

HOW ARCHAEOLOGY IS SCIENCE: A CASE IN POINT

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Archaeology is generally agreed to be a scientific discipline, and in theory it involves a process that goes something like this: the researcher makes observations on some preliminary data, on the basis of which s/he constructs a hypothesis. The hypothesis is then tested to see if it is supported by additional facts. In a perfect world, this would involve going into the field, locating a site containing new raw data relevant to the hypothesis, and seeing if they support or disprove the original hypothesis. If they do support it, it's regarded as a good hypothesis for the time being. If they don't support it, the original hypothesis is modified or scrapped altogether and a revised or entirely new one is formulated in light of the new, countervailing data. Either way, the testing process is repeated in the field so as to develop increasingly better hypotheses concerning the subject in question.

Unfortunately, it isn't a perfect world and it's not always possible to simply proceed into the field at our convenience and recover the data we need to test standing hypotheses. That's because it's often very difficult to predict where to find undisturbed sites that will contain the information we need. This seems to be especially the case with Palaeo-Indian sites. So what the archaeologists generally do is document their initial hypotheses and the data upon which they are founded, and then patiently wait for the opportunity to test them. Nowadays, that opportunity usually arises during cultural resource management projects, when the desired data are fortuitously encountered in areas of high archaeological resource potential during impact assessment and mitigation activities. Hypotheses may also be tested against the literature wherein relevant data may already exist in published form.

A very good example of how the overall procedure works in real life is exemplified in Palaeo-Indian research on the Prairies within the past 45

years. The lead player in the scenario I have in mind is Dr. David Meyer, a dedicated Saskatchewan archaeologist of long-standing.

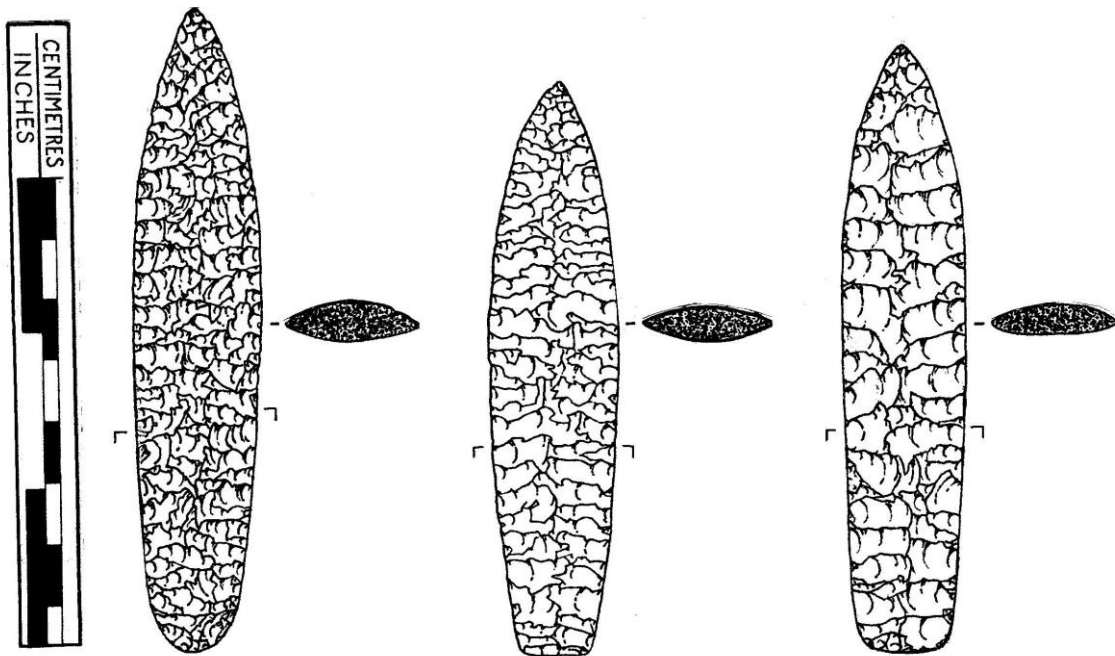
For many years, numerous surface finds of slender lanceolate points were being collected on the Canadian Prairies and classified as “Agate Basin” because they looked more like Agate Basin points than anything else. In 1970, while a graduate student at the University of Manitoba, David was questioning the appropriateness of this classification. In a paper published in the *Saskatchewan Archaeology Newsletter* that year, he described and interpreted an assemblage of Plano-looking lanceolate points that had been surface-collected at various places within a former embayment of Lake Agassiz. The embayment, an extension of the Manitoba Lowland physiographic region, formed part of the Carrot River district of eastern Saskatchewan.

David undertook to provisionally date these finds with reference to the Lake Agassiz chronology as it was then known. He observed that the embayment would have been under water during Agate Basin times and hence inaccessible to persons of that particular cultural persuasion. People could only have occupied the embayment later on – that is, in post-Agate Basin times -- after it had been vacated by the waters of the lake. The upshot was that, in David’s own words, “the Carrot River Plano points are so removed in time (2-3,000 years) from Agate Basin that there is no rationale for calling or equating them with Agate Basin.” Furthermore, he noted a variety of morphological attributes in the Carrot River assemblage that further differentiated the specimens from *bona fide* Agate Basin points.

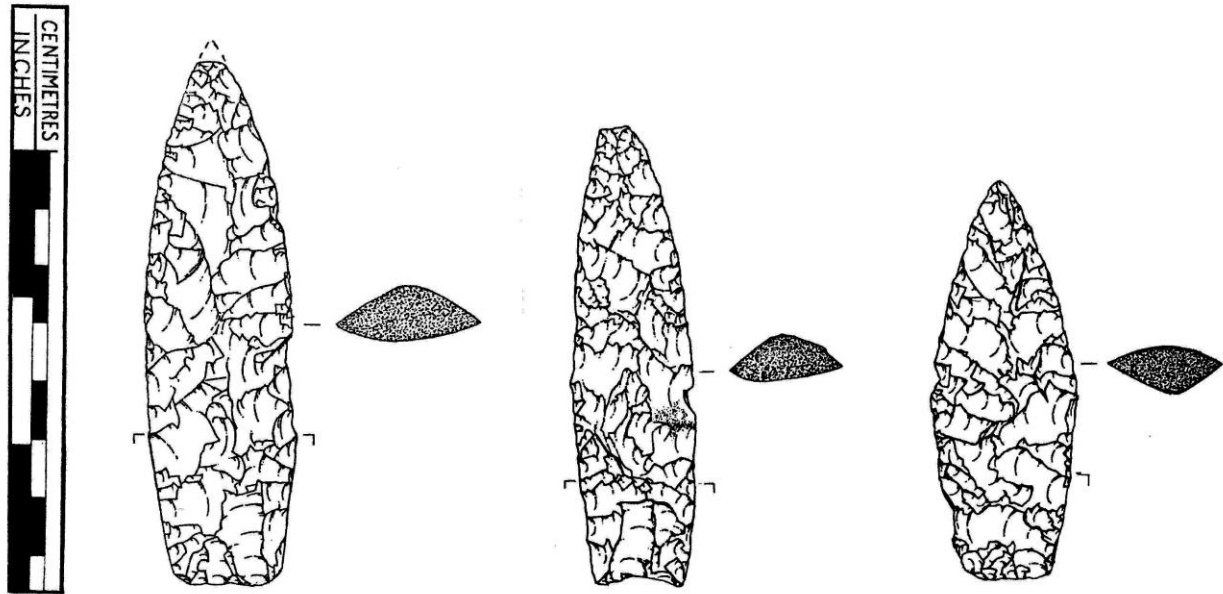
On the basis of the Carrot River recoveries, David hypothesized that there had existed within the region “a late Plano archaeological culture, tenuously related to, but not identical with, the early Agate Basin complex.” Several years later, the Nipawin Archaeological Survey produced a further sample of these points, again, all surface finds. In the ensuing report, David formalized his thinking by assigning these and the earlier findings to what he called the “Nipawin complex.” He was understandably guarded in taking this step, because normally the establishment of an archaeological complex

in the literature is based upon the controlled excavation of one or more components containing a range of artifact classes in addition to projectile points, along with data pertaining to other cultural variables such as economy. Nor did he propose a taxonomic label for the points of the Nipawin complex when he initially introduced it.

His limited data base notwithstanding, David was fully justified in rendering his Nipawin complex hypothesis because (1) it was eminently testable, and (2) as it turned out, subsequent field research in all three Prairie provinces has produced data relevant to the question of whether or not a



▲ The above artifacts are Agate Basin points.



▲ The above artifacts are NOT Agate Basin points.

post-Agate Basin (indeed post-Cody) Plano occupation, distinguished by slender lanceolate points, existed on the Canadian plains. In his comprehensive book “Light from Ancient Campfires,” Dr Trevor Peck lists no fewer than nine radiocarbon-dated sites in Alberta, belonging to the post-Agate Basin Plains/Mountain and Lusk complexes, that contain late Plano lanceolate points. Two more such components are now on record in Saskatchewan, and an additional two have been discovered and researched in Manitoba.

So we can say with a high degree of confidence that David’s original hypothesis has thus far stood the test. Radiocarbon-dated components excavated from one end of the Canadian Prairies to the other over the past three decades or so have consistently supported it, and with these independent data in hand we can presently conclude that it was a very good hypothesis indeed.

At the same time, I would point out that not all facets of the hypothesis have drawn support by the more recent research. For example, David surmised that his late Plano complex dated to between 7,000 and 5,000 RCYBP. The

dates for the sites reviewed above cluster around 8,000-7,500 BP, so his 1970 estimate for the Nipawin complex appears to be a bit too recent in light of the now-available sample.

Also, David suggested that the Nipawin hunters were “big game specialists,” but the faunal inventory from one of the Alberta sites (Boss Hill) included the remains of both large (bison, elk, bear) and small (fox, badger, beaver, jack rabbit, snowshoe hare, muskrat) game, not to mention those of waterfowl (teal, scaup, goose) and fish of an undetermined species. Level VII at the St. Louis site in central Saskatchewan yielded not only bison remains but those of fish and waterfowl as well. Numerous pieces of shell from a large egg were uncovered along with two late Plano points in the bottom level of the Teepee site on Duck Mountain in western Manitoba. These findings suggest to me that the ancient inhabitants of these sites possessed a more diffuse economy than the term “specialist” would imply.

I hasten to add that these results in no way diminish the significance of David’s contribution: disproving hypotheses is as much a part of the scientific process as is finding evidence that supports them. That’s how the system works.

My closing remarks fall more within the “Great Minds Think Alike” department than anywhere else. Right around the time that David was developing his late Plano hypothesis on the Canadian side of the border, Harvard PhD student Henry Irwin was generating a few ideas of his own on the American side. Reacting to Wilfred Husted’s classification of projectile points from sites in the Bighorn Canyon of Montana, Irwin wrote, “we must overcome the indiscriminate assigning of projectile points on superficial morphological resemblance to known and now well-dated types like Agate Basin.”

This was David’s sentiment exactly, based on his handful of surface finds from eastern Saskatchewan and his familiarity with the most recent rendition of the Lake Agassiz chronology. Both men came to the same conclusion independently of one another, based on two distinct and widely separated

sets of data. In my opinion, David's 1970 paper in the *Saskatchewan Archaeology Newsletter* was a ground-breaker, a major contribution to Palaeo-Indian studies on the Canadian Prairies, and one of the best demonstrations of insightful archaeological hypothesizing I have seen in over 45 years of involvement in the discipline.