J Forensic Sci, Mar. 2003, Vol. 48, No. 2
Paper ID JFS2002246_482
Available online at: www.astm.org

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Firearm and Toolmark Identification Criteria: A Review of the Literature, Part II

ABSTRACT: An update to a previously published review of articles pertaining to firearm and toolmark identification criteria is presented. In this update, 22 additional articles were reviewed, including works of a general nature, studies critically assessing the theory of consecutive matching striations, empirical studies involving various firearm components, toolmark studies, as well as articles discussing the utility of statistics in the firearms and toolmark identification discipline. These articles have been reviewed in a format to permit others to learn what has been published in the field in an effort to educate interested parties. Further, a discussion of the importance of articulation and communication within the discipline is presented.

KEYWORDS: forensic science, firearms and toolmark identification, identification criteria

In 1997, this author published Firearm and Toolmark Identification Criteria: A Review of the Literature (1). In that work, 34 articles were reviewed that appeared to represent significant efforts towards the development and communication of identification criteria in the area of firearm and toolmark identification. These articles included empirical studies of consecutively manufactured firearm components and tools as well as various mathematical and computer models. The purpose of the review article was to recognize that these works appeared "to be based at least in part on the scientific method which tests hypotheses by experimenting and making observations" and could be used to permit examiners to develop and better articulate their own criteria for identification (1).

Since that publication there has been a plethora of work performed that relates directly to the issue of identification criteria for firearms and toolmark examination. The purpose of this article is to provide a review of the more recent literature not addressed in the first review.

Some significant differences in the content of the articles will be apparent. The early articles were rather sparse and written to communicate that marks made by different tools or firearms could be differentiated. The authors would discuss the study as designed and then provide information that appropriate identifications could be effected. The early mathematical and computer models were attempts at defining a more objective identification criteria.

The articles since that first review have tended to be much more descriptive. More importantly, many of them have taken a proposition—the criteria for identification as proposed by Biasotti and Murdock, first in 1997 and then in 2002 as a reprint of the 1997 text (2)—and designed experiments in attempts to falsify the proposed hypothesis. The primary purpose has been to define more objective criteria for identification, especially in light of some recent court decisions such as *Daubert v. Merrell Dow Pharmaceuticals* (509, U.S. 579 1993) and *Kumho Tire Company v. Carmichael* (131 f.3d 1433, No. 97–1709). In addition, there has been further discussion of the statistical significance of toolmark identifications.

The articles have been divided according to the primary purpose of the author(s). Since 1997, there have been some well-written discussions with respect to some of the critical points in the discipline that had suffered from previously poor discussion and articulation. In addition, this latter period is marked by an effort to look at the theory of consecutive matching striations (CMS) first proposed by Biasotti in 1959 (3) and develop it into an objective criteria for identification. As in the first review, there are articles dealing with barrels, breech faces, and other tools through which concepts such as identification criteria, class, subclass, and individual characteristics are communicated. Finally, there are the statistical arguments, which appear to be developing around the Bayes theorem.

As with the first article, it is hoped that through this review and summary firearm and toolmark examiners might be better able to articulate their craft to the end-users. This need for articulation has never been more evident considering the very recent court decision in Florida (4). While maybe more evident, the need has always been there but simply not adequately addressed. The first step in addressing this need is a recognition and appreciation for what has been accomplished. This is important because it helps to clarify what can be said. Secondarily, it is important for the purpose of defining what still needs to be accomplished. The next step is to define the future course of action in a manner that is consonant with sound scientific practice and in a manner that is understood by the end-user of the discipline. As did the first review, the bulk of this article addresses the first step. Unlike the first review, this article will also delve into the second step.

General Discussion Articles

John Collins has provided one of the more important recent contributions of a general nature simply for his recognition that one of the primary issues of importance is not necessarily the reported struggle between objective and subjective criteria, but the ability to articulate what one is doing (5). In an attempt to discuss the oft-

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Received 19 July 2002; and in revised form 21 Oct. 2002, 22 Oct. 2002, and 16 Nov. 2002; accepted 16 Nov. 2002; published 6 Feb. 2003.

argued, apparent battles of art versus science and subjectivity versus objectivity, Collins uses the English language with 26 letters as an analogy to toolmark identification. Credit is given to Collins, as it is clear he recognizes the difficult, yet very real, need to communicate these very difficult concepts to an audience that does not have the requisite knowledge or skill. While some may scoff at his use of such an analogy, there is much to be appreciated in its use.

He argues that science and art are not mutually exclusive nor are scientific validity and subjectivity, as many have suggested and argued. Collins suggests professional standards and scientific tests monitor subjectivity such that, "the scientific validity of the identification sciences should not depend on numerical criteria" (5). He summarizes by indicating that the discussions should not be centered on objectivity versus subjectivity and art versus science. Nor should they be focused on specific tool-medium combinations as the courts are in the habit of doing, but concentrating on identifying, establishing, and articulating the scientific validity of the discipline. He states that, "scientific validity occurs when we have incorporated continuous testing, peer review, and documentation to such a high degree that our subjectivity becomes inconsequential. As long as we examiners can show that these safeguards are in place, we can clearly articulate the scientific validity of our conclusions" (5).

Evan Thompson published a short article that dealt with another significant foundational issue of the firearm and toolmark identification discipline, the need to understand the manufacturing process of the tools in order to appropriately interpret the significance of the marks they leave (6). He stressed that understanding the tooling process was the key to avoiding confusion of individual with family characteristics, which he defined as "gross stria that carries from barrel to barrel, or along the flat of the chip as it is removed from the metal parent material" (6). This distinction of individual markings and subclass markings was a major topic of discussion among many of the earlier articles summarized in the first review.

One of the many articles published by Jerry Miller during this period is one in which he presents an overview of the discipline of the forensic examination of toolmarks (7). Developed from training ATF provides to the National Firearms Examiner Academy, he provides a general discussion and assessment of metals and metallurgy, various types of manufacturing processes for tools, basic tool types, their manufacture, and the typical marks that they leave. As will be seen in the later articles, he also provides an important discussion on the concept of subclass characteristics. In this particular article he provides an example of the potential pervasiveness of such characteristics. He compounds the characteristics over multiple levels when he suggests that a mold imperfection that may be reproduced on a tool may actually be the result of a master that reproduced the imperfection on multiple molds.

CMS Theory

In 1998 Tulleners, Giusto, and Hamiel performed a study in which they removed a total of six 1-in. sections of barrel (8). Prior to sectioning and after each sectioning, test fires were produced. Known matching bullets were compared, as were bullets from one barrel section to the adjacent barrel section. In utilizing a single barrel with sectioning, the authors indicated that they were controlling for the potential variable of different tools used for drilling and reaming, even if the same tool was used for rifling. They felt that this procedure of sectioning and test firing was analogous to consecutive barrel work.

They reported that the barrel was button rifled, and subsequent casting demonstrated the presence of circumferential reaming marks. The barrel was consistently test fired after each section of barrel was removed, with a conditioning shot and then test-fired shots for comparative purposes. Once all test firing was completed, a total of 464 land impressions and 464 groove impressions were available for comparison, which included known matching impressions and known non-matching impressions.

Several issues with regard to this study are worth highlighting. The first concerns reproducibility. The actual line counting of the striations was carried out by a group of student examiners. It was discovered that the consistency between different examiners was relatively good, especially with regard to the estimation of consecutiveness and even further as the line count within a particular set exceeded three CMS. The second is that a measure of percentage of matching striations alone is a poor indicator of whether two bullets match or not. This was expected, as it was recognized by Biasotti in his early work and has been consistently recognized and supported by scientists within the discipline. The third is that the threshold for consecutiveness between known non-matches and known matches was three consecutive matching striations. The authors indicated that based on their study one could expect to encounter three CMS in known non-matches but not four. They indicated that the incidence of three and four CMS was quite high for known matches, especially in the land impressions.

The advent of the Integrated Ballistics Identification System (IBIS) enabled Jerry Miller to perform a series of related studies on CMS, the first of which was published in 1998 (9). The focus of his paper and others to follow was to test the hypothesis of CMS as presented by Biasotti and Murdock in 1997 and determine if the hypothesis could be shown to be false. Miller offered that, "If the hypothesis cannot be proved [by the testing of every tool], then it should be tested to determine if it can be disproved or falsified. If the results do not disprove the theory, then the theory is shown to be valid, or accurate" (9). Considering the focus of the paper, it is not surprising that he opened with an extensive discussion of the applicability of the scientific method to toolmark comparisons.

Miller also presented a summary of some of the more cogent remarks regarding CMS along with one from Biasotti and Murdock that appeared to be the impetus behind his work, "'there was insufficient published research to validate the quantitative objective criteria necessary to conclude that a toolmark made by a working surface is unique'" (9). Miller stresses the importance of quantity and quality, re-iterating the stress laid upon the same issues by Biasotti and Murdock. When addressing critique, Miller offers that the concept of counting striations is subjective and based on experience, but it does tend to be consistent within a single examiner.

The methodology employed by Miller incorporated the use of the IBIS system. Fifty pairs of .38 caliber full metal-jacketed bullets test fired from Smith & Wesson revolvers were input into the IBIS system. The system then compared the 50 pairs of bullets against the database of 1600 other test bullets. In this comparison, the system actually performed a total of 2,000,000 land-impression-to-land-impression comparisons. IBIS was used to screen all the possibilities, and then "the top five scoring land impressions for each of the bullets was examined for the data" (9). In this manner it can be suggested that whatever the data are for these known best non-matches, this would be the best possible data for the intent at hand, i.e., falsifying the hypothesis. Miller collected data for two-dimensional and three-dimensional comparisons, utilizing the IBIS viewing screen for the two-dimensional data.

In his study, Miller offered several different criteria for identification. In doing so, he examined each of the proposed criteria in light of the collected data to determine the number of errors that his data would represent among those selected criteria. The first criterion he examined was simply matching line count. If the number of matching lines reached a particular threshold, an identification could be made. The threshold was the average of matching lines in a known match. Using his collected data, there would be six false inclusions for two-dimensional work and none for three-dimensional. Further, there would be 28 false exclusions for three-dimensional work and an unknown number for two-dimensional work. The second criteria of percent matching lines presented similar problems with false inclusions for the two-dimensional data and false exclusions for two and three-dimensional data.

Having discredited the first two criteria, as has been done consistently in the past, Miller then examined the concept of CMS using the aforementioned conservative criteria offered by Biasotti and Murdock. In his data of known non-matches, Miller did not find any groups over six CMS for two-dimensional known non-matches and did not find any groups over four CMS for three-dimensional known non-matches. In summary, there would be no false inclusions using the conservative CMS criteria. He did not offer a summary statement as to the number of false exclusions that might be expected. He indicated that the concept of multiple runs of CMS was particularly helpful in that known matches had multiple sets of CMS unlike known non-matches. Indeed, this was one distinctive feature that was considered highly significant.

Miller then offers some discussion with regard to the usefulness of probability studies, suggesting that their value lies with demonstrating that, "there is an extremely low probability of having two different tools produce the same marks" (9). He touches upon the necessity of training and the ability to articulate what the difference is between a known match and a known non-match. Indeed, because of the many different variables involved, training being primary among them, he indicates, "It is difficult, if not impossible, to establish a specific criteria for a toolmark identification" (9). One of the prime benefits of Miller's article is the wealth of raw data available in the appendices.

In 2000, Miller authored another paper in which he examined the CMS criteria in several calibers including .25 ACP, .380 ACP, and 9-mm Luger (10). Once again, Miller used IBIS to perform initial screening of bullets to provide himself with the best possible known non-match data, e.g., data that had the best opportunity to falsify the hypothesis. A total of 60 land impression to land comparisons were conducted in two and three dimensions for each of the three calibers after screening the entire databases with the computer algorithm. The entire databases for the .25 ACP, .380 ACP, and 9-mm Luger consisted of 34,524 specimens, 92,304 specimens, and 102,276 specimens, respectively.

In summary, when Miller applied the previously discussed conservative criteria for identification, there were occasions in which false exclusions would be made in both two- and three-dimensional comparisons, but there would be no false inclusions. It needs to be stressed that his "false" exclusions were based on counts in single impressions and did not take into account the entire bullet surface, as would happen in a case. More specifically, for .25 ACP known non-match bullets there were no CMS runs exceeding four in either the two-dimensional work and three-dimensional work. The same could be reported for two-dimensional comparisons in .380 AUTO caliber, but for three-dimensional work there were no sets of CMS exceeding three. This threshold of three also held true for both two- and three-dimensional comparisons in 9-mm Luger caliber.

In addition, Miller also provided a similar discussion for the .38 SPL data from his study published in 1998. In that summary he informs the readers that for two-dimensional comparisons there were no CMS exceeding six and for three-dimensional known non-match comparisons there were no CMS exceeding four. He offers a reason as to the higher numbers, suggesting the larger land impression width for the .38 SPL bullets as opposed to the others. In summary, Miller found that caliber was not a factor in the application of CMS.

Later that same year, Miller published a study in which he examined the CMS theory as applied to two consecutively manufactured .44 caliber barrels (11). In an attempt to obtain two barrels that were most likely to share similar characteristics, he procured two barrels that were consecutively manufactured with a gang-broach but had no exterior finishing. For purposes of the study, Miller pushed bullets through the barrels to obtain the test bullets for comparison.

Utilizing Mikrosil casts of the barrels, Miller noted some subclass characteristics in three of the land impressions, but that this correspondence was offset from the shoulders. Random correspondence was noted in the groove impressions and the three remaining land impressions, but not enough that would cause an improper identification to be made. When the bullets were examined, these subclass characteristics were not significant with respect to ascertaining the correct barrel-to-bullet identification.

In summary, using the conservative criteria for identification, there were no false inclusions, but there were some false exclusions. This number of false exclusions would decrease if based on all land impressions as opposed to a single one. In examining his raw data, there were no CMS exceeding a run of two in the known non-matches, while the known matches had nine incidences of three CMS, eight of four, one of five, and five of six.

Miller subjected test fired bullets from ten consecutively manufactured 9-mm Luger barrels to the conservative CMS criteria (12). The bullets utilized for this study were those that were produced for Brundage's study (13). Miller compared the bullets from sequential barrels, i.e., bullet from Barrel 1 versus bullet from Barrel 2, bullet from Barrel 2 versus bullet from Barrel 3, and so forth for the ten barrels.

Overall, he indicated that misidentifications would not occur because the comparisons of bullets fired from different barrels, "shows only random correspondence of striae with no possibility of an erroneous identification" (12). He did observe some correspondence on the groove impressions, which was attributed to subclass correspondence, as well as some agreement of slippage marks, but none sufficient enough to warrant an identification. The best correspondence observed on known non-matching land impressions was nothing more than random, indeed more random "than what could be attributed to subclass characteristics" (12).

He then applied the conservative criteria for identification to the bullets as he had done in previous studies. He found that for two-dimensional known non-matches there were no CMS runs greater than four for the land impressions and none greater than three for the groove impressions, though they were poorly marked. For three-dimensional known non-matches there were no CMS runs greater than two for the land impressions (which he indicated was an obvious exclusion) and none greater than three for the groove impressions. In summary, using the conservative criteria there would be no false identifications but there would be some possible missed identifications if not all the land impressions were available for comparison.

In 2001, Bruce Moran published an article in which he applied the CMS criteria to magazine marks (14). In his opening remarks with regard to the issue of CMS, Moran indicates that he has embraced the theory, "in an effort to distance myself from the influence of subjectivity" (14). He indicated that he did not feel that there was any conflict between pattern matching and CMS, but that he was simply taking the next logical step, finding areas of potential value through pattern matching and then applying CMS to those areas.

In his work, Moran focused on the importance of an examiner evaluating the various marks with regard to how they are made and the potential for subclass influence. Once established as marks that can actually aid in the identification of a particular tool, Moran finds the CMS criteria very helpful, especially when dealing with a small quantity of markings such as one might find with magazine markings on the sides of cases.

General Studies Involving Barrels, Breech Faces, and Other Tools

Valdez conducted a study involving polygonally rifled, HK USP, .40 S&W caliber barrels (15). His primary purpose was to determine how well bullets fired from a particular barrel could be identified to that barrel, as polygonal barrels proved to be a particular challenge in this regard. He collected 30 sets of test-fired bullets from different barrels and found that in 28 of 30 cases he could identify the bullet to the barrel through which it was fired.

During his study he did perform comparisons of known nonmatching bullets to determine how much similarity there may be on bullets fired from different barrels. His work in this regard was neither well documented nor discussed at great length. He indicated that such testing was performed and that, "The number and density of the striated patterns were unique enough to the respective barrels that a misidentification did not seem likely" (15).

In 1998 Brundage published further comments regarding his study of the ten consecutively rifled Ruger P-85 barrels (16). The primary issue for this study was to demonstrate that examiners could correctly associate bullets with the barrels through which they were fired, even if the barrels were consecutively rifled by a cut, step-broached method. In this purpose, he was expressing sentiments similar to this discussed earlier by Collins in which Collins was less focused on the two legitimate means (i.e., pattern matching versus CMS) but rather that the end result was correct. Brundage indicated that the, "data shows that not only are consecutively rifled gun barrels different from each other, but are unique and can be differentiated" (16).

In 1999 Tulleners and Hamiel reported on subclass characteristics associated with .38 SPL caliber bullets (17). These barrels were rifled with a step-broach method. They observed that subclass characteristics did exist on some, but not all sequential barrels and on some, but not all groove impressions. Further, the characteristics were not transferred to copper-jacketed bullets or the land impressions of lead bullets, just to the groove impressions of lead bullets. Tulleners reported that, as late as 1991, Biasotti contended that subclass characteristics were a "rare event" and their presence "easily determined" (17).

Matty examined the stamped breech face inserts utilized by Lorcin in the production of their 9-mm Luger and .380 AUTO caliber pistols (18). He examined several of these stamped inserts and was able to identify some of them to one another, having identified two different sets of inserts. Assuming that these marks could likely transfer to the fired cartridge cases, Matty offers this warning, "Any identification of cartridge cases which utilize these manufacturing stria[e] will be an identification of the tool that stamped the part rather than an identification to the firearm in question. The breechface stria[e] pattern should be viewed as a subclass characteristic on the L9MM" (18).

In apparent response to criticisms leveled by others not regularly involved in the practice of the discipline, Rosati published a short paper regarding the individuality of bunter tools (19). In this brief discussion Rosati commented on the typical production run for bunter tools by Remington and the reason for the rather limited run. He also indicated that the bunter tools utilized by Remington were provided by an outside source and that the four provided to him for study were reportedly manufactured utilizing Electric Discharge Machining (EDM). He examined the bunters using casts and cited each working surface as individual and that he expected each to produce individual markings such that a head stamp could be identified to the bunter tool that produced it.

In 2000, Lopez and Grew published a study in which they examined consecutively machined Ruger bolt faces (20). They examined six consecutively manufactured Ruger rifle bolts, manufactured by the end mill machining process. The process was discussed and it was pointed out that, in face milling, concentric marks were produced that were subclass in nature because the cutters are specially designed to resist abrasion.

They reported on the comparison between the first manufactured bolt face and the rest during which they found an "alarmingly high level of correspondence" in all comparisons, except for Bolt Face 5, but that even that showed "a noteworthy level of similarity" to the initial bolt face (20). This article was well documented with photographs illustrating the excellent correspondence observed. They did indicate that, even with the subclass characteristics, there were features that were individual to each individual bolt face. The first was the abrasion markings near the firing pin hole. These appeared due to an apparent tearing of the metal rather than a clean cut, an apparent result of the different speeds of rotation between the inner and outer portions of the cutter. The second was the markings appearing across the concentric circles. But, they were neither consistent nor did they have sufficient detail to be deemed as significant for purposes of identification.

From 1997 through 1998, there appeared a series of articles having to do with the manufacture of nails. The first of these dealt with square cap nails and was published by Miller (21). Given the circumstances of the case, the question as to the value of the identification became important. This question as to the value of the identification is analogous to the head stamp and bunter question in that, "Although marks produced by the tools are reproducible and individual to the tool producing them, the number of nails produced and the method of distribution may limit the value of an identification" (21).

Miller proceeds to discuss the manufacturing process for the nails, the history of it, and the procedures for re-tooling by the operator. In the case of these Simplex nails, it is estimated that an average of 200,000 may be produced prior to the need for re-tooling. In the specific case in which these nails were an issue, Miller stated that a "definitive" identification was achieved, but that he was concerned about the value or "weight" of the identification evidence (21).

In this context, Miller discussed the concept of subclass characteristics. It is important here to quote Miller at length as it is apparent from later articles that his use of subclass characteristics in this regard has drawn some concerns. In this case he states, "In routine cases, it is important only to realize that, due to the manufacturing process, the value of an identification between the nails found at the scene, and the nails found on a suspect, are circumstantial. The marks, although identifiable, become subclass characteristics identifiable to the specific parts of the machine at that time. As the tools wear, or a part is changed, a new set of subclass characteristics are [sic] produced" (21).

Miller addressed similar issues in another article dealing with nails (22). In this second article on nails, Miller addressed cut nail manufacturing and the significance of toolmark identifications with regard to such nails. He described the machining operation and the action of cutters and headers, which, based upon his description, should leave characteristic markings for a period of time until either wear or re-tooling changes the characteristics of the surface(s). As with the square cap nails, the character of the markings can change over time such that a given nail may not only be able to be identified to the machine as being manufactured during a particular production run, but also to a particular point or interval within that production run. With respect to this issue, Miller states, "Therefore, instead of being individual characteristics, they are actually subclass characteristics" (22).

For his evaluation and assessment, Miller examined sets of nails obtained at 30-min intervals over a 9-h period of time, during which approximately 32,400 nails were produced. He indicated that nails from the first set to the last could be matched, though some differences were observed. He indicated that each machine has its own header and cutter, "producing toolmarks identifiable to themselves" (22). Further, each header and cutter are customized for the individual machine and "finished by grinding, eliminating the possibility of carry-over marks from one machine to another" (22).

In this article, Miller offers the assistance that probability can provide. He suggests that a probability based on the number of nails produced that will have that a combination of features and the chance that it will be at a scene, in a box, etc., would be valuable in determining the value of such an identification. He concludes by saying that, "While identifying these types of toolmarks, it is necessary to know the possible numbers of nails which could exhibit the same subclass characteristics before the value of an identification can be determined" (22).

In his article published in 1998, Collins offers some clarification to the issue of subclass characteristics (22). He discusses extensively the manufacture of wire nails including the potential marks that can be left on the nail and their source within the scope of the manufacturing process. He expresses the necessity of evaluating and understanding the effects of wear and tear on various tool surfaces that come into contact with the nail. With respect to the examination and identification of such nails, Collins says, "Examination of nails recovered from the scene of a blast will likely reveal the presence of characteristics that are not unique to any one nail, but are unique to the machine component that created the toolmarks" (23).

One of the more important contributions Collins made with this article is similar to the contribution he made with the first article mentioned in this review—the contribution of articulation. Collins first defined the class characteristics of a nail as: (1) the type or style of nail; (2) finishing; and (3) the size including the length and diameter. In this definition, all nails sharing such basic characteristics are defined as a class. Marks that may further distinguish them are then termed subclass. Therefore, nails sharing the same toolmarks that allow them to be identified to a particular machine at a particular time are subclass characteristics.

Collins makes some very poignant statements with regard to the issue of subclass characteristics and will be necessarily quoted at length. "The term 'sub-class' is one that is assigned to describing manufacturing imperfections that may or may not be unique to the tool that made them, but will be reproduced on a population of objects produced by that particular manufacturing process. Whether or not that object is ever used as a tool to create other marks has no bearing on whether or not the marks on its surface are defined as subclass" (23). He provides an interesting analogy of marks on a wall being created by the individual nails as they flew through the air after the blast; therefore, the "implication from this is that the term 'sub-class' could be completely eliminated with no significant impact to the discipline of toolmark examination" (23). He continues by saying, "If an examiner confirms the individuality of marks that are examined, knows the source of these marks, and can articulate the uniqueness of that source, then the term 'sub-class' is redundant. But, for the purpose of assigning a name to these marks, which have the potential for being misinterpreted, the term has value" (23).

Miller published a third article dealing with nails (24). In this article Miller focused on the reproducibility of markings on nails and did not venture into further elaboration of subclass characteristics. In this study, Miller simply collected six nails from every 1000 produced from an entire production run. The run produced approximately 1000 nails every 15 min with a total of 114,000 produced during the entirety of the run. Based on his study, he observed that the toolmarks produced by the nail header and those on the nail flat under the head are reproducible and identifiable throughout the entirety of the run. Other marks change through the course of the run, although differently. Those on the right and left edges change multiple times over the course of the run, with the change first observed at approximately 15,000 nails and then every 60,000 nails after that. Those on the nail flat are reproducible through the first 16,000 nails, change, and then are reproducible throughout the rest of the run. Because of these changes, Miller offered that it is possible to classify nails to a smaller set of the entire production run.

Statistics and Their Role in the Firearm and Toolmark Identification Discipline

As indicated in the introduction, the scope of this review extends back to the time of the original review article written in 1996, but published in 1997. The next article to be reviewed falls outside that parameter, but will be discussed regardless because an important later article will specifically address claims made in this earlier article. In 1995, Deschênes et al. published a short article dealing briefly with statistics and their value in toolmark comparisons (25). In their very brief article they offer two reasons why statistics are not sufficient for the purposes of probability of an identification. The first is that "statistics never permit to draw conclusions concerning a particular situation" (25). Secondly, they contend that when resolving the issue of an identification the judgment of the toolmarks examiners is more valuable than the statistician, commenting that possibly the offering of the statistician is regarded as better because "Numbers always look so scientific!" (25). They conclude that statistics do have a usefulness but, "we must realize that statistics will not give an answer to the most famous toolmarks examiner's question: What is the minimum number of corresponding lines needed to get a positive match?" (25).

Taroni, Champod, and Margot published an article later that was written directly in response and as a criticism to the work offered by Deschênes et al., suggesting that some of the propositions and objections leveled by others are not accurate (26). They argue that statistics do have a valuable place in the identification disciplines because the forensic scientist is unable to answer the totality of the judicial question by him or herself. They then cite the judicial question as, "The court wants to know the odds that this screwdriver has produced this toolmark given the circumstances of the case and the observations made by the forensic scientist (the match reported)" (26). They assert further that while the scientist is in position to "express the probability of observing the match if the screwdriver had not produced the toolmark," (26) he or she never has a complete set of circumstances and therefore, "the toolmark examiner is never in a position to identify a tool. But when considering the whole population of the world, the expert estimates that the probability of another match is very close to zero, then it is common sense to declare an identification" (26).

In their article, they do recognize the importance of the experienced toolmark examiner but suggest that the examiner and the statistician each have an important role unlike the contention of Deschênes et al. They assert that statistics are particularly valuable, "firstly to assess the validity of the scientific principles of the area of expertise and secondly to assist the examiner in coping with uncertainty" (26). The context of this article dealt with uncertainty in terms of the inconclusive results that tend to be common among examiners. In essence they felt that statistics potentially offer more meaning to the inconclusive result, thus aiding in the judicial question.

They delve into the issue of consecutive or corresponding lines criteria suggesting that the issue of quantitative criteria should not be the primary contemporary issue. They assert, "A conclusion is reached after consideration of multiple factors (quantitative and qualitative) as recently reviewed by an AFTE committee. The quality in toolmark expertise will never be reached through simply counting striations, but rather through education, supervised training and controlled experience" (26).

In 2000, Bunch published a paper criticizing the consecutive matching striation criteria, using statistics in support of his argument (27). It is important to understand this article in the context as offered by Bunch, a critique performed from the "perspective of Bayesian logic" (27). He offers a comprehensive discussion of Bayesian analysis and suggests it is useful because it allows for an estimate of variables that may affect the probability of a certain event occurring. In essence he suggests that the judicial issue at hand is the establishment of some posterior odds for an event occurring based upon an estimation of a set of prior odds combined with a likelihood ratio (posterior odds $=$ prior odds $\times$ likelihood ratio). In this respect the firearms and toolmark examiner is charged with the determination of the likelihood ratio.

Bunch suggests that the CMS model is not a model for identification because, "there is no rational or scientific ground for making claims of absolute certainty in any of the traditional identification sciences," (27) of which firearms and toolmarks is one. Turning his attention back to the formation of a likelihood ratio, he provides an example of how the likelihood ratio can be calculated for a CMS of 6. His results are only hypothetical because he offers no real life experimental data to support his assertions. For example, using his hypothetical data, if one were to encounter 6 CMS then it can be said that it is "110 times more likely" (27) if the bullet was fired by the same firearm rather than a different firearm. Further, where no data exists for an event, such as 8 or more CMS for a known non-match, then the examiner needs to extrapolate and produce one. Theoretically he suggests that the likelihood ratio should be higher for a match versus a known non-match. He then offers that verbal add-ons to the likelihood ratio can be offered that can define the ratio as constituting "weak, moderately strong, strong, or very strong (pick one) evidence" (27).

He offers other criticisms of the CMS approach as well. He suggests that there is inherent subjectivity in CMS because there is always the question of what constitutes a matching striation. He offers practical limitations including a need for a large databank of values and a need to test many variables even if it is for the purpose of discounting them as insignificant. Further, he suggests that the data gathered to date is not necessarily valid for all considerations. For example, the data gathered for the same gun situation is not necessarily valid for used barrels because examiners would not necessarily expect large CMS runs in used barrels. He indicates that the usefulness of the current research is only "moderately so" (27). The reason is because many variables have not been studied, including barrel manufacturing methods, barrel lengths, barrel hardness, bullet hardness, and bullet surface materials. He contends that researchers do not know enough to determine which, if any, are irrelevant.

The remaining faults he proposes are interpretational. The first and most prominent is that, in the framework of Bayesian logic, CMS can never reach zero probability no matter how long the run. As a result, such probabilities have to be extrapolated from existing data. He also suggests that ignoring the same gun probability overestimates the likelihood ratio and that using the statistics from different guns only causes individuals to think illogically about the evidence. In support of this latter objection, he states, "This occurs when one transposes the conditional, i.e., when the probability of the evidence assuming guilt is mistakenly thought to be, and presented as, the probability of guilt assuming the evidence (again treating guilt as tantamount to the evidence bullet being fired from the suspect barrel). The probability of a dorsal fin, given a shark, is not equal to the probability of a shark given a dorsal fin" (27).

As indicated earlier, Bunch's work was written from the perspective of Bayesian logic. But, as can be seen from the following excerpt, it was also written from the perspective of a traditionalist, i.e., one who is in support of the more traditional approach of pattern matching. He is quoted at length for the purpose of ensuring that his full intent is clear. "It is arguably unfair to draw harsh conclusions about a CMS regime without subjecting its dominant rival—the traditional, subjective regime—to an equally critical examination. Nevertheless, and for now setting aside the practical difficulties, it appears that the inability of this probability model to deal rigorously with barrel changes is a weakness worthy of note, the seriousness of which is debatable (it's quite possible that further research and hard thinking could resolve the issue satisfactorily). Indeed, some questions do arise regarding the scientific status of present day subjective examinations; but with measures such as professional certification and rigorous validation/proficiency testing, the traditional, subjective examination regime can strengthen its scientific grounding. Whether CMS or objective-automated regimes eventually supplant it remains to be seen, and, of course, research and logical analysis should continue, even accelerate. At least for the moment, however, the benefit of the doubt should go to the traditional methods" (27).

Discussion

The reviewed articles represent the significant works regarding firearms and toolmark identification criteria that have been published since this author's original review published in 1997. These articles represent a significant change in purpose from the earlier ones in that there is an increased focus on the ability of an examiner to articulate the basis and application of the firearm and toolmark identification discipline and on the suitability of CMS for the discipline. It is these two issues that deserve further discussion.

As a set, the value of general articles that discuss terms, concepts, and theory cannot be overestimated. The primary difficulty that has arisen in this discipline is the lack of ability of examiners to effectively communicate with one another and the end-users of the service. Communication is further hampered by misunderstanding terms and usage, and the use of excessively "familiar" language—language that has meaning to one person and one person only—the user of that language. The pattern of language and communication in this discipline has been akin to caliber nomenclature—sometimes it just appears. In order to make an effective presentation as to the scientific validity of this discipline, it is critical that examiners come to a mutual agreement and understanding of the language. Articles such as those published by Collins, Thompson, and Miller are attempts to bridge the gap that does exist.

However, there are still some significant difficulties in this regard. One of the more critical ones deals with the use of the term *subclass characteristics*. In Miller's initial article on square-capped nails he indicated that, although he was able to identify a nail to a particular machine, the markings used to render this identification were subclass characteristics because the same characteristics were shared by many, and in this case potentially thousands of, nails. Indeed, when examined against the background of the definition offered by AFTE, the use of the term appears appropriate. In 1992, the following was published as a definition of the term subclass, "Discernible surface features of an object which are more restrictive than CLASS CHARACTERISTICS in that they are: (1) Produced incidental to manufacture; (2) Are significant in that they relate to a smaller group source (a subset of the class to which they belong); (3) Can arise from a source which changes over time. Examples would include: bunter marks, extrusion marks on pipe, etc. Caution should be exercised in distinguishing subclass characteristics from INDIVIDUAL CHARACTERISTICS" (28). Klees made similar supporting statements regarding subclass characteristics in an article concerned with the identification of manufacturing toolmarks on gunpowder particles (29).

Yet, when examined in the context in which the term "subclass characteristics" has been used in the literature, it has a slight, but rather significant distinction. The primary concern has been whether or not subclass characteristics would cause an individual to link a particular bullet or cartridge case to a firearm different from that in which it was fired. The carryover of subclass characteristics, whether or not they were actually referred to as such, from consecutively rifled barrels and other firearm components, was one of the primary concerns of Churchman (30), Skolrood (31), Murdock (32), Matty (33), and Johnson and Matty (34), among others, including ones cited earlier in this review. Collins attempted to address this concern in his article that was reviewed earlier.

Despite the contention made by Collins in his attempt at clarification of the term, including the suggestion that the term subclass could be done away with, the common usage to date of the term *subclass characteristics* has applied to the tool itself and not the item upon which the marks were made. In the classic sense of firearms and toolmark identification, when there is a set of markings that permits identification to a particular tool, or machine in the case of the square-capped nails examined by Miller, then those markings have been considered individual markings. The markings link that nail to that particular machine to the exclusion of all others no matter how many other nails may share those same characteristics; every nail can be identified to that machine through these markings. Yet, in the example and discussion provided by Collins, these same markings indicate a subset of a class of nails that have a particular size and design; therefore, they should be considered subclass characteristics.

Another example would be helpful in this instance. This author considered the nail question to be analogous to bunter marks. Headstamps can be identified to the particular bunter that made them, but it must be remembered that a single bunter can produce thousands of headstamps just as a single machine can produce thousands of nails. The question then is not only the identity, but also the significance of that identification. Yet, in the AFTE definition, bunter marks have been designated as subclass characteristics. When the bunter analogy is brought forth, Murdock has suggested that even though particular markings may permit identification to a single bunter, they qualify under the definition of subclass because the production of each bunter is a sub-set of their group, e.g., all other bunters with the same headstamp (personal communication with John Murdock, April 23, 2002). While his explanation is valid and fits the definition as worded in the *AFTE Glossary*, it now lends itself to confusion.

This is especially the case when a bullet example is taken to an extreme. Assume a gun barrel manufactured and rifled. It has been determined that the markings within the bore of the barrel are not reproduced on any subsequent barrels, therefore distinguishing that barrel from all other barrels. This barrel is now responsible for firing 500 bullets, the first 25 displaying varying markings while the barrel is "broken in" and the remaining 475 showing some differences among themselves, but also displaying some markings that are persistent throughout the entire run of 475 bullets. In common usage, these markings have been considered individual because they permit identification to one barrel to the exclusion of all others. Yet, following the example provided by Miller and Collins they could be considered subclass characteristics as well because they are present on a subset of all bullets fired from that barrel.

The question is, can such marks be both? Based on common usage and the definitions cited the answer is yes. The real question is, then, should such marks be referred to as both? The answer is no, unless those within this discipline wish to continue in frustration when trying to communicate the subtleties and nuances of this work. Collins suggested that this term could be done away with. This author would respectfully disagree. The term subclass characteristics is an extremely useful term; it is simply important to define its use among those in the discipline in a manner that is consistent.

The question really is not whether the term should exist, but simply how it should be used and in what context it should be used. As it stands, the definition as offered in the *AFTE Glossary* is adequate. It is defined such that it makes logical sense in the context of what is meant by class characteristics (characteristics shared by a group of objects) and individual characteristics (characteristics unique to a given object) across the entire spectrum of the identification sciences. The context should refer to the characteristics of the tool itself and not to the object. Therefore, if a tool has characteristics that it shares with multiple tools, it would be appropriate to refer to these as subclass characteristics. However, if a tool has characteristics that uniquely set it apart from all others tools, and those marks are reproduced on the object then these characteristics are individual characteristics. In essence they are not individual characteristics of the object itself, because multiple objects could share the same characteristics if marked by the same tool. Rather, they are individual markings of the tool, which are reproduced on an object (of which there may be multiple objects).

Interestingly, two of the papers reviewed in this article referred to what the authors felt the primary issues should be in the firearms and toolmarks identification discipline and focused on identification criteria. This author feels the primary issue has been and remains an ability to articulate what one means. This was never more evident than in the recent *Ramirez* decision in Florida. It is clear that the examiner had a criterion for identification; otherwise the identification would not have been made. What was just as clear was that it was not being articulated in a manner that could be appreciated by the end-user. Further, despite the coming discussion of CMS criteria, making this discipline more objective is not going to solve the problem. Indeed, it may only exasperate the problem because without a common understanding, without an ability to articulate one's training and experience, without an ability to defend one's own subjectivity, which always exists at some level, without an ability to articulate what constitutes a matching striation, nothing is solved and a whole new set of variables is now introduced.

The use of the term *subclass characteristics* is one issue of many. Other prominent issues have included the use of the words subjective and objective along with the terms art and science. The contention surrounding these terms has become more prominent, especially as the courts and legal system have become increasingly critical of the discipline for the lack of a "more objective" criterion for identification. It is apparent that the courts have linked this criterion to a number and that, as a result, tensions within the discipline have escalated.

Throughout the history of this discipline, it is apparent that the words *art* and *subjective* have been nearly synonymous as have the words *science* and *objective*. Further, these synonymous word pairs have also implied the exclusion of the other, as if the two word pairs cannot co-exist. Well, not only are the word pairs incorrectly associated as synonyms, they are also not mutually exclusive. Indeed, a closer examination of each of these words will show that: (1) there is nothing about any of them such that their use should cause one to look askance at the discipline; and (2) there is nothing about any of them such that they should be considered mutually exclusive of one another.

Webster's dictionary defines *science* as, "accumulated and accepted knowledge that has been systemized and formulated with reference to the discovery of general truths or the operation of general laws . . . esp. knowledge obtained and tested through the use of the scientific method" (35). Keeping the typical word pair together, the same dictionary defines *objective* as, "publicly or intersubjectively observable or verifiable esp. by scientific methods . . . of such nature that rational minds agree in holding it real or true or valid . . . perceptible to the senses or derived from sense perception" (35). *Art* is defined as "the power of performing certain acts esp. as acquired by experience, study, or observation . . . a branch of learning . . . an occupation or business requiring knowledge or skill" (35). Finally, *subjective* is defined as, "peculiar to a particular individual modified by individual bias and limitations" (35). It must be conceded that many other definitions exist for each of these words, but those cited appear to be most pertinent to the issue at hand.

As can be readily concluded, the discipline of firearms and toolmark identification is certainly a science. One need not look far to discover "accumulated and accepted knowledge" that has been "systemized" and apparently developed through testing according to the precepts of the "scientific method" (1). Nowhere in this definition does it preclude subjectivity or solitarily embrace objectivity. Indeed, throughout its history, science has been replete with both. It cannot exist without both.

Every observation made is subjected to the assessment of that observation by an individual; an individual who filters each assessment through one's experience, knowledge, fears, and possibly physical limitations. Subjectivity is present in everything that an individual does and, even if it is possible to link a particular observation to a number, the story is never complete with the use of that number alone. Indeed, an observation does not need to have a number assigned to it to make it an objective observation. This is particularly true for pattern matching. When one is observing a pattern, one makes an assessment as to relative position, placement, and size among other characteristics. While individuals may assign numbers to better communicate what they are observing in the pattern (i.e., artificial grid numbers for placement), by citing the earlier definition of the word "objective," they are still observing something that is "perceptible to the senses or derived from sense perception" and, even without a number, their assessment is an objective one. In a cloudless sky, "the sky is blue" is an objective statement. There is no escaping the fact that there are many elements of the discipline that are subjective in nature. What is important is that these be articulated in such a manner that the end-user can understand and make appropriate assessments as to the significance and meaning of what is communicated.

Not only is the firearms and toolmark identification discipline a science, but it is also an art in every sense of the word. There is a particular skill set that is needed, certain knowledge that is required, and programs of study necessary to develop one's skills and powers of observation. Further, the need for continually increasing and building one's skill set is endless. That is one reason in particular why this discipline is so well suited to a mentoring relationship, but not simply because it is a craft like blacksmithing as some would contend, because it isn't. There is such a broad spectrum of potential experiential knowledge and information that it is critical to have experienced examiners impart their own experiential and institutional knowledge to younger examiners. There is a solid scientific foundation to the discipline, but the very practice of it is an art.

Is firearms and toolmark identification an art or a science? The answer is not one or the other, but both. Is the criterion for identification based on an objective or subjective criteria? Again, the answer is not one or the other, but both, and it matters little whether the individual is using what has been referred to as the traditional pattern-matching method or CMS. If it is clear that each term has an important and significant place in the discipline, then why have the terms *subjective* and *art* become such an anathema in the courtroom? Indeed, there is not one reason in particular, but many from which to choose. The increased public scrutiny is one reason. The fallout from other disciplines in which court decisions have had to be overturned because too much undue emphasis was given analytical results is another. Increased ability for communication among the end-users of the service is a third and others do exist. The point is that the words have become such an anathema because of the continued poor ability of examiners in this discipline to properly and adequately educate. This is not a suggestion that all suffer from this malady, but it is apparent that the problem does exist. This problem is then exacerbated when such examiners retreat to an indefensible position, making unreasonable and unrealistic claims regarding the discipline, to the extent that the court will not only discard the entirety of the testimony, but also the work and potentially the scientific utility of the discipline itself.

The second concern to be discussed is CMS. Developed primarily as an answer to the aforementioned disputes of art-science and objectivity-subjectivity, CMS has many supporters as well as opponents. However, the criterion of CMS addresses only the fruit of the tree. The problem that is at the root of the entire set of difficulties is the lack of appropriate articulation and communication of what one means. This problem is not answered solely by CMS. Further, the use of CMS while these problems still exist can do nothing but add to the problem because it is simply being covered by a layer of objectivity and science that the courts may assume have solved the problem. Put another way, it only hides the problem until a later time in which it will become revealed again. That being said, what follows is a discussion not necessarily of the applicability of CMS but its development in the literature along with comments from its supporters and critics.

Interestingly, the articles published by Tulleners and Miller appeared in the same 1998 issue of the *AFTE Journal* and could almost be considered a landmark for the discipline, as they were the first publications offering significant data regarding the CMS theory since Biasotti's original article appearing in the 1950s. Many studies had been performed in the interim, but none took to task the theory as originally offered by Biasotti and then later refined by Biasotti and Murdock. These two articles offered some hope to the resolution of the articulation of objective criteria for identification and were soon followed by more, especially by Miller. Summarizing very briefly, Miller was able to demonstrate that, while the application of the criterion offered by Biasotti and Murdock may result in some false exclusions, there would have been no false inclusions given the data Miller generated.

This article is not designed to either support or refute CMS as a legitimate alternative to the traditional pattern matching method employed for so many years in this field. The primary reason is that it has not been promoted as an alternative, but as a numerical threshold. This author believes that both are valid, both have received sufficient testing and support, and that every examiner, whether he or she admits to it or not, applies both at some point of the comparison and identification whether or not it is actually documented. For instance, pattern matching involves a process in which the mind is assessing quantifiable features of the pattern whether or not they are ever expressed as numbers in an individual's notes. On the other side of the coin, proponents of CMS have indicated that it is not in conflict with pattern matching, but is simply the next logical step (14).

What is of primary concern is the manner in which this debate has been handled and with some of the statements made during this debate. One individual has claimed, "I have done so [embraced CMS] in an effort to distance myself from the influence of subjectivity" (14). Given the previous discussion on this subject of objectivity versus subjectivity, it would appear that such a statement is akin to window covering. Later discussions with the individual offering that statement have indicated anything but such window covering. However, statements such as these simply lend fuel to the fire and are not helpful in articulating the primary issue—the scientific validity of the discipline.

Other comments have been as disconcerting if not more so, and these articles have come in the context of statistics. While a better treatment than offered by Deschênes et al., the article by Taroni et al. lays forward some assumptions that simply do not appear accurate or, alternatively, this author is more naïve than at first thought. It was never expected that the forensic scientist could offer the answer to the judicial question, but even more basic than that, this author would argue that the judicial question as posed in the article is not the ultimate judicial question. The ultimate judicial question for which the jury or the judge has the responsibility for answering is whether or not the individual accused of the crime is indeed responsible as charged. That is the question that needs to be answered. In the scope of that question, everything else is used as either evidence in support of or in refutation of the charge, including the examination results of the toolmark expert.

Further, what the authors offer in support of statistics in the toolmark discipline is severely offset by their overly cavalier handling of the issue of consecutive corresponding striations. This author would wholeheartedly agree and has argued on many occasions that the quality in this discipline comes through quality education, supervised training, and controlled experience. However, the use of the term "*simply* counting striations" (26) (emphasis added) simply points to a lack of a fuller appreciation of what is involved in the issue of consecutive corresponding striations.

As well intentioned as the article by Bunch appears, its usefulness was immediately handicapped by introducing another variable into the picture. From the outset, it was apparent that the article was written from a Bayesian approach, an approach that has not yet been accepted in the discipline. Nor was there sufficient discussion as to the utility of the Bayes theorem for the discipline. Nor was there any discussion as to the applicability of the traditional pattern-matching approach in the same context that CMS was criticized.

Bunch offered numbers in the context of likelihood ratios. But, his offerings solve nothing. One of the primary issues at hand is articulating exactly what the examiner means by weak, moderately strong, strong, or very strong evidence and sticking a number on that does absolutely nothing to communicate it better, other than making it look "more scientific" as suggested by Deschênes et al. Further, and this is a very serious and grave concern, because of the relative infrequency of higher counts of CMS in known matching situations, the likelihood ratio will critically underestimate the value of such sets. This is not a limitation or weakness of CMS as Bunch has suggested in his writing, but of the Bayesian approach itself. It has been clearly established that such a high run of CMS is so significant as to render the possibility that it could have resulted from a known non-match so small as to render the possibility insignificant, a statement the strength of which could never be approached using the Bayesian approach and likelihood ratios.

He indicated himself that it was "unfair" to solitarily subject CMS to such a critique. Yet, he did little to subject the more traditional approach to the same objections, even though similar charges made against CMS could be levied against the pattern-matching approach as well. Interestingly, his final statement implied that pattern matching should get "the benefit of the doubt" (27). The question that remains is why? He articulated a set of arguments against CMS, but did nothing about articulating support for the traditional pattern-matching method. One does not offer a proof of validity based on an argument of invalidity, or lack of sufficient proof, of an alternative.

The problem with this lies exactly with Bunch's summary statement. Whether or not the traditional pattern matching should get "the benefit of the doubt" is not really the issue because it is clear that in the court system it is NOT getting the benefit of the doubt. That does not mean it is in error, but that the end-users simply desire a better explanation of what is going on than they have been accorded in the past. The problem has been, is, and will remain a problem of articulation.

Conclusion

In an effort to bring current a review published in 1997, a review of the most current articles in the discipline of firearms and toolmark examination, specifically criteria for identification, has been presented. Such articles included some general articles focusing on different aspects of communication, studies concerned with the theory of identification as presented by Biasotti and Murdock, studies specifically designed to examine a particular tool or firearm component, as well as discussions in the application of statistics for the discipline. The cogent points of each of these articles were presented to simply offer the reader some highlights. Such a review should not be an excuse to not read the originals, but should actually provide an impetus to critically read and review all of the pertinent literature in the field.

It was during the review of these articles that a concern expressed in the first review became more readily apparent—the lack of ability to articulate the important aspects of the firearms and toolmark identification discipline. This is the one issue that continues to confront the discipline, and there has been little to resolve the issue. Much has been done to address the fruit, but little has been done to address the issue at its very root. As a result, what has developed is an air of contention rather than mutuality in purpose. It is hoped that this article helps put the focus where it truly belongs.

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