Telling Tales: Interpretive Trends in Southern Ontario Late Woodland Archaeology

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The publication in 1966 of Jim Wright's The Ontario Iroquois Tradition *and his construction of the Late Woodland culture history for southern Ontario has had a tremendous impact on how this part of the archaeological record has been thought about since. Yet a critical component of this construct, often referred to as the Conquest Hypothesis or Theory, has always proven problematic and contentious, and generally has been rejected by Wright's colleagues. This seeming paradox in rejecting a critical interpretative construct while embracing the model serves as the starting point here for examining the ways in which archaeologists have come to think about and interpret, and re-interpret the Late Woodland history of southern Ontario. In moving away from the normative assumptions inherent in the Ontario Iroquois Tradition model and actively engaging in broader theoretical discussion, Ontario archaeologists are beginning to ask different questions of— and apply new perspectives to — the archaeological record. What I offer here is an example of the kind of story that can be told based on such changing interpretive approaches to the Late Woodland period. Through application of a long-term, historical perspective to the archaeological record, and recognition of the role of agency, the period of the supposed Pickering Conquest is seen as representing both a range of variable local responses to on-going change, and strategies of response based on over a millennium of cultural development, rather than a sudden, militaristic "hiccup" in between long periods of cultural equilibrium. I also intentionally offer here fictive vignettes, an informal voice, and play, all of which are intended to underscore the point that archaeology, ultimately, is about telling stories about the past. In the end, it is up to you, the audience, to decide whether this story "works," and if so, where it will go next.*

Introduction

Across the world over the last thirty years and more archaeologists have increasingly been involved in self-reflective debates and discussions. Focussed on archaeological theory, concepts, assumptions, and ultimately on methodological, interpretive and personal limitations, these debates have radically challenged how we, as archaeologists, think about what it is we do, conceive of the material remains we work with, and construct our understanding of the past (see Johnson [1999]; Trigger [1989] for reviews of these trends; see as examples Binford [1989]; Hodder [2001, ed.]; Knapp [1996]; Preucel [1991, ed.] and Schiffer [2000, ed.] for a sampling of this cacophony). To varying degrees (Kelley and Williamson 1996; Wright 1985), these issues slowly have found their way into Canadian and Ontario archaeological studies,

and have affected how archaeologists perceive the record they are working with and re-constructing.

My interest here is to review the manifestation of these broader conceptual trends in the study of the Late Woodland in the southern Ontario region north of Lake Ontario and Lake Erie over the last 35 years, and the potential for these trends to enrich future investigations of this period. Commonly thought of as the ancient or pre-contact history of particular Iroquoian-speaking peoples, this part of the archaeological record has dominated the attention of archaeological research in the south of the province (e.g., Ellis and Ferris 1990 [ed.s]). This is not surprising, given that the record for this period is massive, rich and covers significant cultural developments, comparable in research potential to many other regions of the globe (Ramsden 1996).

Archaeologists here have also worked with a

long-lived cultural-historical framework for this period (the Ontario Iroquois Tradition or OIT: Wright 1966), which has served to influence the direction taken for much of the subsequent research and interpretative debates of the Late Woodland in southern Ontario. At the same time, recent decades have seen an explosion in the data generated for this region, as Archaeological Resource Management activities related to regulated land use development processes have become entrenched and consult-ant archaeologists have added to the database by over a hundred fold from what was available originally to construct the OIT framework (Ferris 1998). This in turn has ultimately led to the emergence of a tension between the established OIT framework for the Late Woodland, and the massive amounts of new material data whose fittedness to this paradigm becomes increasingly problematic.

As archaeologists have struggled to negotiate this tension, while trying to tell their stories of the archaeological record, the various assump-tions that were inherent in the OIT, particularly around change and cultural development, have emerged and become points of contention. The degree to which archaeologists have succeeded or failed to address these is a reflection of how inter-pretive trends in Ontario have utilized or not the broader theoretical advancements occurring in archaeology, and how willing researchers are to accommodate differing ways of re-thinking and re-telling the past.

To explore this I will review the interpretive trends that have emerged in discussing the cul-tural developments that occurred through the first half of the Late Woodland period, especially arising from Wright's original suggestion of a conquest marking the transition between the early and middle periods of the OIT (Wright 1966; Wright and Anderson 1969). While I do wish to critically review this concept and exam-ine how it has both engaged and stifled debate, my intent is also to use the issues inherent in the conquest interpretation as a point of departure to explore more broadly the way Ontario archaeol-ogists of this "Iroquoian" past have (re)presented their data and conceptions. And in exploring

these interpretative trends of the southern Ontario Late Woodland, I wish to critically engage at least some of the assumptions inherent in the normative cultural historical model that is the OIT — interpretive assumptions which oper-ate below the data and often are accepted unre-flectively by many working here. Such a perspec-tive is critical, I think, since the last 35 years have shown that any data to data debate on the accu-racy of the OIT is instantly mired by having to operate within the OIT framework. And since the OIT is much more a product of archaeologists than of archaeological data, simply debating data misses a critical component of the OIT framework.

I will first review the specific and general inter-pretive biases that I believe operate in the current construction of the Conquest Theory and the early Late Woodland period as representing Iroquoian local history. But I don't simply want to aim a spotlight on the interpretive crutches we all have utilized previously — I also want to high-light how archaeologists have advanced their interpretations from the original OIT, and offer a re-telling of this past based on these efforts. After all, it is an essential tenet of all archaeological research that it necessarily advances from that which has come before, and our under-standing of the archaeological data for the Late Woodland period of southern Ontario is quite a substantial foundation upon which to build.

I should also caution that I don't *deny* other stories than the one I present here could be told of this period, nor do I profess exclusivity of interpretive authority. Indeed, much of what I review here represents the collected, published thought of the many past and present researchers working in this field. In fact, while I can't prom-ise full citation of every individual who ever pub-lished a particular thought on a particular subject at every instance raised in this paper, I will try to represent as wide a community of contributors to the on-going construction of this part of the archaeological record as my fallible research can cover. What I hope to add to this is a voice not normally a part of the conventional telling of southern Ontario Late Woodland archaeology.

My alternative telling will be aided by histori-

cizing the events of the early Late Woodland. While at first glance this may sound odd for the presentation of culture history, nonetheless much of the conventional descriptive archaeology for this period has tended to be presented a-histori-cally — events described as episodic occurrences within otherwise continuous cultural stability. This perspective usually fails to account for the constancy of change and the historical reality it creates and within which all people must operate, and which, through personal agency and social structure, served to constrain decisions and ulti-mately shape the direction of this change (Bordieu 1977; cf. Blinkhorn [1997]; Deitler and Heidrich [1998] for discussions of this concept in archaeology, and Jamieson [1999]; Rankin [2000a] for examples of historizing Northeastern archaeological trends).

Historicizing interpretations are provided here through the use of a multi-scalar, long-term per-spective that examines the archaeological record. This is simply adopting an Annales school of his-torical analysis (Braudel 1980; cf. Bintliff [1991, ed.]; Cobb [1991]; Duke [1991]; Hodder [1987]; Knapp [1992, ed.] for archaeological applications, and Rankin [2000b] for an example of its use in Ontario). Essentially, by taking a broader historical context, we can step back from temporally-specific events manifest at particular sites, and see how they relate to broad continu-ities through time, or the longue duree of history (Duke 1992). This simply allows us to see the interconnectedness of individual events. But it also allows us to move away from just asking questions such as when or what (i.e., the material remains of the archaeological record), to also questions of why and how (i.e., the agency and contingency underlying human history; see Dobres and Robb 2000 [ed.s]). In other words it allows us to start thinking about the direct role or agency of *people* in shaping the archaeological record, rather than their debris, as well as to think about the historically-informed strategies and structures these people employed to respond to the present or predict the future, rather than thinking about those responses in terms of what *we* as archaeologists "know" occurred later (e.g., Dobres and Hoffman 1994; Hodder 1986;

Johnson 1989; Last 1995; Thomas 2000). These differing analytical approaches to interpreting the archaeological record should not feel too unfamiliar to most of us, as certainly recent trends in Ontario archaeology have adopted at least multi-scalar, long term perspectives, if only implicitly.

Another aspect of what I will offer not nor-mally to be found within the pages of this jour-nal, is in the way I present the archaeological interpretations that make up my re-telling of the early Late Woodland. Archaeology, at its core, is about telling stories about the past. This use of narrative is, I believe, a strength of archaeology, though it is often masked by a reliance on objec-tified language, formal presentation, and descrip-tion of minutia — the objectified distance of "sci-ence" (Terrell 1990; Wylie 2000). There is much to question in any assertion that science itself operates through an objectified voice (e.g., Dupre 1993; Gould, 1989; Lewin 1994), but germane here is the fact that this simply cannot be asserted for archaeology, where interpretations rarely can be reduced to a set of neat alternative equations and hypotheses (Bowler 1991; Terrell 1999:671). Rather, as archaeologists we build our interpretations on the recovered fragments of a past people we can never really know, and in so doing hopefully recognize the complexities and uncertainties inherent in the basic act of inter-pretation. And because we are attempting to con-struct past human behaviour, we also must infuse our interpretations with considerations of ancient cultural context or social meaning, as sloppy and inaccurate as are the processes avail-able to us for this purpose (e.g., Benn 1995; Shanks and Tilley 1987). As a result, rather than translate past human behaviour into predicable formulae, archaeologists become story-tellers, though we anchor our narratives to the recovered fragments of the past. Of course, the very nature of this social context and fragmentary database also means archaeologists benefit from having many ways of telling — multiple stories and differing perspectives all built on the same data sets that can complement, rather than rule each other out (Pluciennik 1999).

To emphasize the act of narrative, and thus the

direct agency I, as author, have in constructing the story of the past I (re)present, I will use the authorial first person voice and informal presentation, eschewing the third-person, passive voice commonly used in archaeological texts, which implicitly serves to distance the author from the material presented and to create a false sense of objective description (Pluciennik 1999:667). I will also situate myself in the foreground by providing personal anecdotes and impressions of this community I belong to. After all, knowing the ancient past is also a negotiation and understanding of the archaeologist's transitory present, as Tilley (1993:8) points out. So as a member of this contemporary community, my reflection of the world I am a part of is critical for understanding the basis of my commentary on the *archaeologists* of the Ontario Late Woodland.

Finally, as a part of my narrative I will also offer fictive stories — interpretative imaginings of the past, if you will. The intent here is to take the interpretative models being reviewed of the early Late Woodland, and illustrate their implications from the perspective of the ancient actors in this narrative (Pluciennik 1999; Schire 1995; Spector 1993). Hopefully this aids in seeing how our interpretations would have operated in this past we create from archaeological data, and in so doing also illustrates the potential flaws these imaginative interpretations will always have in coming to an understanding of a past beyond knowing.

I don't, however, suffer from a hyper-relativist belief that archaeology can never be anything more than just so stories of the past. The stories we offer are indeed telling in that they are based on the recovered material remains of the ancient and recent past. Cautious interpretations which acknowledge the limitations and biases inherent in the database we use, and in ourselves, allow archaeology to link history, geography and anthropology together to tell, and re-tell, the past in a way of knowing that cannot be achieved otherwise. I believe the rich archaeological record of the Late Woodland of southern Ontario is well suited for such a re-telling. I invite you, as reader, to judge whether or not my tales are compelling and convincing.

To Start, A Story...

The sun was hot, and without any wind to cool things off, Talks Little had decided to move from the work area by the opening of the lodge, to a shaded location by the palisade. It was still warm but the shade was better, and the flies not so bad. It was also quiet, which she preferred. Once she had settled in, she wiped the sweat that was dripping into her eyes and reached for the clay.

"This will be the last pot I make today," she said to herself, "then I'll have to join everyone else in the field, hilling the corn."

Because the work in the field would be hard and in the open sun, and because no one seemed to be around, she took her time with the vessel, enjoying this rare calm and quiet inside the village. The wet slaps of her paddle against the clay were the only sounds that could be heard. Slowly the clay was shaped into the familiar form of a cooking pot, like the three others over by the lodge waiting to be decorated, then fired. Everyone said Talks Little's pots were some of the best made, and she took pride that so few of hers ever broke without first being well used.

As she completed the simple task of forming the pot, her reluctance at heading to the fields grew. She looked around and saw that one of the other pots had dried enough to be decorated. Feeling only slightly guilty, she readied the tools next to her and prepared to mark the top of this vessel. As she began this task, her mind wandered back over the changes that had occurred to her and her people since she was a girl, before her husband's people from the east invaded and made everything their own.

"It's such a strange thing, them coming like that," she thought as her hands smoothed over the rim of the pot, "and I've never understood exactly why it came about. There seems so much that didn't make sense. When these people arrived, they were hungry, and a bit wild looking, and seemed to be so desperate for our good corn-growing fields... *but then, surely they must have grown corn themselves to know why our fields were so good. How bad could things have been that they would have marched so far for food? And they seemed so organized to do what they did... why did-*

n't they simply plan better how to live?"

Talks Little searched among her sticks and bones used to mark the clay, trying to find a relatively clean and sharp one, but without a pointed end. She thought to herself, "well, whatever their problems, they sure beat us dead to rights. In no time they were running the show and had killed or chased away most of our people...but *then, where did they bury the bodies? Shouldn't those be places we all now need to avoid? And why have those who were chased away not come back? Where could they have gone to live a similar life to that they had known here?"* She started to etch her first line in the wet *clay,* picking away the little bits that peeled off as the bone made its cut. It also seemed so incredible to her that her husband's people were able to achieve their invasion without burning down a village, and indeed, simply built new villages for themselves and their captives to live in.

She shook her head to herself as she looked around this new village. "My mother-in-law thinks this ordering of houses in the village is so much more modern and efficient than how we lived before. *But then, there isn't* that *much difference overall; how can she think this so sets her people apart and makes them better than us?"*

Finally, she snapped out of her reflection and looked at her now decorated pot. By not paying attention to what she was doing, she saw that the horizontal lines she had drawn across the top were unevenly spaced apart and jagged — indeed in some places so jagged and broken that they reminded her of her mother's old way of decorating vessels, which was to put slanted dashes in rows across the vessel. "My mother-in-law would so disapprove," she thought. But suddenly she found herself filled with anger for her mother-in-law, husband, and their people from the east that had caused so much change. "Why do they insist that I put these groups of straight, flat lines across the vessel? And why does it always have to be three or four rows, not two or even eight? What is that all about, some kind of secret code?"

With grim pleasure, she quickly defaced the pot, pushing in the horizontal lines, and expanded the short, angled slashes into two rows encircling the top of the pot. "There," Talks Little

thought triumphantly, "let this pot remind them of the people that came before them!" Or so *that* story goes....

I remember the first time I was introduced to the Conquest Theory (referred to as the Conquest Hypothesis at the time). It was during my first field season, and I was looking at J.V. Wright's *Ontario Prehistory* (1972) and reading about the last 1,000 years of the archaeological record — that period known as the Late Woodland when people lived in villages and longhouses, grew corn in abundance, and lived that Iroquoian way of life that we would come to read about in the early writings of the sixteenth and seventeenth century Europeans who first arrived here. I asked the archaeologist I was working for if this was an up to date summary of things. He said, "yeah, except for this one change." He then took the book, pulled out a pen, and scribbled on the diagram depicting Late Woodland culture history, drawing a solid line between the Glen Meyer and Pickering early Late Woodland boxes, so that each box then progressed to the next, or middle Late Woodland stage. The Pickering box no longer was to encompass the Glen Meyer box. I remember at the time being less impressed with the scribbling out of the conquest than with the fact that all the rest of the boxes were, by omission, free from the requirement of edits.

Those boxes that made up Wright's construction of the Late Woodland culture history of southern Ontario, which emerged with his publication of *The Ontario Iroquois Tradition* (1966), have indeed enjoyed longevity in the face of subsequent, massive increases in data. They have experienced only minor re-tooling, and have very much served as the base paradigm for researchers, who primarily have sought to plug new findings into these constructed boxes. This despite Wright's oft-quoted prediction that his interpretations would eventually be "...subject to marked alterations" (1966:101).

Yet at the same time, a principal explanatory hypothesis of change proposed as a part of the

OIT – that of a conquest of peoples living west of the Niagara Escarpment by similar peoples living east of the Escarpment – has never been widely accepted by other researchers. This rejection persists despite repeated efforts by Wright to support and refine the idea (e.g., Wright 1984, 1987, 1990, 1992, 1994), and the occasional supporting voice (e.g., Finlayson 1998). Indeed, most other researchers have chosen to simply dismiss the idea as problematic or conceptually unsound, and have declined to engage Wright in any real debate on this point.

This seems to be a curious phenomenon since, as Wright (1990:498) pointed out, the Conquest Theory was meant to offer a means of understanding the development of this Late Woodland cultural group as they evolved to a more intensively horticultural economy, along with all the resultant impacts to social structures and organization that change brought. It seems incongruous that so critical an underlying explanatory construct could he so casually dismissed, while much of the remaining structural framework was accepted so completely. Then it could also be argued that this seeming contradiction reflects both the strengths and weaknesses of the Wright paradigm; the use of which always having been something less than he intended (i.e., an explanatory model of social development), and more of a chronologically-specific classificatory lexicon of material culture (e.g., MacDonald and Williamson 1995:10; Smith 1990:288).

But it strikes me that much of the research to have emerged subsequently, by covertly subverting the conceptual framework inherent in the OIT while keeping the lexicon, has created a degree of "baggage" and miscommunication between some researchers that has limited interpretative advancement. The following reviews the formation and impact of Wright's OIT and Conquest Theory in influencing subsequent interpretative trends of the southern Ontario Late Woodland, and the biases conventional Late Woodland studies in southern Ontario have tended to leave unchallenged.

A `Pre-History" of the Ontario Iroquois Tradition
The study of Ontario's archaeological record has itself a long history, extending back over 100 years (Smith 1990). Much of this work has been concerned with the archaeological record of the last 1,000 years, perceived to be the time during which historically known Iroquoian cultural groups first emerged and subsequently developed (Latta 1999; Trigger 1970). This concern with the emergence of specific tribal/ethnic origins has proved to be a common theme throughout much of the development of our understanding of Late Woodland archaeology in Ontario (Trigger 1970, 1999).

Work in the 1950s is notable for serving to frame much of the subsequent research of the Ontario Late Woodland. This includes Richard MacNeish's (1952) detailed seriations of ceramics based on the identification of culturally and temporally distinctive types from Late Woodland sites across the lower Great Lakes, and his proposal of an artifact based classification that linked historically known Iroquoian groups with archaeological manifestations. He also argued for the local or "in situ" origins of these groups, refuting earlier migration hypotheses (Latta 1999:18; Smith 1990). Emerson (1954), in developing a pre-contact chronology for Ontario, also incorporated concepts such as "tradition" and "horizon" into the discussion of the Ontario Late Woodland.

The Wright Stuff
The publication in 1966 of J.V. **Wright's** *Ontario **Iroquois** Tradition* represented the culmination of perceptual changes that had been emerging in earlier studies of the Ontario Late Woodland. Building on earlier terminology, Wright proposed a chronological/spatial classification for the Late Woodland of southern Ontario, as defined primarily by the relative frequencies of ceramic attributes appearing through time and space. This work was a water-shed for Ontario archaeology, in that all subsequent Late Woodland research within this historic Iroquoian homeland came to operate with-in the Wright classificatory framework, plugging new data into the appropriate box within the

tradition and describing ceramic features with reference to the attributes initially recognized by Wright as significant in determining cultural/temporal placement.

What Wright proposed was a Late Woodland Tradition starting around A.D. 1000 extending from the lower Great Lakes to Lake Nipissing (Figure 1), though he also included Late Woodland data from the Pic River site on the northeast shore of Lake Superior as a sort of pre-cursor to A.D. 1000 developments (Wright 1966:101). Also worth noting is that Late Woodland manifestations along the St. Lawrence River were excluded from his model, since he considered this to be an independent tradition (Wright 1990:498).

He used data from over 50 archaeological sites, some of which were represented by excavated assemblages and some from surface collections. All these samples were from reputed village locales. Wright laid out a classification within the OIT of temporally specific stages (Early, Middle Late), as well as spatially-defined branches within the Early and Late stages. The Early Ontario Iroquois stage was seen as a 300 year phenome-non, a time when people relied on corn agricul-ture, hunting and fishing. Wright also believed that by A.D. 1000 various historic Iroquoian patterns could be observed, including matrilineal-based longhouses and precursors to ossuary burials. He also felt that the general artifact pat-

terns observed matched closely with historically known patterns (Wright 1966:22), which to him meant that the "cultural core" of Ontario Iroquoian life had largely emerged at A.D. 1000, and subsequent developments were really refine-ments towards the historical specifics known for these cultural groups.

In the Early Ontario Iroquois stage, Wright perceived two separate branches. A western branch, labeled Glen Meyer, was defined as extending from Long Point in Lake Erie, to the southeast Lake Huron shore. This branch was defined from nine site collections, none of which had undergone significant excavation. Most of the material had been collected by Thomas Lee of the National Museum of Canada, who had originally identified these sites as being a Glen Meyer focus of the New York Owasco (Lee 1951, 1952, 1958). East of this region, Wright defined a geographic area containing the Pickering branch, proposed to extend along the entire north shore of Lake Ontario and north to Lake Nipissing. It was defined on the basis of seven sites, two of which were multi-component — East Sugar Island on Rice Lake, and the Frank Bay site on Nipissing. It is important to note here that at the time Wright was writing his dissertation, he was also being exposed to a mass of new data yielding fresh insights into Toronto area (a.k.a Pickering) Late Woodland archaeology, but had yet to be analyzed in a detailed, system-

Figure 1. *Map of southern Ontario depicting various geographic locales referred to in text. Note that the dashed line from the western head of Lake Ontario to the southern end of Georgian Bay reflects what Wright (0966:23) pro-posed was the western extent of the Pickering culture.*
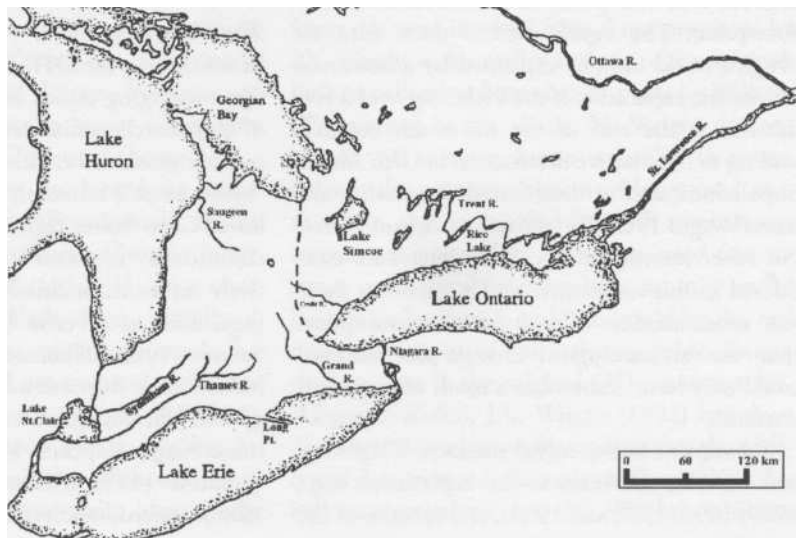
atic way. As Wright (1966:23) pointed out, the limited Glen Meyer material had been analyzed, but the Pickering data, while new, offered much more in the way of large artifact samples and extensive settlement data.

For Wright, the Middle stage of the OIT represented the results of a militaristic absorption of the western Glen Meyer branch by the eastern Pickering branch, thus creating a more homogenous archaeological stage across all of southern Ontario. He saw this because in the data he looked at, there appeared to be a continuous progression from Pickering to the succeeding Middle Ontario Iroquois (Uren substage) in eastern Ontario. He also saw the emergence of Uren sites in southwestern Ontario that apparently bore greater similarity in their ceramic assemblages to earlier Pickering sites, and had only minor affinities to Glen Meyer sites. The presence of Glen Meyer traits on Uren sites was due, he felt, to the captivity and subsequent adoption of Glen Meyer women by conquering Pickering people. Wright also argued that his interpretation was correct based on the recovery at Uren sites of what he felt were diagnostics of Pickering culture (e.g., gaming discs, deer phalange beads or "cup-and-pin" pieces). Thus the Uren site, clearly in the heart of Glen Meyer territory, but exhibiting closer similarities to various artifact traits found on the distant Pickering Bennett site in Hamilton than to the nearby Glen Meyer Gossens site, was proof of the direction of the absorption. The significance of these data, to Wright, could only be explained by a sustained militaristic expansion of the Pickering over a few decades at the end of the thirteenth century, leading to the massive destruction of Glen Meyer populations, and/or their forced dispersal to the west (Wright 1966; Wright and Anderson 1969). No other interpretative explanations were considered in this early work, and neither was there any substantiation offered for the assumption that the archaeological change documented could only have occurred as a result of organized warfare.

Subsequent to this initial research, Wright has reasserted his adherence to this explanation (e.g., 1987, 1990, 1992 and 1994), and remains of the

view that no substantiated alternative has been offered to explain the change he sees in the archaeological record. His most direct defense of the Conquest Theory (Wright 1992) argues that his interpretation can now be supported by a more comprehensive data set, though, in fact, it still relies heavily on the geographic location of Glen Meyer/Pickering/Uren sites, and the presence/absence of various artifact traits. Indeed, though the article claims to take into account 25 years of new archaeological data to substantiate his interpretation, the artifact analysis (Wright 1992:Tables 3-7) is largely a reiteration of the Gossens-Bennett-Uren site comparison provided earlier. Wright (1992:13) does provide a more explicit explanatory basis for the theory, arguing that Pickering groups developed a degree of inter-site social-political cohesiveness that allowed for the formation of a highly organized and effective military confederacy, all attributed to a rise in horticultural subsistence and concomitant shift in the role of women as food producers. Wright also offers possible reasons for this occurrence, including a male need for warfare to re-affirm prestige following the loss of their role as primary food providers, or crop failures in this more marginal part of southern Ontario forcing Pickering peoples to seek more reliable crop land and climate conditions. Wright also suggests a possible individual agency due to an exceptional leader or leaders.

The Wrong Response

In developing the OIT, Wright was able to build upon emerging trends and thinking at the time of his research, utilize newly uncovered data, and provide researchers with a taxonomic tool that "made sense" across the large expanse of the lower Great Lakes region. The longevity of this classificatory framework is demonstrated in relatively recent summaries of Ontario archaeology (e.g., Ellis and Ferris 1990 [ed.s]; Ferris and Spence 1995; Williamson and Robertson 1994). Nonetheless, disputes over particular terminology and chronological brackets have long been raised (e.g., Kapches 1981; Sutherland 1980; Timmins 1985; Williamson 1990; M. Wright 1986). Indeed, much effort has been expended at

debating and refining basic temporal start and end dates to the framework, a natural by-product of the massive amounts of data generated since its publication.'

Despite this broad acceptance of the classificatory framework, the explanatory interpretations of change at the end of the Early Ontario Iroquois stage of a militaristic conquest have never been embraced. Indeed, while Wright has argued that detractors have yet to prove him wrong on this topic (e.g., Wright 1992:3), it appears from a simple review of the literature that, in fact, his colleagues have felt that he never really proved himself *right*. For example, in her review of the Bennett site report, Marian White (1971:222) dismissed the notion of a conquest as questionable, raising three critical faults with the concept. First, there was no direct evidence of any kind that a militaristic conquest (e.g., burnt villages, mass graves of war dead, etc.) had been documented for this transition. Second, she pointed out that it is difficult enough to demonstrate claims of war and conquest in the archaeological record, even in cases where it has been known to have occurred, and certainly impossible to do so on the basis of artifact change alone (White 1971:223). Third, she felt the claim that the three sites used in Wright's comparative study were roughly contemporaneous — using 50 year intervals — was problematic,' and contributed to masking likely temporal variations, and evidence of local cultural continuity from Gossens to Uren (White 1971:223). These three criticisms raised by White (Evidence of Continuity; Problematic Data Analysis; Lack of Direct Evidence of Warfare) broadly categorize most subsequent objections raised to the Conquest Theory.

Evidence of continuity has long been noted for southwestern Ontario. For example, in his initial analysis of southern Ontario Late Woodland materials, Lee (1952:71) saw a clear continuity from the Early Late Woodland through to later periods, relying on much the same data Wright would use over a decade later to argue for discontinuity. Wright (1966:24) challenged Lee's observation by arguing that he had mistakenly assumed two Pickering sites (Boys and Barrie) were essentially identical in

material culture to the southwestern Ontario Glen Meyer materials. As such, Lee had failed to recognize what was, to Wright, a socio-cultural border, and thus he missed observing the sudden appearance of eastern Early Ontario Iroquois manifestations in southwestern Ontario after the conquest.

Nonetheless, subsequent researchers have implicitly supported Lee's views. For example, while Noble (1969, 1975a, 1975b) did concur with a ca. A.D. 1300 convergence of archaeological cultures, he also felt that clear continuity could be demonstrated in settlement-subsistence, burial programs and some material culture traits for southwestern Ontario between early and middle Late Woodland groups. Others have also supported local continuity and argue that there is little or no evidence of Pickering-like traits in local sequences in southwestern-most Ontario (Fox 1976). Subsequent detailed regional studies would further affirm local continuity between early and middle Late Woodland sites in southwestern Ontario (e.g., Kapches 1981; Pearce 1984[3];Williamson 1985), and even evidence of continuity on a single site (Williamson 1998 [ed.]).

Problematic use of data in the original construction of the OIT has been raised by several researchers to criticize the Conquest Theory specifically, and more broadly the OIT itself. Milt Wright (1986) found that the basic ceramic frequencies from his 1977 excavations at the Uren site were inverted from those presented by J.V. Wright in his earlier analysis of the original, limited collection from the site made by William Wintemberg in the 1920s. M. Wright demonstrated that this was due to variability in assemblage make-up across the site. He pointed out, therefore, that such variability will mean representative samples from these sites, used to construct presence/absence trait lists, may suffer from sampling error, and thus could be the real cause for purported cultural-geographic distinctiveness seen in the original OIT construct (M. Wright 1986:66). J.V. Wright (1992) has since dismissed this criticism by arguing that the additional data from this site only reaffirmed his original interpretations, but M. Wright's criticism

also echoes cautions others have raised regarding the adequacy of site sampling in making broad regional comparisons (e.g., Trigger 1981:10).

Recently, Lisa Rankin (2000a) has convincingly demonstrated that initial interpretations of another key site in Wright's original framework can also be questioned in light of re-analysis. The Nodwell site (Wright 1974), located by Lake Huron in south Bruce County, was seen originally to represent a Middle Iroquoian village of migrating conquerors at the end of the conquest, establishing a base in a frontier region. Rankin's work shows the Nodwell site to be a much more complex and long lived community. She argues the site was occupied for several centuries by members of a local and long established population, and shows only a small in-migration of a limited group of a more southerly based people into this larger indigenous community. Since this site was used by Wright in part to argue for the resulting homogenous material culture and settlement subsistence system that arose with the successful conquest of the Glen Meyer by the Pickering people, Rankin's work seriously undercuts that premise. Moreover, it reiterates M. Wright's concern that at least some of the data used to construct the OIT, and upon which explanatory interpretations of change were first proposed, might be on less solid footing than originally supposed.

The basic method of analysis Wright used for material culture from these large, long-term occupied village sites raises more general concerns, as well. The problems with Nodwell, for example, occurred in part because of the decision to analyze and summarize the material culture from the site as a single unit, as was the case for Uren and the other sites originally used to formulate the OIT. The issue here, of course, is that these locales were occupied for decades or longer (Timmins 1997a; Warrick 1988), representing thousands of individual, mostly unintentional acts, of deposition. But rather than being analyzed as the palimpsest of human actions and reactions, the data from these village sites are presented as a single entity to be described, catalogued and summarized. This normative emphasis on general description is intended to help

determine the placement of the compiled findings into the regional cultural historical norms already developed on the basis of the excavation of other such sites. But this descriptive summary means that sites are often described a-historically, structures and material remains implicitly presented as all being in use at the same archaeological moment, or in two or three archaeological moments when obvious evidence of site expansions are encountered. This masks internal variation between individual houses and activity areas on the site, and even within one of these units (Jamieson 1989). Certainly detailed intra-site analyses have revealed much more complexity across a site and through the length of occupation at a given locale than a simple listing of material can ever convey (e.g., Howie-Langs 1998; Lennox et al. 1986; Timmins 1997a; Williamson 1998 [ed.]; M. Wright 1986).

A corollary to this criticism of normative description for village sites relates to the field excavation methodology adopted to facilitate this kind of site analysis, and which has become almost a standardized universal for Late Woodland village sites in Ontario. This consists of mechanically stripping off the topsoil and sacrificing the data in this upper layer of site deposition, and recording two dimensional settlement patterns and sampling midden deposits. As Peter Ramsden has pointed out (1996:106; cf. Fogt and Ramsden 1996), while this provides data on the end result of 20 or more years accumulated settlement, whole data sets conducive to depicting the very intra-site variability and diversity critics of Wright have identified as important, are still sacrificed. It is true that careful analysis of end of house middens and use-specific features like semi-subterranean sweat lodges can help compensate for the sacrifice of data. Nonetheless, when mechanical stripping becomes the standard, unreflective operating method for all such sites, the systematic elimination of entire dimensions of these occupations can mean archaeological research risks becoming repetitive and of limited utility when archaeologists subsequently start to ask new questions of the data.

Other problematic data analysis concerns related to the construction of the OIT frame-

work have been raised regarding chronological control of site placement, specifically whether or not several of the archaeological sites initially used by Wright were properly ascribed as either Pickering, Glen Meyer or Uren. White is not alone in questioning the cultural-temporal affiliation of the Bennett site as Pickering, or at least as "classic" Pickering given its temporal placement right at the transition from the early to middle Late Woodland (Bursey 1994, 1997; Dodd et al. 1990; Sutherland 1980; Williamson 1990). Also the dates ascribed to particular sites, and more broadly to the basic unfolding of cultural developments through the OIT (e.g., Fox 1980) have been questioned. For example, these researchers have suggested that the appearance of at least some of the distinctive traits found on Middle Ontario Iroquois stage sites may actually have originated first in southwestern Ontario, then spread eastwards during the early-middle Late Woodland transition (e.g., Timmins 1985; Trigger 1985; Williamson 1990; M. Wright 1986:66).

However, as Wright (1990, 1992) has argued, some of these chronological interpretations, particularly arising from Peter Timmins' (1985) work, are problematic and based on possibly flawed radiocarbon analyses. Moreover, as Susan Jamieson points out (personal communication 2000) tight chronological control remains difficult to achieve for such a brief period, which makes any temporal ordering circumspect, especially when researchers inconsistently use cultural-chronological labels (see also footnote 1). Jamieson suggests that, given the current data-base's skewed sampling that heavily favours southwestern Ontario site assemblages, any ascribing of a southwestern origin for a particular trait risks being subject to the same sampling biases for which Wright has been criticized.

Indeed, debating southwestern or southcentral Ontario as the origin for particular traits can miss considering the wider region of the Great Lakes and Northeast these cultures interacted within (Jamieson 1992, 1999). Adopting a wider geographic perspective to an understanding of the appearance and diffusion of various material culture traits tends to show that so-called hall-marks of the conquering peoples, such as the rise in horizontal decorative motifs and ribbed paddle vessel treatments, also arose within areas further to the west and south, and indeed through-out the Northeast between ca. A.D. 1200 and 1400 (e.g., Fitting 1965; Jamieson 1991, 1992; Murphy and Ferris 1990). This presumably was not due to Pickering peoples conquering the entire region, so obviously at some level the notion of conquering peoples as manifested in the appearance of particular ceramic traits is a misidentification of the diffusion of a larger stylistic trend.

The on-going debate and differing definition of the chronological brackets for each phase of the OIT is part of a more substantive concern that has been raised over the basic viability of applying generalized cultural chronological phases, or "boxes," over a wide geographic region. This issue has emerged as a central challenge to Wright's model, as extensive data, particularly from regionally-based studies, has come to light (Smith 1990:288). Essentially, the spatial and temporal boxing of OIT phases and stages tended to imply the existence within specific geographic regions of homogenous, well-integrated cultural units (a la an organized pan-Pickering military group). This emphasis on regional homogeneity arising from particular trait similarities is a common critique of the kind of normative culture history the OIT represents (e.g., Dobres 1998; Stahl 1993; Trigger 1989). Such broadly defined "norms" in archaeological patterning will mask considerable variability, in each region and time period, and offer little insight into how individuals and their specific communities behaved and evolved through time (cf. Jamieson 1989:308; Niemczycki 1986). Ramsden (1977), for example, argued that geographic homogeneity was a constraint to properly investigating the Late Woodland of southcentral Ontario, and stated that the archaeological manifestations in this region should be viewed as reflecting several distinct cultural-regional groups, each following a complex, multi-scalar ebb and flow of particular social, political and environmental circumstances. Such intensive regional studies thus allow the researcher to

define and explain variation in the archaeological record, rather than to explain it *away* by plugging the data into a larger, arbitrary, cultural chronological framework.

Ramsden's work, as well as that of Bruce Trigger (1976), and James Tuck (1971) and Mary Ann Niemczycki (1984) in New York State, contributed to the increase in regionally-specific perspectives being applied to the examination of Late Woodland developments (e.g., Chapdelaine 1993a; Kapches 1981; Pearce 1984; Rankin 2000a; Sutton 1996; Timmins 1997a; Warrick 1990; Williamson 1985). These studies, in turn, have tended to blur the neat boundaries between generic categories such as Glen Meyer and Pickering, showing as much or more variation between particular Late Woodland regional sequences than between Wright's broader Glen Meyer and Pickering phases.

Of importance along this line of investigation, Mike Spence (1994) provided a summary of all the known mortuary practices for early (Glen Meyer/Pickering) and early-middle (Uren) Late Woodland groups across the OIT region. Spence pointed out that there was a great deal of variability in burial practices, not just under the broad categories of Glen Meyer and Pickering, but also within local sequences for each of these broader regions. His point is straight-forward, yet critical: counter to Wright's claim to the contrary (1992:12), there is no convincing evidence of a uniform set of burial practices for either the Pickering or Glen Meyer "cultures" (Spence 1994:17). This would imply that these constructs do not represent singular, cohesive social organizations of the kind that could have operated at the collective level required to undertake something like a planned military conquest. This would also suggest the cultural historical boxes drawn around these two phases operate somewhat arbitrarily to the actual archaeological patterns emerging from southern Ontario research. In short, the absence of any kind of regional cohesiveness to mortuary programmes, an area of cultural behaviour Spence feels plays a major role in symbolizing and articulating social networks (cf. Ferris and Spence 1995:115; Williamson and Robertson 1994), challenges any basic cultural-

historical reality the OIT construct was assumed to have reflected.

Surprisingly, Wright (1994) dismissed out of hand Spence's comprehensive review and attempt to critically examine the Conquest Theory with data, despite his own previous complaint (Wright 1992) that no one was engaging him in a direct, data for data debate of the topic. By assertion of personal opinion, Wright (1994) accused Spence's study of data misinterpretation, of lacking a complete enough data set to provide a meaningful challenge, and then simply re-asserted his own views without meaningful consideration of the alternatives proposed. This is unfortunate, as it suggests attempts to continue to work within this construct cannot be disengaged from personal investment in particular ideas, and this ultimately will detract from the importance of the OIT's initial contribution to the direction of research in Ontario.

In terms of the lack of direct evidence of warfare that was of concern to White and other detractors of the Conquest Hypothesis, more than thirty years of additional investigation of sites in southern Ontario has done little to alter this situation; evidence for large scale conflict remains absent from the archaeological record. As well, other researchers have also picked up on White's initial doubts as to the legitimacy of claiming warfare as the vehicle for explaining archaeological change, at least as constituted by Wright. Trigger (1985) and Gary Warrick (19846; 1990), complementing Spence's (1994) later observations, have questioned the archaeological record for the Pickering phase as reflecting a social-political cohesiveness with the ability to organize and sustain such a complex military operation. Ramsden (1977, 1991) and Warrick (1984a) also point out that the kind of conflict envisioned by Wright is inconsistent with what is generally known about Iroquoian warfare, or even for comparable societies elsewhere.

Finally, an issue of the OTT not initially raised by White has been the unease of accepting the link between Wright's archaeologically defined branches as real social groups, and connecting them to historically defined ethnic units, along with the effect that uncritical application of the

direct historic approach has on analysis and interpretations (Smith 1990:288). Early on, Trigger (e.g., 1970:43) questioned the appropriateness of applying historically derived ethnic labels to archaeological data, warning of the confused and unsound research that could emerge as a result. Though this warning was largely Cassandra-like, his admonishment rightly predicted the pre-occupation of archaeologists with determining the historic ethnicity of pre-contact sites and artifact classes.

Ethnic labels have been bandied about to the point that archaeologists have predicted, on the presence or absence of a single prosaic artifact class, or even by the decorative treatment of a rim sherd, whether or not site inhabitants spoke Iroquoian or not, and even a specific dialect of Iroquoian. Indeed, it is interesting to note that, as an attempt to underscore the argument that Glen Meyer and Pickering represent distinct cultural groups, Wright may have hinted (1990:497), and certainly William Finlayson (1998) has baldly asserted that the Glen Meyer "people" were actually Algonquian-speakers. Finlayson offers no data to substantiate his opinion and fails to explain what exactly in the archaeological record distinguishes a Late Woodland archaeological site made by Algonquian-speaking peoples from one made by Iroquoian-speaking peoples. To me, Finlayson's assertion in particular seems less one based on hard evidence of a clear variation in the database, and more a quickly grasped brace to continue supporting the sagging viability of a Glen Meyer/Pickering duality.'

This typically normative approach of equating spatial variations of specific material traits, such as rim sherd decoration or point forms, as signifying actual, specific ethnic or cultural identities, has long been identified as problematic, especially for non-state level societies (e.g., Binford 1965; Clarke 1968; Hodder 1978, 1979; Shennan 1989; Stark 1999). This is a particularly problematic assumption given that anthropological research suggests ethnic identity is a largely self-defined and transitory referent that will stretch beyond material culture and linguistic borders (e.g., Banks 1996; Barth 1969; Stahl

1991; cf. Buchignani 1987; Jones 1997; Shennan 1989), and further suggests that the idea that culture itself as an immutable concept is one which is fraught with conceptual assumptions that reveal more about the investigator than the investigated (e.g., Clifford 1988). Nonetheless these are pitfalls of normative cultural historical assumptions that are continually ignored in constructions of the past.

Within ongoing OIT research, this largely unchallenged equation of material traits with ethnic identity is an assumption that has greatly flavoured ceramic analyses, specifically by encouraging a tendency to ascribe differential importance to particular ceramic attributes such as supposed ethnic signifiers in the decorative designs along vessel rims. Clearly there is temporal and spatial variation manifest in ceramic decoration, as Wright's OIT construction demonstrates. But the continuing assertion in many OIT ceramic analyses — that ceramic decoration was specifically intended by artisans to reflect a specific ethno-linguistic identity at some kind of large scale — has operated intuitively and simply has yet to be substantiated, whether or not the author's assertion is being made between completely separate cultural entities (e.g., Finlayson 1998; Stothers 1979) or between tribal communities of the same ethno-linguistic group (e.g., Latta 1987; Wright 1966).

What critics have pointed out is that various interesting dimensions of analysis are often ignored in favour of supposedly determining what language or dialect these artifacts spoke. These other areas of cultural behaviour range from style and artistic play of individual artisans, imitation of broad decorative trends, or formal and informal organization of family or community production and trade. As well, as materials operating within complex cultural processes, decorative elements and vessel forms and functions (separately from each other) can convey a host of culturally infused messages, intentionally or unintentionally read by any number of distinct audiences. And, given that pots are functional and are — presumably — relatively stationary, the limitations of reading pots as ethnic signifiers, if pots do not travel, if the decoration is

not visible except close up, or if the decoration is obscured by food spillage, makes ethnic marker arguments non-starters on simple practical grounds (e.g., Arnold 1999; Dietler and Herbich 1994, 1998; Gosselain 2000; Sackett 1990; Wobst 1977, 1999). Increasingly, Late Woodland researchers in the Northeast (e.g., Brumbach 1995; Chilton 1998; Engelbrecht 1999; Fox 1990b; Niemczycki 1988, 1995; Watts 1999) have also questioned these assumptions about ethnicity and ceramics.

The preoccupation with trying to understand how data fits into developmental boxes that are presumed to equate with historically specific ethnic groups has also led to archaeological interpretations that suggest archaeological manifestations "anticipated" historically documented patterns of behaviour that occurred hundreds of years later. This can encourage the use of a mostly "cut and paste" form of analogy — an uncritical application of historic observations used to explain particular material culture patterns of the ancient past (Stahl 1993; Wylie 1985). More significantly, the broad narrative underscoring these culture histories takes on a sort of unidirectional, inevitable quest through archaeological time — pre-contact antecedents struggling to eventually become the historically specific people we are supposed to know them for (Pluciennik 1999; Ramsden 1996). So for example, despite the obvious time and circumstantial specifics of the events, the devastating disease and warfare of the seventeenth century has led to the assumption that such devastating conflict was a common part of Iroquoian life in the distant past. In that light, it would then be a plausible stretch to think "Iroquoians" of over half a millennium earlier in time would want to — and could mount — an "ethnic cleansing" of a neighbouring group. With such historically-derived ethnic blinders on, archaeologists forego opportunities to explore truly exciting and complex cultural pat-terns within and between sites by futilely, it seems, trying to bring everything forward to historical realities (cf. Jamieson 1989). As Ramsden (1996:105) suggests, we may better understand the pre-contact archaeology of the Late Woodland if we conceptually approached the

data as representing pre-industrial horticultural societies in southern Ontario, and put aside who we may or may not think their ancestors and descendants were.

Summary

Today, the remaining utility of the OIT rests primarily as a technical shorthand for local researchers — a terminology of temporal labels for various blocks of time within the Late Woodland — though the assignment of historically derived ethnic names (Neutral, Huron, etc.) to pre-contact archaeological manifestations continues to be problematic (Ferris 1999; Jamieson 1989; MacDonald and Williamson 1995; Ramsden 1977, 1996; Trigger 1970, 1999; von Gernet 1995; cf. Gibbon 1995 for a similar discussion about Oneota). But increasingly researchers are either calling for an abandonment of old paradigms and terminologies (e.g., Jamieson 1989; Williamson and Robertson 1994:39), have proposed revised taxonomic structures that remove ethnic-historical baggage of earlier constructs (e.g., Ferris and Spence 1995), or are seeking alternative ways of classifying Late Woodland groups and cultural systems (e.g., Timmins and Staeck 1999). As the utility of the OIT wanes, presumably at some point even the terminological remnants of the construct will disappear from archaeological lexicon.

The criticisms and objections raised against the OIT mirror in many ways the challenges to normative cultural history, and the limitations of this approach, made elsewhere (e.g., Binford 1989; Clarke 1968; Gardin 1989; Hodder 1986; Shennan 1989; Trigger 1989). In fairness, it is worth pointing out that many archaeologists critical of normative archaeology, be they "processual" or "postprocessual," have also been accused of actually re-adopting some of the same "crutches" of culture history, such as the uncritical embracing of the direct historic approach and a ready acceptance of the immutability of traditions (e.g., Feinman 1997:372-373; Stahl 1993:243). Indeed, acknowledging this has led some to propose integrating the strengths of multiple theoretical approaches, in order to achieve more robust, meaningful interpretations

(e.g., Duke 1995; Trigger 1991).

In particular, an emerging central theme in the re-theorizing of archaeology, as is argued throughout this paper, is the overt centering of history in archaeological interpretation, and specifically the historicizing of social agency and process in archaeological record (e.g., Barrett 2000; Cobb 1991; Dobres and Hoffman 1994; Hodder 1987, 1999, 2000; Last 1995). This integration, application and advancement on the thinking of Braudel, Bourdieu and Lemonnier, as seen in the works of Deitler and Herbich (1998), Dobres (2000), Gosselain (2000), Stark (1999), and an increasing host of others, is being heralded as an emerging new paradigm in re-conceptualizing archaeological interpretation (Pauketat 2001; cf. Shennan 1993): a paradigm that bridges and moves beyond specific theoretical camps, accounts for subjectivity while still demanding empirical research, and working with the same data that contributed to regional, cultural historical classifications, but without reliance on those normative blinders (e.g., Stark 1993; Wylie 1993).

So, it is important to realize that the development and use of the OIT was never a "had" path for Ontario archaeology to follow. It has served as a critically necessary step to allow subsequent thinking to emerge, and descriptive archaeology in general continues to be the initial way archaeologists come to know the archaeological record. But the OIT was a product of its time, and the discipline and the database have grown beyond, and thus undermined, the framework, just as the thinking of today will weaken as research progresses. Thus no one should be surprised that, despite its longevity and effectiveness, the construct would eventually no longer accommodate the burgeoning database, and the inherent biases and weaknesses incorporated in the approach would become obstacles to new paradigms. Current thinking simply reflects a moving on so as to tell stories that require differing perspectives and differing data analyses.

Telling A Different *Story*

"We'll rest here awhile," Running Deer announced to his sons. He had spotted the cool shade of the maple by the small creek and wanted nothing so much as to sit under that tree and close his eyes and listen to the voices in the water. He knew if they pressed, they'd be home by nightfall, but he just couldn't resist stopping; it was such a fine day, after all.

He eased himself down under the tree, moving slowly from the stiffness in his back. His sons watched quietly, but with a smile on their faces. "Yes, yes," he said to them, "your father's an old man and has a body of an old woman. And since you are so much stronger than your old father, you can head to the berry patch we passed awhile back and prepare this old grandmother a feast!"

"Right away, *grandmother,"* Blue Rock, his eldest son, laughed, "and does *grandmother* want an extra skin for her nap?!"

Running Deer just smiled and shook his head, and the boys — men, really — were off.

In the quiet that arose after they departed, Running Deer thought again that the aches in his back and feet were indeed making him feel his age, and these long journeys were not something he'd be able to do for much longer. But though he enjoyed these trips, he probably wouldn't get much of an opportunity to miss them, what with the demands of the community increasingly being made of him. He took pride in the way he'd been recognized by those around him in recent years as someone with a good mind and fair judgement, leading inevitably, it now seems, to the council asking him to join them. He liked adding his voice and being involved in the affairs of the community, influencing those decisions that would end up affecting everyone. Not bad for someone who had come from away just over 20 years ago.

He recalled back to when he was a young man living in the old place with his parents and family and that fateful day which seemed, now, to have been pre-ordained. If he had went off with his brothers, he never would have met Half Moon, his future wife, and her family, when he had stumbled across their camp while out hunting by himself. He had known the father and one of her uncles from before, as they often came to the old village to talk and trade, and they recog-

nized him, too. So they welcomed him and invited him to stay with them for a few days and help build their weir in the river, and to feast afterwards of the catch. The stay stretched to a whole month, and it only took those few weeks for Running Deer to know he felt as comfortable with these people as he did his own family, and his future mother-in-law made it clear that Half Moon would be crazy if she didn't like Running Deer.

He laughed, recalling how both he and Half Moon had blushed when she had said that, since they had taken every chance they could in those early days to meet and talk about everything and anything, and were already feeling things that have never gone away since. So when he had accompanied them back to their village, which had just been built, he was very impressed and started even then to think of joining them. It didn't hurt, either, when he saw that his mother-in-law's lodge was so big, and she pointed out that it was an empty place that needed more people and especially more children, "which your present and *future* family could provide," she had said with a wink and push on his shoulder. Though it took several months from when his mother-in-law had joked with him, it seemed but an instant from then to when he went back to the old place and told his family he wanted to marry Half Moon, talked them into coming with him to the new place (actually his mother-in-law had sold the idea to his mother, mostly), then married Half Moon and moved. He had made the new town sound so good that, in all, four other families from the old place petitioned to join not long after his family had moved in. It was easier since all were of the Bear, or were willing to be of the Bear, which his mother-in-law's family was, too. Of course, back then most people in the area were of the Bear or of the Wolf, but now there were no less than four clans in the village.

Not long ago he had revisited the old place where he had become a man while out hunting with Blue Rock. It was just an open space now, with a few poles standing here and there that were too rotted to salvage and too heavy to carry away for firewood. He explained to Blue Rock about his ancestors and how this had been their place to hunt along the river going back to grandfather's grandfather, and even further back, and would always remain so. He also pointed out where the ancestors rested, so that Blue Rock could stay away from there. Blue Rock asked what had happened to the others from the old place who hadn't join everyone else at the new town, but Running Deer didn't really know the answer to that question, and had wondered, too, where those families had gone. He had heard indirectly that a lodge of Wolf families had gone north quite a long way, not wanting to spend so much time worrying about crops and more time hunting and fishing as the ancestors had done. But there were many stories like these around, and not all were true.

Running Deer was drawn out of his thoughts by the sounds of his sons returning with a skin full of berries. Of course they were throwing berries at each other and were a mess, but there was still plenty for a good early lunch. As they ate, Blue Rock talked excitedly about their recent trip, and all that they had seen. The people of the villages by Thunder Falls and south of the lake were all talking about the need to protect their territories from encroachment, and the need to be stronger, and some were talking about developing bigger, shared territories with adjacent groups so *they* could become stronger, as is the way far to the south. Though Blue Rock had obviously been impressed, Running Deer couldn't see the need for it. As long as a village was strong and could attract people to join them, people would respect their authority. Entering into some kind of alliance with others would be complicated; just how would you figure out how to make decisions? And what if you disagreed? No, Blue Rock may think this is the way to go, but he couldn't see it himself. And besides, what do these people to the south know? They were good people to trade with, had many interesting things from far away, and were interested in the toolstone, skins and meat Running Deer and his sons had brought with them, but he and his community lived far away, so the issues to the south couldn't possibly touch them.

He wasn't too worried about these new ideas,

after all there were always new ideas to contemplate. Some had to do with improving hunting or planting, some had to do with toolstone or even making clay pots — after all, he was just old enough to remember when the idea of those thickened pot tops with the flat markings were all the rage everywhere he went. And people often told stories of how things are different else-where, either because they had seen it them-selves, or because others had told them. Sometimes these ideas helped make things work better. Certainly being of Bear, Wolf or Turtle had become much more important in the last while, and hadn't this, too, been an idea at some point? People talked, that was simply the way things were. What were the good ideas people kept and what were the bad ideas people simply tossed away. He just wondered if Blue Rock could recognize the difference.

"Ho, Blue Rock, you may want to keep some room in that head of yours for more than just tales of far away. When we get back there is plenty to deal with."

"What of, father," Blue Rock asked.

"Well, it may soon be time to think about moving the town, you know. Your mother says it's getting harder and harder to keep the fires going, and I'm tired of always being told to repair this wall or that post of the lodge, and the smell from the back gets so bad on hot days I can hardly hear myself think!

"Anyway, moving is always so difficult, there is much to do, like looking after the ancestors and making sure everyone will remain with the community and come along to the new town, so the council and I will be quite busy. I was wondering about who would go and scout out the places that would be good for the new location, and I thought of suggesting you and your brother for the job."

Blue Rock looked at his brother with a big smile on his face. That his father would suggest him for a task that surely others would have to do was nonetheless a strong vote of confidence, and made Blue Rock swell with pride at being considered important enough.

"And that's not all," Running Deer said. "You know those people from the east who hunted on our land last winter. Well the council's idea has been to go to their town and tell them to stop this. But I know you and your friends would rather wait till winter, and if they come back, attack the party and fight. This is serious business you want, but it would make us stronger, and maybe these days that would be a good thing given what we heard talk of on this trip. So maybe you and your friends should talk to the council and see if you can convince them. After all, since your father is becoming a grandmother in body if not in spirit, I need strong men to act, men who someday will be the elders of the community themselves."

Running Deer had said much to Blue Rock, and he was pleased to see that Blue Rock had understood much. He had basically told Blue Rock that he was no longer a child, but a full member of the Town and of the Bear. And with men like Blue Rock participating in the new town and building prestige for the community, Running Deer had no doubt their people had a bright and proud future.

Seeing Blue Rock deep in thought, digesting what his father had said, Running Deer felt pleased with himself, and stretched out under the tree, announcing, "you know, home will still be there tomorrow, so this old woman is going to close his eyes and take a nap while you two catch him his dinner in that creek. Then I'll spend the evening telling you tales my grandmother used to tell me long ago that will make you sleep like dogs!"

Yes, it was indeed a fine day.

Or so *this* story goes....

It is perhaps too easy in discussing the Late Woodland archaeology of southern Ontario to find myself talking more about the archaeologists who investigate that past, than to try and tell a story about that past. After all, this is well-traveled ground and individuals and their ideas often tend to define "social strata" within the broader Ontario archaeological community. These are the people I first met, who gave me an entrée into archaeology, and they were the "the" people

I was in awe of in my early days ("Look over there, that's *the* Jim Wright or *the* Bill Noble or *the* Bill Fox"). Some of these people would become mentors, and some would become the villains of various tales told of the "great *men* of Ontario Archaeology." In many ways this closed, insular community has perhaps been one of the problems with Late Woodland Ontario archaeology. We talk to each other, we publish to each other, and repeat ourselves over and over, pursuing answers to questions, or support for personal suppositions, that can make Late Woodland archaeology little more than local history and of little interest to anyone besides ourselves (Ramsden 1996:105). Perhaps this is why I've personally tended to shy away from anything to do with Iroquoian archaeology – no small feat in a community so dominated by this part of the archaeological record.

If Ontario Late Woodland archaeology has indeed been relegated to the local history heap, it would be unfortunate, as the stories of these 1,000 years or more of cultural developments are rich, varied, and allow interesting questions of human behaviour to be posed. In particular, the events of the twelfth to fourteenth centuries do appear to have been a time of significant change – albeit more the continuation of centuries of cultural behaviour than arising from a single event – and thus offer the archaeologist, in Ontario or elsewhere, much to explore. It is perhaps the case previously that, with the Conquest Theory on the table, research overly focussed on the debate and the baggage of this one interpretation, pushing aside alternative views of cultural development for the period. But the last decade has seen a number of researchers move away from trying to accommodate or refute the Conquest Theory, and they have begun to ask different questions of the Late Woodland archaeological record. What I would like to do in the remainder of this paper, then, is to tell a story that arises from this other work, and piece together the story of Ontario Late Woodland archaeological development through A.D. 1300.

Before proceeding, however, I need to decide here how to reference the events, periods and cultural remains discussed below. While I have been

using many of Wright's labels in my discussion so far, for the remainder of this discussion I need to move away from them, acknowledging their problematic baggage. Following Trigger (e.g., 1970, 1999) and others, I would like to abandon the use of ethno-linguistic labels. I also wish to perpetuate the notion that, spatially, there are separate Late Woodland Traditions in the lower Great Lakes Region. As well, I need to convey to you that when I reference the Late Woodland materials previously associated with the OIT, I am largely talking about the archaeological record for south central Ontario. Lastly, I want to be able to easily refer to large chunks of time to differentiate, broadly, chronological changes discussed.

Now you might think I would have a clear position on this and thus find it a simple task to complete; but instead I remain perplexed, perhaps because I too am deeply indoctrinated into the dominant, traditional paradigm for Ontario archaeology. So let me offer the following revised terminology, which is still flawed, but is offered here only for the purpose of this exercise. To generally refer to the archaeological manifestations traditionally encompassed by the OIT, I will simply use the term "Inter-Lakes Tradition," or ILT, referring to the Late Woodland archaeological record broadly within the region between Lakes Erie, Ontario and Simcoe. I place no fixed boundary to this, and in fact I believe we need to think less of fixed borders or culturally discrete and exclusionary regions, and think more of wide transition zones in the lower Great Lakes for archaeological material culture. While I do think the record can be used to discern broadly separate traditions (as Murphy and Ferris [1990], Cunningham [1999], Riddell [1993, 1998], and Watts [1997] illustrate for the Western Basin Tradition of southwestern-most Ontario), I also recognize that people and communities likely straddled these zones, and certainly traveled, traded, married and died between and beyond them. This generally limits our ability – archaeologically – to neatly separate out particular, bounded social units. Indeed, I wouldn't preclude the ILT zone from encompassing materials in what is now the United States, and I haven't a

clue about, nor would I want to try to sort out, any possible archaeological distinctions falling in the Trent-easterly Lake Ontario-St. Lawrence region, though I look forward to the day when someone presents substantial data of the Late Woodland from this region and sheds light on the subject.

Also, if for no other reason than a sense of obligation to be consistent, I will follow Ferris and Spence (1995) and utilize the chronological labels we adopted there. This includes the Middle Woodland (ca. 500-400 BC to A.D. 500-700), Transitional (ca. A.D. 500-700 to A.D. 900-1000), Late Woodland (ca. A.D. 900-1000 to A.D. 1400), and Terminal Woodland (ca. A.D. 1400-1700). And, to talk about broad trends in change through the ILT, I will adhere to the tripartite chronological designations we used then of Early (EILT, ca. A.D. 900-1300), Middle (MILT, ca. A.D. 1300-1400), and Late (LILT, ca. A.D. 1400-1700).

Needless to say none of these categories should be considered anything more than convenient labels for archaeological material patterns, and should not be construed as somehow capturing cultural units, and I will reiterate this throughout. Nonetheless, you may still feel obliged to groan and accuse me of putting a new dress on the same old mannequin, but I have found that, for me, eschewing all labels and simply referring to specific places and points in time to be "narratively" cumbersome. I need the crutch of some kind of terminological shorthand. I do not, though, advocate any widespread adoption of these labels beyond their use here as quick referents, and look forward to someone sharper than I resolving this dilemma.

Finding New Stories to Tell
An interesting thing about current thinking on the Late Woodland in Ontario is that, while Ramsden (1996:105) remains essentially correct that much of it has proven impervious to theoretical developments in archaeology, it is nonetheless true that some researchers, especially over the last decade, are bringing a more sophisticated level of analysis and interpretation to the archaeological data. Neither are they shy to turn

to theoretical literature to help conceptualize particular thinking within a broader context, pragmatically borrowing from various theoretical camps when the ideas can help inform interpretations (as discussed more generally by Wylie [e.g., 1989a, 1989b]). Lacking from this intellectual cherry picking or pragmatic eclecticism, though, is an overt theoretical paradigm, which has been argued to be a common trait of Canadian archaeology and an intellectually healthy position to follow (Kelley and Williamson 1996; Wright 1985).

While the intellectual "health" of such eclecticism could be debated, it is clear that, in southern Ontario, such research has increasingly focussed on intensive regional studies, multi-scalar levels of analysis, and long term historical perspectives of the archaeological record. This implicitly invokes the kind of Annales School conception of history and a long term perspective to interpretations. Likewise, again in an implicit way, contingency and agency have begun to be featured prominently in recent explanations of dynamic social change. In telling the stories that are emerging from this current research, the thinking of these researchers clearly has shifted from more traditional paradigms.

To understand the events that occurred in the thirteenth and fourteenth centuries it is necessary to understand the historical context within which they occurred (e.g., Jamieson 1992, 1999). In a sense, simply knowing what happened immediately before and immediately after is not enough. For example, a comparison of the Middle Inter-Lakes Tradition (or MILT) period with the Early Inter-Lakes Tradition (or EILT) period is to compare cultural trends that occurred over what is generally considered to have been a period of no more than 100 years (Dodd et al. 1990), with trends that occurred over 300-400 years (Williamson 1990). It makes no more sense, I would argue, to talk of these two blocks of time as representing single, momentary expressions of culture across a wide geographic area, than it does to talk of a habitation site occupied for decades as if all deposits were generated in a single day. For southern Ontario, the use of a historical, multi-scalar per-

spective allows us to see that the events of the Late Woodland did not occur in a vacuum, either regionally or chronologically, and it allows us to do what Ramsden wishes, to look at the pre-contact record removed from the filter of historically described ethnic units.

Notwithstanding this need to move past ethnic labels, it is also worth considering Wright's (1966:22) view that by the start of the EILT period all the telltale markers of an "Iroquoian-like" society (i.e., multiple family longhouses, palisaded communities, agricultural activity) were manifest. If this is indeed the case, it would seem that to best appreciate specific change between the thirteenth and fourteenth centuries it would help to have an understanding of how such an "Iroquoian-like" way of life came to be, at the site, regional, and broader level of the lower Great Lakes and Northeast. This too requires understanding the archaeology of the long term, looking much further back in time, and certainly beyond the first arrival of corn if it did indeed impact on social, political and economic organization, to properly contextualize Late Woodland continuities and changes.

Unfortunately, most areas of southern Ontario presently lack sufficient data to document detailed, continuous development for the time range I wish to review here (Rankin's [2000a, 2000b] work along the Saugeen River is a notable exception). So for present purposes what I will try to do instead is to look broadly at the wider "sea" of archaeological cultural patterns documented for the lower Great Lakes region for the period under consideration, and move down to regional social structures and site specific events as opportunity provides.

Beginning in the Middle

As a benchmark — albeit an arbitrary one I acknowledge was connected to histories still further back in the time — archaeological data across the lower Great Lakes does uniformly suggest that by the first century A.D. human occupations consistently were following regionally specific variants of a diversified hunting-gathering-fishing subsistence pattern (e.g., Cleland 1982;

Finlayson 1977; Spence et al. 1990). Evidence of domesticates or purposeful cultivation of maize has yet to be discovered for this period, and isotopic studies tend to support the idea that cultivated plants were not part of human diets (Katzenberg et al. 1995; Milner and Katzenberg 1999). Nonetheless, groups to the south and west of the lower Great Lakes had certainly been intensifying "incidental" cultivation activities of indigenous plants (e.g., Asch and Asch 1985; Brashler et al. 2000; O'Brien 1987; Rindos 1984; Simon 2000; Yarnell 1993), and it may be that some groups along the lower Great Lakes were also manipulating local plant species, perhaps including wild rice (e.g., Fecteau 1985; Spence et al. 1984).

There tends to be, at a broad level, a sharing of prosaic material culture on Middle Woodland sites across the lower Great Lakes, including clay vessels exhibiting a range of impressed and stamped decorative motifs extending over the exterior and interior of the vessel (Finlayson 1977). It had been argued that, as a result of a perceived broad homogeneity, Middle Woodland sites could be grouped into large, regional archaeological "cultures" (e.g., Saugeen, Point Peninsula), much as Wright had originally proposed for the Late Woodland. As more data has emerged, however, the notion of spatially broad, homogenous cultural groups has been questioned (Wilson 1990, 1991). As Spence points out, given the degree of interaction, intermarriage and seasonal mobility expected of these hunter-gatherer groups, regionally-based research has revealed much more of an archaeological continuum than fixed cultural boundaries. What is emerging is a picture of local groups who varied slightly from their closest neighbours, but increasingly so from people further away (Ferris and Spence 1995:98; Spence 1986; Spence et al. 1990).

Regional settlement-subsistence patterns, based on archaeological constructions, are also quite varied. Finlayson (1977) sees Middle Woodland groups in the Saugeen area existing as a series of bands, each occupying a major drainage and lakeshore. The proposed seasonal round was spring gatherings at rapids to harvest

fish, then dispersal up and down the lakeshore over the summer to exploit a range of edge resources, followed by inland occupation over the winter. Elsewhere, in the Rice Lake region of eastern Ontario, it has been suggested that macro bands occupied key locales though much of the warm season before dispersing to individual, extended family groups in the winter (Spence 1986; Spence et al. 1984). Two differing models have been proposed for adjacent sections of the Thames River near London. Timmins (1989) sees evidence of warm weather macro band occupations along the river, followed by large cold season occupations at inland pond areas, with irregular forays by portions of the community to other key resource harvest locales. Wilson (1990, 1991, 1994) feels, particularly at the Boersma site located on extensive river flats some 20-30 km west of Timmins' study area, that he has documented evidence of a macro band settlement occupied for most of the year, including the winter. Sites like Boersma are believed to have served as base camps, with other areas visited by task groups whose brief, repeated use of locales over time account for the large size of some interior sites. Given the diversity of environmental settings for these groups across southern Ontario, it should be expected that there would be a high degree of variability in adaptive strategies to local settings and circumstances, as is seen for forager groups elsewhere (e.g., Chatters 1987; O'Brien 1987; Price and Brown 1985 [ed.s]), or even that adaptive variability would exist from year to year for a specific forager group (e.g., Jochim 1991). Indeed, all of the scenarios described here, based on observations of local patterns, may be correct for those particular regions of southern Ontario, or for given years in those regions.

Whether due to repeated use, longer period of occupation, larger populations, individual and group strategies to manage social organization and subsistence, or some combination of factors (Ferris and Spence 1995:99), the increase in size and number of known Middle Woodland sites in these areas is seen as evidence of increased sedentism compared to earlier periods. While further research obviously needs to confirm that this pattern is fact and not simply an artifact of the exist-

ing database, Middle Woodland sites like Boersma do strongly point to some form of increased sedentism. This pattern is seen as indicating both a greater territorial cohesion and a constriction in the size of an individual group territory through the Middle Woodland (Spence et al. 1984; Trigger 1985), though these concepts clearly pre-date the Middle Woodland (e.g., Ellis et al. 1990; Spence et al. 1990; Williamson and MacDonald 1997 [ed.s]). As sedentism and territorial definition increases through the Middle Woodland, this is seen as leading to a "filling in" or "packing" of band territories along major river drainages and lakeshores (Spence 1986:92).

Though it has been suggested that this packing was a consequence of a population increase during the latter part of the Middle Woodland, Warrick (1990:329-330) is not certain that populations actually did increase, or if increased sedentism and more restricted territorial size for a band simply led to more intensive use of the same sites, leading to their increased visibility in the archaeological record. He estimates band size to be about 450 people throughout the Middle Woodland (Warrick 1990:329), relying on Wobst's (1974) estimate of the minimum number required of a supporting population to sustain a forager community. This is consistent with the estimates of others (Spence et al. 1984:128), but seems high compared to historically recorded population figures for southern Ontario hunter-gatherer bands, which range between 150-300 (Ferris 1989). Notwithstanding the limited utility of the historic pattern, it is worth pointing out that there is a difference in terminology here between "band" and "supporting population," in that several bands with exogenous marriage pat-terns and flexible membership mobility between adjacent bands could comprise a single "population" in Wobst's sense of genetic sustainability. Certainly this was the case for the southwest Ojibwa in the early nineteenth century. So it may simply be that in the later Middle Woodland there *was* population increase, but territorial packing may also be indicating an increase in the number of the smaller-sized *band* populations emerging across the region. Thus adequate population levels would have been achieved through

the collectivity of these smaller, neighbouring territorial bands; genetic viability would have been sustained through intermarriage and perhaps family mobility between neighbouring territorial bands.

This certainly implies that sustained, friendly, inter-group social relations would have been established during this period of solidifying territorial boundaries (Trigger 1985:76). Trigger also suggests that trade helped maintain friendly inter-group relations. There is plenty of evidence of Middle Woodland groups participating peripherally within large scale Hopwellian trade networks up to about A.D. 250 and certainly, at a more regional scale, in exchange networks with groups to the south and west (Jamieson 1999; Spence and Fox 1986; Spence et al. 1990). Of course, social exchange extends well back prior to the Middle Woodland, and served as an important vehicle for passing on innovative ideas and social strategies that influenced changing subsistence and community organization, and about defining cultural and political inter-connectedness (Jamieson 1992, 1999; Nassaney and Sassaman 1995; Schortman 1989).

The critical point here is that the rise in sedentism and defined territorial boundaries for small bands appears through the Middle Woodland, at varying rates and to varying degrees, across southern Ontario. Specific groups at the local level, in responding to contingencies as they arose, did so based on previous experience and accrued local and regionally shared knowledge. In turn, this all becomes the historically-based knowledge people will rely on to inform actions taken subsequently, and thus are the precursors to the emergence of a more "Iroquoian-like" way of life.

The Princess and the Kernel
As is wont to be the case, understanding events subsequent to the Middle Woodland is complicated, obscured by limited data and variable patterns from region to region. Nonetheless, in some areas of southern Ontario, patterns of continuity are seen. Rankin (2000a) can demonstrate continuity of Middle Woodland groups into the succeeding Late Woodland after A.D.

1000 along the Saugeen River, as well as the maintenance of established settlement-subsistence practices through that time. Spence sees continuity from the Rice Lake Middle Woodland through to local early Late Woodland groups (Spence 1986; Spence et al. 1984; personal communication 2000). And, at a general level, Molto's (1983) osteological work provides evidence of biological continuity across southern Ontario from the Middle Woodland into the succeeding Late Woodland.

Smith's (1997) recent summary of calibrated radiocarbon data would seem to suggest that Middle Woodland manifestations in southern Ontario disappear somewhere around A.D. 800, and early Late Woodland manifestations appear to start around A.D. 900. Of course, the pattern is more complicated than those two observations at first suggest. Certainly the vast number and consistent results of radiocarbon dates for what I refer to here as the Early Inter-Lakes Tradition suggests a general post A.D. 900 start.' However, the most recent Middle Woodland dates (i.e., A.D. 600-800) are few, and come mostly from sites in eastern Ontario, the Thames River drainage around London, and the Saugeen River in Bruce County. In at least a part of the inter-lakes area (the western end of Lake Ontario and Humber River drainage, the Grand River drainage and the north shore of Lake Erie from the Niagara Peninsula as far west as Long Point), there appears an archaeological manifestation that overlaps, chronologically, with the late Middle Woodland materials found elsewhere in the province. This archaeological manifestation has been labeled the Princess Point complex (Fox 1990a; Stothers 1977), and has been referred to as the beginning of the Late Woodland (e.g., Smith 1997; Stothers 1977), or as a part of a distinct Transitional period between the Middle and Late Woodland (e.g., Ferris and Spence 1995; Fox 1982a, 1990a; Spence and Pihl 1984). Smith's radiocarbon data would suggest Princess Point dates between A.D. 500 and 900, and perhaps as late as the early eleventh century based on recent findings (Pihl and Williamson 1999:104-105). Also, this Princess Point manifestation traditionally has been thought to be associated with

the earliest appearance of maize in Ontario archaeological deposits, the earliest finds of which so far date to around A.D. 500 (Crawford et al. 1997a).

The Princess Point complex is known for a distinctive ceramic style characterized by collarless, everted rims and conical bases. Vessels were manufactured by modeling clay, bodies were heavily cord marked presumably as a result of this method, and necks and rims were usually decorated with impressions made by a cord wrapped stick stylus, and often a row of circular punctuates (Fox 1990a:175). Numerous sites on the Grand River drainage have yielded this distinctive pottery. More limited frequencies of cord wrapped stick decoration are found on early Late Woodland sites along this drainage and else-where in southern Ontario (Smith and Crawford 1995:67; Williamson 1990). As well, one of the late Princess Point sites, Porteous, was found to have small, longhouse-like structures and a palisade (Noble and Kenyon 1972; Stothers 1976). As such, it has long been argued (Fox 1982a; 1990a; Smith and Crawford 1995; Stothers 1977) that the Princess Point manifestations are where one finds the material origins of Ontario's historic Iroquoian peoples, and certainly Wright (e.g., 1984) has accepted the addition of Princess Point to his model.

Unfortunately, while recent work has done much to advance our understanding of the Princess Point archaeological record (e.g., Crawford and Smith 1996; Crawford et al. 1997a, 1997b; Pihl 1999 [ed.]; Smith and Crawford 1995, 1997), overall we still know very little. For example, there is only a generalized sense of material culture or settlement change through this period, with some notion of what early (e.g., Stothers 1977) and late (e.g., Noble and Kenyon 1972; Pihl 1999 [ed.]) patterns may be. Surface collected and test excavated sites may help clarify matters (e.g., Crawford et al. 1997b; Smith and Crawford 1995, 1997; Walker et al. 1997), but detailed data is still pending. As well, the general lack of data for the earlier Middle Woodland period in the region, despite local research indicating large numbers of Middle Woodland sites all along the Grand River (W.

Fox, R. Williamson, personal communication 2000), makes it difficult to understand the relationship of Princess Point manifestations to local (overlapping/earlier?) Middle Woodland materials.[6]

From what has been documented, two interpretations have emerged regarding the settlement-subsistence pattern Princess Point community(ies) followed along the lower Grand River. Large sites located along the river and on floodplains have been interpreted to be warm weather macro band settlements, situated so as to take advantage of an abundance of available resources at these locales (Stothers 1977). Smaller sites in upland locales were presumed to be the result of cold weather dispersal. Stothers also sees the appearance of corn at these sites as something that was essentially grafted on to a pre-existing hunting-gathering-fishing way of life. Later work on Long Point discovered evidence of a Princess Point occupation that was clearly situated for the specific purpose of harvesting fish and other lakeshore resources (MacDonald 1986).

Smith and Crawford (1995:65; cf. Crawford et al. 19976) don't disagree with Stothers' description of Princess Point subsistence, but have argued that the big, riverine Princess Point sites were occupied year round, and served as base camps for a macro band or community that did not formally disperse, although smaller groups would have gone to upland sites for specific resource harvesting purposes. Smith and Crawford refer to these base camps as a "pre-village" form of settlement, and see this as the emergence over the 400 years or so of the Princess Point of a less Middle Woodland-like use of the landscape, anticipating later, formal Late Woodland village-centric concepts of territoriality.

Of course, given currently proposed interpretations of the Middle Woodland "packing" of drainages, long term use of' fixed locales and a more restricted territoriality, Crawford and Smith's distinction between Princess Point and Middle Woodland is subtle rather than severe. So it is worth considering that the settlement-subsistence pattern they propose, minus the presence of corn, is what Wilson (1990) sees as the

pattern of Middle Woodland settlement-subsis-
tence for the community he examined on the
Thames River, particularly as seen at the strati-
fied Boersma site, with radiocarbon dates stretch-
ing from the fifth century to eighth century A.D.
(Smith 1997:44). In effect, regardless of the
Stothers or Smith and Crawford interpretations,
both are essentially arguing for corn to have been
incorporated into a Middle Woodland-like way of
life that existed in southern Ontario around A.D.
500 — a way of life largely maintained by
Princess Point groups, like Middle Woodland
groups elsewhere in the province, until A.D. 800
or 900. This would suggest that the Late
Woodland, "Iroquoian-like" pattern of settle-
ment-subsistence essentially emerges with the
evidence of increased sedentism and resulting
"packing" manifest during the Middle Woodland
period (cf. Trigger 1976); i.e., a social phenome-
non that developed prior to the formal incorpo-
ration of maize horticulture into local ways of
life. This is consistent with the Middle-Late
Woodland transitions seen elsewhere in the
Northeast and Midwest (e.g., Brashler et al.
2000; Ceci 1990; Wymer 1993).

Also implicit in these models is that, with the
exception of a dozen or two corn fragments from
their refuse deposits, the only real distinction
between Princess Point archaeological manifesta-
tions, say prior to the ninth or tenth century A.D.,
and other Middle Woodland manifestations of the
same time would be the ceramics, since
diagnostic lithics are generally similar (i.e.,
notched Port Maitland Points and triangular
Levanna-like points; Spence et al. 1990; Fox
1990a). There may also be some variation in site
selection preferences between Middle Woodland
and Princess Point settlement patterns (i.e., the
lack of inland occupations proposed by Smith
and Crawford [1995, 1997]). However this dis-
tinction seems to be one that is more an artifact
of the limitations of archaeological research than it
is of any real cultural variability at this time.
While currently existing only within ARM "grey"
literature, inland Princess Point locations do exist
and have been investigated within the Grand
River drainage, contra to Smith and Crawford,
(e.g., MacDonald 1990; Timmins 1992, 1993),

and now also on the Thames River drainage (Jim
Wilson, personal communication 2001). These
are typically smaller encampments, and as such,
may reflect why they have not been as visible as
the floodplain macro band camps. For example,
a Princess Point site found by surface collecting,
which lacks any diagnostic rims or much in the
way of large ceramic fragments in the artifact
sample collected, could well appear to be a
Middle Woodland site by the investigating
archaeologist, and thus be identified as such. So,
smaller campsites where diagnostic rims may not
be plentiful, such as interior camp locations, may
not be commonly identified as Princess Point.
Thus their representation as an important part of
the fuller Princess Point settlement pattern may
be omitted simply because of contemporary field
identifications, rather than any intentional site
selection preferences back then.

Does That Slipper Fit This Princess?
The appearance of the distinctive Princess Point
pottery; the fact that these vessels appear to be
made by modeling, whereas Middle Woodland
ceramics are generally thought to be made by
coiling; the limited appearance of corn; and the
seeming temporal overlap of Princess Point with
the continuation of Middle Woodland manifes-
tations in other parts of southern Ontario, have
all been pointed to as evidence that Princess
Point represents the appearance of an intrusive
cultural group. Indeed, it has been argued by
Snow most strongly (1992, 1995, 1996; cf.
Bursey 1995) that these archaeological data rep-
resent the northward migration of Iroquoian
speaking peoples into southern Ontario and New
York from Pennsylvania and points south. Some
historical linguists such as Fiedel (e.g., 1999)
have also argued for a migration on the basis of
linguistic evidence, and their work is often cited
as support for such archaeological interpreta-
tions. I find it difficult to look at the discussion
of the mass migration or non-migration of
Iroquoian-speaking peoples, particularly as asso-
ciated with the emergence of Princess Point,
other than askance. There are big holes in basic
data sets required to shed light on the subject,
and as Starna and Funk (1994) and Engelbrecht

(1999) have argued, the "either/or" created by this proposition reduces incredibly complex and variable cultural behaviour into what is seemingly little more than a debate of basic presence/absence trait lists.

But decrying the futility of the debate does not make it go away. For some, these "circumstantial" strands of evidence seem enticing support of a migration scenario. But I find that the pat nature of these interpretations makes me suspect, and I do doubt the strength of the migration argument as an explanation for change in the archaeological record at this time. I should caution that I am not rejecting the possibility of migrations having occurred in the past, of course, and believe there is evidence in the Late Woodland for migrations in southern Ontario (discussed below). But my read of the relevant literature (e.g., Anthony 1990, 1992, 1997; Burmeister 2000; cf. Sutton 1995, 1999) suggests the proposed long distance, displacing advancement of an entirely differing ethno-linguistic group into an already occupied region is extremely rare in non-state level societies, and archaeologically would be much more visible than the sparse record brought to bear here.

Beyond simple questions like why and what (i.e., why would this group have moved; why would they have confined themselves to this peninsular region for half a millennium; why did they continue on with a Middle Woodland way of life; what happened to the extensive pre-existing Middle Woodland population of the region?), there are, I believe, some data that challenge an interpretation of Princess Point representing evidence of a displacing population into southern Ontario. First, with corn now having been documented as early as A.D. 500 (Crawford et al. 1997a), and given that this will likely mean corn first actually appeared in the region somewhat earlier (Hart 1999), the first appearance in southern Ontario of corn is undeniably during Middle Woodland times. This would remove the unique association of corn with the first appearance of Princess Point ceramics. Also, with an early appearance, we are then looking at a very, very slow rate for the increased use of corn as a meaningful contributor to over-

all diet, say minimally 500 years (cf., Chapdelaine 1993b; Hart 2001). This hardly makes that first appearance the earth-shattering event the otherwise dramatic story of a migration of people and their corn appearing "suddenly" on the Grand River would suggest.

Also, in terms of the pottery, while cord wrapped stick ceramics are distinctive, it is not clear that this class of artifact actually appears "suddenly." Certainly some of Stothers' (1976, 1977) findings from the relatively early Grand Banks and Cayuga Bridge sites include plain cord marked or roughened rims with a row of punctuates and with or without some additional decorative treatment. While a detailed study of Princess Point ceramic change through time is needed, it does seem that some of the rims from these early sites represent Princess Point decorative expressions on Middle Woodland pots. Likewise, Robertson et al. (1997:501-503) report finding a vessel in a feature at the Peace Bridge site in Fort Erie on the Niagara River that exhibits a typical Middle Woodland form, but with a "later" style of surface treatments. The feature also contains other Princess Point ceramics and has been radiocarbon dated to the seventh century A.D. This could well argue for a transition in ceramic traditions, rather than a replacement.

As for arguments suggesting that since the Princess Point wares are manufactured differently than earlier wares they represent the migration of a distinct people into the region (e.g., Bursey 1995; Snow 1995), this seems to me to be facile. Certainly elsewhere this technological change in ceramics also occurs through the Middle-Late Woodland transition, and is often cited as a subjective means of making the distinction between the two periods (e.g., Fox 1990a; Murphy and Ferris 1990; Wilson 1990). Are we to assume all these areas experienced in-bound migrations at this time? If so, just when can technological change ever be seen as an internal phenomenon and not evidence of displacement?

Finally, Princess Point-like wares do appear across all of southern Ontario, and indeed ultimately across the Great Lakes (Fox 1990a:181-185). The dates for many of the sites yielding

these wares have been calibrated to the eighth century A.D. in southwestern Ontario, and to the late ninth or tenth centuries A.D. on the Bruce Peninsula or in eastern Ontario (Fox 1990a:180; Smith 1997). Rather than indicating further advancement of Princess Point peoples throughout southern Ontario, presumably this simply reflects a diffusion of a stylistic and technological innovation in pottery making in the centuries after it was adopted in the Niagara peninsula.

Telling Tales I — The Story Before the Story

If we can put to rest the idea of Princess Point manifestations representing some kind of Iroquoian migration, or at least put it aside until we have a more meaningful handle on the archaeological record between, say, A.D. 300-800 for the inter-lakes region of southern Ontario, what story do we then have? Well, as Smith's (1997) data suggests, what are essentially Middle Woodland groups, already packing in the Grand River and pursuing a diversified settle-ment-subsistence pattern from seasonally or year-round fixed locales, end up adopting a distinctive ceramic assemblage within a few centuries of also being introduced to corn. And with the beginning and ending of this manifestation overlapping with other Middle and Late Woodland materials in southern Ontario, then clearly this *is* part of a Transitional Woodland period. Simply, Princess Point starts out *as a* local Middle Woodland manifestation with an interest in a new pottery style and form, and with direct access to an exotic food. Over several centuries, this group appears to gradually change settlement-subsistence patterns, eventually exhibiting a pattern that would be typical of what we under-stand the early Late Woodland in this region to be. But why do these communities adopt this ceramic tradition to begin with, and why, by the tenth century, had their use of corn increased?

To explore these questions, we need to keep in mind that Transitional Woodland manifestations identified as Princess Point occur beyond just the Grand River. Sites with Princess Point ceramics are found within the inter-lake region of the

Niagara Peninsula, encompassing the drainages of the extreme west end of Lake Ontario and east end of Lake Erie to Long Point. I suspect that there may be some variability as to when distinc-tive ceramics first appear locally in this area (e.g., Redhill Creek in Hamilton, where Middle Woodland-like ceramics have been documented later at the HH site [Woodley 19961), but the point is that bands on the Grand River, in the Hamilton area, and along lakeshores and drainages of the Niagara peninsular region were likely inter-connected as a broad territorial group, with individual mobility being fairly fluid between bands, as had been the case extending at least well back into the early Middle Woodland.'

Now, it has been argued (Jamieson 1991:4, 1992:73, 1999:184) that the Niagara Peninsula has a long antiquity of serving as a key geo-graphic conduit by which trade and social exchange entered southern Ontario. To me, this suggests that local populations in this peninsular region were participating in a social interaction network unique for southern Ontario,' one that also encompassed parts of the lower shores of Lakes Erie and Ontario in New York and Pennsylvania. So let us assume that, into this interconnected territory, knowledge and experi-ence of a new form of ceramics and corn first appeared in Ontario, passed along by communi-ties to the south which were linked both to this peninsular group, and to adjacent regional groups extending further south and west. Also, if Middle Woodland peoples of the Niagara penin-sula had indeed been involved in a social interac-tion network dating back hundreds of years that oriented them south of the lower Great Lakes, rather than east and west across southern Ontario to the north of the lower Great Lakes, then perhaps this may explain why Princess Point-like pottery does not appear outside of the peninsular region at the same time. Essentially, this historical predisposition to communities elsewhere meant they did not come into frequent contact with peoples to the west beyond Long Point or north and east beyond the head of Lake Ontario, or did so only during instances of for-mal, infrequent contact (i.e., not informally, not often at domestic locales, and infrequently

through intermarriage).' This may suggest that groups to the west in southwestern-most Ontario (i.e., Lake Erie shore, Thames River and similar drainages) were involved in an interaction network that extended west around the end of Lake Erie and were not directly tapped into the early appearance of Princess Point ceramics. This, in turn, may also indicate that, contrary to previous suggestions (e.g., Fecteau 1985), this was not where corn first entered Ontario to be passively shipped eastward without accepted locally.[10] This would be partially the reason for why the appearance of corn on Western Basin Late Woodland sites happened several centuries later (Murphy and Ferris 1990).

This distinctive regional interaction zone may suggest why the Middle Woodland peoples of the Niagara peninsular region were the first in southern Ontario to be introduced to cord-wrapped stick decorated, modeled pottery and corn, but not why they would have adopted either. For this, let me offer a story of the Middle Woodland peoples who lived along the Grand River, a long, wide, generally slow moving watercourse (out-side of the snow runoff period), characterized by extensive river flats interrupted occasionally by areas of high bluffs. We know that larger Princess Point sites are found on these wide floodplains (Stothers 1977), and that earlier Middle Woodland sites are also known from these locales (e.g., Parker 1994), especially at Dunnville and at the mouth of the river by Lake Erie. This area of river flats, small islands and marsh edges is one portion of the Grand River that has been noted previously as having extensive wild rice stands," and archaeological deposits in this area of the Grand carpet either side of the river. I do not think it unreasonable to assume that wild rice, as well as the various game attracted to this area, would have been harvested by local Middle and Transitional Woodland groups, as suggested by McAndrews (1969) for the Great Lakes generally, and documented for the Middle Woodland elsewhere (Arzigian 2000; Rajnovich 1984). Certainly the plant had the potential to be beneficial. Yields from a wild rice stand could have been considerable: 100-300 pounds per acre using modern technologies (Aikens et al. 1988),

and 40-100 pounds (Lofstrom 1987:7) or 50-75 bushels (Arzigian 2000:246) per acre, using traditional harvesting techniques. So this plant likely was an important component of a diversified subsistence base, especially as surplus yields could be stored for the winter. Additionally, nut resources, notably acorn, hickory and walnut, would have been plentiful at various locales up and down and away from the river, and were certainly harvested by at least Princess Point groups (e.g., Monckton 1999:83; Smith and Crawford 1995:66). In short, from archaeological data it is clear that local peoples on the Grand River during the Transitional Woodland, and likely during the Middle Woodland, were harvesting a wide range of resources, including diversified species hunting with some emphasis on deer, fishing, and extensive gathering of plant species.

At some point before A.D. 500, some members of the communities along the Grand first learned of corn — I'll suggest that is was from people with whom they interacted further along the Niagara Peninsula or in New York. This could have occurred through chance encounters, or during direct forays to or across the Niagara River, or visits from these groups to the Grand. If the lower Grand, for instance, constituted the home territory of a single group (band?) that was exogenous, we can assume that, in a typical Middle Woodland pattern, interaction with their neighbours would have been frequent and informal. Marriage between groups, and perhaps even inter-group mobility, would have been common. As such, individual knowledge of events and changes in the materiel of societies connected further to the south would filter up and through this community. In other words, people on the lower Grand likely knew about this plant long before it appeared locally.

Between the first century A.D. and prior to A.D. 500, then, corn itself would have appeared in the region, most likely through incidental or formal exchange as a curiosity if not immediately as a foodstuff. Once it was in the region, though, there is no indication that anyone thought to heavily invest in growing the plant at first, and it likely did not really introduce any radical culinary changes to the family cooking

fires. Nutmeat certainly, and wild rice possibly, were likely already being boiled to extract food value in the form of mash and oil, so maybe corn, in the early days on the Grand, was little more than an exotic food to add to the pot, something akin to the American eel found at the Holmedale site: a food not available locally that provided something unusual and different tasting to a meal.

Perhaps within two to three centuries or so of corn finding its way onto the Grand (say around the seventh century A.D. in light of the Peace Bridge site findings), people were also becoming familiar with differing pottery decoration and manufacturing techniques. I'm assuming the ideas about these pots, or their decoration, came through trade, or by people who had learned to make pots in this manner coming to the Grand (by way of individual or family mobility, not migration) from groups living in New York and south of the lakes, again through local interregional social networks.

While recognizing the absence of large and comparative data, the minor evidence of some Middle Woodland form ceramics with apparent Princess Point-like decorative applications suggests that, like corn, the adoption of Princess Point vessels was a gradual process in the peninsular region; local artisans incorporating new decorative elements into existing vessel manufacture practices. Given the historically-based conservatism that can define ceramic traditions, especially in vessel form and construction (e.g., Gosselain 2000; Sassaman 1993), it seems entirely consistent with the diffusion of creative innovations among local artisans to first see its manifestation in the adoption of new styles or decorative applications. So while only an impressionistic observation at present, ceramic decoration and even form seem to imply continuity of local artisan traditions through this period.

At the time the new ceramics were becoming known locally, a function of the traditional, coil manufactured Middle Woodland vessels in the home was likely the processing and cooking of foodstuffs. Given the frequency and diversity of plants found on sites, and the possible harvesting of the rich wild rice stands on the Grand, pots

would have been the means to boil these items into a mash, or to make meat or fish soups or stews, using ground nutmeat as a thickener (e.g., Gardner 1997). It has been suggested before that the heavy, thickly coiled vessels of the Middle Woodland were appreciably deficient in these tasks when compared to the more durable and efficient, thinner vessels made by modeling (e.g., Braun 1983; Muller 1986:142). This makes me suggest that, either through use of pots traded in, or by watching someone new to the group make modeled pots, community potters would have come to know, over time, better efficiencies from this new style of vessel. That it seems Princess Point-like pottery appears suddenly in the archaeological record may simply reflect the relatively short time (say less than a century) within which women making and using pots were convinced that the thinner vessels were likely easier to make, held together better in firing, and/or helped make a better mash. Once convinced, it would not have been long till all potters within the communities interacting with one another in the peninsular region would have been familiar with, and tried their hand at, this new technology. Such a rapid, adaptive response is not unheard of in the archaeological record. Certainly iron trade axes, first introduced in the late sixteenth century, quickly supplanted stone equivalents once the obvious benefits of the new versions were realized (Lennox and Fitzgerald 1990:423). As well, given the eventual, universal spread of modeling as the manufacturing method of choice for potters across the Northeast, technological advancement does seem to have been an important factor in the adoption of thinner vessels.

Notwithstanding functional value, the fact that new decorative designs were also associated with this new breed of pot probably helped to make them even more of a curiosity with local potters, who may have soon tried out innovations in this decorative technique that would, in time, develop into local traditions of decorative expression. Indeed, this was likely a significant dimension in the transition to modeled pots. Heeding Loney's (2000) caution against the simple equation of technological change as

"improvement," thus negating non-functional or even anti-functional reasons for technological change (cf. Sassaman 1993), I certainly do not want to suggest that the acceptance of modeled pots was only ever about the improved efficiency of a particular task. After all, the production of prosaic material items such as pots is a complex process of social agency, in which historically defined constraints continually are shaped by, and shape, individual decisions regarding production (e.g., Crown 1998; Deitler and Heidrich 1998; Dobres and Hoffman 1994; Lemonner 1993). This is where need, function and raw material choices interact with personal choice and social order, tradition and innovation, and artistic convention and creativity. As well, pots play an important role in community foodways, specifically the preparation and presentation of food within a social context, and these factors can certainly influence design and use, ultimately leading to non-technological reasons for change and innovation (e.g., Johannessen 1993; Loney 2000; Mills 1998; Wright 1991).

By way of example, we can consider the observation that in the increasingly settled community of base camps, negotiation of both power and co-operation between families and between genders would have necessitated increased shared-participation in day-to-day life, including the efforts of preparing food. And, of course, in human culture the sharing of food and eating together is universally infused with social etiquette, taboos, and reciprocity (Johannessen 1993; Meadows 1997; Mills 1998; cf. Levi-Strauss 1978). Pots, their design and function, would play a part in the social presentation of cooked food in the community, especially if they were light enough to be easily moved from the cooking fire to eating areas. Likewise, the role of sharing prepared food probably played a part in the intricate rituals of interaction with neighbouring communities and individuals. If that pot also contained a meal prepared with an exotic food, such as corn, how even more sensational the event and greater the interest in the objects themselves — the pot, artisan, chef, and even wider family authority all acting as cultural seeds encouraging and passing along play and innova-

tion to the next family and community, to para-phrase Spence (1999).

Regardless of such imaginings of the possible complex factors that may have played a role in ceramic technological change, it did occur. Corn, meanwhile, also would have been adopted into the household culinary repertoire, but when and to what degree is difficult to measure. For example, it is impossible to really know what the simple presence of a few kernels of corn from a single archaeological site indicates: traded or locally grown food, in small quantities or great? I am dubious that the simple presence of corn on sites can be equated with a significant change to subsistence, but its appearance does suggest that it had been adapted to the temperate climate of the inter-lakes region by A.D. 500, so presumably could have been grown locally (Hart 1999). And Chilton (1999) and Hart (1999) are certainly more confident than I that the simple presence of a cultigen on a site, no matter how limited, is a reflection of formal, relatively high use. Regardless, I would simply emphasize the point that the quantified archaeological data documented so far suggests that corn never supplanted nutmeats, berries and probably wild rice as the main non-meat staple in local diets during this time, but rather complemented those other foodstuffs (e.g., note frequencies as reported by Monckton [1999:81-82] and Smith and Crawford [1995:66]).

Likewise, whether local groups also embraced at this time any ideological "context" corn may have operated within further to the south cannot really be answered. It seems unlikely to me, however, that people would have been any quicker to incorporate complex ideological concepts or wholesale changes to seasonal scheduling, calendric rituals and other like behaviour than they were of corn itself. Presumably any ideological importance linked to the growing and harvesting of corn likely grew gradually, along side the growing adoption of corn itself (but cf. Hart 1999).

In the absence of archaeological data suggesting corn appeared full blown in the Niagara peninsular region as a result of invading corn-centric peoples, and given a likely very slow rate

of its adoption over several centuries as an important contributor to diet, it would seem that the initial decision to plant corn locally was likely quite informal. But once done consistently, practical accommodations for seasonal scheduling of planting and harvest would need to be accounted for within the wider range of practices and activities carried out by individuals and groups. Significant, I think, would have been the realization that the place of planting necessitated repeated visits (planting, tending, harvesting), which would have had to be scheduled within more general seasonal mobility. Also, this place of planting likely needed to be at a locale where there would be some confidence that the corn would be protected from total destruction by animals or insects, and perhaps even recognized as personal property.

It seems to me the most appropriate place for that initial cultivation would have been near the base camps on the floodplains of the Grand, regardless of whether these places were warm weather or year round occupations. This would be the fixed locale that would be returned to repeatedly, and, by being next to the settlement, answers to the question of who planted (owned?) the plants would have been obvious and common knowledge. Likewise, the plants could be watched throughout the seasonal growth cycle, so the gardener would quickly learn how best to raise and tend the crop at that particular locale (cf. Hart 2001), and spot animal or insect dam-age. Of course planting near one's home would have blurred somewhat the distinction between camp as a purely residential and perhaps processing locale, and gathering/harvesting areas, which traditionally would have been places removed from the camp, a change in disposition that certainly, long term, would have contributed to the kind of village-centric re-orienting of settlement that Smith and Crawford (1995, 1997) see emerging through the Transitional Woodland.

If, as is generally assumed, the harvesting, processing and cooking of plant foods was a task performed by the women of a community, then the ownership of corn, responsibility for its care, and linking of corn with residential locales all would have been an initiative of the individual

women of the camp (Benn 1995; Hastorf 1991; Watson and Kennedy 1991). Presumably the mastery of this exotic plant and development of expert, local knowledge, and the incorporation of it into the food she prepared, likely carried with it some level of prestige within that community, or at least the household, for the individual. Certainly seeing those stalks growing near the camp, perhaps close to the lodge of the gardener herself, would have been a visible indication of that success. Once established as an incidental source of food and perhaps associated with a gardener's personal ability and prestige, the adoption of corn in this minor focus could have occurred quickly throughout the individual's connected family (Hart 2001). Perhaps the motivation would simply have been a case of individuals replicating for themselves the expression of status a nearby plot of corn would have conveyed, as well as seeking similar kudos for the new cuisine. Eventually, these informal practices would have been variably spread over the regional interaction network, ultimately leading to wholesale adoption of the practice.

However, the impulse to eventually increase efforts to grow corn and ultimately the acceptance of corn as an important contributing source of food to the diet likely also came from the perceived adaptive efficiencies and contingencies of this plant over others. In particular, it is important to realize that corn is harvested in late summer or early fall (i.e., before killing frosts), which is generally the same time nuts and wild rice would have been harvested. Nuts are easily harvested in a relatively short period of time, pro-vide a valuable source of food, perhaps processed as flour or oil, and stands would have been repeatedly visited year after year by women to harvest (Gardner 1997; Jackson 1991). Wild rice, though, is a very labour intensive harvest, requiring repeated re-visits to the stand over the course of a few weeks before a sizeable yield has been generated. This is due to the variable ripening of individual grains on the plant (Aiken et al. 1988; Vennum 1988). As well, wild rice yields are susceptible to fluctuating water levels and climate, so yields could vary significantly year to year (Fecteau 1985; Vennum 1988).

Corn would have required a fair degree of tending from planting to harvest, yet, if planted adjacent to the warm weather base camp, most of the work during the growing season would have been incidental tasks, undertaken by anyone in the family, including children or the elderly, likely supervised by the women who were the knowledge-keepers for the plants. As well, as a secondary rather than primary part of the diet, none of this effort would have compromised other subsistence efforts or yields. And, after some period of experimentation under local conditions, this effort would have resulted in a relatively consistent yearly harvest, which, in turn, would have led to people to eventually expect similar-sized harvests from year to year. Also, corn provides a similar, though lesser, caloric component to the diet than wild rice does (Fecteau 1985:247; Vennum 1988:40), so in years of low rice yields those people growing corn likely came to realize and ultimately count on this crop filling an otherwise critical gap in food supplies. Over time, this consistency in harvest could have led to women encouraging first their individual families, and then the band as a whole, to invest in growing larger plots of corn. If so, this would have led to an increase in the use of corn in the diet.

Both Trigger (1985:109) and Warrick (1990:336) favour the idea that people adopted a horticultural economy ultimately to have food stores in sufficient quantity so that winter dispersal of the band would no longer be required, and that high winter mortality due to periodic food shortages would be alleviated. But given the recent data suggesting corn was *very* slow to be used in this way after it first arrived in the region, and Smith and Crawford's (1995, 1997) and Wilson's (1990) ideas that Middle/Transitional Woodland peoples occupied at least floodplain sites through most of the year, winter food shortages may not have been as critical an issue as previously thought. It could also be argued, though, that Middle Woodland packing along a drainage may have led to greater pressures on winter food supplies such as big game, creating greater winter privation than experienced in earlier generations (Bill Fox, personal communication 2000).

Nonetheless, the diversified subsistence followed by these people, including the harvesting of plant stuffs like nuts and wild rice, as well as the use of storage facilities, as also seen earlier on many Middle Woodland base camps (e.g., Parker 1994; Wilson 1990), would have limited the risk of winter food shortages, regardless of whether or not bands dispersed in the winter. Of course, across the Northeast and Midwest, groups had developed various risk management strategies long before the Transitional Woodland to ameliorate the impacts of short-term food shortages (e.g., Halstead and O'Shea 1989; Winterhalder and Goland 1997). Indeed, it has been argued for wild grasses and tubers in the Midwest (e.g., Wymer 1993; Yarnell 1993), and for wild rice in the northern Great Lakes (Lofstrom 1987), that critical social change had occurred during and even before the Middle Woodland as a result of an increased emphasis on native plant husbandry (e.g., fixed settlement and shared subsistence, re-gendered division of labour, imposition of more formal social control). The arrival of Mexican cultigens into the region was simply plugged into already innovative strategies of subsistence and social organization (Yarnell 1993). In other words, looking at the long term, corn was simply accepted into an existing, historically-based conception of plant husbandry. The importance of its role as a primary food source would have emerged only after hundreds of years of slowly usurping the primacy of native plants in local diets, and then intertwined with the emergence of the kind of intensification of use seen in the later Late Woodland and tied to further changes in social organization (e.g., Chapdelaine 1993a; Hart 2001; Niemczycki 1984, 1988).

Indeed, for the first several centuries that corn was grown locally, the historically-based social reality of needing to maintain subsistence diversity would have fostered a conservatism limiting wholesale and rapid dependence on this one food source (O'Shea 1989:59). In fact, the continued reliance on wild resources through the Late Woodland (e.g., Lennox and Fitzgerald 1990; Ramsden 1990) reflects this many millennia old strategy of buffering single resource dependence (O'Shea 1989).

Interestingly, it has been pointed out (e.g., Finney 2000; Halstead and O'Shea 1989; Nassaney and Sassaman 1995; Winterhalder and Goland 1997) that interregional interaction networks — that is, formal and informal exchange – were important components of minimizing risk. Certainly sharing knowledge and resources are opportunities local groups can exploit when social bonds have been built with their neighbours. More generally, across the Eastern Woodlands these regional interaction networks were the linked conduits that diffused many of the improved concepts and products of new subsistence strategies, as well as the cultural-conceptual framework to integrate these ideas into local circumstances.

So, rather than a wholesale and rapid shift to a horticultural way of life, corn appears to have remained a supplementary food source throughout the Transitional Woodland, augmenting a very diversified subsistence economy. However, the importance of corn as an alternative to local resource harvests, especially during periodic failures of the local harvest, contributed to this food becoming an essential, though still complementary, part of that subsistence base by the end of this period. As well, the long term impacts to community organization would slowly have been manifest over this time. For instance, if this crop was indeed grown near summer or year round base camps, tied perhaps to individual families, corn would have entirely emerged within the social domain of women, who would have initiated the experimental planting of the crop, care for it, incorporated and used the crop in food, and linked it to the residential domain (Watson and Kennedy 1991). Moreover, shared efforts in caring for the crop and benefiting from the yields were likely managed by the women based at the camp, thus underscoring the gendered ownership of both the plant and place. In other words, the social significance of the crop, perhaps long before its full subsistence and economic importance was realized, would have been linked to, and championed to the wider community by, the women in the band. Over generations, owner-ship of crops and the planting areas could have been passed down from a mother to her daugh-

ters who had helped her as children in the plots. And if ownership of the crop and land it was planted in, and by extension the residential base adjacent to these fields, became tied to some or all of the women of the band, well these were things that could not be simply relocated to the husband's home after marriage, and there thus would have been clear benefits to the husband relocating to the wife's residence, notwithstanding any hierarchical group or male head of the family control of the ownership of others efforts (cf. Benn 1995). Ultimately, it is tempting to see that the continuing role of the women in these groups to shape and advance the importance of corn to the community's diet would have been the change in habitus that in the long term would have been a significant catalyst to the emergence of the matrilineal/matrilocal societies seen to emerge later in the Late Woodland (cf. Chapdelaine 1993a:198; Hart 2001).

Summary

In cultivating this story, of course I realize that it reaches beyond the archaeological data to offer a view of pre-Late Woodland social development. Certainly we must await the kind of intensive investigation of Middle Woodland occupations on the Grand River that has been done for places like central Illinois (Asch and Asch 1985) and central Ohio (Wymer 1993), including extensive soil flotation, before any sense of whether or not wild rice and other native plant species played an important part of local Middle Woodland subsistence can be gained. And even then, notwithstanding modern methodologies of intensive flotation and recovery (cf. Moffat and Arzigian 2000), earlier cautions about the difficulties of recovering or recognizing charred wild rice (e.g., Ford and Brose 1975) still apply, so coring (e.g., McAndrews 1969) and site location inference (e.g., Rajnovich 1984) may remain the only avenues of inference for this supposition.

Nonetheless, the key point that I have been trying to draw out here is that the developments on the Grand River did not occur rapidly. Corn was not adopted with a vision of village life and endless cornfields in mind. Rather, it was a subtle and conservative introduction, plugged into

historical realities and constraints, which were bridged only over the long term likely through the agency of both individual women, and subsequently mothers and daughters. This eventual greater acceptance of corn, especially as a contingency to other food sources, fits neatly with Last's (1995:148) notion of an agency of action — individuals responding to immediate needs and recognized advantageous strategies that also trigger a long term change that can be visible in the archaeological record, but unanticipated and unplanned for in initial actions (cf. Barrett 2000, 2001). Certainly as a forager society, these Middle and Transitional Woodland communities practiced a subsistence strategy that was both flexible enough to incorporate additional practices, and conservative enough not to abandon wholesale any one activity for any one other. The consequences that manifested themselves in the longer term, such as matrilocal residence and reallocation of village-based labour towards horticultural efforts would have been logical developments only realized long after shifts in that direction had begun. And more distant consequences, such as villages, tribal level socio-political organization and conflict, would never have been anticipated in the decisions of the day.

The Late Woodland

From the previous discussion, it should be clear that, though you are now reading a new section of this paper, I do not intend to imply by use of a different heading that I have begun to talk about something completely new and separate. Rather, my story continues on with the next generations of grandchildren, connected by history to the earlier generations of grandchildren of the previous millennia. I will talk broadly of the archaeological record that chronologically falls later, and therefore into later designations I am using for ease of narrative. But I don't wish to suggest anything other than temporal fittedness for placing any one site into the discussion here or elsewhere, or any kind of ethno-linguistic affinity for one site or another.

The Early Inter-Lakes Tradition

By the end of the Transitional Woodland and start of the Late Woodland, say ca. A.D. 900-1000, there is archaeological evidence that more substantial residential, macro band base camps had emerged. The limited excavations at the Porteous site, which has been variously placed within the Transitional Woodland (i.e., Princess Point [Noble and Kenyon 1972; Stothers 1976, 1977]) and early Late Woodland (Fox 1990a; Williamson 1990), revealed a number of small, circular or square houses, along with two overlapping oval structures, roughly ten metres long by four to five metres wide. There was also a palisade surrounding the site, which was situated away from the Grand River by a high bluff above a small creek. Differing opinions as to the age of the site, and the possibility that it is a long term and multi-component occupation, limit its interpretive value (Smith 1997:48-51). Recently the Holmedale site, identified as Princess Point, but with radiocarbon dates in the eleventh century A.D., was excavated by Archaeological Services Inc. Situated on a floodplain only a couple of kilometres from Porteous, excavations revealed a less clear settlement pattern (Robertson 1999), reminiscent of that reported for the Middle Woodland O'Hara A site (Parker 1994), also situated on a river flat. At Holmedale, houses are interpreted, as is a palisade/fence, though they are not as distinct as at Porteous, but are assumed to be comparable.

Elsewhere in southern Ontario, there is a great deal of information available on EILT manifestations, much of which has come to light following Wright's OIT development. To the east, one of the possibly earliest EILT sites is Auda, located midway along the north shore of Lake Ontario and purported to date late in the eighth century A.D. (Kapches 1987). However a lone radiocarbon date for the site would put it in the eleventh century, and others have favoured this later placement (Fox 1980; Timmins 1985). Further complicating matters is that the immediately adjacent Hibou site has yielded a ceramic assemblage that seems to seriate slightly earlier than Auda, but returned radiocarbon dates of the late thirteenth century (MacDonald and Williamson

1995). A possible explanation for this discrepancy is that the assemblage from Hibou is strikingly similar to New York State late Owasco materials, which also date to the thirteenth century (MacDonald and Williamson 1995:21). This may suggest an intrusive New York population moving into the area after the Auda site occupation, or it may suggest a blending of more westerly and southerly expressions. For example, Mima Kapches (1987) and Spence (personal communication 2000) both see local continuity from Middle Woodland groups into EILT manifestations in the area. Given the location of Auda and Hibou, and their distance from the Niagara peninsular social interaction network, it is tempting to suggest that local developments in this area were linked to a social interaction network that circled east around the eastern end of Lake Ontario and into New York State, which would explain the regional variation and similarity of the Hibou materials to eastern New York State manifestations.

There are a number of EILT sites around the western end of Lake Ontario, including early sites such as Lightfoot, a possible late eleventh century A.D. informal occupation of small oval and feature-less structures (Poulton et al. 1996; D. Poulton, personal communication 2000). Later in time, and of more substance, are the Miller and Boys sites (Kenyon 1968; Reid 1975). Miller, in particular, exhibits similar feature-less structures to that seen at Lightfoot, as well as longer longhouse-like structures, palisade, and complex tertiary[12] settlement patterns. Also known from the west end of Lake Ontario and extending to the Grand River are sites that have been investigated in the Burlington (e.g., Bursey 1997; Fecteau et al. 1994), and Hamilton areas (e.g., Fox 1967; Wright and Anderson 1969), and along the Grand River itself (e.g., Bursey 1996; Williamson 1998 [ed.]).

West of the Grand River EILT sites — at least the larger occupations — tend to exhibit more complex settlement patterns, though these sites mostly date to the eleventh century or later. Partial excavations at Van Besien, located north of Long Point (Noble 1975a), revealed an over 20 m long longhouse, crowded with various fea

tures. Two other somewhat smaller structures were also uncovered. Radiocarbon dates (Smith 1997) would appear to place this site in the eleventh century, presumably only a couple generations or so later than Holmedale and Porteous. About a kilometre upstream is Dewaele (Fox 1976), which dates later in time (ca. twelfth century A.D. [Smith 1997]). It also exhibits overlapping structures, including portions of four longhouses, along with another four small, ovate structures. As with many EILT sites, both Van Besien and Dewaele exhibit a great deal of tertiary settlement patterns. Other large sites investigated and north of Long Point on the Norfolk sand plain include the Elliott villages (Fox 1986a, 1986b), and the Reid site (M. Wright 1978).

The other investigated "clusters" of EILT sites are found east and west of the City of London along the Thames River drainage, including the multiple village occupations of the Calvert site just east of London (Fox 1982b; Timmins 1997a), dating to the twelfth and thirteenth centuries. West of London Williamson (1985, 1986) conducted an intensive regional survey of Early Ontario Tradition sites on the Caradoc sand plain, which extends north towards the Sydenham River. This included excavation at the thirteenth century Roeland village occupation, and work at a number of smaller sites Williamson (1986) identified as special function locales.

Traditionally the larger, settlement occupations of this period are referred to as "villages." However, as they exhibit multiple overlapping structures, significant variability in size and shape of structures, and plenty of tertiary settlement patterning, these sites have struck archaeologists as reflecting little or no conscious planning of community layout (e.g., Noble 19756:40; Warrick 1984a). This is particularly the case when archaeologists compare these settlement patterns to the later patterns observed during the Middle Inter-Lakes Tradition period, suggesting that EILT settlement patterns are a transition from what came before to what came later (e.g., Trigger 1985; Williamson 1990).

While simple chronological ordering makes

this observation fact, it is also the case that recent research has shown these early patterns to be more complex and organized than was previously thought (Timmins 1997a; Timmins and Staeck 1999). In the first detailed intra site analysis of its kind for the EILT, Timmins (1997a:210-213) was able to demonstrate that the Calvert site represented close to a century of occupation, and his use of multiple lines of context-specific comparative analyses confirmed the site had four distinct periods of occupation. He argued that both the planning of the re-organization and subsequent close alignment of houses through the second and third phases of occupation are strong indicators of organized social planning and a high degree of co-operation, suggesting a well-integrated community. Also worth noting is that Timmins (1997a:I98) identified four distinct functional structure types at the site, including the typical (i.e., Dodd 1984; Warrick 1996) EILT "short" longhouse, nuclear family or task specific group cabins, non-residential structures for storage or to accommodate specific tasks, and structures which combine residential and task needs.

Timmins' review of other regional clusters of EILT sites in southwestern Ontario suggests that these communities exhibited the same degree of social complexity and organizational co-operation as seen at Calvert (1997a:223-226; Timmins and Staeck 1999). And by way of comparison, with the exception of Lightfoot, the Toronto area EILT sites (e.g., Miller and Boys) would score comparably on Timmins' and Staeck's (1999) measurement of social organization/complexity. While Timmins and Staeck rightly caution against the almost inevitable potential for judgmental evaluation to creep into this "scoring" of complexity, it does appear that, unlike previous interpretations, this method shows regional sequences of EILT development across southern Ontario were following broadly similar trends at roughly the same time. There would seem to be little evidence of a "more" complex and organized regional sequence emerging first in the east. Likewise, Timmins also shows that the use life of even relatively small village sites could be as much as a century or more. With past tendencies

by archaeologists to consider and present such sites as a "moment in time" and analyze them as a single unit, occupational longevity masking community organization could be overlooked.

It is also worth considering that the settlement patterns published from earlier sites such as Van Besien (Noble 1975a) and Porteous (Stothers 1976) may also suggest some episodes of co-operative and organized management of residential space and construction (e.g., similarly oriented and aligned structures, use of a encircling palisade, differential feature concentrations presumably associated with specific activity areas). While both sites could benefit from a detailed re-examination, if they do indeed reflect similar episodic histories, as at Calvert, this would suggest that the organization and management of space and residential layout Timmins uncovered at Calvert might already have had some antiquity. Indeed, the macro band settlements of the Middle and Transitional Woodland, whether occupied year round or seasonally by some or all of the territorial community, would themselves still have required some limited maintenance, organizational planning, decisions about when to move on, and other such logistical co-operation. The strategies that were employed by the residents of those communities simply continued to be operational into the succeeding early Late Woodland period.

Notwithstanding the dating problems with Auda and Porteous, at least by A.D. 1000 more formal structures, such as a wall surrounding some or all of the residential area of a base camp and the appearance of clearly visible residential dwellings, can be found on sites in the inter-lakes region of southern Ontario. Palisades, as archaeological features and as walls that appear in a forest clearing, are powerful architectural additions to a site. While a few, heavily palisaded EILT sites suggest to Jamieson (1992:74) some evidence of internecine warfare, the more commonly found examples of a single rowed palisade or partial wall are interpreted as wind breaks, or as a means to keep animals out (e.g., Jamieson 1992:74; Reid 1975). These walls, then, were less stockades and palisades, and more functional surrounds. Regardless of how substantial these

structures were at any given site, Ramsden (1991) also points out that the most significant aspect of walling in a residential area is both a tangible and symbolic definition of place, membership, and presumably rights and access to the things and people inside that wall. In effect, contributing significantly to further village-centric understandings of community.

At about the same time palisades start to encircle residential areas, house structures also emerge as visibly distinct patterns from the broader tertiary pattern of scattered posts on sites. The simple fact that they are visible suggests that their construction is of a more substantial nature (Chapdelaine 1993a:185; Warrick 1996). At this early period there are several distinct structural types in use. Beyond residences, smaller structures have been interpreted as providing storage or were locales for specific tasks (e.g., Fox 1976; Timmins 1997a; Williamson 1985), nuclear family or task group hunting lodges (Timmins 1997a), or places set apart for religious or medicinal rituals (Fox and Salzer 1999). As well, given the presence of one or two hearths in some of these, it may be that smaller residential structures were also used in tandem with short longhouse residences (also noted by Kapches [1984] as occasionally found on sites of the late pre-contact era).

It is generally assumed that the short longhouse contained a single extended family (e.g., Dodd 1984; Warrick 1996). Not in agreement is whether or not these houses yet represent a matrilocal residence pattern, as seen historically for Iroquoians. Kapches (1990, 1995) has argued that this does not occur until after the EILT period, and Williamson (1990:317) cautions that his Caradoc data suggests a fair degree of female mobility to special function task sites away from the village, which might suggest matrilocal residence had not yet been established. Timmins (1997a) and Chapdelaine (1993a) do see matrilocality in the Ontario data, and Timmins points out that this may be variable from one region to the next. Williamson (1990:318) offers an additional observation that there is a general ceramic *homogeneity* in the data recovered from the Caradoc community, and Timmins (1997a)

notes a striking degree of ceramic *heterogeneity* between regional EILT sequences; both facts cited (cf. Whallon 1968) as evidence of women staying within regional communities (i.e., matrilocal residence), so that cross-regional sharing of stylistic traits is minimal. Actually determining when matrilocal residence first occurred, as opposed to when it became formalized across the lower Great Lakes, is a bit of a moot point however, since there likely was regional variation and experimentation occurring through this time (e.g., Niemczycki 1984; cf. Hollinger [1995] for Oneota comparisons), and certainly the evolution towards matrilocal preferences, even if not fully and consistently manifest yet, had been evolving for several centuries by this time (Hart 2001).

Of course, distinct local ceramic traditions may also arise as a consequence of possible group fissioning and relocation, especially in the southwest (Fox 1976; Jamieson 1992; Timmins 1997a; Williamson 1985). For example, what is known of the Middle Woodland west of London on the Thames River is limited primarily to the Delaware area (Spence et al. 1990; Wilson 1990), which is just east of the Caradoc sand plain. These researchers believe Middle Woodland manifestations in the area show local progression into the Riviere au Vase phase of the Western Basin Late Woodland Tradition. Though impossible to determine the exact process, it is also suggested that these people moved west after A.D. 1000 (Murphy and Ferris 1990), and within a generation or two EILT archaeological materials appear in the area. While heeding Rankin's (2000a) caution against simplistic models of invading horticulturalists displacing or absorbing local populations, the archaeological record around the Caradoc sand plain has yet to reveal any data to support an archaeological manifestation antecedent to the EILT, despite extensive, on-going work in the region." Timmins, too, wonders if the EILT community represented at the Calvert site may also have been a migrating group, who perhaps arrived from north of Long Point.14.

Fox (1976, 1982b) has suggested that such a westerly movement of EILT people from the

Norfolk sand plain north of Long Point occurred in response to an intensification of agricultural activities. These fissioned or "daughter" communities moved to a similar environmental locale (i.e., the Caradoc sand plain) to continue practicing intensive agricultural subsistence strategies. However, the age of some of these sites pre-dates the assumed thirteenth century occurrence of this intensification among EILT groups (as discussed below). Likewise, Williamson's data suggests a very diversified subsistence regime followed by the Caradoc group, suggesting that no single component of that subsistence system should have so completely affected wholesale change. Nonetheless, other examples of migrations are known, such as the MILT movement into Simcoe County (Sutton 1995, 1996, 1999). As well, Sutton has argued (1995, 1999) that migration would not be a simple outward expansion, but rather would be characterized by "leapfrogging" to regions of which people had some previous knowledge and familiarity. If so, this may recall the first phase of the Calvert occupation, which consisted of a single house, and which may represent a pioneering settlement from the Norfolk sand plain or elsewhere. 'While this story needs more of a plot, it may be part of the reason why there appears to be distinct, regional ceramic traditions emerging in EILT times, as artisans of newly fissioned communities made individual and locally-based choices with respect to decorative motif and technique, which over time diverged from the traditions of "mother" communities to create new local or micro-traditions of ceramic expression.

Regardless of the suite of factors that contributed to this, the existence of regionally distinct ceramic assemblages compromises the utility of ceramic attributes or types to define broad geo-cultural units, and emphasizes the need for detailed regionally-specific trait analyses to see, at a fine scale, if there are any interregional connections beyond broad temporal similarities (Timmins and Staeck 1999:173). Regionally specific ceramic traditions may also suggest fairly limited corporate organization between regions, as Spence (1994) has demonstrated for EILT burial practices. Indeed, Spence's point that these

practices were even variable within particular regional sequences, which suggests idiosyncratic practices, likely some temporal variation, and perhaps even an indication of multiple "communities" or bands represented within a particular regional sequence of sites is worth noting. Certainly such multiple communities might have occurred in the Caradoc area, for example. While Williamson (1985) believes the Caradoc EILT sequence represents the development of a single band through time, Timmins (1997a:217) suggests the basis for this (i.e., determination of the number of central villages present on the sand plain through the Early Late Woodland on the basis of site size exceeding a hectare) may have unnecessarily missed a number of other villages which collectively could represent a village occupation sequence for a second corporate group. The Calvert site, he points out, was less than a hectare in size. So if this is the case, some of the Caradoc sites not subject to excavation, or only partial excavation, and initially identified as special function camps, may in fact represent Calvert-like smaller village locales. This would be notable if it were the case, as it suggests there would have been, among individual corporate groups, an ever changing conception of territory through this time, in order to accommodate newly established communities. It would also imply that conception of the territory beyond the village likely had to be regularly redefined through negotiation or conflict with neighbours over the sharing of hunting, fishing and other resource locales away from the formal village.

This shift in a group's conception of territoriality likely fostered the emergence of a more narrowly conceived, village-based definition of community, and may be reflected in changes to burial patterns. Ramsden points out (1991:175) that some earlier Middle Woodland groups maintained visible corporate cemeteries such as burial mounds. Burial mounds likely were instrumental in defining both band membership (i.e., who got to be buried there and who did not), and serving as a visible marker on the landscape of territorial ownership for that group. But the use of burial mounds disappears by EILT times. Instead, burials are usually found in or around village locales.

So the visible marker of place and membership on the landscape that would have been a burial mound for the macro band of the earlier Middle Woodland becomes the village itself in the Early Late Woodland. In effect, the total population of this earlier territorial band becomes subsumed within the palisade.

The appearance of palisades and definable house structures on early Late Woodland sites, along with the recovery of corn, have all previously been cited as demonstrative of the formal establishment of a semi-sedentary, horticulturally-dependent way of life by A.D. 1000 (e.g., Noble 1969, 1975b). More recently, however, as extensive information on regional sequences of EILT sites emerged, this notion has been challenged (e.g., Chapdelaine 1993a, 1993b; Trigger 1985; Williamson 1985, 1990). Williamson (1985, 1986), for example, points out that the EILT occupations on the Caradoc sand plain demonstrated quite diversified subsistence strategies. Special function sites were occupied by either a single extended family, or several extended families (a proto clan?), undertaking a specific task (hunting, fishing, etc.), and ultimately returning with harvested yields to a village locale. Chapdelaine (1993a, 1993b) also questions the assumption of a horticulturally dependent economy throughout much of this period, and isotopic data (Katzenberg et al. 1995; Milner and Katzenberg 1999; Schwarcz et at. 1985) tend to confirm that a significant increase in corn as part of an individual's overall diet only occurs in the thirteenth century. As well, while Warrick (1990:342-343) sees a steady population increase through the EILT period, he notes that the rate effectively triples late in the twelfth century A.D. This implies that for much of the EILT period, corn is an important *part* of a diversified subsistence regime, but that communities only intensified their cultivation of the plant to the point of it becoming the primary food source by the end of the period.

Telling Tales II — The Story Continues

Taking an historical perspective to the events of the Early Late Woodland shows that the archae-ological manifestations after A.D. 1000 arose from peoples' experiences and actions of the preceding millennium. Early "villages" were a continuation of the macro band occupations seen earlier, and exhibit similar settlement patterns, with the addition of an enclosure and more substantial structures. The precursors of matrilocal residence, corn becoming an increasingly important part of a diversified subsistence, and changing social organization all contributed to a period of intermediate semi-sedentary settlement, during which time discontinuous development towards a more formal, sedentary, horticultural way of life occurred (Chapdelaine 1993a, 1993b; Niemczycki 1984). Niemczycki suggests that the "social" tools, at least within Seneca communities, were not yet in place during EILT times (prior to A.D. 1250) to effectively manage the required social integration of growing populations, and these communities lacked the ability to intensify subsistence to support their expanding, permanent settlements. As a result there was a ceiling to village growth that was continually being hit, causing group fission, allowing the resulting smaller groups to return to more successful, diversified subsistence strategies and to more effectively manage social organization of the group (Niemczycki 1984:99). Such a scenario, if it also occurred in southern Ontario, might well offer an explanation as to why there appear to have been micro migrations of groups, such as an EILT migration onto the Caradoc sand plain.

I am struck again at the implications this has for the role women of the community played as agents of continuing change. Returning to the Grand River, for example, we had left this group with corn associated with individual households, and probably within the jurisdiction of the women in the community. As Chapdelaine (1993a:198) suggests, the appearance of short longhouses or extended family houses could be the first archaeological manifestation of matrilocal residence. But it is also likely that there was a fair deal of residential variability at any given village through the Early Late Woodland, as seen in the abundance of different house structures documented. Indeed, hard and fast rules for things

like who resided in the longhouse and who did not, or how descendant ownership of corn plots worked, were likely experimented with on a community by community or even family by family basis. Any of these may have led to variable residence patterns, and to changes in those patterns even in the life of a single structure.

It does seem to me that the increased organization of residential locales is evident with the appearance of more substantial dwellings and palisades or surrounds. And this likely has to do with the increased organizational skills of women, a by-product, if you will, of either the women of individual extended families or of the group as a whole working together, interacting and discussing the management of the village, people, corn plots, domestic tasks and other aspects of community life. So the variability within settlement patterns for any particular site may well reflect the degree of effectiveness of women's agency for that group, responding to things like the relative willingness of community members to accept matrilocal residences, placement of in-bound son-in-laws in the house, rules of inheritance, doing more work in a more fixed place, and a range of other practical issues emerging from on-going changes to home and village living.

It also does not seem too far-fetched to see things like surrounds appearing initially because of individual actions. For instance, whatever the initial functional reasons were, this imposing structure quickly came to operate as much more than just a wall. Approaching, walking through, and living within a walled-in community would have daily helped to re-conceptualize and reinforce what was surrounded, both for the residents of the place, and for those from elsewhere visiting it. An encompassed living area would have conveyed a sense of permanence and fixedness to a specific place in a much more visible and tangible way than the more open Middle and Transitional Woodland macro-band settlements. Inside the wall was where people stayed, outside was where people went. So, in actuality or not, this walled place would conceptually be the centre of the band's territory, rather than the band moving around the territory. The re-con-

ceptualization of residence encouraged by surrounding it with a wall likely made the idea of sedentariness, and indeed permanence, much more of a self evident truth, and thus internalized changed definitions of place, personal membership to place, home, or, in other words, the community's sense of identity — altering the community's habitus while effectively maintaining it (Bordieu 1977).

Of course the appearance of more substantial structures on sites after A.D. 1000 likely tells a similar story. Practically, living in a single structure for more of the year due to increased sedentariness would have created the need for more substantive structures. But living in the same structure longer, and staying in the place where these structures were kept for more of the year, would have infused these things with meanings beyond that of simple tools of shelter. For example, if people found themselves living for more of the year in a specific dwelling, they likely would see the house as the permanent place to keep personal belongings, food, and family members (i.e., young, infirm and old). In other words, it would become a tool for ordering and managing the family through the ordering and managing of the familial space. Kin co-operation and compromise (or acquiescing to the rule of the house) would have been essential. In this context, organization of roles and responsibilities, defining and reaffirming the hierarchy of authority within the extended family, resolving disputes for competing individual interests (e.g., division of tasks, ownership and possessor rights of siblings, etc.) all would have had to be addressed on a daily basis. Of course, disputes could also fester in such settings, so in that regard, it is tempting to see seasonal task-specific camps away from the village as serving a secondary function of being a convenient and regular opportunity for people to get away and diffuse the tensions between members of a community, or even of a family.

It is also tempting to see the smaller dwelling structures on EILT villages, so many of which are located adjacent to longhouses, as representing ad hoc or interim solutions to a shortage of dwelling space in the extended family longhouse. Certainly a father and mother, unmarried chil-

dren, maybe a brother or sister or two and elderly parents, plus married children and their off-spring, all would be a sign of a robust and grow-ing extended family. But a 10-15 metre long-house likely would become too cramped to accommodate everyone, especially if over time newly married couples, their subsequent children, perhaps relations from an adjoining village, or some other individuals sought to live in the same structure. These people may have occasion-ally required temporary accommodation, and perhaps were provided with such a smaller dwelling until the time that space could open up in the family home (i.e., deaths or departures). This variable wait for a spot in the family long-house might explain why some of small structures have relatively few internal features or hearths (i.e., a short wait), while others have many. As well, presumably the people in the hut desired to be in the main longhouse, so as such they would become active agents lobbying the rest of the community, or parts of it anyway, to rebuild or even relocate the settlement (ultimately so that a larger structure could be built so they could join the rest of the family). This may also be a partial factor (beyond needed structural repairs) behind the evidence of multiple rebuilding episodes within even one phase of a village occupation. It may also have contributed to variable durations for some sites (i.e., successful consensus being achieved to relocate the village earlier than would have otherwise been necessary).

Lastly, as Kapches rightly notes (1995:94; cf. Wright 1995), longhouses are impressive feats of architecture. It seems highly likely that these structures served as an important and very visible statement about the people who resided in them, and ultimately a statement about the extended family as a corporate member of the village. In effect, the longhouse was the "outer packaging" of individual lineages; advertisements of how well that family was prospering. Most of this prestige would fall back to the women of the family, again reinforcing their dominion over the family residence. These structures also symboli-cally embodied many of the essential tenets of emerging village social life — co-operation, dura-bility and family growth and prosperity. But it

also contributed to maintaining a distinction between the extended family in a village and the village proper. As such, and beyond the commu-nity fissioning Niemczycki (1984) proposes, there likely was also individual extended family fissioning from a community as a response to tension or conflict (i.e., a successful family either not being able to influence community decisions or having too much influence). Indeed, this may well have been the case for the later MILT period Savage site, situated well west along the Thames River drainage in southwestern-most Ontario and among otherwise Western Basin Tradition archaeological sites (Fraser 2001; Murphy 1985). This site consists of a single long-house, clearly occupied year round, and apparently without any nearby contemporaneous village site with which it might be associated. It is tempting to see the founding of this settlement as the community origin for the Wolfe Creek cluster of village sites, which are found slightly to the west and dating a century or so later, and also thought to be affiliated with the Inter Lakes Tradition rather than the Western Basin Tradition (e.g., Foster 1990).

The Middle Late Woodland

Given what we have learned in the last 35 years about the archaeology of southern Ontario, there seems no way to continue to buttress a concept such as the Conquest Theory. Not because a sin-gle data set or artifact trait challenges the original interpretation, but because the conceptual underpinnings behind the notion are no longer in operation. A first point to make about the thirteenth and fourteenth centuries is that change in the archaeological record was not uniform across southern Ontario. Regional sequences, though reflecting broadly similar developments, exhibit a great deal of local variability, and clearly unique circumstances directed particular responses in different regions (e.g., the Saugeen River [Rankin 2000a]; the Trent River valley [Ramsden 1990]; the St. Lawrence River valley [Pendergast 1975]; Simcoe County [Sutton 1996, 1999]; and the Thames River valley [Pearce 1984, 1996]). Neither can this

change be characterized as sudden, not if one takes into account the long term historical developments of each region. This is not to say change is not manifested in the archaeological record, however, or that there were no important differences from the EILT period (e.g., Dodd et al. 1990). Also, as has been suggested by many (e.g., Chapdelaine 1993a; Kapches 1995; Jamieson 1992; Niemczycki 1984; Trigger 1985; Williamson and Robertson 1994) it is during this time that what could be considered "classic," historically recognized traits of Iroquoian society become fully manifest in the archaeological record.

Multiple agents have been cast in the starring role of the drama that was the thirteenth and fourteenth centuries in southern Ontario. Dincauze and Hasenstab (1989; cf. Hasenstab 1987) have argued that the archaeological manifestations seen by the end of the MILT period are a reflection of a process of "Iroquoianization," due to these lower Great Lakes Late Woodland peoples being involved in a broad communication and exchange network directly controlled and influenced by Mississippian centres to the south in the Ohio valley and beyond. Communities of the MILT period were involved in providing these Mississippian centres with raw resources such as deer hides and other commodities. Dincauze and Hasenstab (1989) also suggest that the impact of this Iroquoianization process would have spread from west to east, since westerly groups would have been first to be swallowed into this network due to their closer proximity to the "core," and then ultimately co-opting their more easterly neighbours. Of course, somehow Western Basin Tradition Late Woodland groups had to have been either passive carriers of this influence, or were bypassed in the process. An earlier, economically determinant agent of change proposed by Hayden (1977, 1978), where control over external trade and accumulation of wealth led to differential authority and expressions of wealth inside the village, could easily be accommodated in the Dincauze and Hasenstab core-periphery model, simply as being the specific local manifestation of involvement in this exchange network.

Core-periphery variants of world systems models have been largely rejected (e.g., Stein 1998), and most Ontario archaeologists have also failed to accept the Dincauze and Hasenstab model (e.g., Timmins 1997a:208-210; Williamson and Robertson 1994). Generally, these researchers fail to see any evidence in the local archaeological record of involvement in this long distance trade. As well, Muller (1986), amongst others (e.g., Finney 2000), has questioned the population estimates for these Mississippian centres and the degree to which they were not self-sufficient (thus needing imported supplies), or the evidence that peripheries were generating surpluses of the scale needed to supply the core. Griffin (1993: 9-11) is also quite dismissive of the Dincauze and Hasenstab conception of core-periphery, questioning their understanding of Mississippian cultural development, and pointing out chronological and material culture inconsistencies ignored in the model.

Among Ontario researchers a number of external/internal agency models have been proposed to account for change through this time. Jamieson (1989, 1991, 1992, 1999) has outlined an interregional interaction model of internal, local cultural change driven in part by the ready flow of external ideas and material into and through southern Ontario Late Woodland groups (as defined more broadly by Caldwell [1964]; cf. Kelly 1991; Schortman 1989; Schortman and Urban 1987, 1992). The flow would have been facilitated by these groups participating in regional interaction networks, and these individual networks themselves would have been linked across the Eastern Woodlands and beyond. Some of the flow of concepts and material innovations passing through these interregional interaction networks would have been differentially internalized locally and ultimately manifest in change. This process of internalization, which Jamieson labels "Mississippification" (1992:70), incorporated concepts of hierarchical social organization, economic intensification, and defensive and offensive expressions of militaristic authority that first developed among the major centres of Mississippian culture to the south of the Great Lakes.

Now, it is true that Jamieson's earlier work, especially the 1992 paper, has been criticized (Brose 1993:108-109; Ferris and Spence 1995:111; Timmins 1997a:208-210; William-son and Robertson 1994). Such criticisms have suggested that her model is just a variant on the core-periphery notion, arguing that local Late Woodland groups were seemingly deficient in possessing independent informational structures to "localize" external concepts, acting as passive agents to external influence. Jamieson (personal communication 2000) has been adamant, though, that these criticisms are a misinterpretation of the points she makes in the 1992 article, and in fairness, these criticisms do ignore her portrayal of how interaction plugged into internal change. For example, Jamieson (e.g., 1992:71,74) argues that diffused traits were not only adopted, but also translated and integrated into local ideologies and local historical developments. In other words *actively* reconstituted and internalized in a manner consistent with local social realities. In fairness to the critics, however, it is also the case that this article can be read as supporting or at least complementing the Dincauze and Hasenstab model (e.g., by suggest-ing that external "acculturative" forces on local groups did not come directly from Mississippian centres, but did from adjacent interaction net-works [Jamieson 1992:71]). The argument that localized development towards village-centric social organization and settlement-subsistence occurred following the pronounced appearance of Mississippian interaction (Jamieson 1992:71), her suggestions regarding the "eventual domination" of southern cultural elements (Jamieson 1992:76), and even the term "Mississippifiction" itself, is all language that *could* be read as intimating a fairly reactive local response to entirely specific, external influence.

But I am not interested in conducting a textual analysis here, and suffice to say there was room in the 1992 piece for some unintentional ambiguity between writer and reader. Anyway, the whole debate is rather moot now, in light of Jamieson's (1999) most current thinking on the topic. This work greatly expands on her earlier views of interregional interaction and historical

context, and redresses earlier misperceptions of the model. She points out that societies, and individuals in these societies, operate within a web of social relations that crosscut families, individual settlements and regional groups, and wove local communities into a wider social fabric of ideas and materials that ultimately connected groups throughout the Northeast. The spread of innovations, symbolic meaning, and conceptual messages, as packaged in material items, could be read across this region by groups, regardless of linguistic or geographic distance, because all groups belonged to a shared, ideologically con-servative Native belief system (Jamieson 1999:177; cf. Caldwell 1964; Nassaney and Sassaman 1995; Seeman 1995). Jamieson points out that internal group values and ideological meanings are not denied in this model, but actu-ally are active components in the on-going com-munication, revision, and re-communication of concepts through this interconnected cultural network. In effect, as individuals existing both as members of a particular community *and* as members linked to this much broader belief sys-tem, the implications of ideas and things being exchanged could easily be understood, selected or rejected, internalized, and potentially reconsti-tuted in local actions. Diffusion of these ideas and materials through this web of networks worked though simple mobility, consisting of everything from informal contacts, exchange net-works, intermarriage, group segmenting or fis-sioning, and formal migration. Indeed, she makes the valid point that this network, and the diffusion of concepts through it, was a basic con-struct of pre-contact society, and shows the his-torical vitality of this component of life in play-ing a significant role not just in the events of the thirteenth and fourteenth centuries A.D., but throughout the pre-contact era. This certainly accords with the earlier discussion about the introduction of corn and modeled ceramics seen to have entered Ontario through such social net-works, and then ultimately internalized, leading to increased sedentarism and matrilocal organi-zation of residential settlement.

It is important to realize that this model reflects the basic fact that past peoples, though

striving and coping within local constraints, also existed within a much wider pan-regional network of like local communities. The presence of exotic trade goods, the broad scale changes in prosaic material culture seen across the Eastern Woodlands, food production or even pot decoration, all attest to this simple fact. This is a critical point that has been made more generally in recent discussions of the role interaction plays in internal cultural dynamics (e.g., Finney 2000; Peregrine 1995; Sassaman and Nassaney 1995; Schortman and Urban 1992). Previously, interpretations based on systems theories and models of exchange have been criticized, especially those normative conceptions of interregional exchange, for an over-emphasis on economic determinism, dependency, the importance of exotics, as well as an overly passive understanding of the agency of culture (e.g., Kohl 1987; Milner 1990; Nassaney and Sassaman 1995; Stein 1998). However, recent discussion has situated internal agency, historical context and multi-scalar analyses as critical elements to understanding the interconnectedness of cultural groups over wide geographies, when understanding internal cultural agency. This approach accommodates not only local patterns within wider cultural contexts, but even regional discontinuities and variable responses to information and material exchange from local area to local area, and even within different sectors of the same community (e.g., Cobb 1991; Nassaney and Cobb 1991; Peregrine 1995; Sassaman 1993; Scarry 1993). In effect, this provides for the kind of Annales examination of the long term process of cultural agency as shaping and shaped by human interaction and mediation missing from previous, normative trait lists of exotics and the basic economic motivations for these things (Nassaney and Sassaman 1995:xxiv) — a kind of thinking globally to interpret locally, to paraphrase Nassaney and Sassaman (1995:xix-xx).

In the context of these approaches to interregional interaction, Jamieson's work neatly maps out the "global" framing of locally based agency and change. As such, other recent interpretations of change for the inter lakes Late Woodland (e.g., Chapdelaine 1993a; Kapches 1995; Timmins

1997a; Williamson and Robertson 1994) can be seen as focussing on the localized half of Jamieson's social duality, and complement her model, serving as other acts in the same play.

Of these locally-focussed models, only Kapches speaks of change as sudden and traumatic (chaos, rather than Chaos Theory) in the fourteenth century. Kapches (1995:90) argues that rapid change occurred as a result of extreme cultural stress and reaction to the emergence of matrilineal social organization and all its attendant implications. This stress, for Kapches, appears rapidly (within one or two generations during the start of the MILT period), the end result being the emergence of "classic" historic Iroquoian-like social structures. I have no quarrel with the notion of the emergence of pre-industrial horticultural societies reflected in the archaeological record during this period, including evidence of likely ameliorating strategies to redress the agitation over emerging matrilineal order. However, the characterization of change being "sudden" and "traumatic," and its sup-posed brief duration, runs counter to the picture that emerges from an historical perspective of developing social patterns over the preceding centuries. I think suddenness exists in Kapches' model because she argues that although pre-fourteenth century Late Woodland groups in south-central Ontario were likely Iroquoian-like, they were not "classic" in expression, and, thus to Kapches they were pre-matrilineal. As such, she proposes that the period of the EILT should be labeled Ontario Owasco (as representing a sort of proto-Iroquoian cultural development stage), and so placed within a different cultural-historical box (Kapches 1995:87). Of course, this conceptually removes from consideration the developments that occurred during this earlier period from subsequent events, and allows the events of the fourteenth century to be characterized, a-historically, as sudden. In other words, by re-adopting the boxing-in of broad, temporally defined cultural groups and developments, Kapches revitalizes the Ontario Iroquois Tradition paradigm and repeats the same assumption of sudden change inherent in the Conquest Theory, though replacing men fighting men on the battlefield

with men "fighting" women on the home front.

The other internal agent models all point to increasing corn consumption, intensification of horticultural activities and the need to manage decision-making and social organization within the village as leading to local responses affecting change. Certainly the isotopic data (Katzenberg et al. 1995) support the notion that corn consumption increased significantly in the late thirteenth century, and if Warrick (1990) is correct, population growth intensified in the thirteenth century, which no doubt would have placed pressure on any informal or ad hoc rules in place regarding residency, authority, inheritance, and the basic functioning of the community.

Telling Tales III – When a Conquest Isn't

In the absence of a catastrophic or militaristic episode leading to wholesale change, what then is the basis for interpreting the archaeological record within the inter lakes region of southern Ontario during the thirteenth and fourteenth centuries? Well the historical context of local developments through the preceding centuries outlined so far simply continue on through this time, and any changes in the record are linked to this pattern of ongoing agency and contingency effecting change. This is what contemporary interpretations of change have focussed on, whether in context of pan-regional trends and stimulus (e.g., Jamieson 1999), or in context of local factors of stimulus (e.g., Williamson and Robertson 1994).

So village communities that were essentially re-constituted Middle Woodland macro bands a few centuries earlier at the start of the Late Woodland, consisting of a population of perhaps 100-200 people, had evolved to something else by the end of the thirteenth century. In a sense, this is because previous strategies of coping with village life were becoming difficult to sustain. Certainly Niemczycki's (1984, 1988; cf. Chapdelaine 1993a) pattern of increased cohesion, threshold, then fission as the means of managing increasing complexity and social stress in the preceding centuries, likely was no longer viable later in the period as regions were "packed"

or filled in with communities. Or rather, this was no longer a viable option unless one was willing to relocate to less ideal or new regional settings (e.g., Rankin 2000a; Sutton 1996). Nonetheless, other options, such as one or more extended families, newly married couples, a community segment, or just individuals relocating to other villages or other regions, likely were common (e.g., Fraser 2001; Murphy 1985), as people tried to find better opportunities for themselves, especially if present prospects were felt to be less than promising.

But what was happening inside the palisade as village communities became larger? Well, bigger populations would have likely put greater demands on all aspects of the subsistence regime in the territory surrounding the village, a territory that could extend only so far until bumping into the territory of the next village over. As some food sources failed locally, or just could not be made more productive, this would likely reinforce the notion that continued efforts on agricultural supplies was the one aspect of the subsistence regime that could be manipulated into producing more. So in absence of other foodstuffs, planting more corn ensured continued sustenance for family and village neighbours. This would necessarily require intensification in corn production and more co-ordination of the village labour devoted to the task (Chapdelaine 1993a, 19936; Williamson 1985, 1990), and greater sophistication of power structures and other tools of implementing and validating authority in the community.

Of course, in order to maintain an increased supply of food, the informal and flexible resource procurement strategies of earlier generations would have been curtailed (e.g., why should you get to go to the hunting camp for a month when you're only going to come back with enough meat for yourselves?). As well, with more collective efforts to produce food, this in turn would create a greater need for task differentiation within the community (e.g., this group goes to get fish, this group stays to repair the village and look after the crops). Over time, this would also encourage the recognition of individual skills and increased differential status for people and their

families in the village (e.g., you make better pots, she makes better soup, he makes better points, and they couldn't hit a deer if they were on top of it). Likewise, all these decisions to assign tasks required agreement, and individuals who could influence and get consensus for unpopular decisions and tasks. As with any informal collective, the individual who can achieve this secures recognition and prestige within the group. Whether decision makers were men and women, or if different topics constituted differing decision makers (e.g., longhouse space, crop care, hunting or trading forays), ultimately any decisions of substance would impinge on both male and female domains (e.g., do we go south to trade or do we stay home and repair that long-house), necessitating discussion and ultimately decisions on which actions were of more value to the individual, family and village. Positioning decisions in this way ultimately would have needed to acknowledge the primacy of the village's needs and continued vitality over any individual or particular family need. This, in turn, would have increased the authority of any collective decision-making group perceived as speaking beyond their own self interest, and thus had the authority to impose restrictions on individual behaviour (albeit to some degree) or at least reduce flexible behaviour of the individual constituents of the village.

While such an intensification of agricultural production would have had the short term benefit of addressing the pressure of an increasing number of mouths to feed in a village, it also likely triggered a feedback loop for that community: we need to increase food supplies to keep people together and fed; we can increase food by having people plant more corn and spend more effort on cultivation; when we increase food we eventually get more mouths to feed; so we need more people to grow more corn. In some cases, this need for more people perhaps led to neighbouring villages joining together in order to work co-operatively and to quickly obtain a larger work force. This is what Pearce (1984, 1996) and Timmins (1997a) see happening in the London area. Timmins, for example, points out that around the Calvert site the archaeological record

is notable for the increased site size of subsequent occupations associated with the MILT period, but a decrease in the overall number. This likely is partially due to the apparent decline in task specific camps, but can also be due to the merging of two or more distinct village communities (or portions thereof) inside the wall of one subsequent, MILT period village.

This implies both co-operative and friendly neighbourly relations, likely reinforced through family ties. However, it is not necessarily the case that, region by region, negotiation of growth and competition with neighbouring villages always followed an amicable path. It may also be the case that the need for more people and/or resources contributed to increased and localized competition between neighbouring villages, and perhaps poaching of foodstuffs or use of another community's resources (e.g., hunting or fishing on someone else's patch, or raiding stores). This could easily have given rise to occasional disputes, skirmishes, raids and even open war within a specific region. For example, at the Uren site, multiple rows of palisade, and the presence of human remains in midden deposits assumed to be evidence of cannibalism, are cited as indications of at least internecine warfare (M. Wright 1986; cf. Warrick 1984a:65). Other researchers are less sure this evidence does suggest warfare (e.g., Dodd et al. 1990:357), but the key point is that localized, episodic disputes may have had a role in specific regional development during this period. If this was the case, such conflict could be manifest in the archaeological record for a particular site, such as Uren, but not seen on other roughly contemporaneous sites in the region. Even if conflict was the means of resolving social stress and growth for particular local communities, the broader archaeological record for southern Ontario still fails to indicate evidence of pandemic warfare at this time.

With the intensification of agricultural activities and population increases, local responses to these developments are reflected generally in the archaeological record during the early MILT period. First, while there remains variation in overall assemblage make-up (Dodd et al. 1990) there does appear to be an increased *degree* in the

homogeneity of ceramic design throughout the inter lakes region of southern Ontario, though this remains an impressionistic rather than a quantified observation. Notably, the horizontal motifs found commonly on vessel rims from the late thirteenth and early fourteenth centuries appear to be a local manifestation of a broader, pan-Northeast trend (e.g., Jamieson 1992:72). Indeed, local knowledge of this stylistic innovation may predate the late thirteenth century, presaged in the juvenile ceramics from occupations dating late in the EILT period. Timmins argues (19976) that the pots of children in communities like Calvert may have reflected the children's role as trend-setters in welcoming and adopting this stylistic design. Of course, as part of a broader pan regional trend, this isn't to say horizontal design sprung independently and simultaneously in the minds of local young artisans, but rather offers an intriguing alternative suggestion as to who, in communities, were the agents most receptive to changing ceramic style, and most capable subsequently of implementing that change.

But how were previously regionally distinct ceramic trends homogenized, or at least more so? One favoured notion contributing to ceramic homogeneity was the idea that, during the conquest, captive brides were taken into villages, leading to a diffusion and homogeneity of local ceramic traditions. Marti Latta (1991; cf. Engelbrecht 1974) however, has demonstrated this "phenomena" to be more a syndrome of normative archaeological assumptions than a demonstrable archaeological or ethnographic fact.

An alternative explanation could be linked to the coalescence of formerly distinct groups within a single village, which likely would have led to the sharing of knowledge, including ceramic designs, among the potters of the village. Likewise, as villages amalgamated, two things would have happened (Ferris and Spence 1995:111): the new community's territorial range would have expanded by encompassing formerly distinct areas, and second, the neighbours that people used to interact with most often were no longer there (they were now a part

of the same community). This likely led to these newly amalgamated villages reaching further afield to the next amalgamated (or not) village, widening the "neighbourhood," and strengthening those regional strands, a la Jamieson's (1999) social web or Williamson and Robertson's (1994) peer polity links. As well, this likely extended the range of mobility in terms of intermarriage, bringing people into communities with differing views and concepts on decorating pottery, behaving with one's in-laws, approaches to catching fish or storing corn, and a host of other actions, a la Jamieson's more pluralistic communities (1989, 1992). As Spence notes (1999:277), such people were cultural seeds, bringing ideas that would hybridize with existing practices. And as a fixed member of the community rather than as a visitor, their individual influence would be constant, and manifest in the subsequent enculturation of the next generation of village residents.

Other ways the archaeological record from the MILT period reflects the results of community change is in more ordered villages and longhouses. M. Wright (1986) for instance, argues that at the Uren site there appear to have been distinct village segments across the site, likely reflecting formerly separate village groups prior to amalgamation. And the increased size in longhouses also appears to reflect an increase in the spatial organization of the living area (Dodd 1984; Kapches 1990), and perhaps changed attitudes about personal space (Jamieson 1989:309). This would likely have been a response to the previous inefficient means of dealing with growth in the extended family. The practice of simply building a larger structure with enough additional space to accommodate new members of the house — space which was perhaps used for task or storage areas in the interim — may also explain the decline in the wide variety of smaller residential structure types on these later sites. These big longhouses would also be visible statements to the rest of the village community about the health, wealth and vitality of the lineage embodied within the longhouse, perhaps leading to competition for residents, as suggested by Varley and Cannon (1994).

Likewise, as suggested by MacDonald (1988)

and Kapches (1995), the appearance on the earliest MILT sites like Bennett (Wright and Anderson 1969) and Myers Road (Williamson 1998 [ed.]) of large rectangular to circular features in or attached to longhouses, and interpreted as semi-subterranean sweat lodges, is significant. These structures, historically known for curative purposes and for maintaining social relations, may well have served an important role in resolving disputes and building social connections arising from the changes in family and village organization. That they are clearly attached to longhouses suggests that their utility, at least initially, was tied to internal lineage maintenance and pressures of family life, rather than mediating broader, village-wide issues.

Finally, the first occurrence, in the thirteenth century of secondary burial pits, or ossuaries, and their subsequent use, played a significant role in affirming membership in these reconstituted communities (Ramsden 1991; Spence 1994). Ramsden (1991:174-175) notes that a communal burial pit containing the deceased from across the village, being bundled and laid to rest together, would have helped create and strength-en the concept of a common ancestry among village residents, regardless of their pre-village make-up. Indeed, it is tempting to see in community-wide ossuaries a real tangible expression of the emergence of fictive family lineages and the appearance of clans.

Of course, history did not end in the fourteenth century, and Kapches (1981), Timmins (1997a) and Williamson and Robertson (1994), amongst others, all argue that these multi lineage villages were the precursors to the emergence of regional tribes, or possibly chiefdoms (Jamieson 1992, 1999), and ultimately the pan-regional Iroquoian confederacies seen in the seventeenth century. These historic realities were not ultimately tied to a past, single episode of organized warfare, however, but were simply more recent emerging trends of local and regional cultural developments that were very much tied to the long term history of these people, and their individual and group responses to opportunity and contingency through time.

Some Final Thoughts

Taking an historical perspective that can accommodate a multi-scalar examination of broad events through time and the agency of the people who left behind the archaeological record, rather than just focussing on the debris of that record, tells a different story to that which arose from the construction of the Ontario Iroquois Tradition 35 years ago. But that is not to say the OIT was somehow a "wrong" effort. We all of us, after all, inform our responses to contingencies and opportunities within an historical context. The OIT was developed based on an under-standing of the world and the local archaeological record as it existed then, operating within accepted conventions and understandings of cultural behaviour and archaeological formation processes — sort of an archaeologist's habitus, if you will. As well, there is no denying the utility of the OIT construct to order and classify a complex and diverse Woodland archaeological database. Certainly the deep chronology and broad geographic boundaries imposed by the OIT were a convenient way of grouping and splitting intuitive similarities seen in the archaeological record. This step simply had to occur for archaeological thinking to advance (cf. Trigger 1999).

However, these spatial and temporal orderings of material culture were also believed to convey social and cultural meaning about past peoples, reflecting "real" planned group behaviour and overt expressions of identity. This belief has been assailed almost from its birth, as newer, and more detailed regional data emerged. This, indeed, is a fatal flaw in all established normative culture histories, in that any descriptive presentation of trait lists can become out of date with the next bag of artifacts or site map brought back from the field. So, for the OIT, newer data increasingly revealed that cultural-geographic labels arbitrarily incorporate many actual past political, community, family and religious boundaries. Moreover, as archaeologists began to think about and understand differently what the archaeological database was and what it is we are doing with it, we have come to understand that the archaeological

record may reflect little of how people actually defined themselves and those around them, and more our assumptions of how people and artifacts are connected.

So over the past 35 years, the limitations of the OIT increasingly have become evident and the permanence of the construct questioned. Eventually, the geo-cultural boundaries proposed in the OIT wavered and faded in the face of much, much more data. As this happened, a key assumption in the OIT — that broad geo-cultural political identity could be extracted and mapped for this Late Woodland material record — became untenable. The implication, of course, is that constructs like Glen Meyer and Pickering never really could have reflected actual past cultural units. More practically, as data expanded, these labels have proven to be too arbitrary and unwieldy even as basic categories archaeologists could use to help sort the archaeological data being recovered across southern Ontario for this time period. In a sense, then, the early questioning of Wright's Pickering conquest could be characterized as a first reflection in the archaeological community of the limitations inherent in the OIT construct, and the struggle involved in accommodating or changing the construct to fit newer data. To me, this explains why the conquest debate never boiled over into a truly animated data-for-data exchange. Archaeologists, conceptually, had simply moved on. We ask different questions now, expect less of the data and try to recognize our own assumptions and preconceptions more overtly. As such, the stories being told today arise from differing perspectives of the past, of how that past was formed, and how we shape our understanding today of that past.

In thinking about the notion that we are all but products of our time, I recall Last's (1995:154; cf. Gero 1995; Renfrew 1978:94-95) discussion of the contingency of archaeological discoveries. This questions a key formational construct of cultural historical models — the tendency to define an archaeological culture on the basis of a type-site, which has become the type-site by virtue of the primacy of its discovery. Renfrew argues that the entire, subsequent dell-

nition of an archaeological culture, and its geographic extent, based on perceived similarities with the point of reference that is the "type site" will be arbitrarily defined by the coincidental and fortuitous first investigation of that one particular site, regardless of whether or not it is in fact an appropriate benchmark. Last suggests Renfrew may be overstating his criticism, and more generally underscores Gero's call for archaeologists to assess their own subjectivity. But in reviewing the interpretive trends in southern Ontario Late Woodland archaeology, I can't help wonder what path interpretations would have followed if Wright's original formation had come without aid of the Miller site excavations or his work at Bennett, or if, instead of Miller and Bennett, he was using data freshly recovered from, say, the Van Besien and Calvert sites. Likewise, what would our understanding of the beginning of the Late Woodland be if it were the Holmedale site that had been investigated in the 1970s, and Porteous only just in the last couple of years? No doubt Wright and others would have made different interpretive assumptions on those altered data sets, and I suspect we would be reviewing a very different play now.

Of course, this ability to create a narrative when the archaeological data falls short in revealing "the way it was" is both a necessary device of archaeological history, and a strength of the discipline to speak to the ancient past. But we need to keep in mind that our interpretations are just our own constructed reality of the past, one that can be reassembled on the same data to tell different stories. Indeed, it is safe to predict that in the future we story tellers of today will also find ourselves victim of our time and the blinders that are our current theoretical perceptions and data limitations. If we recognize this fact and that this simply means we need to allow new stories to be told, then we can embrace this essential dynamic of archaeology and actively look for it in our research, rather than fight its inevitability.

and insight, both on this paper, and more generally through an extremely interesting and entertaining term. I would thank my employer, too, specifically for giving me the opportunity at professional development that allowed me to actively think and write, and more generally for providing me with the extremely fortunate and exciting employment position from which to be familiar with so much of the range of data coming out of the field, and to see how that data relates to current understandings of the past. I am, nonetheless, solely responsible for what has appeared here. Not withstanding this, several individuals graciously allowed me to bounce ideas off them and ask questions on views and opinions they've put down on paper previously. They include Chris Ellis, Bill Fox, John MacDonald, Holly Martelle, Dana Poulton, Lisa Rankin, Michael Spence, Peter Timmins, Gary Warrick, Ron Williamson, Jim Wilson and Phil Woodley. As well, the comments of David Robertson, Rick Sutton and Sue Jamieson on the initial submission of this manuscript were insightful and extremely helpful in identifying areas where I needed to tidy up my own sloppy thinking...I hope "The Look" is less sour now. I also am indebted to Rob Von Bitter who graciously compiled a mass of data very quickly, and some on his own time I think, so that specific site location data could be generated. I would also like to make a collective thanks to all who have, or are or will be contributing to the on-going reconstruction of the past; we all rely on each other and our collective findings, whether we like it or not, to achieve any meaning in our individual enterprises. More specifically, I wish to also thank Jim Wright for so significantly advancing our understanding of Ontario archaeology. Notwithstanding my differing view of the archaeological record, I only have that view due to his significant, continued contributions. More importantly, as I have suggested throughout, archaeology is also about archaeologists. I have always admired the way in which Jim conducts himself as being of the archaeological community, not removed from it, and defines, I think, how we all should conduct ourselves as professional archaeologists, as colleagues, and as advo-

cates for archaeology. Lastly, as always and newly, let me thank Little "G" and Little "B."

Notes

[1] However, much of the debate around the "chronological size" of the boxes within the OIT has been of a personal nature e.g., "are you a late Glen Meyer or early Uren *man?*" Thus conflicting data has often been dismissed as coming from an opposing camp ("so-and-so has misidentified this site or is using unpublished data"). This has had the effect of both prohibiting the development of a broad consensus around basic chronological framing, and allows for many minor variants of the same basic construct to be proposed and then ignored by other researchers.

[2] Indeed, it has long been felt by most other researchers that the Bennett site is actually Middle Ontario Iroquois, as demonstrated by ceramics and the presence of semi-subterranean sweat lodges, which only first appear on other such middle phase sites. Given this, it is not surprising that the site should appear more similar to the Uren site than to the Gossens site – but this is a result of chronological closeness, rather than socio-political affiliation.

[3] It is worth mentioning here the curious phenomenon that is the Pearce study. The 1984 reference is to his dissertation, which has long been cited as the first and critical mass of data that demonstrated clear local continuity between Glen Meyer and later Middleport times in the London area – in fact Pearce argued for multiple community coalescence during that time, thus explaining the perception that middle stage villages were larger than earlier ones (e.g., Trigger 198; Dodd et al. 1990; Williamson 1990). This dissertation was eventually published in 1996, largely unchanged from the 1984 text, and with the same argument against a conquest hypothesis (1996:257). However, in an addendum also included in the 1996 publication, Pearce provides a long list of Early and Middle Ontario Iroquois sites from the London area that had been investigated since he completed his dissertation. He then makes the remarkable statement that, in a reconsideration of his earlier work and the additional data presented in the addendum, he now is of the opinion that Wright's Conquest Theory best explains the changes he sees in south-west Ontario at around A.D. 1300. Thus he refutes him-self... *in the same publication. As* Timmins points out, how-ever, while the addendum data affirms that early Middle Ontario Iroquois phase sites exist in the London area (i.e., Uren substage), these data appear to simply further reaffirm local continuity, rather than support discontinuity (1997a:207).

[4] In fairness, it should be pointed out that I have been embroiled in "ethnic" debates in the past (Murphy and Ferris 1990:271-277). This was in response to earlier assertions (Stothers 1979) of an Iroquoian ethno-linguistic affiliation for Late Woodland materials associated with the Western

Basin Tradition, geographically extending around the western end of Lake Erie from southwestern Ontario, southeastern Michigan and into northwest Ohio. Our argument was that the archaeological record for this Late Woodland manifestation significantly diverges from the Late Woodland record seen more to the east in Ontario (i.e., the OIT), which continues to be supported by more current findings (e.g., Cunningham 1999; Lennox 1995; Riddell 1998). Nonetheless this position has bred an on-going campaign of intellectual "one-upmanship," (e.g., Stothers et al. 1994) that has become quite nasty at times (Brose 1997a, 19976 versus Stothers et at. 1997). This is unfortunate since this serves little to advance meaningful discussion and interpretation of the archaeological record, fails miserably to find a way of extracting useful indicators of cultural identity, and speaks volumes of the impact of contemporary archaeological personalities on our practice of constructing the past.

[5] This is also similar in timing to the appearance of the Younge phase in the Western Basin Tradition, which Smith (1997:60) suggests is really the start of the Late Woodland in southwestern-most Ontario.

[6] To date, only Parker's (1994) ARM report of a number of Early/Middle Woodland site excavations on the lower Grand River is available. This report indicates that at least one site (O'Hara A) was a long occupied seasonal camp with only vague settlement data, including a large scattering of formal and informal cultural features. Diagnostic ceramics were limited, but included dentate stamp decoration such as rocker dentate, i.e., fairly typical ceramics for the Middle Woodland.

[7] The presence of American eel remains at the Holmedale site in Brantford — a species that is thought not to have extended beyond Lake Ontario to the rest of the Great Lakes because of the barrier of Niagara Falls — suggests direct travel to the Lake Ontario drainage by this Grand River group, or trade with groups who did have access to this area (Thomas 1999:90).

[8] Indeed, it seems the distinctiveness of the Niagara region can be seen first in various Archaic manifestations that occur there and nowhere else in Ontario (e.g., Ellis et al. 1990:98, 100, 101, 106).

[9] Jim Wilson's (personal communication 2001) discovery in the summer of 2001 of a hunting camp in northeast London with typical Princess Point rims, but a local pattern of lithic utilization may help document when this westward diffusion occurred or may challenge such a notion, depending on what any dates obtained from the site reflect, and whether or not the ceramics were produced locally.

[10] Though it may well have been the route that a later cultigen, beans, first entered Ontario (Fecteau 1985).

[11] I thank Paul Lennox (personal communication 1999) for bringing to my attention the extensive wild rice stands that

were mapped in this area by the nineteenth century archaeologist Peter Pringle.

[12] I use this term as a means of referring to the ubiquitous scattering of posts and isolated features often seen across EILT village sites, as distinguished from more primary (houses or palisade walls) or secondary patterns (formal internal walls, other discrete buildings or functionally specific areas such as middens, burials, etc.). This tertiary scatter, occurring both inside and out of formal houses, presumably reflects episodic construction of temporary structures, small fences or windbreaks, structural repairs or isolated post placement occurring before, during and after individual house use.

[13] Jim Wilson's ARM company (Archaeologix Inc.) has been investigating a series of early Late Woodland sites on the northwest end of the Caradoc sand plain over the last few years. His investigations have revealed a series of seasonally limited occupations with material culture, especially ceramics, consistent with material culture further west (i.e., Western Basin Tradition), and not similar to the Caradoc EIIT materials Williamson (1985) documents. While still awaiting analysis and radiocarbon dating, this data may further support the notion of the Caradoc EILT being a recently arrived community onto the sand plain (Jim Wilson, personal communication 2000).

[14] It is worth noting that extensive EILT and later archaeological manifestations are known in the east and central Elgin County region between the Thames River and Lake Erie, within 30-40 km of the Calvert site (e.g., Poulton 1980; Jim Wilson, personal communication 2001). This community may have also "fissioned" off from the Long Point region, or could be the source group for either the Calvert or Caradoc Sand Plain communities. The archaeological record in this area, however, has been subject to so little intensive investigation that nothing more can be said at this point.

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