Figure 1 – a Folsom Preform

The Invisible Signature of the Folsom Point Knapper

Tony Baker July 1, 2010

Folsom points are the gold standard for finely crafted lithic projectile points and modern knappers find them very difficult to replicate. As a result archaeologists, collectors, and knappers alike, have suggested over time that they were the product of a few craftsmen; a person who made all the points for the band or possibly for several bands. I was a strong proponent of this craftsman theory in the early years of my archaeological studies. When I attended graduate school in the 1980s I wrote about the craftsman and continued to believe in the concept into the late 90s. See *Art and the Folsom Point* (http://ele.net/art_folsom/art_fols.htm). By the turn of the century, my beliefs were changing and by the fall of 2000, I gave a paper at the Plains Conference in St. Paul, MN in which I argued that the vast majority of the Folsom hunters were making their own points. [1]

When I think back over these years, I realize I never had any hard facts or evidence for either position. I only had opinions. Today as I write this, I have some additional information from which one can form an opinion and the purpose of this webpage is to share it with the reader. The information is in the form of probabilities and statistics, so if the reader is adverse to these things one may not want to continue. On the other hand, I will point out that probabilities and statistics are the reasons one ultimately loses money in Las Vegas. Also, if the reader is not familiar with the process of creating a Folsom point, I suggest they read *Folsom Point Manufacture* (http://ele.net/folsom.htm) or the *Hanson Site* (Frison and Bradley 1980).

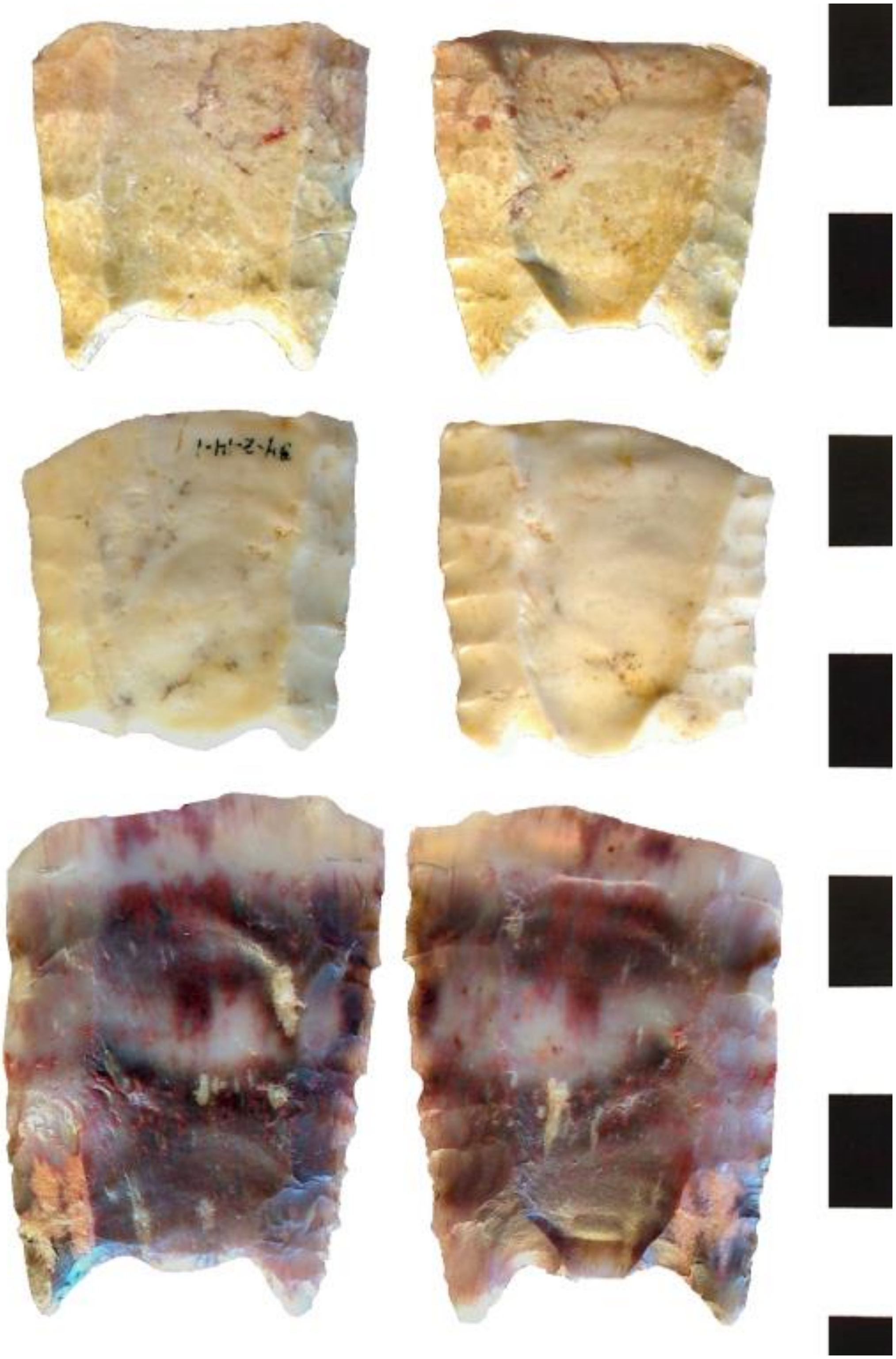
Background (research prior to 1/1/2001)

In the 1990s and early 21st century I was using finite element analysis (mechanical engineering software) in an attempt to understand flake mechanics. Specifically, at that time, I was focusing on the fluting process of the Folsom point.

To be mathematically correct, I wanted to use measurements of preforms and channel flakes that matched the archaeological record. Fortunately, I had sufficient examples in the Baker Collections to obtain these measurements. However, I soon discovered that there was one measurement I couldn't obtain. This was the thickness of the channel flakes from the two opposing faces of the same point. I obviously could measure the thickness of the channel flakes, but I didn't know which ones came from Face A (first removal) and which came from Face B (second removal). In different words, how did the dimensions of channel flakes from Face A differ from those from Face B?

One day my luck changed. While looking at Folsom preforms with Bob Patten (http://www.stonedagger.com/index.htm), we discovered the preparation work for fluting Face A differed from that of Face B. Figure 1 is a fragment of a preform abandoned during manufacture.[2] Face A had been fluted and Face B was almost ready to be fluted. I know it was almost ready to be fluted because Face B has be prepared or shaped with dressing flakes (pressure flakes), which are perpendicular to the long axis of the preform. These dressing flakes create deep scars at the edge and have a feather termination at the center line. As a result these dressing flake scars create a ridge along the longitudinal-centerline of the preform. Most everyone knows that flakes follow ridges and this longitudinal-centerline ridge is there for the channel flake to follow. Additionally, the dressing flakes regularize the face so that a straight edge can be laid on the face, parallel to the longitudinal axis, and there will be no high or low location on the face. As a result, the channel flakes from the archaeological record are straight and uniform in thickness. The preparation of the face to be fluted with the dressing flakes is probably overwhelmingly responsible for a successful channel flake removal.

As I wrote in the previous paragraph, we discovered the dressing flakes scars on Face A were different from those on Face B. Basically, the dressing flake scars on Face A are wider (larger) than on Face B. This can be easily seen in Figure 2, which depicts three more Folsom preform fragments destroyed during manufacture. Unlike the one in Figure 1, which was destroyed before Face B was fluted; these were destroyed during the fluting of Face B.[3] Notice in each case the dressing scars (between the edge of the flute and the edge of the preform) are wider on Face A. And, this observation holds true for most of the preforms in the Baker Collection. So at that time, I proposed that the dressing scars on Face A were wider because the preform was wider when Face A was dressed. When it became time to dress Face B (after fluting Face A) the edges of the preform were "turned" to dress Face B. "Turning the edge" naturally removes material from the edges and the preform becomes narrower.[4]



Face A **Face B**

Figure 2 – **examples of differences in dressing scar widths on Face A and Face B**

With the discovery that the two faces had different dressing scar widths, I now had a way to obtain the thickness of the Face A and B channel flakes. Channel flakes naturally carry the dressing scar signature from their Faces as they come from the center of it. See the channel flake in Figure 3. So, <u>channel flakes with wide dressing scars are more likely to have come from Face A, while those with narrower scars come from Face B.</u> I underlined this statement because it is the bases for the following analysis.

Figure 3 – **channel flake fragment**

I chose 80 channel flakes from the Baker Collection that were intact enough to see the dressing scar pattern. I then visually forced ranked them by dressing scar width. After the forced ranking Channel Flake #1 had dressing scars that were narrower than the other 79 channel flakes. Additionally, Channel Flake #37 had scars that were wider than those numbered 1 to 36 and narrower than those numbered 38 to 80.

After the forced ranking, I measured the width and thickness of the 80 channel flakes. I knew the channel flakes numbered 1 to 40 should relate to Face B, and those number 41 to 80 should relate to Face A. And, I fully expected that there would be a difference in the average values of width and thickness between the two groups. To my surprise there was not. Based on t-tests channel flakes from Face A and Face B have a 84% chance of being equivalent based on width and a 48% chance based on thickness. Table 1 presents the numeric results along with data from the literature for three other Folsom Sites (Tunnell and Johnson 1991). To further emphasize the statistical sameness of the two faces, I also prepared Figures 4 and 5. These present the actual values for width and thickness of each of the 80 channel flakes in their forced ranking order. Figures 4 and 5 make a convincing argument that the first 40 channel flakes (I consider Face B) are identical to the second 40 channel flakes (Face A).

Table 1 – **Channel Flake Dimensions**

	Width (mm)			Thickness (mm)		
	N	Average	Standard Deviation	N	Average	Standard Deviation
Face A (41-80) Baker Collection	37	15.95	2.57	40	1.97	0.39
Face B (1-40) Baker Collection	32	15.08	2.50	40	1.92	0.30
Both Faces -- A and B Adair-Steadman, Lindenmeier, and Hanson Sites	531	16.08	2.72	592	1.98	0.421

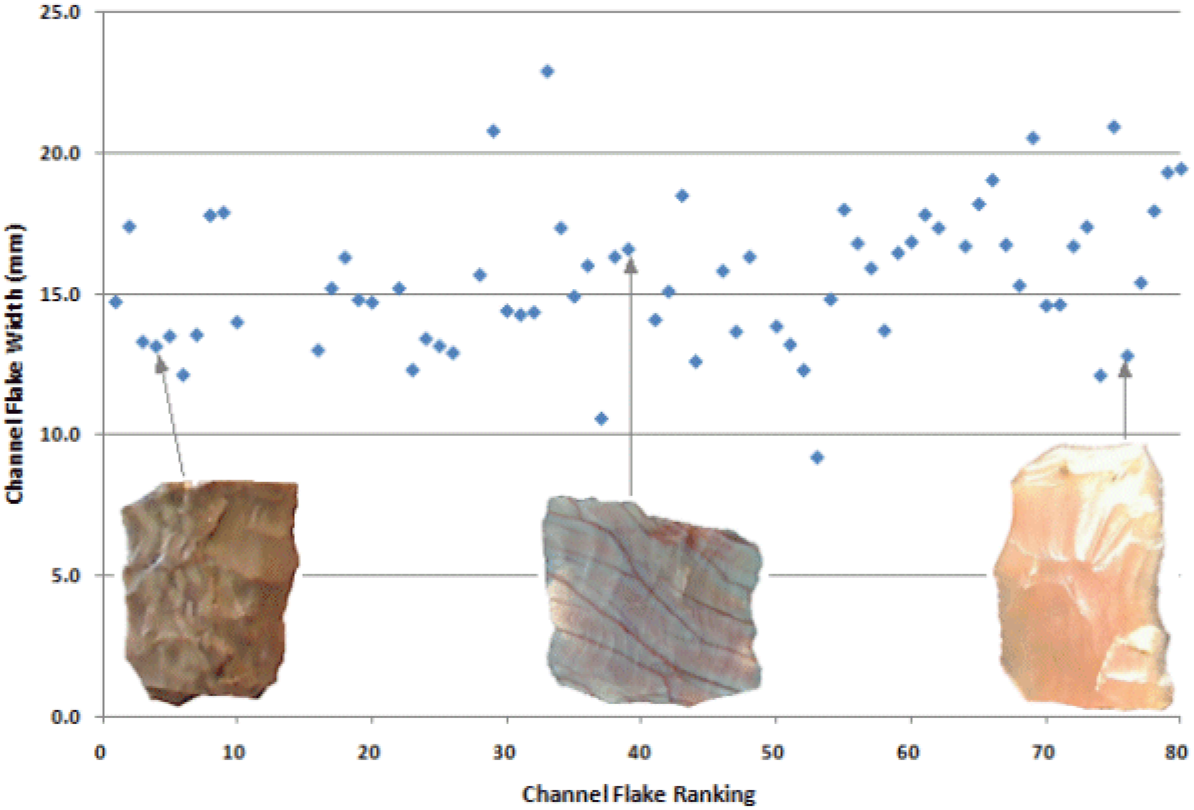


Figure 4 – **channel flake width**

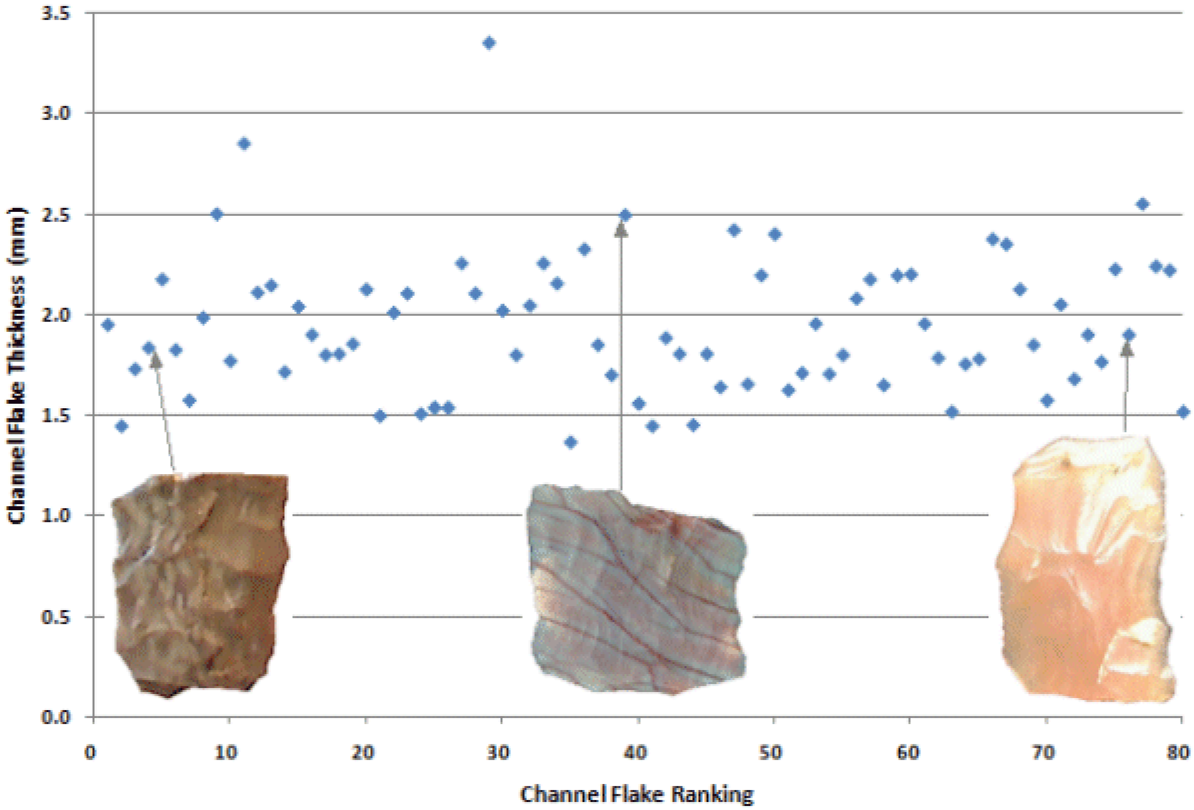


Figure 5 – **channel flake thickness**

The research presented up to this point was completed by the fall of 2000 and I presented it at the Plains Anthropological Conference that year in St Paul, MN. In summary, Bob Patten and I had observed that the dressing flakes scars on Face A of the Folsom preform were wider than those on Face B. I assumed this was due to the preform width being wider when Face A was being dressed. I use this information to separate channel flakes, which carry the dressing scar signature, into a Face A group and a Face B group. From these two groups I then determined that the width and thickness of the channel flakes from Face A and Face B are virtually identical. The width and thickness are approximately 15.5 and 1.95 millimeters, respectively. These were the values I needed at the time for my other research, so this work was filed away.

Current Research (more than nine years later)

In January, 2010 I was discussing Folsom Point manufacture with John Garrett, a fellow engineer and amateur archaeologist. I showed him preforms and channel flakes including those in Figures 1, 2, & 3. I pointed out that the dressing flake scars on Face A were wider than those on Face B. I explained to him that this was because the preform was narrower when dressing Face B. I said it in a manner that sounded like it was fact. Yet, I didn't know this to be correct, it was only a theory and I had never tested it. These discussions with John compelled me to test the theory. Additionally, I wanted to quantify the difference in dressing scar widths from the two faces.

To accomplish these objectives, I needed preform fragments that were broken as a result of Face B fluting attempts. Only those would have remnants of the dressing scars on both faces. Additionally, the fragments needed to be large enough to be able to measure the width of at least two adjacent dressing scars on each edge on each face. I would have preferred to have had more than only two, but that would have reduced the number of preforms in the test population. With the requirement of only two adjacent scars I was able to find 16 preforms in the Baker Collection. Figure 6 depicts these preforms, and as the reader can see, the size and material type varies considerably within the sample. Additionally, 12 of these preforms were found within 22 mile radius of each other and 15 within a 50 mile radius. One (1) was found more than 300 miles from the rest.

I measured preform width and dressing scar widths on each face of the 16 preforms. Figure 7 depicts the dressing scar width on Face B plotted against the preform width. The solid line is the best fit linear regression line that has an r^2-value of 0.26. In different words, the regression line explains 26% of the variation between the preform width and the flake scar width on Face B. Even in lithic studies 26% is not very impressive, but in contrast, the r^2-values for Figures 4 & 5 were less than 0.01 or 1%. At least the value of 26% indicates that preform width could be partially associated with Face B dressing scar width. However, I had expected a greater association and I definitely implied that there was one when I discussed this with John.

Figure 6—preforms broken during the fluting of Face B

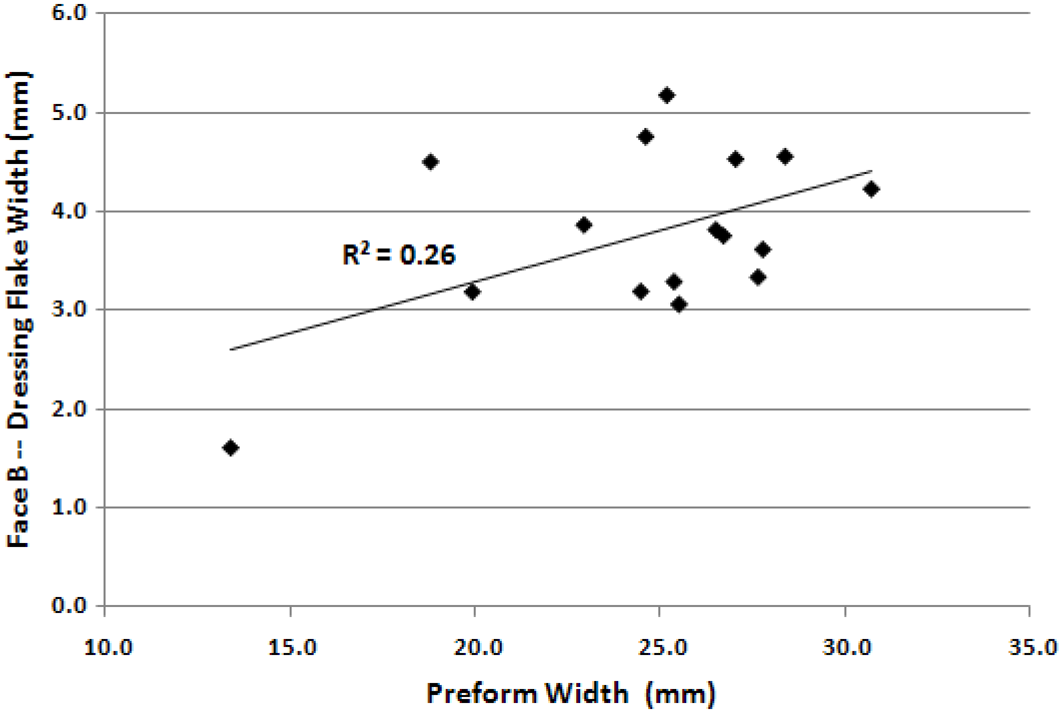


Figure 7 – **Face B scar width vs. preform width**

Due to the poor correlation between Face B dressing flake scar width and preform width, I decided to investigate the correlation between these two variables and Face A scar width. This is shown in Table 2. To my surprise the correlation between Face B scar width and Face A scar width was much greater than with preform width. Figure 8 depicts the linear regression between the scar width on the two faces. Here the regression line explains 58% of the variation between the two variables. However because of the correlation between Face A scar width and preform width, see Table 2, I knew that 58% was too optimistic. So I ran multiple regression analysis with Face B as the dependent variable. This indicated that 60% of the variation in Face B scar width was explained by the two variables, and that Face A scar width explained 50% of that variation. The multiple regression analysis also provided me with an equation that permitted me to remove the effects of the preform width on Face B scar width.[5] Figure 9 depicts Face B flake scar width, less the effects of preform width, verses Face A scar width.

Table 2 r^2- values

	Preform Width	Face A
Face B	0.26	0.58
Face A	0.20	

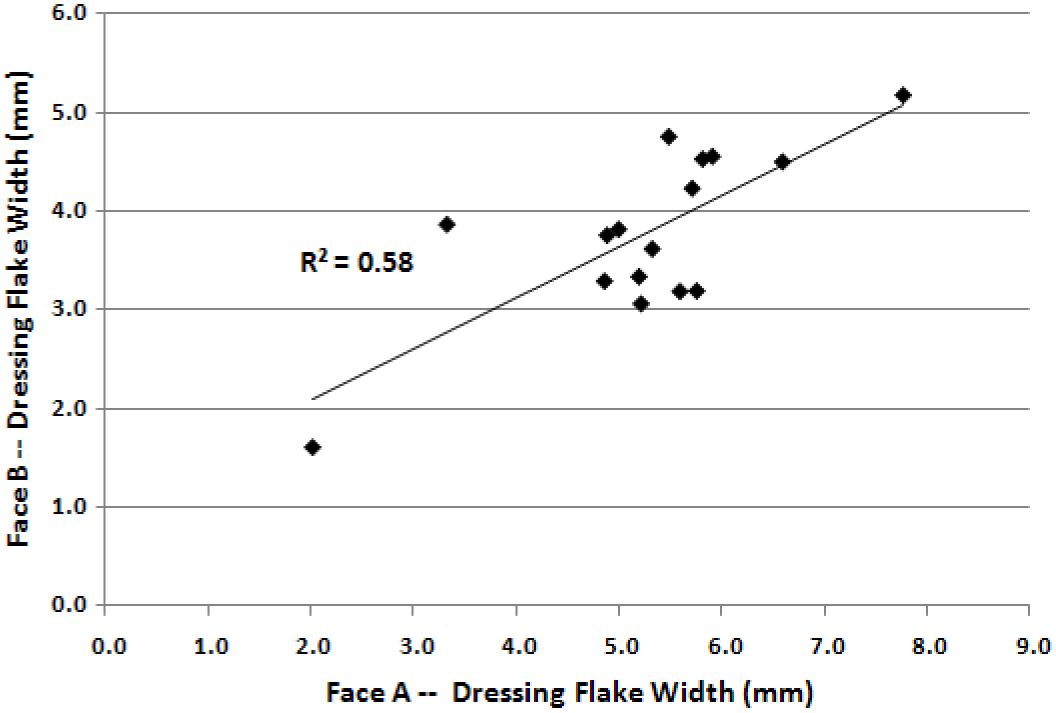


Figure 8 – Face B scar width vs. Face A scar width

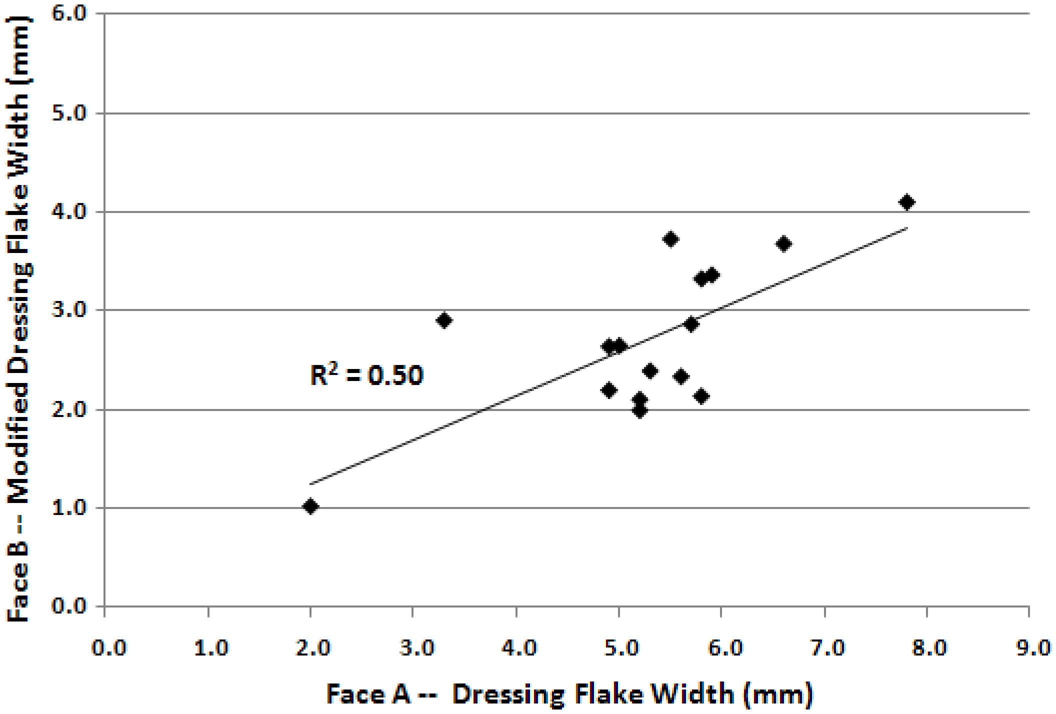


Figure 9 – Face B scar width (less preform width effects) vs. Face A scar width

Removing the effects of preform width (Figure 9) did not alter the fact that Face B scars are narrower than Face A scars on all 16 preforms. I wondered why this was the case. Nor, did it alter the trend that has Face B scars increasing in width when Face A scars do. So, a second question is *why* does this trend exist? Originally, I believed that preform width was dictating the dressing flake scar width. However, the statistical work strongly suggests this to be incorrect. Something else was going on and that something was common to both Face A and B. Therefore, I found myself asking what does Face A and B have in common? The only two things I could think of was the rock type and the knapper.

To test if different lithic material types might have some influence on flake scar width I returned to my data set of 80 channel flakes in Figures 4 and 5. Remember these had been ordered (ranked) by dressing scar width. In the 80 there were 71 that were represented by 10 material types with more than one channel flake in each type. The other nine were singletons representing nine different, additional materials, which I placed in a group I called singletons. Table 3 represents the results of the single factor analysis of variances (ANOVA in Excel) of the 80. As can be seen material type does not seem to be a factor in dressing scar width. Instead, it is far from being important with a p-value of 0.36. I made a second run without the singleton group and the p-value changed to 0.32.

Table 3 – analysis of material effects on flake scar width

Anova: Single Factor

SUMMARY

Groups	*Count*	*Sum*	*Average*	*Variance*
CHALCEDONY	5	113	22.6	238.3
CUMBRES PASS	21	895	42.61904762	562.8476
EDWARDS LIKE	2	49	24.5	480.5
RANCHERIA	3	176	58.66666667	536.3333
JASPER	14	506	36.14285714	616.1319
LIVER RED	3	103	34.33333333	745.3333
OBSIDIAN	3	68	22.66666667	374.3333
WASHINGTON PASS	14	649	46.35714286	440.0934
PEDERNAL	6	263	43.83333333	586.9667
SINGLETONS	9	418	46.44444444	574.2778

ANOVA

Source of Variation	*SS*	*df*	*MS*	*F*	*P-value*	*F crit*
Between Groups	5397.363492	9	599.7070547	1.126584	0.35609	2.017
Within Groups	37262.63651	70	532.3233787			
Total	42660	79				

The results of the ANOVA work in Table 3 is worthy of a few more comments. Ultimately it is saying that rock type does not influence the Folsom knapper's product. This realization then raises another question. Does this mean the physical properties, such as elasticity, hardness, brittleness, etc. do not vary from rock type to rock type? Most proficient modern knappers would strongly argue that these properties do vary. They could offer the example of heat treating and how it greatly alters these properties in some materials and I will agree that it does. Hypothetically then, would a Folsom knapper working heat treated stone still make a channel flake with the same width, thickness, and dressing scar widths as he does on untreated stone? I suggest that he would. I further suggest that the Folsom knappers, along with proficient modern knappers, obtain feedback from working the stone and this feedback permits them to adjust their application of force to produce the same product they produce the previous day with a different stone type. And, they probably are not conscious of the tiny adjustments they are making. They may say "this rock is different or harder", but their product remains the same.

I had eliminated rock type as a possible answer to my two questions, so the two still loomed large. Why were the dressing scars on Face B narrower than those on Face A? And, why was there a linear trend in the 16 preforms in Figures 8 & 9? The answer had to be associated with the knapper. So I asked a knapper. Specifically, I asked Bob Patten (http://www.stonedagger.com/index.htm) who I mentioned earlier. Bob can replicate a Folsom point and he had a good answer to the first question. He suggested the knapper can be bolder when dressing Face A, because the preform is thicker and stiffer. Additionally, the knapper has little invested in the preform at this stage of the process, so in a sense he throws caution to the wind. After Face A has been successfully fluted, the preform is thinner and more fragile. So, the knapper is more cautious with his work and that caution results in smaller, less risky flake removals. Although I am not a knapper, I can relate to this and I believe it is correct. So, my first question was answered.

More than one knapper (?)

Accepting the idea that the knapper creates smaller scars on the second face, still doesn't explain why there is a linear relationship (trend) between the scar widths on Faces A and B. In different words, why don't the scar widths on Face B cluster around a central point in Figures 8 & 9. The central points in each Figure being the average value for each face that the knapper had trained himself to strive for over the years. Finally, I realized I was possibly seeing more than one knapper in Figures 8 & 9.

To test for multiple knappers, I performed some stochastic computer modeling. First, I made the following assumptions:

1) There was only one knapper, which was my null hypothesis (H_o).

2) The knapper attempts to make the same product (dressing scar width) every time, but variation occurs and it is normally distributed around an average.
3) The averages for scars widths on Face A and B are 5.3mm and 2.7mm, respectively. These are the numeric averages of the 16 preforms in Figures 6, except Face B average has been modified for the preform widths as seen in Figure 9.
4) The standard deviations for the scars widths are 1.3mm and 0.8mm, respectively. These are the values associated with the averages in Assumption 3.

I then mathematically created 200 hypothetical preforms by randomly sampling the two normal distributions that represent the dressing flake scar widths of Face A and Face B. Figure 10 depicts how these 200 preforms distribute on a graph.

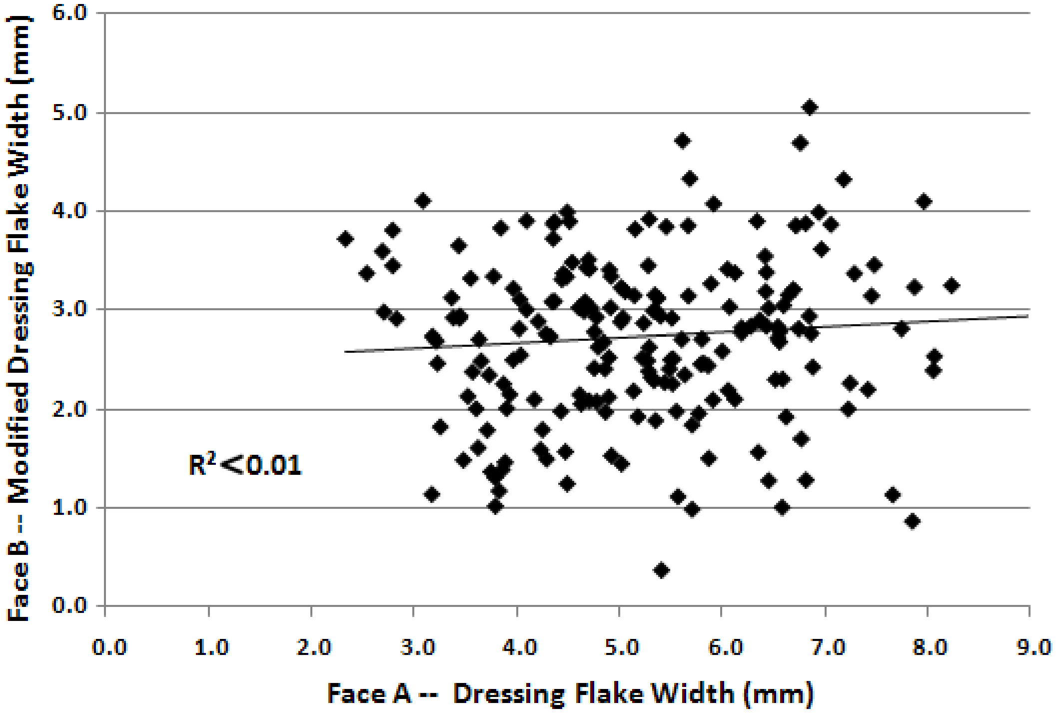


Figure 10 – **200 hypothetical, randomly created preforms**

The 200 hypothetical preforms in Figure 10 cluster in a cloud around the intersection of the two averages of 5.3mm and 2.7mm. In statistics if one can't visually see a trend in the dots, there isn't one. And there isn't one in Figure 10. The mathematics bears this out as the linear regression line explains less that 1% of the variation. Therefore, with the assumption that the knapper tries to make the same product each time, it is not possible for a single knapper to have created the trend seen in Figures 8 and 9.

It occurred to me that my 16 preforms represents only a sample of the archaeological record. Maybe it was possible that the trend was a result of the sampling and it did not exist in the larger population? So, I sampled the 200 hypothetical preforms, 1000 times, by randomly pulling 16 preforms at a time. As a result, I can state the odds of creating a linear trend with an r^2-value of 0.50 or higher (Figure 9) is close to nil. Therefore, I must conclude that the 16 preforms in Figure 6, which are the subject of this paper, are not the product of a single knapper.

How many knappers?

To answer this question, I developed a stochastic method to measure probablities. To test the validity of the model, I ask Erik Otárola-Castillo (www.public.iastate.edu/~eotarola/homepage.html) to review my work and give me his opinion. He said my approach was not wrong, but I was ignoring several of the assumptions of the normal distribution. He offered a couple of different approaches to circumvent my laxity, which I tried. However, they did not alter the outcome that I achieved with my method.[6] So, I am presenting my method as I believe it is the easiest for me to explain and the reader to understand.

Returning to Figure 9, I removed the regression line, and added a radius line from the center of the cluster (5.3 mm, 2.7 mm) to the most distance preform. This is shown in Figure 11. I reasoned that if there was a second knapper responsible for any of the preforms then the one furthest from the center would most likely be one of his. Using stochastic modeling again, I assumed the null hypothesis was that there was only one knapper. Then I returned to the 200 hypothetical preforms in Figure 10 and increase the number to 1000. I calculate the radii of all 1000 from the center of the cluster (5.3 mm, 2.7 mm) and determined how many were equal to or great than 3.7 mm, the radius to the furthest preform in Figure 11. Only three of the 1000 met this criterion, which is a p-value is 0.003. This p-values is quite small, so I rejected the null hypothesis and accepted the hypothesis that this most distant preform was made by a second knapper. At this point I had convinced myself that there were at least two knappers visible in the data, Knapper A who made 15 of the preforms, and Knapper B who made the most distant preform in Figure 11.

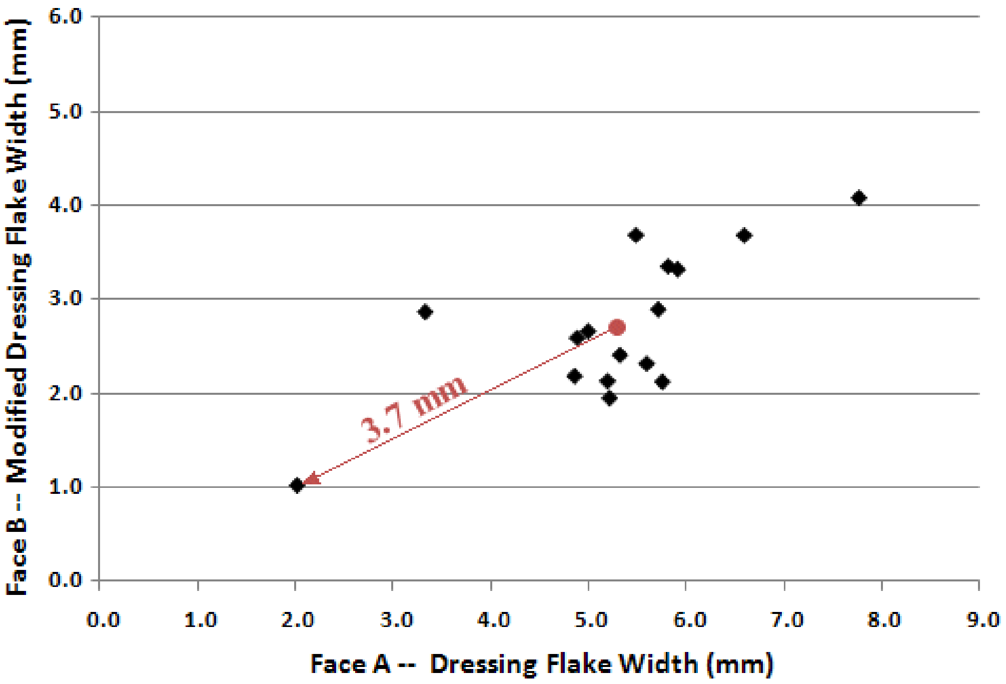


Figure 11 – distance to furthest preform from center

Is there still another knapper visible in Knappers A's 15 preforms? To answer this question, I assume the null hypothesis that Knapper A made the 15 remaining preforms. I calculated a new center for the 15 preforms and a radius to the most distant. I re-created the 1000 preforms with the new center. This time the odds of this most distant preform occurring increase to eight out of 1000 or p=0.008. This again is a small p-value, so I chose to reject the null hypothesis that Knapper A made the 15 preforms and accept the alternate hypothesis that there was a Knapper C who made the most distant of the fifteen. I continued this process and found that two more preforms that have very small p-values. However, the p-values rose rapidly after removing these four. So, I am convinced there are five Knappers (A, B, C, D & E) visible in the 16 original preforms. Knapper A made 12 of them and Knappers B, C, D & E each made one. Figure 12 & 13 illustrates which preforms belong to which knappers.

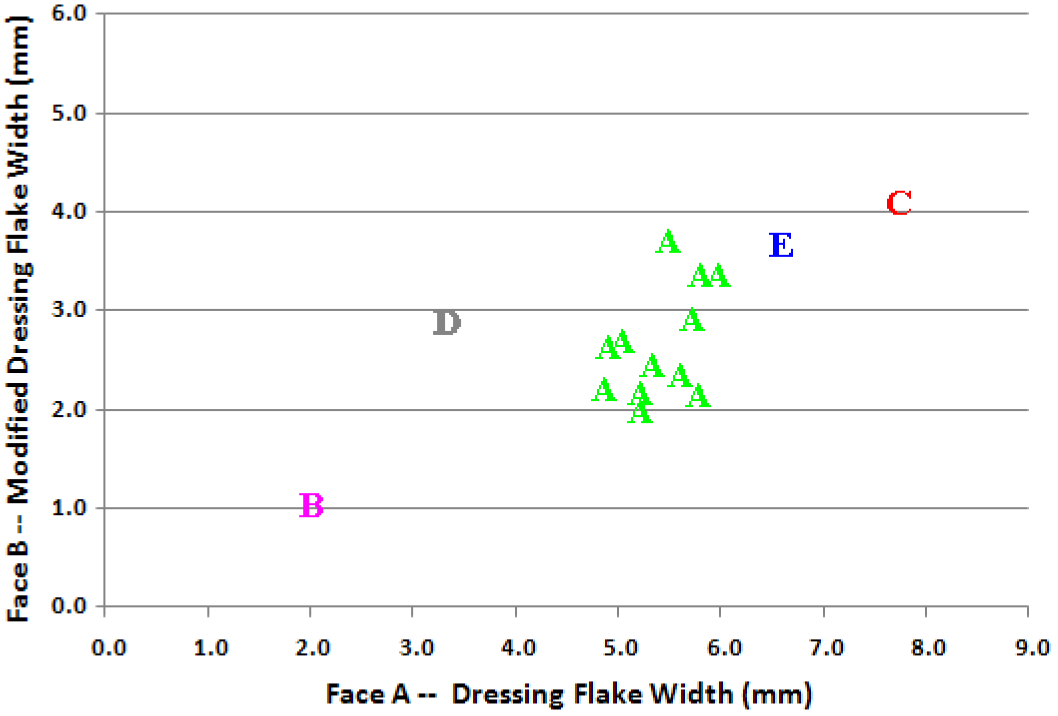


Figure 12 – identified knappers

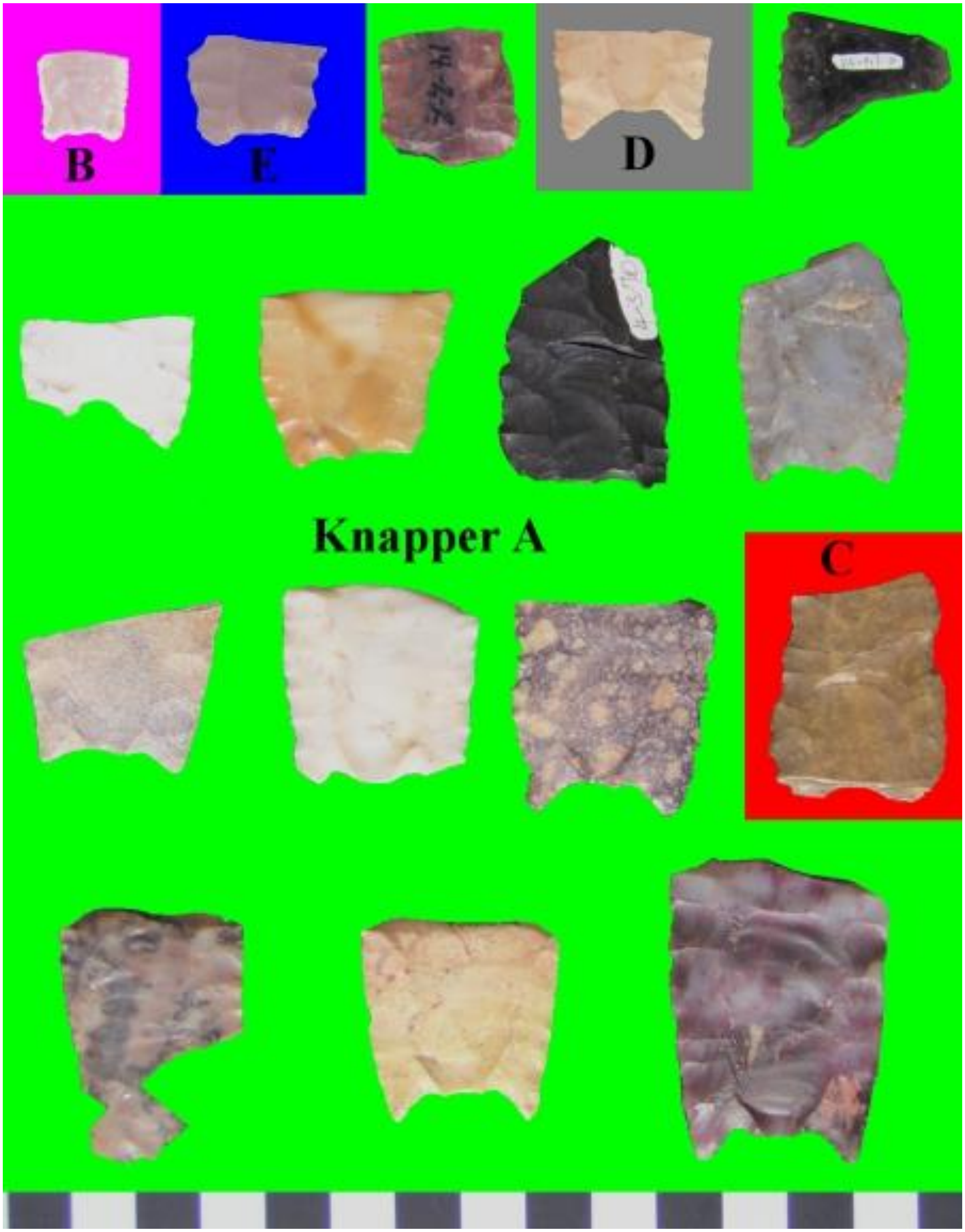


Figure 13 – identified knappers

Where were the knappers located?

A partial answer to the question in the above subtitle can be derived from the find locations of the preforms. Since they are preforms, they were never finished, hafted, or used, and their find locations are probably very close to where they were made. Figure 14 is a map of the locations where the preforms were found. All landmarks have been removed, but the map is to scale. All the preforms made by Knapper A were found within a 22 mile radius of each other. The preforms of Knappers B, C, & D were found approximately 70 miles from those of Knapper A. Finally, the preform make by Knapper E came from over 300 miles away from those of Knapper A.

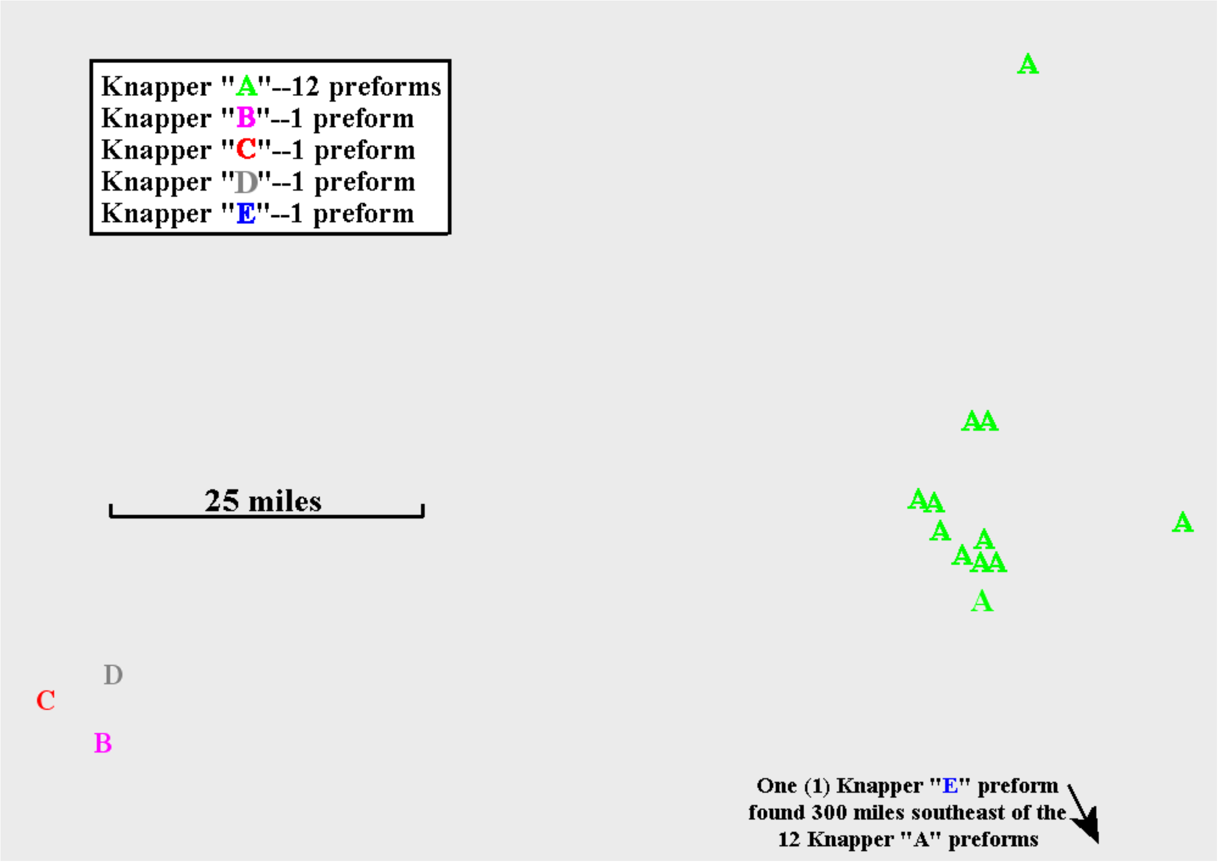


Figure 14 – preform find locations

After looking at Figure 14, the reader is probably thinking that the find locations of the various preforms influenced the assignment of the preforms to the various Knappers. I can tell you that they did not. In fact, the map in Figure 14 was an afterthought created out of curiosity. I was as surprised as I suspect the reader is, as Figure 14 is remarkable support for the discussion. What are the odds of finding the evidence of only one knapper in the radius of 22 miles? To put this question into better perspective, consider the facts that this area also produced 77 finished Folsom point fragments, 147 additional preform fragments, and 415 channel flake fragments.

And this represents only the material that was found by my father and me. How much more material is still on the ground or in other collections? That said, what is the possibility that the 12 preforms of Knapper A are the result of collection bias? Again, what are the chances my father and I were just lucky enough to find only the products of one knapper? To answer this question let's assume that there were only two knappers in the subject area and that they made an equal number of points over time. This hypothetical case is identical to flipping a coin where only heads or tails are possible outcomes. So what are the odds of flipping heads twelve times in a row? This question has an easy answer, which is 2 out of 10,000 attempts, or $p=0.0002$. As a result, I chose to reject the null hypothesis that there were two knappers in the area and accept the fact that there was only one. For what is worth, if there were more than two knappers, then the odds of finding 12 belonging to a single knapper becomes even less.

The Knapper A phenomenon does have another explanation. Suppose the Knapper A preforms were actually made by a number of different individuals, but the variation in the dressing scars of the different individuals is so large that the statistics can't separate them. This would be a classic example of the variation within the group (the individual) being greater than between the groups (individuals). The only way I can see how this might be tested is with experimental archaeology and modern knappers. Unfortunately, I don't know of that many good Folsom point knappers. So, at this juncture, I will continue to believe that Knapper A is a single individual who is responsible for the 12 preforms in Figures 12, 13, & 14.

The other area of preform concentration in Figure 14 is the West Area, which is the location of Knappers B, C, & D. Although it is not apparent in the Figure, this area is similar in size to Knapper A's Area, or about 20+ miles in radius. However, the density of artifacts is less than 1/10 of that of Knapper A's Area based on finding only three finished Folsom point fragments, nine additional preform fragments, and 35 channel flake fragments. So, at first glance, the West Area and Knapper A's Area seem to be telling conflicting stories. The area with the most diagnostic Folsom stuff has only a single knapper, while the area with more knappers has the least Folsom stuff. The answer is that the amount of Folsom stuff is not related to the number of knappers in the area, but to the time the knappers spend in the area. In different words, the volume of stuff is related to knapper-years in the area. (See *Pleistocene Bones and Stones in the New World* at http://www.ele.net/arch_record/stones.htm.) Knapper A and his associated people (bands) were in their area 10 times longer than Knappers B, C, & D and their people were in the West Area. So while Knapper A and associated people remained in their area, Knappers B, C, & D and associated people came and went. Maybe B and people came first and left, then C and finally D. In any case, Folsom people occupied the West area about 1/10 of the time of that of Knappers A's Area. And, this is my story and I am sticking with it.

Concluding Remarks

If the reader hasn't figured it out by now then you probably are wondering why I titled the paper *The Invisible Signature of the Folsom Point Knapper*. The answer is the signature, which is the dressing flake scars on the preform, doesn't exist on the finished point. As a result any follow up to this work will require additional preforms. It is possible there exists a signature on finished Folsom points and I encourage the reader to look for it. However I have no idea what it might be.

The most important thing about the work presented here is the strong support for a single expert knapper who is making all the points for the various Folsom hunters in an given area. As I stated in the beginning of this paper, I used to believe that this was the case because the making of the point is so difficult for modern knappers. However, in the 1990s I began to change my opinion to a belief that each hunter made his own points. I began to believe that all the everyday Folsom hunters were just exceptional flint knappers. The research presented here has caused me to return to my earlier belief. Such is science.

Notes

1 Abstract of paper presented at 2000 Midwest Archaeological / Plains Anthropology Conference, St. Paul, MN.

> *Folsom Point Manufacture – A Common Task Preformed by All*
>
> Recent computer modeling of the Folsom fluting process indicates that fluting with high-angle-percussion can yield almost "automatic" success across considerable variation in input variables. This process is simple and can be learned by anyone with knapping skills. The implication of the "automatic" process is that Folsom point manufacture was probably a common task performed by all hunters in lieu of the product of a single craftsman in the group. This paper will discuss the results of the computer modeling and how they match the archaeological record.

2 Figure 1 depicts a preform that is a very rare artifact. It is rare because unlike most Folsom preform failures, which occur during the high-risk steps of fluting Faces A or B, this failure occurred during the low-risk steps of preparing Face B for fluting. The fact that there are only seven (7) exhibiting this type failure of the 264 preforms in the Baker Collection is indicative of how low-risk preparing Face B really is.

3 Distinguishing Face A from B on a Folsom preform can easily be demonstrated in Figure 2. The most obvious indication is the deep scoop on Face B immediately above the striking platform. This is the negative scar of the bulb of percussion that is on the channel flake. On Face A, the striking platform and this negative scar (scoop) has been

removed in preparing for fluting Face B. It is removed by shortening the preform and "turning the edge."[4] If it is left on the preform, Face B could not be properly fluted because the Face B channel flake would not run past this thin section of the preform.

4 "Turing the edge" is a term I picked up from the modern knapping community. I understood the process before I learn what the knappers called it. In 1998, I called it "reversing the bevel." See "Stage 8--Channel Platform Preparation (Face B)" (http://www.ele.net/stage_8.htm).

5 The multiple regression equation is:
$\text{ScarWidth}_{\text{FaceB}} = (0.4463)*\text{ScarWidth}_{\text{FaceA}} + (0.0435)*\text{PreformWidth} + 0.35$
The Face B values plotted in Figure 9 come from:
$\text{Figure9values} = \text{ScarWidth}_{\text{FaceB}} - (0.0435)*\text{PreformWidth}$

6 Shortly after developing my stochastic method, I attended the 2010 Paleoanthropology - SAA meetings in St. Louis. At the meetings I heard a statistical paper by Erik Otárola-Castillo and I felt this was the person who could critique my methodology. So, I approached Erik and to my surprise he was a tremendous help. Plus, he was more gracious that I would have ever expected.

One of the issues Erik had with my methodology was that I was creating my stochastic preforms by randomly sampling a normal distribution and flake scars widths are not normally distributed. Flake scar widths can never be equal to or less than zero mm. Yet, the normal distribution has the possibility of creating negative values all the way to negative infinitive. Erick suggested I convert my scar width values to logarithmic values and then re-apply my stochastic method. When I did this it raised the p-values of the preforms of Knappers C, D, & E. For example Kanpper E's p-values increased from 0.012 to 0.106. This increase in p-values did not alter their occurrence in the process nor the fact that they were different from the 12 preforms of Knapper A.

The other critique that Erik offered was focused on my strong adherence to rejecting the null hypothesis if the p-values were 0.05 or less. He reminded me that "statistically different" or "statistically significant" were actually meaningless. We in the social sciences have arbitrarily and almost outspokenly established this threshold so we can make decisions and answer questions with a forthright yes or no. But there really is a gray area here that we are ignoring. Erik further suggested that the 0.05 p-value threshold to reject the null hypotheses is really dangerous if the sample size is small and my 16 preforms is a small sample size. As a result of this critique, I rewrote the paper and removed words like "statistically significant". This philosophy also permitted me to present my stochastic method in good conscience.

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