reference estimates that can inform
decision making and guide future research; the results are not
representative of all situations, and do not account for operational
context and safeguards.

The probability that an examiner will repeat a decision, or that
another examiner will reproduce a decision, depends on many
factors. One factor is the type of examination performed: for
example, whether comparing a single latent to a single exemplar,
or multiple latents to full sets of exemplar prints. We can expect
repeatability to vary from examiner to examiner, and may expect
reproducibility to vary by subpopulation (such as those with similar
training, or by organization). We can expect that when the quality
and quantity of corresponding information present in a pair of
images is either very high or very low, repeatability and
reproducibility will be higher than when the information content
is marginal or when the examination is complex due to factors
such as distortion or background issues. We should not expect
equal rates of agreement for individualization decisions as for
exclusion decisions for two reasons: an exclusion can be justified
based on a single discrepancy, whereas individualization requires
sufficient features in agreement to conclude that the two
impressions originated from the same source; the mated and
nonmated image pairs represent distinct populations whose test
samples were selected by distinct procedures. Finally, rates of
agreement depend on how agreement is defined, which in turn
should reflect the question under investigation: for example,
distinguishing inconclusive from individualization is important
when asking whether examiners agree as to the sufficiency of the
evidence, but this distinction is not relevant when asking whether
blind verification has the potential to detect false negative errors.

To date, there has been little empirical research on the
repeatability and reproducibility of decisions by latent print
examiners. What has been published demonstrates that examiners
usually agree, but are not entirely consistent; the relative
importance of the various contributing factors has not been
established.

Materials and Methods

(See also Information S1)

Ethics Statement: The collection of fingerprints from human
subjects was approved by the FBI Laboratory Institutional Review
Board and the Noblis Institutional Review Board. Use of latent
print examiners in the study was approved by the FBI Laboratory
Institutional Review Board, and written informed consent was
obtained from all participating examiners.

The repeatability retest used the procedures and fingerprints
from the initial study, and a subset of the participants. The
examiners were presented with fingerprints they had seen in the
initial study; they were not told that they had previously seen these
prints. Latents and mated exemplars included a broad range of
attributes and quality, within a range typical of casework. Each
comparison was of an image pair that consisted of one latent and
one exemplar. Image pairs were selected to be challenging;
nonmated pairs were based on difficult comparisons resulting from
In this analysis, we reassign the same image pair to at least two examiners. Each examiner was reassigned up to 7 image pairs on which the examiner had made false negative errors (\(\text{FalseNeg}_M\), \(n = 105\) decisions; 69 mated image pairs); one examiner who initially committed a false positive error was reassigned that image pair (\(\text{FalsePos}_M\), \(n = 1\) decision).

In Information S3, we report results using one such metric, \(kappa\) [16], in a summary of the main results.

Rather than attempting to model randomness in the decision process, one can model the classification process itself. There is a considerable amount of literature on models in which the observed ratings are partially determined by unobservables that themselves have been randomly sampled. The beta-binomial distribution results from a particularly simple model, under which each item (e.g., image-pair) has associated with it an unobservable probability of being classified as “A” instead of “B.” Shuckers [17] discusses its use in fingerprint examination analysis. In Information S9, we use the beta-binomial distribution to derive confidence intervals as a means of providing some indication of our measurement precision.

A much larger class of models fall under the rubric Latent Structure Analysis. Uebersax [18] reviews these models in the context of agreement analysis. For example, under one such model, each item takes on a value for an unobservable continuous variable. Each rater has his/her unobservable threshold for this
variable, which determines the rater’s personal probability of assigning a classification to the item. Among other advantages, this enables estimating the distribution of these thresholds.

Percentage agreement ($P_i$) is defined as follows. Let $p_j$ represent the extent of agreement on the $i^{th}$ image (or image pair):

$$P_i = \frac{1}{n(n-1)} \sum_{j=1}^{k} n_j (n_j - 1)$$

where $n$ is the number of decisions, $k$ is the number of decision categories, and $n_j$ is the number of decisions assigning the $i^{th}$ image (or image pair) to the $j^{th}$ category. $P_i$ is a proportion and can take on values from 0 to 1. When calculating reproducibility, $n$ represents the number of examiners deciding on the $i^{th}$ image (or image pair). When calculating repeatability, $n = 2$, representing the test and retest decisions made by one examiner.

$P$ is simply the mean agreement over a set of $N$ test questions (images or image pairs):

$$P = \frac{1}{N} \sum_{i=1}^{N} P_i$$

This measure weights each test question (image or image pair) equally. Similar results derived from the contingency tables presented would implicitly weight each response equally, resulting in slightly different values.

Both $P$ and kappa implicitly treat all disagreements as being equally serious. So, for example, the disagreement “individualization vs. exclusion” is not weighted differently than the disagreement “individualization vs. inconclusive.” Because various types of disagreements have very different operational consequences, we report separate statistics for each by applying the percentage agreement statistic in multiple ways to address distinct research questions. For example, we measure agreement based on population (mates vs. nonmates), and decision granularity (e.g. 2-way decisions such as {VID, not VID}) vs. 3-way decisions such as {VID, VEO, NV}). It is important to recognize that chance alone would account for some level of agreement: as with any true/false test, the percentage agreement would be expected to be substantially greater than zero even if examiners were guessing. When the response frequency is unequal among the categories, we expect a lower level of agreement.

Results

We report intra-examiner (repeatability) results and compare them to the inter-examiner (reproducibility) results from the initial test. The results include analyses of latent value decisions, comparison decisions, and comparison difficulty. Because the relative proportions of mated and nonmated image pairs are test-specific, comparison decisions are reported separately for mated and nonmated data. Except where specific reference is made to the Within-test dataset or the Multi42 dataset, the reproducibility results are based on the main retest (72 examiners); all reproducibility statistics are from the initial test, and are limited to those 72 retest participants for comparability.

The responses provided on these tests were decisions of individual examiners, which may not reflect the final decisions that an agency would have reported with the benefit of organizational quality management (e.g., verification, or technical and administrative reviews).

Analysis of latent value

Examiners determined the value of each latent print before proceeding to comparison. Together, the initial test and retest resulted in 1,403 pairs of intra-examiner latent value decisions among randomly selected latents that were assigned twice to the same examiner (latents from the Random/Mates and Random/NonMates datasets; see Information S6 for further discussion of data selection for latent value analyses). The extent of repeatability depends on the number of decision categories, based on the treatment of the category “value for exclusion only”. On the question of whether a latent was of value for individualization (2-way decision: {VID, not VID}), repeatability of initial responses was $P = 99.7\%$ (Fig. 1A). When examiners were required to further differentiate NV from VEO (3-way decision: {VID, VEO, NV}), repeatability dropped to $P = 84.6\%$ (Fig. 1B). Complete reversals (between NV and VID) occurred at the rate of 1%. The charts in Fig. 1 depict the contingency table of examiner value decisions (Table 1) as mosaic plots, where the area of each colored region represents the proportion of a combination of initial and retest decisions. For example, in Fig. 1A, 61% of initial value decisions on latents were VID; this corresponds to the height of the row labeled “VID.” On retest, 93% of those VID decisions were repeated; hence, 93% of that row is colored green to indicate VID decisions on the retest. Reading across any one row of a mosaic reveals the conditional probability of a second response given the initial response.

Table 1 reveals two asymmetries. Examiners appeared slightly more willing to call latents VID on the retest than on the initial test, with most of the shift from VEO to VID. There is also a conditional asymmetry resulting from the fact that VEO is an intermediate decision category. Many latents were inarguably VID and NV decisions, and therefore were much more stable than VEO (no latents were unanimously VEO): 85% of NV decisions and 93% of VID decisions on the initial test were repeated, whereas only 55% of VEO decisions were repeated.

The Within-test repeatability data showed very similar results when examiners were retested over a period of days (median 7 days) rather than months (Information S7). On the question of whether a latent was of value for individualization, repeatability was $P = 92.2\%$. This rate is only slightly higher ($p = 0.026$, one-sided) than the rate measured on the retest ($P = 89.7\%$). When examiners were required to further differentiate NV from VEO, repeatability dropped to $P = 88.8\%$. Complete reversals (between NV and VID) occurred at the rate of 1%.

Reproducibility of VID decisions was unanimous on 42% of the latents. The extent of unanimity reflects the data selection: this test was designed to focus on difficult image pairs; if the test had included more latents that were obviously of value or obviously of no value, the overall reproducibility of value decisions would have been higher.

Changed value decisions were almost entirely restricted to latents on which there was some disagreement among examiners (Fig. 2). On the retest, changed decisions occurred on nearly half of the latents on which there was not unanimous agreement among examiners (mean of 5.0 retest decisions per latent). Among the 197 retested images on which there was not initially unanimous agreement, repeatability was $P = 83.3\%$; on these same 197 images, reproducibility was $P = 75.2\%$. This association demonstrates that in almost all cases, the specific images on which examiners individually were not consistent in their own decisions also resulted in disagreement among examiners.

On the initial test, some comparisons resulted in individualization decisions (true positives) even though the latent value decision was VEO, for a rate of 1.8% (40 out of 2,220 VEO comparisons of mated pairs). The retest yielded similar results, albeit on a much
smaller sample: 4/142 VEO comparisons of mated pairs resulted in individualization decisions (true positives).

Comparison Decisions

Repeatability on the RandomNonMates dataset (Table 2), based on three decision categories {VID individualization, exclusion, no conclusion}, was $P = 85.9\%$: 90.6\% of (true) exclusion decisions were repeated; 73.1\% of no conclusion decisions were repeated. We should not expect the proportion of exclusion decisions repeated to equal the proportion of no conclusion decisions repeated: some image pairs will result in more consistent decisions than others, and the test was not designed to result in equal proportions of exclusion, no value or inconclusive decisions.

Repeatability on the RandomMates dataset (Table 3), based on the same three decision categories, was $P = 90.3\%$: 89.1\% of VID individualization decisions were repeated; 90.9\% of no conclusion decisions were repeated. Most of the difference in the repeatability of no conclusion decisions between the RandomNonMates and RandomMates sample populations may be explained by the fact that the RandomMates dataset included a much higher proportion of poor-quality images than did the RandomNonMates [5]. We do not report an overall repeatability percentage: because the study design was based on stratified partitions of data, any such overall rate would reflect the relative sizes of the partitions, not any meaningful result.

In those cases where examiners changed their decisions on whether or not there was sufficient information to individualize, such changes almost always occurred on those image pairs that resulted in non-reproduced decisions (Fig. 3); results are very similar to those shown for value decisions (Fig. 2). The majority of decisions that were not repeated changed to or from inconclusive or VEO decisions: most of the intra-examiner inconsistency was with respect to sufficiency to make a conclusion.

As illustrated in Fig. 2 and Fig. 3, many images and image pairs are associated with highly reliable decisions (repeatable and reproducible); among those images (image pairs) where the group does not achieve highly reproducible results, we observe a high level of intra-examiner inconsistency. Much of the lack of reproducibility is associated with decisions that individual examiners do not reliably repeat.

Examiners were asked to indicate the difficulty of each comparison performed on a scale from “obvious” to “very difficult”. Difficulty proved to be a good predictor of decreased

Table 1. Test-retest repeatability of latent value decisions (3-way contingency table).

| Initial Test | Retest NV | Retest VEO | Retest VID | Total | Repeated |
|--------------|-----------|------------|------------|-------|----------|
| NV           | 249       | 34         | 10         | 293   | 85%      |
| VEO          | 38        | 137        | 75         | 250   | 55%      |
| VID          | 8         | 51         | 801        | 860   | 93%      |
| Total        | 295       | 222        | 886        | 1,403 |          |

The table summarizes 1,403 pairs of decisions made by 72 examiners on 339 distinct latent images. Examiners changed their 2-way (VID, not VID) latent value decisions on 94 distinct latents.

doi:10.1371/journal.pone.0032800.t001

doi:10.1371/journal.pone.0032800.g001

Figure 1. Test-retest repeatability of latent value decisions (mosaic charts). (A) 2-way (VID, Not VID) latent value decisions: $P = 89.7\%$. (B) 3-way latent value decisions (NV, VEO, VID) including category “value for exclusion only”: $P = 84.6\%$. These mosaic plots depict the tabular data from Table 1, indicating for each category of initial test response (y-axis) the proportion of each category of retest response (x-axis).

Repeatability of Latent Fingerprint Decisions
Reproducibility (C and D) are quite similar, but the rates of inter-exclusion, VEO individualization, respectively, for 7-way decisions {NV, VEO inconclusive, VEO exclusion}. The corresponding patterns for exclusion, VEO individualization, VID inconclusive, VID exclusion, respectively, for 7-way decisions {NV, VEO inconclusive, VEO exclusion}. The corresponding patterns for repetition and reproducibility of both individualization and exclusion decisions (Fig. 4; Table 4; see also Information S7).

Fig. 5 and Fig. 6 summarize the aforementioned repeatability statistics and contrast these with corresponding measures of reproducibility to reveal the broad trends (see also Information S8). As expected, we see that agreement decreases as the number of decision categories increases. On latent value decisions, most of the intra-examiner variability was already evident on the Within-test dataset; the additional seven months added only a small increment. Fig. 5 and Fig. 6 also show that most of the observed inter-examiner variability can be attributed to intra-examiner effects. This pattern is especially strong on nonmate decisions (Fig. 6, Nonmates), where the intra-examiner rate of disagreement is about 70% as large as the inter-examiner rate of disagreement. On comparisons of mated pairs, intra-examiner effects account for most of the observed variability on individualization decisions (Fig. 6, 2-way Mates), while false negative errors are a major source of inter-examiner disagreements (compare 2-way Mates to 3-way Mates).

The overall patterns of agreement and disagreement tended to be similar for intra- and inter-examiner pairs of responses (Fig. 7; see also Information S9). In Fig. 7, mosaics A and B show in detail the patterns of repeatability on nonmated and mated pairs, respectively, for 7-way decisions {NV, VEO inconclusive, VEO exclusion, VEO individualization, VID inconclusive, VID exclusion, VID individualization}. The corresponding patterns for reproducibility (C and D) are quite similar, but the rates of inter-examiner disagreement are higher than the rates of intra-examiner disagreement.

One half of the 3-way disagreements {VID individualization, any exclusion, other} on mated pairs were due to false negative errors (Fig. 7D): 9.6% of the paired responses among examiners were disagreements involving false negative errors.

Repeatability and Reproducibility of Errors

For the purposes of operational quality assurance, there is a particular interest in understanding repeatability and reproducibility with respect to false positive and false negative errors.

Six false positives were committed by five examiners on the initial test (Table 3): none of these errors were reproduced in the initial test, and none were repeated in the retest (n = 4). No new false positive errors were committed during the retest among 645 randomly selected nonmate repeat assignments (Table 2), which is consistent with the false positive rate of 0.1% on the initial test.

The retest participants committed false negative errors at the rate of 8.8% (FNR_{cmp}) on the initial test. The majority of those errors were not repeated (Table 6): of the 226 false negative errors that were retested, 68 were repeated (30.1%). We estimate the probability that another examiner would reproduce one of these errors to be 19% (Table 7). We understand these comparative results as follows: “self-verification” (several months later) detected 69.9% of the false negative errors, whereas independent examination of the same images by another examiner (analogous to blind verification) would have detected an estimated 81%.

**Table 2.** Repeatability of comparison decisions on RandomNonMates dataset.

| Retest (Nonmates) | Initial Test | No Conclusion | Exclusion | VID Indiv. | Total | Repeated |
|-------------------|--------------|---------------|-----------|------------|-------|----------|
| No Conclusion     | 128          | 47            | 0         | 175        | 73.1% |          |
| Exclusion         | 44           | 426           | 0         | 470        | 90.6% |          |
| Total             | 172          | 473           | 0         | 645        |       |          |

P=85.9%. Contingency table of the 645 repeat assignments of nonmated image pairs, on which the examiner did not initially commit a false positive error. No conclusion includes NV, inconclusive, and VEO individualization. Exclusions include comparisons of latents rated VEO and VID.

doi:10.1371/journal.pone.0032800.t002

**Table 3.** Repeatability of comparison decisions on RandomMates dataset.

| Retest (Mates) | Initial Test | No Conclusion | Exclusion | VID Indiv. | Total | Repeated |
|----------------|--------------|---------------|-----------|------------|-------|----------|
| No Conclusion  | 479          | 15            | 33        | 527        | 90.9% |          |
| VID Indiv.     | 20           | 9             | 236       | 265        | 89.1% |          |
| Total          | 499          | 24            | 269       | 792        |       |          |

P=90.3%. Contingency table of the 792 repeat assignments of mated image pairs, on which the examiner did not initially commit a false negative error. Examiners repeated 89.1% of true individualization decisions. No conclusion includes NV, inconclusive, and VEO individualization. Exclusions include comparisons of latents rated VEO and VID.

doi:10.1371/journal.pone.0032800.t003

Figure 2. Repeatability and reproducibility of 2-way latent value decisions {VID vs. Not VID}. Percentage of examiners rating each latent VID (y-axis), in rank order (x-axis), color-coded by repeatability; n = 252 latents on which at least 3 examiners were retested. Examiners were initially unanimous on 107 of these 252 latents; value decisions changed on 3 of these. Reproducibility rates were based on 53.2 mean examiners per latent (s.d. 21.7); repeatability rates were based on 5.0 mean examiners per latent (s.d. 2.3).

doi:10.1371/journal.pone.0032800.g002
Interestingly, much of the relative benefit of blind verification over this type of self-verification relates to the wide variability in FNR by examiner: false negative errors are produced disproportionately by those examiners with high FNRs, so another examiner selected at random to perform verification is likely to have a lower FNR. Difficulty was not predictive of whether false negative errors would be repeated; the data suggest that greater difficulty is weakly associated with lower reproducibility for false negative errors. Although most errors were not repeated on the retest, examiners did introduce new false negative errors (Table 3). After correcting for the difference in test mix between the initial test and the retest, no significant net change in false negative error rate was observed.

Table 7 compares the repeatability and reproducibility rates for mated pairs contingent upon whether the initial decision was an

---

**Figure 3.** Repeatability and reproducibility of 2-way individualization decisions (VID individualization, other). Percentage of examiners individualizing mated image pairs (y-axis), in rank order by VID individualization (x-axis), colored-coded by repeatability. Y-axis is based on 4,006 initial decisions (excludes false negative responses; 10.3 mean examiners per image pair; s.d. 2.6). Color-coding is based on 792 retest decisions on 389 mated image pairs (RandomMates dataset; 2.0 mean examiners per image pair; s.d. 1.1). Non-repeated decisions occurred on 46 of the 389 image pairs. Examiners were initially unanimous on 257 of the 389; decisions were not repeated on 2 of these.

doi:10.1371/journal.pone.0032800.g003

---

**Figure 4.** Repeatability (A) and reproducibility (B) of individualization decisions by difficulty. (A) Retest decisions by difficulty where the initial test decision was an individualization (269 paired decisions (test-retest) on 147 image pairs, 144 of which were mated). (B) Reproducibility of individualization decisions by difficulty (1,615 individualization decisions (15,990 paired examiner responses) by the 72 examiners on 249 image pairs, 246 of which were mated). Results for exclusion decisions were similar ([Information S7](#)).

doi:10.1371/journal.pone.0032800.g004
erroneous exclusion. These data indicate that blind verification (estimated by reproducibility) is more effective than self-verification (repeatability) in detecting false negative errors (81% vs. 69.9%). Based on a baseline FNRCMP = 8.8% (as measured among retest participants on the initial test) and Table 7 (first row), we estimate that if every exclusion decision were verified, the resulting rate of erroneously corroborated false negatives would be 2.7% (self-verified) and 1.7% (blind-verified).

Discussion

In order to better understand limitations to the reliability of examiner decisions, and to develop strategies for improvement, we need to understand the types of errors and disagreements that occur and the circumstances under which they occur. Analyses of repeatability and reproducibility can provide indications of the causal factors contributing to disagreements among examiners and erroneous conclusions. For example, differences in examiner skill or judgment would be consistent with errors and disagreements that tend to persist, whereas differences that do not persist might reflect inadvertent errors or borderline decisions.

While the rates we report reflect the specific test data and the performance of participants, several general conclusions may be drawn. Most but not all examiner decisions were highly repeatable and reproducible. The overall patterns of agreement and disagreement tended to be similar for repeatability and reproducibility. Much of the lack of reproducibility was associated with specific images and image pairs on which individual examiners were not highly consistent. Most of the inter- and intra-examiner inconsistency was with respect to whether the information available was sufficient to make a conclusion. Examiner assessments of comparison difficulty were a good predictor of low repeatability and reproducibility.

Why do examiners not always repeat their own decisions? Most of the inconsistency pertains to whether the examiners considered that the information available was sufficient to reach a conclusion (such as between individualization and inconclusive decisions). Our interpretation is that there is a continuum of the quality and quantity of features as interpreted by examiners. Much of the variability arises from making discrete decisions in this continuous decision space in borderline or complex cases (“complex” decisions are defined in [10]). When decisions were not repeated or reproduced, the majority changed to or from inconclusive or VEO decisions. Lack of repeatability for complex or borderline decisions may be attributed to differences in the examiner’s assessments of features in each print, or to differences in how the examiner uses those features in making value or comparison decisions. An examiner’s assessments of the quality and quantity of features in a given print may vary. Schiffer and Champod [19] found that the number of minutiae detected by an examiner increases with training; Dror, et al. [20] found that the number of minutiae observed by an examiner varied from test to retest.

Differences in assessments of features may be especially critical when key features are ambiguous. However, if the examiner

![Figure 5. Percentage agreement on latent value.](image_url)

The figure shows the percentage agreement on latent value, comparing 2-way (VID, Not VID) and 3-way (VID, VEO, NV) latent value repeatability measured within the initial test (“Days”), and between the test and retest (“Months”). Reproducibility is computed from the initial test results. All statistics are limited to the 72 retest participants; “N” indicates the number of decisions and, parenthetically, the number of distinct latents. Confidence intervals for these estimates are discussed in Information S9.

Table 4. Repeatability and reproducibility of individualization and exclusion decisions, by examiner assessment of difficulty.

| Difficulty Level | Individualization | Exclusion |
|------------------|-------------------|-----------|
|                   | Repeated | Reproduced | Repeated | Reproduced |
| Obvious/Easy/Medium | 92%     | 85%        | 88%      | 77%        |
| Difficult/Very Difficult | 69%     | 55%        | 70%      | 50%        |

Table 4. Repeatability and reproducibility of individualization and exclusion decisions, by examiner assessment of difficulty.

| Difficulty Level | Individualization | Exclusion |
|------------------|-------------------|-----------|
|                   | Repeated | Reproduced | Repeated | Reproduced |
| Obvious/Easy/Medium | 92%     | 85%        | 88%      | 77%        |
| Difficult/Very Difficult | 69%     | 55%        | 70%      | 50%        |

doi:10.1371/journal.pone.0032800.t004
reaches a different decision without changing assessments of the quality and quantity of features, then the examiner is not applying decision criteria consistently. This may be attributable in part to the lack of quantitative criteria and limited qualitative criteria for decisions: in some difficult cases it is not apparent to the examiner whether a conclusion or inconclusive decision is appropriate. Other plausible explanations for why decisions would not be repeated may include inadvertent mistakes, changes in outside influence or bias, or changes in expertise over time. While any of these may apply to casework, given the study design we do not consider contextual bias and changes in expertise to be significant contributing factors to the findings in this study.

Why do different examiners reach different decisions? Much of the observed lack of reproducibility is associated with prints on which individual examiners were not consistent, rather than persistent differences among examiners. When inter-examiner disagreements on decisions are not associated with a lack of repeatability, we suggest the following explanations: examiners differ as to which features are present in each print [9,19,20]; examiners differ on the relative costs or implications of decisions (e.g., weighing the benefit of a true positive against the cost of a false positive, or against the cost of an inappropriate inconclusive decision); examiners differ as to whether the information present is sufficient to support a specific decision, while agreeing on features and costs; examiners differ in skill and experience (e.g., we previously found that conclusion rate increased with experience [5]); or examiners differ in their use of terminology (the exact meaning of a decision varies by agency, often related to variations in operating procedures).

Repeatability and reproducibility are of particular importance with respect to false positive and false negative errors. Six false positive errors were committed on the initial test. None of these were reproduced, implying that blind verification should be highly effective at detecting such errors. Four of these comparisons were performed again months later by the examiners who initially committed each error; none of the errors were repeated. The lack of both repeatability and reproducibility suggests that quality control procedures would detect false positive errors such as these, assuming that contradictory decisions would be subject to a rigorous review.

False negative errors contributed substantially to both inter- and intra-examiner disagreements on mated comparisons. The false negative error rate (FNRCMP, among the retest participants on the initial test) was 8.8%. When these examiners were retested months later, 69.9% of false negative errors were not repeated. Our corresponding estimate of reproducibility indicates that independent examinations (analogous to blind verification) would have resulted in disagreements on 81% of the false negative errors committed by the original examiner, presumably resulting in a conflict resolution review. This implies that blind verification by another examiner should be expected to catch the majority of false negative errors, but a substantial proportion (19%) would not result in a contradictory decision, and therefore would be corroborated rather than detected. Interestingly, the effectiveness of blind verification is partly due to the wide variability in FNR by examiner: false negative errors are produced disproportionately by those examiners with high FNRs, so another examiner selected at random to perform blind verification is likely to have a lower FNR. While false negative errors were associated with examiner assessments of difficulty [5], the reproducibility of errors was not well predicted by examiner assessments of difficulty, and repeated false negative errors were not highly concentrated on specific image pairs.

Repeatability and reproducibility are useful surrogate measures of the appropriateness of decisions when there is no “correct” decision, as when deciding between individualization and

![Figure 6. Percentage agreement on comparisons of mated and nonmated image pairs. 2-way Mates (VID individualization, other), 2-way Nonmates (exclusion, other), 3-way (VID individualization, any exclusion, other), and 7-way (NV, VEO inconclusive, VEO exclusion, VEO individualization, VID inconclusive, VID exclusion, VID individualization). Repeatability is computed from the RandomMates and RandomNonMates datasets; reproducibility is computed from the initial test results. While the 2-way and 3-way decisions correspond to common operational practice, only a subset of the 7-way distinctions would correspond to any specific operational practice. All statistics are limited to the 72 retest participants; “N” indicates the number of decisions and, parenthetically, the number of distinct image pairs. Confidence intervals for these estimates are discussed in Information S9.](https://doi.org/10.1371/journal.pone.0032800.g006)
The reproducibility of decisions has operational relevance in situations where more than one examiner makes a decision on the same prints. Reproducibility as assessed in our study can be seen as an estimate of the effects of blind verification [21]—not consulting or non-blind verification. Verification is an agency-specific quality assurance measure conducted with the intent of detecting any errors before decisions are formally reported by the agency. Verification of individualization decisions is standard practice [10], but whether other decisions are verified varies by agency. Typically, the verifier is aware of the first examiner’s decision ("non-blind verification"). Blind verification, in which the verifier performs an independent examination without knowledge of the first examiner’s decision, is practiced by some agencies either in addition to or instead of non-blind verification. In casework, examiners also may consult with each other, benefiting from a second opinion prior to reaching a decision. The repeatability of decisions has a more subtle relation to casework: in practice, examiners typically have hours or days to catch any mistakes and reassess complex decisions before reporting them. Our study did not provide an opportunity for examiners to reconsider their decisions at a later time before making a final decision, and therefore might underestimate the repeatability of decisions in practice.

Our estimates of reproducibility and repeatability may differ from operations for several reasons. The comparisons in the test were selected to be representative of difficult comparisons from searches of an Automated Fingerprint Identification System (AFIS), including few comparisons where the correct conclusion was obvious. The responses provided on this test were decisions of individual examiners without the benefit of verification or quality assurance, and therefore may not correspond to the final decisions reported by an agency. Examiners also were not permitted to revisit their own decisions during the test. Because practices vary from agency to agency, the test required some examiners to make distinctions that may have been unfamiliar, or at least outside their routine practice. Because participants knew that they were being inconclusive. The reproducibility of decisions has operational relevance in situations where more than one examiner makes a decision on the same prints. Reproducibility as assessed in our study can be seen as an estimate of the effects of blind verification [21]—not consulting or non-blind verification. Verification is an agency-specific quality assurance measure conducted with the intent of detecting any errors before decisions are formally reported by the agency. Verification of individualization decisions is standard practice [10], but whether other decisions are verified varies by agency. Typically, the verifier is aware of the first examiner’s decision ("non-blind verification"). Blind verification, in which the verifier performs an independent examination without knowledge of the first examiner’s decision, is practiced by some agencies either in addition to or instead of non-blind verification. In casework, examiners also may consult with each other, benefiting from a second opinion prior to reaching a decision. The repeatability of decisions has a more subtle relation to casework: in practice, examiners typically have hours or days to catch any mistakes and reassess complex decisions before reporting them. Our study did not provide an opportunity for examiners to reconsider their decisions at a later time before making a final decision, and therefore might underestimate the repeatability of decisions in practice.

Our estimates of reproducibility and repeatability may differ from operations for several reasons. The comparisons in the test were selected to be representative of difficult comparisons from searches of an Automated Fingerprint Identification System (AFIS), including few comparisons where the correct conclusion was obvious. The responses provided on this test were decisions of individual examiners without the benefit of verification or quality assurance, and therefore may not correspond to the final decisions reported by an agency. Examiners also were not permitted to revisit their own decisions during the test. Because practices vary from agency to agency, the test required some examiners to make distinctions that may have been unfamiliar, or at least outside their routine practice. Because participants knew that they were being
testing, some may have reacted to the test ("Hawthorne effect") by trying harder than usual to reach conclusions, or by being more or less cautious than during casework.

Many of the issues raised by these findings could be addressed through enhancements to quality management systems such as the following, some of which are currently used in some forensic laboratories:

- Blind verification can be expected to be effective in detecting most errors and flagging debatable decisions, and should not be limited to individualization decisions.
- Examiner assessments of difficulty may be useful in targeted quality control, which could focus on difficult decisions: operating procedures could provide means for an examiner to indicate when a particular decision is complex. Quality control measures, however, should not focus solely on difficult decisions, since even easy or obvious decisions were not always repeated or reproduced.
- Borderline and complex decisions may benefit from collaboration and consultation among examiners to take advantage of inter-examiner variation on feature selection or decision thresholds.
- Metrics derived from the quality and quantity of features used in making a decision may assist examiners in preventing mistakes, and in making appropriate decisions in complex comparisons. Such metrics may be used to flag complex decisions that should go through additional quality assurance review and arbitration of disagreements between examiners.

There is a need for dialog in the community to address the extensive differences in terminology and procedures in the latent print community (see survey responses in [5]). For example, the relatively high FNR suggests the need to come to agreement on appropriate criteria for exclusion decisions, including decision thresholds based on costs and operational implications.

Further research is needed to better understand how inter- and intra-examiner variability arises. One approach to understanding the source of inter-examiner disagreements would be to conduct a "white box" test in which examiners document the basis for their decisions in the form of image markup. The objectives of such a study would be to investigate similarities and differences in examiners’ interpretations of the features in a latent; how examiners assess sufficiency to reach a conclusion; and to assist in the development of guidelines and automated metrics to use the

| Table 5. Examiner responses on the six image pairs (labeled A–F in [5]) that resulted in false positive errors. |
|---|---|---|---|---|---|---|
| Test Response | Image Pair (Nonmates) | A | B | C | D | E | F |
| VEO | Exclusion | 2 | – | – | – | – | – |
| | Inconclusive | 11(II) | – | – | – | – | – |
| | Individualization | – | – | – | – | – | – |
| VID | Exclusion | 6 | 24(II) | 22 | 21 | 20(II) | 21 |
| | Inconclusive | 6 | 5 | 3 | – | 1 | 1(II) |
| | Individualization | 1(II) | 1(II) | 1(II) | 1(II) | 1(II) | 1(II) |

*Multi42 dataset. Cell counts indicate the distribution of responses from all 169 examiners on the initial test. The initial (I) and retest (R) responses are indicated for the examiners who committed the false positive errors. One examiner who committed two errors (image pairs C and D) did not participate in the retests; the retest response for one image pair is from the.*

doi:10.1371/journal.pone.0032800.t005

| Table 6. Repeatability of false negative errors on (A) FalseNeg and (B) FalseNeg_M datasets. |
|---|---|---|---|---|---|---|
| A | Retest (Mates) | Initial Test | Exclusion | Inconclusive | Indiv. NV | Total | Repeated |
| Exclusion | 68 | 97 | 47 | 14 | 226 | 30.1% |
| B | Multi42 (Mates) |
| Exclusion | 29 | 37 | 23 | 16 | 105 | 27.6% |

*Data limited to mated pairs that were erroneously excluded in the initial test; includes comparisons of latents rated VEO and VID.*

doi:10.1371/journal.pone.0032800.t006

| Table 7. Repeatability and reproducibility for mated pairs, contingent upon whether the initial decision was false negative. |
|---|---|---|---|---|---|
| | Repeatability (Retests) | Reproducibility (Initial Test) |
| FN (n = 226) | 30.1% | 19.2% |
| Not FN (n = 792) | 97.0% | 94.5% |

*For comparability, all estimates are limited to responses of the retest participants. "Not FN" includes NV, inconclusive, and individualization. Confidence intervals for these estimates are discussed in information S9, as well as modeling assumptions for the reproducibility results.*

doi:10.1371/journal.pone.0032800.007
quality and quantity of features in an image to predict whether a decision is likely to be debatable or highly reproducible.

Supporting Information

Information S1 Overview of initial study design.
(PDF)

Information S2 Structure of the retest.
(PDF)

Information S3 Representativeness of retest participants.
(PDF)

Information S4 Repeatability and reproducibility contingency tables.
(PDF)

Information S5 Within-test repeatability of value decisions.
(PDF)

Information S6 Data used in the latent value repeatability analysis.
(PDF)

Information S7 Difficulty predicts repeatability and reproducibility.
(PDF)

Information S8 Summary agreement statistics.
(PDF)

Information S9 Confidence intervals.
(PDF)

Information S10 Use of “Value for Exclusion Only” category.
(PDF)

Acknowledgments

We thank the latent print examiners who participated in this study, as well as William Fellner, Calvin Yeung, Ted Unnikumaran, Harold Korves, and William Chapman. This is publication number 11–18 of the FBI Laboratory Division. Names of commercial manufacturers are provided for identification purposes only, and inclusion does not imply endorsement of the manufacturer, or its products or services by the FBI. The views expressed are those of the authors and do not necessarily reflect the official policy or position of the FBI or the U.S. Government.

Author Contributions

Conceived and designed the experiments: BTU RAH JB MAR. Performed the experiments: BTU RAH JB MAR. Analyzed the data: BTU RAH JB MAR. Contributed reagents/materials/analysis tools: BTU RAH. Wrote the paper: BTU RAH JB MAR.

References

1. National Research Council (2009) Strengthening forensic science in the United States: A path forward. (Natl Acad Press, Washington, DC).
2. Evett IE, Williams RL (1996) A review of the sixteen points fingerprint standard in England and Wales. J Forensic Identification 46: 49–73. Available: http://www.thefingerprintinquiryscotland.org.uk/inquiry/files/DB_0769-02.pdf.
3. Gutowski S (2006) Error rates in fingerprint examination: the view in 2006. The Forensic Bulletin Autumn 2006: 18–19.
4. Langenburg G (2009) A Performance study of the ACE-V process: a pilot study to measure the accuracy, precision, reproducibility, and the biasability of conclusions resulting from the ACE-V process. J Forensic Identification 59(2): 219–257.
5. Ulery BT, Hicklin RA, Buscaglia J, Roberts MA (2011) Accuracy and reliability of forensic latent fingerprint decisions. Proc Natl Acad Sci USA 108(19): 7733–7738. Available: http://www.pnas.org/content/108/19/7733.full.pdf.
6. Dror IE, Charlton D, Peron AE (2006) Contextual information renders experts vulnerable to making erroneous identifications. Forensic Sci Int 156: 74–78. Available: http://www.sequentialunmasking.org/su/Dept_Contextual_FSI_2006.pdf.
7. Dror IE, Charlton D (2006) Why experts make errors. J Forensic Identification 56(4): 600–616. Available: http://users.ecc.soton.ac.uk/id/JFI%20expert %20error.pdf.
8. Risiger DM, Saks MJ, Thompson WC, Rosenhal R (2002) The Daubert/Kumho implications of observer effects in forensic science: hidden problems of expectation and suggestion. Calif Law Rev 90(1): 1–56. Available: http://dash.harvard.edu/handle/1/3199069.
9. Langenburg G, Champod C, Werthem P (2009) Testing for potential contextual bias effects during the verification stage of the ACE-V methodology when conducting fingerprint comparisons. J Forensic Sciences 54(5): 571–582.
10. SWGFAST (2011) Standards for conducting friction ridge impressions and resulting conclusions, version 1.0. Available: http://swgfast.org/Documents.html.
11. Efron B, Tibshirani RJ (1993) An introduction to the bootstrap. New York: Chapman and Hall, 1993.
12. Fleiss JL (1971) Measuring nominal scale agreement among many raters. Psychol Bull 76(3): 378–382.
13. Gwet K (2002) Kappa statistic is not satisfactory for assessing the extent of agreement between raters. Series: Statistical Methods for Inter-Rater Reliability Assessment 1(1): 1–5. Available: http://www.agreestar.com/research_papers/kappa_statistic_is_not_satisfactory.pdf.
14. Rust RT, Good B (1994) Reliability measures for qualitative data: theory and implications. J Marketing Res 31: 1–14. Available: http://www2.owen.vanderbilt.edu/bruce.cooil/Documents/Publications/JMR%201994.pdf.
15. Perrault WD, Jr., Leigh LE (1989) Reliability of nominal data based on qualitative judgments. J Marketing Res 26: 135–140. Available: http://www.viktoria.se/~dixi/BISON/resources/perrault-leigh-1989.pdf.
16. Brennan RL, Prediger DJ (1985) Coefficient kappa: some uses, misuses, and alternatives. Educational and Psychological Measurement 41: 687–699.
17. Schuckers ME (2005) Using the beta-binomial distribution to assess performance of a biometric identification device. Int J Image Graph 3(3): 323–329.
18. Uebersax JS (1992) A review of modeling approaches for the analysis of observer agreement. Investigative Radiology 17: 738–743.
19. Schiffer B, Champod C (2006) The potential (negative) influence of observational biases at the analysis stage of fingerprint individualization. Forensic Sci Int 167(2–3): 116–120. Available: http://www.sequentialunmasking.org/su/Schiffer_Observational_Bias_2007.pdf.
20. Dror IE, Champod C, Langenburg G, Charlton D, Hunt H, et al. (2011) Cognitive issues in fingerprint analysis: intra- and inter-expert consistency and the effect of a ‘target’ comparison. Forensic Sci Int 208: 10–17. Available: http://cognitiveconsultantsinternational.com/Dept_FSI_cognitive_issues_ fingerprint_analysis.pdf.
21. SWGFAST (2011) Standard for the application of blind verification of friction ridge examinations, version 1.0. Available: http://swgfast.org/Documents.html.