From 1997 through 1999, the Middle Mississippi Survey field school conducted a set of archaeological tests in Ste. Genevieve, Missouri. Founded circa 1750, Ste. Genevieve was the first permanent French settlement on the west side of the Mississippi River. Its original location, in the bottomland area known as Le Grand Champ or the Big Field, turned out (not surprisingly) to be vulnerable to Mississippi flooding. The town moved to its present, slightly higher position in the 1780s and 1790s. A number of French colonial structures still stand in Ste. Genevieve, making it a tourist draw and a significant architectural district.

Ste. Genevieve was occupied under three national governments: French, Spanish, French again briefly, then American after the Louisiana Purchase. The town retained its essentially French nature through the Spanish period, the Spanish governing lightly and immigrating few settlers. On American acquisition, however, there was an influx of settlers from the east. According to the historians, Ste. Genevieve had
made a transition from a French colonial society to an essentially American one by the mid-19th century.

My paper today is a preliminary attempt to tackle two problems simultaneously. One is to test archaeologically the historians’ view about culture change in Ste. Genevieve. The other is to figure out a method of comparing assemblages meaningfully, so that I can approach the first problem.

We were able to test the yards of three historic houses in Ste. Genevieve, thanks to the permission and hospitality of the Missouri Department of Natural Resources’s Felix Valle State Historic Site. The first was the Delassus-Kern house, actually south of town in what once was the community of New Bourbon. The house is on the land of a French nobleman who arrived in 1793, and it’s built in the vertical log French style, but it turns out that this structure probably was not built until the 1830s. A family of German descent was the main occupant, into the 1960s. The site of the original French house was on the bluff above the Delassus-Kern house, and was recently investigated by Southwest Missouri State University, but I have not received the report.

The second site we tested in Ste. Genevieve was the Benjamin Shaw house, which is downtown. The front structure here was built around 1820, and the rear part added in about 1840. We excavated six test units in the back yard and one at
the front of the side yard. The artifacts generally reflect an 1820s to late 20\textsuperscript{th} century occupation.

The third site, the Felix Valle house, was built in 1818 and occupied until taken over by the Missouri state park system. It has a couple of distinctions relevant to this study. It is the only site we tested that actually had French colonial residents and their descendants. It also was the only site that produced an early context. The privy probably was filled about 1830 with debris from the destruction of an earlier house, so the assemblage represented a redeposited domestic occupation of roughly 1780s to 1830. The other test units represent a mix of everything from 1780s up to late 20\textsuperscript{th} century, emphasis on post-1818. Materials from several of these units are still being catalogued, so my data from the Valle house are preliminary.

For comparison, I have five sites from Western Kentucky, tested in a rather sporadic fashion over the last 20 years. The first was Whitehaven, now a tourist welcome center off I-24 at Paducah. Whitehaven was built in the 1860s and occupied into the 1960s, then restored in the 1980s as the welcome facility. I tested the yard around Whitehaven in 1982, just before the restoration. I excavated five test units and a series of posthole tests here.

The Moore house, in Ballard County, was a two-story double-crib log structure, built in the 1840s, that burned in the early
1980s. It was the seat of a large plantation, by local standards, before the Civil War. With a crew of volunteers, I excavated a set of posthole tests and three test units at the Moore house site.

The Tilghman house, in downtown Paducah, was built in the 1850s for Lloyd Tilghman, who became a Confederate general and died in the Civil War. With some volunteers, I excavated posthole tests and two test units in the front and side yards. Grant Quertermous told you something about his continuation of this project a few minutes ago. This is my smallest sample, and I have reservations about its comparability to the others because I don’t have anything from the back yard, where I would expect more domestic midden to be.

In 1996, with field school students, I added two more sites to the sample set. The Curtis-Sullivan house, in Ballard County, was built about 1840 and occupied into the late 20th century. We excavated four test units in the back yard. This house was odd in the lack of midden deposits around it, so I can question how comparable this sample will be, also.

Finally, I have four units behind and beside the David Morrill house, in Graves County. It was built in the mid-nineteenth century, and offered some fairly productive deposits.

To summarize, I have three sites from Ste. Genevieve and five from western Kentucky. Two Ste. Genevieve sites are in town
and one was a farm or plantation house. One western Kentucky site was in town, the others were farm or plantation houses. In terms of ethnic descent, the western Kentucky sites and Ste. Genevieve’s Shaw house represent generalized Anglo-American occupants, the Delassus-Kern house a family of German descent, and only the Valle house a French family. All eight produced deposits dating from the early to mid-19th century into the later 20th century. Only the Valle house had a distinguishable early context, the privy fill zone. So in terms of the question I started with, is the Valle house distinguishably French at some level? More broadly, can the German Kerns and the French Valles be distinguished from the mongrel Americans on an assemblage basis? Or can the urban be separated from the rural occupations?

How do I test this proposition? I can say right away that there aren’t any artifacts that scream, “Hey, I’m French!” There is a trace of faience brun, late tin-glazed earthenware, in all three Ste. Genevieve sites, and the Valle house is in no way distinguishable in this attribute. Even the Valle privy fill is clearly dominated by English wares. We expect this in French colonial sites, of course, but it doesn’t help with the problem at hand.

So I turn to thinking about assemblage patterning. I don’t know how many of you remember much about Stan South’s original formulation of his patterning technique, but one of the things
he was after was trying to get at ethnic differences through assemblage patterning in colonial sites. South’s patterning technique never really took hold for creative analysis. It was extremely influential as a call for quantification and systematic collection and reporting, and also as an organizing device for describing and summarizing artifact collections.

But, as a number of colleagues, including South, have noted, the application of pattern stalled pretty quickly. South defined two patterns, the Frontier (later changed to Architecture) and Carolina Artifact Patterns, the latter representing 18th century British domestic deposits. South hoped that historical archaeologists would explore variability in these patterns through time, space and society. Instead, we tended to use them as static standards. Either X assemblage fit the Carolina pattern, or the Architecture pattern, or it didn’t. If it didn’t, all too often the archaeologist would define a new pattern, say the 19th Century Mining pattern or the California Brothel pattern or some such. Fortunately the proliferation of named patterns didn’t last long. Unfortunately, most of us just ended up saying, so what? rather than finding ways to explore variability. I did point out at Whitehaven that there seemed to be a transition from Architecture to Carolina pattern with time and with distance from the house, but in the absence of a series
of controlled contexts representing relatively tight time spans, there wasn’t much more I could do with it.

So, whither pattern? in Stan South’s words. Part of the problem was that we had two opposed sets of percentage ranges, with no obvious way to assess whether other assemblages were significantly like or unlike the type data, and no obvious way to explore where significant differences lay. I started thinking about whether we could use the pattern data in more rigorous, that is statistical, ways. I even went to the trouble of auditing a class in multivariate statistics. What I found was that most of the standard statistical approaches to social science data didn’t fit my needs very well. To use the raw data requires fairly consistent sample sizes, which we rarely have in archaeological assemblages. To allow for sample size biases, we either use percentages or do some fairly involved, and I think dubious, statistical manipulation. But the statistical techniques don’t do very well with percentage data, for the most part.

There are of course a number of papers that approach the problem of “Quantifying Diversity in Archaeology,” to quote the title of a 1989 book edited by Leonard and Jones. There are various techniques, borrowed from ecology, that measure something the archaeologists who use them call heterogeneity. Measures of heterogeneity combine measures of richness, having
to do with the number of categories in the assemblage, and evenness, which has to do with to what extent the categories are equally represented within the assemblage. Evenness is a horror in archaeological pattern studies, where a few artifact categories—like plainwares, or the Kitchen group—tend to dominate assemblages and the real action is in small variations in low-count, low-percentage categories.

To these “concepts related to diversity” George Cowgill also added the notions of range, standardization, and uniformity of standardization. All of this can get pretty involved statistically, but I tend to agree with Bon Dunnell, who called these a set of measures in search of an application. For one thing, they haven't been applied to anything that really makes them look useful. More important for my problem, they measure heterogeneity within assemblages, not among assemblages. What I want to measure is similarity between assemblages, and homogeneity or heterogeneity among groups of assemblages.

That brings me to the Brainerd-Robinson coefficient of similarity, which was derived specifically as a measure of similarity between assemblages where artifact counts are expressed as percentages. It measures on a scale of 200, with 200 signifying identical assemblages and 0 signifying completely different assemblages.
The Brainerd-Robinson coefficient, like South’s pattern, was proposed some time ago, but hit a wall in terms of application. It was proposed as a seriation device, but there are easier ways to do seriation, and we tend to think of seriation as an application to chronology. Seriation may reflect other dimensions than time, and a coefficient of similarity doesn’t specify what dimension of time, space or assemblage content is being measured. So Brainerd-Robinson coefficients are a technique in search of an application, also, something that a few people like Cowgill dust off for heuristic purposes now and then but haven’t applied very usefully. Parenthetically, historical archaeologists haven’t thought about Brainerd-Robinson because it was derived for prehistoric sites, and historical archaeologists like to think of their field as something separate from prehistory—but that’s a diatribe for another time.

Recently, Mike O’Brien applied Brainerd-Robinson to a new purpose: showing the wide variability of collections from the same site, to make a point about sample biases due to different collection techniques. But I think there’s a further step we can take. If we control for time—as with my sites from Ste. Genevieve and western Kentucky—and use the same collection techniques, we can use Brainerd-Robinson to measure assemblage similarity. We can then use very simple statistics to look at
the distribution of similarity coefficients among groups of assemblages: this becomes an index of homogeneity, and a way to assess whether a single or a group of assemblages is different from the rest.

Here are my examples. I start with South’s original sites from which he derived the Carolina Artifact Pattern. By default, these are the sites that set the standard for variability among historic sites considered to represent a coherent cultural group, that is, a pattern. The Brainerd-Robinson coefficients among South’s sites have a mean of 163.7 with a standard deviation of 10.6.

I can compare homogeneity among western Kentucky sites at two levels. On one level, I can look at homogeneity within sites, among the different test units. The index of homogeneity for the Moore house is 182.9 +/- 8.9, which says to me that deposits tested at the Moore house are very similar to each other (182.9 average coefficient of similarity, versus 163.7 for the Carolina sites), and very consistent assemblages, with a +/- of only 8.9, less than the Carolina sites’ variance of 10.6.

For Whitehaven, the index of homogeneity is 179.6 +/- 13.1, more internally similar than the Carolina sites but slightly more variable. For the Curtis-Sullivan house, the index is 165.1 +/- 17.3. In all three cases, similarity among deposits within the sites is greater than those among the Carolina sites.
Then we get to the David Morrill house, which has an index of homogeneity of 146.3 +/- 31.9. Wow. The Morrill house deposits are tremendously variable. Why this should be the case will require further analysis, but it certainly pops this site out as unusual.

I did not do an internal index of homogeneity for the Tilghman house due to the small number of samples, only two test units.

Now, I’d like to derive an index of homogeneity among the western Kentucky sites, comparing site totals. I get an index of 157.6 +/- 24.0. The western Kentucky sites are less homogenous than the Carolina sites.

I can do the same set of calculations for the St. Genevieve sites. The index of homogeneity within the Delassus-Kern site is 160.6 +/- 21.2. For the Shaw house, the index is 169.3 +/- 19.8. For the Valle house, the index is the highest of any I’ve seen so far, 186.2 +/- 5.2.

This last one amazed me. The Valle house is the only one with a specialized early context, the privy fill. Yet even including that, the assemblages from Valle are more similar to each other, and more consistent, than any other site I can measure. We’ll have to see if the final figures, from three more test units, maintain this level of homogeneity.
But what I’m really after is whether the French Valle house is different from the German Delassus-Kern house and the Anglo-American Shaw house. The homogeneity index for the three Ste. Genevieve sites is 161.2 +/- 20.9. That is, heterogeneity among these three sites is not greater than internal variability within the Delassus or Shaw house sites. The index of homogeneity among the three sites is approximately the same as for the Carolina sites, although with a wider range. Valle is not distinguishable at this level of assemblage patterning.

I also calculated the index for the combined set of western Kentucky and Ste. Genevieve sites, to see if Ste. Genevieve was different. The index for all eight sites is 162.1 +/- 22.1. In particular, coefficients of similarity between Valle and Tilghman, and Valle and Morrill are both 195, very high levels of similarity. Ste. Genevieve in general, and Valle in particular, are not demonstrably different from the west Kentucky sites—again, at this level of assemblage patterning.

To me, this result substantiates the historians’ perspective that Ste. Genevieve was fully Americanized by the mid-19th century.

I also calculated the indices of homogeneity comparing the Carolina sites to West Kentucky and to Ste. Genevieve. In both cases the indices fell in the 130s, with standard deviations over 30. This excited me for a little while, in that it seems
to show that the Carolina sites are demonstrably different from my sites. This I would expect, given that the Carolina sites belong to the 18th century and my sites to the 19th and 20th centuries.

But then I realized that collection strategies might account for it as well. That is, I might be counting, say, nail fragments where South’s lab counted only whole nails. This would make my Architecture group counts considerably higher than South’s, accounting for the differences.

So, if this technique is going to be useful, anybody applying it needs to be careful to compare assemblages directly only if the collection strategy is the same. However, as long as recovery and cataloguing methods within groups of assemblages are the same, the index of homogeneity should be comparable between projects, as in comparing the index for the Carolina sites to the indices for the western Kentucky and the Ste. Genevieve sites. I will be trying this with sub-assemblages, also: for instance, ceramics and glass, to see if those kinds of data show similar levels of similarity, or are more discriminatory among sites.

I will be developing these ideas further, so I hope you’ll forgive a very preliminary presentation of an idea that’s early in the thought process. I’ll appreciate comments. Thank you.