Writing (pre)history: narrative and archaeological explanation in the New Guinea Highlands

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Abstract

The role of narrative in explanation has received considerable attention in most of the disciplines concerned with questions of historical process, including history, geology, psychoanalysis and palaeo-anthropology. Archaeologists, however, have been curiously reluctant to consider the proposition that their reconstructions of the past are fundamentally narrative in character. An argument is put forward for the serious study of narrative in archaeology, and three case studies from the prehistory of the New Guinea Highlands are presented in support: a brief review of the debate over the impact of sweet potato on Highland society; an analysis of the changing interpretations of the Kuk Swamp agricultural site by Jack Golson; and a summary of the role of indigenous narratives in accounting for the history of wetland drainage amongst Huli speakers in the Southern Highlands of Papua New Guinea.

Introduction

This paper considers the ways in which Jack Golson and other researchers working in the New Guinea Highlands have sought to explain the past — how they have conceived of the relationship between archaeological data and explanation, and how they have achieved consensus at different junctures over the past 35 years. My working assumption is that archaeological practice already contains ‘a significant wisdom about the nature of archaeological reasoning’ (Ballard 1995a:212). The challenge is not to trawl through other disciplines in search of better theories with which to tackle the problem of explanation in archaeology but rather to understand more closely the structure and nature of existing forms of explanation. Archaeological explanations evidently work, insofar as the discipline is able to proceed with shared understandings of plausibility, but it is not clear that archaeologists necessarily understand how or why their own explanations succeed in convincing their colleagues. What I seek to explore in this paper is the contention that archaeological explanation develops within and through frameworks that are fundamentally narrative in character.

Furthermore, recognition of the role of narrative in archaeological explanation, far from undermining archaeology’s pretensions to the status of a science, may allow us to generate explanations which are more fertile in terms of the questions that they raise and the spaces that they open up for further enquiry. To realise this potential, we need first to describe the way in which narrative functions in archaeological explanation. As the philosopher Paul Roth observes, ‘without a theory of narrative explanations, there exists only an unanalyzed practice, a habit tolerated but not at all understood’ (1989:453).

Another, more pragmatic consideration has to do with the current state of archaeological research in the New Guinea Highlands, where difficulties of access have seen a dramatic decline in active research since the heyday of Golson’s work at Kuk and elsewhere during the 1970s and 1980s. In the absence of regular infusions of fresh data from new excavations and surveys, historians of the Highlands will increasingly have to develop novel and more subtle ways of interrogating and combining existing materials from archaeology and related disciplines; refining our analyses and interpretations in a return, perhaps, to the period during the early 1960s when researchers such as James Watson, and Susan and Ralph Bulmer speculated about the pre-colonial past on the basis of little or no archaeological evidence (see discussion of Watson below, S. and R. Bulmer 1964). What I want to suggest is that this need not necessarily be a bad thing. This last point essentially prefigures the general argument of this paper which is that archaeology, like other historical disciplines, has never been reliant on fresh material, to the extent that certain of its practitioners might have us believe, in order to advance its understanding of the past; advances in our understanding of the past can, and invariably do proceed in the absence of new material.

It is necessary first to sketch something of the field of debate in narrative theory, if only to situate my own position on narrative while allowing for the possibility of alternative approaches. The core of the paper then consists of three case studies in Highland archaeology: the first a debate during the 1960s about the impact of sweet potato, the so-called ‘Ipomoean Revolution’, and the second an examination of Golson’s explanations for wetland reclamation and abandonment at the Kuk Swamp site. In the third case study, I draw on the narratives that

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Huli-speakers of the Southern Highlands of Papua New Guinea tell about their own past, in an attempt to show how archaeology (and by extension other historical disciplines) might explore the use in combination of different cultural narrative forms in explanation.

Historical events, narrative facts and consensual truth

Like memory, history and archaeology essentially produce, rather than reproduce, the past (Spence 1991). But if most historians and archaeologists now accept and happily intone the dictum that they cannot reconstruct the past in the historicist or Rankean sense, 'as it really was', they are often less comfortable with some of the implications that flow from this position. Unknowable in its full complexity, and largely unrecoverable, the past is never available to us in a coherent form, simply waiting to be documented by historians and archaeologists. Even where the documentation available for an event is excellent, the scope for different interpretations and different re-tellings of the same event is vast: 'any given set of real events can be emplotted in a number of ways, can bear the weight of being told as any number of different kinds of stories' (H. White 1987:44).

Narrative theorists such as Hayden White (1978a, 1992) distinguish between three broad categories of accounts of the past: the archive, the chronicle and the narrative. Information about the past is not necessarily historical — past events become historical only as they become incorporated within our accounts of the past. There is then a sense in which past events, unmediated by reflection on their historical salience, constitute an archive. This distinction is not unproblematic but provides a useful base on which to develop the contrasting notions of chronicle and narrative. The chronicle is an account of the past in which events are ordered in the sequence of their occurrence, but it is only in narrative that we find the events of the archive and the chronicle engaged in sequences that seek to establish a formal coherence for the past.

History, from this perspective, consists not simply of events but rather of events presented in a meaningful sequence. The act of arranging or narrating a series of events in a particular sequence is the process described as emplotment (H. White 1978a) or configuration (Carr 1986a). In thus connecting events to one another, emplotment implies a relationship amongst the events that is not 'immanent in the events themselves' (H. White 1978a:94); its significance is not simply chronological. The act of emplotment is thus a critical interpretative moment in the writing of history. If the same set of events can be presented in a variety of sequences, then it is the structure of the sequences, rather than the nature of the independent events, that gives meaning to a particular narrative: 'Histories, then, are not only about events but also about the possible sets of relationships that those events can be demonstrated to figure' (H. White 1978a:94).

Narrative, formally defined, is 'a linguistic form for revealing temporal significances imbedded in human action, significances that are constitutive of what the action is' (Johnson 1984:222), for 'what is of historical interest, is characteristically known only after the fact' (Roth 1988:7). Where an 'ideal chronicle' seeks to present, as completely as possible, a factual record of events as they occurred and as they were understood at the time, a narrative selects particular events as significant, in light of earlier and subsequent events, and in line with the particular moral cast which the narrator wishes to impart to the narrative.

The moral quality of narratives is crucial both to their definition as narratives and to their success in promoting a particular account of the past. As Roth observes, 'There is no truth-value... to the statement that such and such a happening is tragic: there is only a telling which so presents it' (1988:12). As we shall see shortly, a revolution is revolutionary only under the terms of a specific type of narrative context. Misia Landau makes the same point in relation to the identification of 'transitions' and 'crises' in narratives of human evolution: 'Events are not inherently crises, however, nor are they transitions; they acquire such value only in relation to other events, in a series' (1984:267). The selection of one narrative from amongst others, or of one form of narrative closure over another, is an ethical or moral choice which tends to betray the narrator's understanding (conscious or otherwise) of the relationship between the individual narrative and grander, over-arching meta-narrative forms. Examples of meta-narratives include Christian meta-narratives of redemption, prefigured most obviously in the Biblical Fall, and the multiple forms of modernist meta-narratives of progress, including Marxist theories of social evolution, and conventional archaeological accounts of transitions from simple to complex, or from egalitarian to non-egalitarian social formations. Thus the transition from hunter-gather to agricultural economies is construed by Neolithic specialists as a progressive historical move, while their Mesolithic counterparts write of it as something akin to the Biblical Fall, nicely illustrating the differing moral casts of different narrative perspectives.

But if narrative is more than a simple chronicle, more than a mere sequence of events, it is also more than just a story, with a beginning, a middle and an end. Narratives have a structural unity unto themselves, such that they form more than the sum of their parts, more than an inventory of the events selected or emphasized in the narrative: 'a historical narrative claims truth not merely for each of its individual statements taken distributively, but for the complex form of the narrative itself' (Louis Mink, in Roth 1989:456). Narrative structures are often sufficiently familiar, or predictable, for readers to discern their moral intent even in the absence of a clearly delineated sequence from beginning to end. Individual elements of a narrative thus possess the ability to invoke the whole through a part: the journey home can stand for the homecoming, the storm cloud for the deluge. Indeed, the more sophisticated the narrative, the less obvious the

specification of its narrative qualities, of a formal beginning, middle and end.

For our purposes here, two broad approaches to narrative can be distinguished: a constructivist or formalist approach, and a narrativist or phenomenological perspective. Although these two perspectives are commonly featured in opposition to one another, insights from both can be combined to provide a more comprehensive theory of narrative. The work of Hayden White is often characterised as constructivist owing to his emphasis on the formal properties of narrative and the fundamentally rhetorical form of history-writing. White insists on the encompassment of history — of any knowledge of the past — by language: 'Every verbal proposition about a particular thing existing in an absolute past... refers us to an entity which we know only through another verbal proposition' (1978b:7, original emphasis). If we accept this proposition, however, the existence of any morally uninflected account of the past that could be designated as either an archive or a chronicle is open to question. Events are initially communicated and knowledge of them is rendered communal via language, and they are thus already and unavoidably selected, moralized and narrativized.

According to White, the narrative coherence of our accounts of the past is not a function of verisimilitude with respect to the available facts, but rather an imposition on the historical past, structured along the lines of one of a limited number of rhetorical strategies. White turns to classical European theories of rhetorics to identify these rhetorical or figurative strategies as one or other of four basic master allegories or 'tropes': metaphor, metonymy, synecdoche and irony. All history, White would argue, is written in one or other of these four tropological modes, and it is the familiarity of these modes, even to those unaware of their function, that furnishes histories with plausibility and authority. If the raw material of history always comes to us in the form of one trope or another, the construction of a narrative, White argues, consists of the translation of a narrative encoded in one tropological mode to another — an endless process of decoding and recoding (1978a:96). But White's method, which he describes as 'tropology', is in many ways too formal for the analysis of all but the most complex narratives or meta-narratives, and offers little to the analysis of narrative fragments or utterances, or to our understanding of narrative structures in everyday life.

In contrast to the constructivist perspective on narrative as an exclusively intellectual act, phenomenologists such as Paul Ricoeur (1984–88) and David Carr (1986a, b) insist that there is a pre-narrative structure to human action in the world; that our narratives come to us through the practice of experience. Invoking Husserl's description of time-consciousness, in which our experience and understanding of the present is situated within an endlessly dialectical process of anticipation (or protention) and remembering (or retention), Carr argues that 'narration... is constitutive not only of action and experience but also of the self which acts and experiences' (1986b:126). In seeking to 'occupy the story-tellers' position with respect

to our own lives' (1986b:125), the resulting narratives go beyond simple chronicles in attempting to furnish our lives with moral or ethical meaning.

'Second-order' narratives, such as historians' accounts of the past, draw on these daily or 'first-order' practices of narrative construction through which people make their lives meaningful (Carr 1986b:131). Actions are inevitably interpreted through their conception and performance (in an experiential register), and on subsequent reflection (in a reflective register). Humans, possibly universally, thus narrate their lives to themselves and to others, and — crucially — act upon their narratives. Psychoanalysts find useful the notion of a tension between narrative order and disorder, arguing that people construct sense from the chaos of their lives, and noting the terror 'that lies behind not knowing who you are, what happened yesterday, and what will happen tomorrow' (Spence 1983:458).

In extending our understanding of narrative from its more limited constructivist function as historiography to the wider world of daily, practical activity, the phenomenologists also usefully introduce the role of other social factors in forming and affirming narratives. The common elements of individual narratives, those elements that find recognition in an audience, form the basis for communal narratives and a common identity. The crucial insight that this introduction of social factors offers is the role of consensus in affirming the 'truth' of a narrative. Narratives must thus speak both to common standards of 'factual' truth, and to historically and culturally specific rhetorical conventions of representation.

Historical truth, as we have observed, is an essentially unknowable and irretrievable category. The more relevant index of truth, as the psychoanalyst Donald Spence puts it, is 'narrative truth' (1983:471). For a narrative to be considered 'true', and to serve as the basis for practical action in the future, as well as further narratives, it must be persuasive and be accorded a consensual recognition by the community as 'true'. Persuasion hinges upon a variety of means, including arguments from exclusion (those that explain previously inexplicable events), parsimony or reduction, the principle of frequency and familiarity, and the 'here-and-now' fit, or the ability of a narrative to mesh with current experience (Spence 1983:461–2). In the longer run, the acceptance or success of a narrative turns upon its value in continuing to raise further questions, and its flexibility in terms of accommodating new information or incorporating other narratives: 'Narrative explanations are accepted for fundamentally the same sorts of reasons as any explanations are — because they achieve our purposes, because they fit the accepted evidence, because they provide fruitful new lines of inquiry' (Roth 1991:190).

A particularly illuminating analogy for narrative is that of the constellation, offered by Roth (1991:186):

Historical narrative may be likened to constellations. No one, I presume, denies that there are constellations. Yet there are no constellations without the presence and cognizing activity of human perceivers. Constellations are

not created *ex nihilo*, but neither does it make sense to claim that they exist apart from an order imposed on certain objects. The fictitious nature of constellations is no bar to their playing pragmatically important roles in human affairs; for example, they may aid in navigation.

Like events, individual stars can be arranged in an infinite number of series. Note too, however, that events, like stars, are never entirely randomly ordered; the size and proximity of stars within a constellation, like the chronological sequence or apparent significance of events, lends them a semblance of 'naturalness' or immanence. Like constellations, narratives — as narratives — cannot be right or wrong; there is no natural plot for them to follow.

This said, a narrativist critique of history does not necessarily entail an extreme relativist position on the question of truth. References to events, to historical or archaeological data, are individually verifiable, at least within tolerable limits; but the processes of emplotment, the arguments that link events together in a narrative structure, are not. Yet there are obviously means by which we evaluate the relative truth or value of different narratives. Some stories "work" for us, or "speak" to us, and others don't. However, the relative worth of different stories is not fixed, either immutably for all time, or for all readers or purposes. A particular story may come in and out of favour and fashion without significant change either to the events or the sequence of their telling. Instead, we judge amongst narratives from a position of contextual truth, determining which story "speaks" to us at a given moment, in the terms of our interests and purposes at that time.

Finally, like constellations, narratives have a historical and cultural specificity — just as there is little match between ancient Greek and contemporary Polynesian constellations, so too we could hardly expect our classical European narrative tropes to encompass the narrative registers of other times or other places. If narratives morally invigorate practical activity in the world, then it would seem essential not only that we attempt to historicize our understanding of past rhetorical strategies (see De Bolla 1986:56), but also that we concede the likelihood of significant cultural variation in the composition, constitution and acceptance of narrative. This, then, is the importance I would attribute to a project that seeks to understand the cultural specificity of other forms of narrative, other conceptions of the past, other historicities — something I attempt in a preliminary manner in the third case study of this paper. To consider the extent to which my argument is any way distinctive in its claims for the value of narrative in archaeology, a brief overview of archaeological writing on narrative is now required.

Narratives in "prehistoric" archaeology

The narratives of prehistoric archaeology are doubly estranged from the first-order narratives of the lives of historical subjects. Archaeological narratives by their nature usually rely upon a decidedly more tricky reading of partial traces of acts or materials (monuments, arte-

facts, landscapes etc.) which are themselves partial accounts of past lives, and thus lack the apparent formal coherence and exegesis of documented narratives. Possibly because these first-order narratives are not commonly accessible to prehistoric archaeology or preliterate history in the manner, say, of mediaeval chronicles, there has been a tendency amongst archaeologists to view the reconstruction of biographical or alternative cultural forms of narrative with some suspicion, as an unnecessary diversion from a proper attention to material 'facts'.

Owing perhaps to the advantages of hindsight and distance on past debates, historians of science such as Misia Landau and Martin Rudwick have led the way in introducing narrative analysis to palaeo-anthropology and geology, respectively. Landau's (1984, 1987, 1991) treatment of historical debates over human evolution draws heavily on Vladimir Propp's structuralist analyses of folk tales, and adopts the formalist approach of Hayden White to narrative. In Landau's view (1987:112), events in the course of human evolution, such as the descent to the ground, are not 'inherently transitional ... however momentous the occasion'; they only acquire the status of turning point 'in relation to our overall conception of the course of human evolution'. The significance of an event in human evolution thus rests on the particular narrative in which it is positioned, and Landau is able to marshal her evidence from the writings of nineteenth- and early twentieth-century writers to considerable effect. In particular, she demonstrates how critical 'episodes' in human evolution, such as terrestriality, bipedalism and encephalization, can acquire very different meanings according to their sequence in different narratives.

Rudwick's (1985) less formalist analysis of the Devonian controversy in geology focuses more directly on the shaping of consensus amongst early 'gentlemanly specialists', and thus demonstrates a keen awareness of the role of culturally and historically specific forms of narrative in debate, albeit still within the broad tradition of Western European scientific thought. The Devonian controversy, argues Rudwick, was ultimately settled not solely through appeals to the 'facts', but rather through a 'social process of argument' culminating in a 'virtual consensus around an interpretation that had not been anticipated by anyone at the outset' (1985:450). The raw material of facts in the debate, Rudwick concludes, is rendered knowledge only through this social process of debate: 'the actively interpretative work of perception is increasingly constrained by the addition of further information, yet at no time is it determined by it' (1985:455, original emphasis). There is, in addition, a reflexive sense in Rudwick's account of the narrative qualities of his own argument and thus its role within an equally social process of contemporary debate.

Both Landau and Rudwick emphasize the dual challenge posed by teleological thinking: the role of assumptions about the inevitability of a particular end-point in a historical process in the thinking of scientists engaged in a debate (such as that over the evolution of anatomically modern humans), and the danger for the historian of

treating the final consensus in a debate as the only possible conclusion (and thus disregarding alternative lines of argument or evidence). Narratives which 'naturalize' a particular outcome in this way tend to conceive of history, and of causality more generally, as a single chain of problems and responses, leading ultimately to an almost unavoidable solution — a logic which, as I suggest below, has characterised much archaeological writing in the New Guinea Highlands.

Although an awareness of the role of narrative in archaeology is a relatively recent phenomenon, a distinction between formalist and phenomenological approaches can be discerned. John Terrell (1990, 1996, Terrell and Welsch 1997), evidently strongly influenced by the work of Landau, has presented a formalist analysis of the debate over Lapita. Terrell usefully introduces a heightened awareness of language to the writing of Pacific prehistory, showing how Lapita figures inevitably as an 'ancestral' moment in the later prehistory of Remote Oceania, and neatly identifying some of the rhetorical strategies pursued in a bid to assert consensus: 'most prehistorians agree...' is a Terrell favourite (1996:58). Proposing that the power of a particular narrative structure, which features 'Lapita culture' in a heroic role, has come to overwhelm other narrative possibilities, Terrell also observes that the exceptional wealth (by regional standards) of archaeological material generated through recent work on Lapita has ironically produced a situation in which 'we know less about Lapita as an empirically observable record of past human activities than some thought we did between 1961–1989' (1996:58). Terrell's own treatment of narrative has become increasingly sophisticated, moving from a stricter formalism (1990), to an interest in rhetoric and consensus (1996) and subsequently to consideration of the merits of multiple narrative strands (Terrell and Welsch 1997). Throughout, however, Terrell appears unsure about the relative worth of narrative analysis in contrast with a more conventional comparison of competing models (e.g. 1990:21), a doubt that I seek to address in the conclusion to this paper.

European archaeology, with particular reference to the debate over the Mesolithic-Neolithic transition, has produced what may be a more productive approach, sensitive equally to the multiple contexts for narrative meaning and to the more reflexive possibilities offered by an awareness of narrative. Mark Pluciennik's (1999) recent review of the role of narrative in archaeology is the most comprehensive treatment of the topic to date, building on the work of Thomas (1991) and Hodder (1993) amongst others. Pluciennik offers a history of the ways in which the transition to farming in Europe has been written, tracking the shifting moral valuations of the Mesolithic-Neolithic transition, from gradualist evolutionary narratives framed by a romantic meta-narrative of progress, to the challenge of more ironic, fractured anti-narratives produced during the 1990s which have sought to disrupt the teleologism of earlier writing. In conjunction with this growing awareness of the presence of meta-narra-

tives in archaeological thought, a process of exploration of writing and of the role of the author in archaeology more generally has emerged, influenced strongly by similar moves in cognate disciplines such as anthropology (Clifford and Marcus (eds) 1986); works within a new vein of reflective and reflexive writing are amongst those with a particular focus on narrative (e.g. Gero 1991, Spector 1993, Hodder 1993, and Bender, Hamilton and Tilley 1997, Joyce 2002). There are important links between the concern in these latter works for the personal and the ethical, and the harnessing of narrative in order to understand archaeological explanation more broadly, but I want here to focus more narrowly on the link between narrative and questions of causality and sufficiency in explanation, to which I return in further detail in the conclusion to this paper.

Each of the three case studies to which I now turn illustrates a different quality of, and a different approach to, the question of narrative in historical explanation in the New Guinea Highlands. The first study seeks to establish the value of thinking in a narrative mode, and of maintaining multiple narrative possibilities; the second proposes that attention to the narrative elements of archaeological explanation might allow us to refine our appreciation of the contingent nature of reconstructions; and the third opens up a cross-cultural arena of radical narrative alterity.

Case 1 — The Ipomoean Revolution: James Watson and the "perilous quagmire of conjecture"

The first of my three case studies, which concerns the debate over the impact of the introduction and adoption of sweet potato in Highland history, is of interest here because it was conducted in a near-vacuum of archaeological evidence. In two papers published in 1965, the anthropologist James Watson (1965a, 1965b) proposed that, 'the Highlands which ethnographers have been examining for about three decades do not, in many fundamental respects, represent a long-established or stable situation, socially or culturally' (1965a:442). The dependence of Highland societies on sweet potato was apparent even to the first Europeans to visit the region. Yet, argued Watson, as sweet potato was known to be South American in origin, its adoption in the Highlands, presumably at some date after its introduction by the Spanish to the Philippines during the sixteenth century, must have occasioned a revolution not unlike the adoption of the horse in the Great Plains of North America, or of the white potato in Ireland or Eastern Europe. Nothing written since on the adoption of sweet potato in the Highlands has matched the wealth of ideas that Watson presented in these first two papers.

Watson raised a series of topics for debate that remain critical for current research. Amongst these are his suggestion that sweet potato, as a central factor in the production of the primary Highland wealth item, the pig, would have revolutionized society at large (1965a:444). Observing

that many groups attribute specific genders to different crops, notably, maleness to taro and femaleness to sweet potato, Watson suggested that the adoption of sweet potato may have led to a revolution in the gender division of labour, from a male labour-intensive taro economy, to a female labour intensive sweet potato system (Watson 1965a:445). He also proposed that the new crop could have enabled a dramatic increase in population and consequent increase in warfare, but he tempered this with a crucial qualifier: that the acceptability of sweet potato would have varied from group to group, because the advantages that sweet potato is now recognised to have over other tubers may not have been apparent either universally or immediately. Pre-Ipomoean variation would thus have structured post-Ipomoean changes, a thesis subsequently developed and elaborated by anthropologist Daryl Feil (1987). The importance of indigenous perception formed a constant theme in Watson's papers: 'We need a notion of this revolution', he stated, 'that does not neglect the sociological factors in favour of the simple concept of a more efficient commissariat rationally chosen by men afforded the choice' (1965a:442); this is an argument which geographer Harold Brookfield (1972) would later expand upon in proposing a distinction between 'social' and 'subsistence' forms of production.

These and other insights Watson then brought together in proposing four different models, or narrative possibilities, for the impact of sweet potato (1965b:302–303). The first envisaged a pre-Ipomoean economy founded on foraging and radically transformed by the adoption not just of a new crop but also of the concept of agriculture. The second allowed for pre-Ipomoean agricultural systems in which sweet potato replaced the earlier staple, taro, but also extended agricultural settlement to new environments. The third, more subtle possibility incorporated the first two models in envisaging a suite of differing pre-Ipomoean economies, which ranged from agricultural to foraging strategies and thus experienced a variety of different impacts. The fourth, a 'logical possibility' (Watson 1965b:303), was that dense pre-Ipomoean agricultural populations simply, and more gradually, incorporated the new tuber within their subsistence economies. In retrospect, we might now conclude that each of Watson's models held true for at least some part of interior New Guinea. However, at the time and in the absence of relevant archaeological evidence, Watson opted exclusively for the first model, for a foraging pre-Ipomoean economy and a radical revolution.

Unfortunately (note here the moral cast of my own narrative!), within a year of the publication of Watson's papers, a digging-stick from a drainage ditch at the newly discovered Manton's site in the Wahgi valley had yielded a radiocarbon result of 2300 BP (Golson *et al.* 1967, see Appendix). A three-day seminar was duly organized by Brookfield and White at the Australian National University in Canberra in 1967 to discuss Watson's proposed Ipomoean revolution. From the various extant copies of papers given at the seminar (Dornstreich 1966, J. P. White 1967, Wheeler 1967) it

seems that attention was directed solely to Watson's preferred model, of a radical revolution from hunting and gathering to agriculture in the course of a few centuries; no-one addressed Watson's three alternative models.

In their published report on the seminar, Brookfield and White stated their preference for 'a more evolutionary interpretation, which is more closely analogous with better-documented agricultural transformations elsewhere' (1968:44). The new archaeological evidence for agriculture as early as 2300 BP seemed to them 'most compatible with an hypothesis of slow growth' (1968:47), and they concluded by affirming 'a position in which the introduction of the sweet potato might cease to be of major relevance to the development of agricultural methods in the New Guinea Highlands. No Ipomoean revolution is necessary to explain the evolution of intensive methods of agriculture, and the question is thus turned back properly to that of population concentration' (1968:50). Brookfield and White appear to have been hedging their bets here, in focusing solely on the impact of sweet potato on intensive agricultural systems, which was not entirely the focus of Watson's thesis. They speculated briefly on the possibility that the adoption of sweet potato might have led to 'some empirical modification of already evolved cultivation techniques, some shifts in population, and especially an expansion into areas of higher altitude' (1968:50–51), but here they felt themselves 'already far out on the perilous quagmire of conjecture... we should perhaps go no further than record our view that this evolutionary hypothesis... is in greater harmony with the evidence than any theory of recent, sudden, and sweeping revolution' (1968:51).

For all their rhetoric about harmony with the evidence, Brookfield and White were essentially substituting their own preference for an 'evolutionary' narrative for Watson's 'revolutionary' account. Yet, with the wisdom of hindsight, it seems that Watson's notion of an Ipomoean Revolution has had more influence subsequently on research into the history of Highland society than any other body of writing. The Manton's site digging-stick aside, Brookfield and White's evidence for gradual change was not altogether different from Watson's 'evidence'. If I suggest that the Manton's site discovery was in some respects unfortunate, it is because the significance of Brookfield and White's response was to close down debate, for a decade, on the range of Watson's other, more astute, observations and his sense of narrative possibility and variation. Not until Nicholas Modjeska's (1977) seminal thesis on production among the Duna, and Jack Golson's (1982b, see Appendix) response to Modjeska, would these issues again play a part in the writings of archaeologists. Watson's 1965 papers, and his 1977 elaboration of the notion of the "Jones Effect" in promoting change among neighbouring communities, have also exerted a profound influence on the subsequent work of anthropologists and human geographers concerned with the historical genesis of Highland society, such as Modjeska (1982), Feil (1987), Weiner (1988), and Allen (2001).

The critical difference between the two competing narratives (the evolutionary and the revolutionary) resided in Watson's more expansive sense of narrative possibility and crucially, I would argue, his sense of the continuing dynamism of contemporary Highland societies — a rejection of narrative closure, in other words. While the evolutionary narrative appeared, at least in 1967, to be 'in greater harmony' with the archaeological evidence newly to hand, it lacked the flexibility and the scope for extension into the future possessed by Watson's more sociologically nuanced revolutionary narrative, and his awareness of narrative variety as a spur to elicit alternative explanations.

Case 2 — Reclamation and abandonment: Jack Golson and Kuk Swamp

The second case study considers Golson's writings on the major agricultural site of Kuk Swamp in the Wahgi

Valley, which offer thirty-five years of interpretation drawn primarily from archaeological evidence. The sheer glut of material evidence from Kuk has proved an embarrassment of riches, and basic data are still emerging in publication. Constrained to deliver provisional interpretations of the Kuk site for successive collections and conferences, Golson has authored or co-authored more than thirty-five different papers about the site (see Appendix). Together, these papers constitute what is perhaps the single largest corpus of writing on any one site, and one of the larger bodies of related output by a single archaeologist, in the Pacific.

The Wahgi Valley swamp ditch systems were first rediscovered in 1966 at Warrewau Tea Plantation. Excavations in the same year at what came to be known as Manton's site confirmed the presence of a stratified series of complex ditch networks and provided the first date of 2300 BP (Golson *et al.* 1967, see Appendix, Lampert 1967). From Manton's, attention shifted in 1968 to the Kindeng site. Then, after Jim Allen found the Kuk

Phase	Process	Period	Narrative Interpretation
1	R	>9000 BP	An early experiment in wetland modification (Golson 1992c), probably with indigenous cultivars (banana, sugarcane, pitpit), and possibly with taro.
	A	c.9000 BP	Reason for abandonment unknown. Possibly just a shift to other wetland locations (Gorecki 1986). Known channels silt up.
2	R	?6000 BP	A further experiment which "mimics" Phase 1 (Golson 1992c), this time with taro as the principal crop.
	A	5500 BP	Abandonment is forced as silt from dryland clearance clogs channels.
3	R	?4000 BP	Pressure on dryland resources, marked by the low point for forest/non-forest values, forces wetland reclamation.
	A	2500 BP	The adoption of tillage, marked by the appearance of soil aggregates, permits intensive use of the drylands, relieving the need for wetland agriculture.
4	R	?2000 BP	Pressure on dryland resources again forces wetland reclamation (Golson 1977–1989); more recently, this has been seen as evidence for a colocasian (taro) revolution (Golson and Gardner 1990, Bayliss-Smith and Golson 1992).
	A	1200 BP	Due either to the early arrival of sweet potato (Gorecki 1986, Golson 1977a, 1990), or to the adoption of casuarina fallowing techniques (Golson 1977a), further extending the scope for dryland use and thus precipitating the abandonment of the wetlands. Abandonment is not explicitly linked to the concurrent Olgaboli ashfall.
5	R	?400 BP	Pressure on dryland resources again forces wetland reclamation.
	A	250 BP	Partial abandonment sees a reduction in wetland use to 25% due to the adoption of sweet potato.
6	R	250 BP	Continuous, but reduced use following from Phase 5, employing raised bed technology.
	A	?100 BP	Total abandonment, possibly related to malarial or other epidemics (Gorecki 1979). Raised bed technology from the wetlands is then transferred to dryland systems.

Table 1: Narrative interpretations for swamp reclamation (R) and abandonment (A) at Kuk. Dates for the phases are uncalibrated and as given in Golson 1982b. All references to papers by Golson are given in the Appendix.

site in 1969, six major field seasons there between 1972 and 1977 produced the bulk of the archaeological material now available.

Although major fieldwork had been completed by 1977, and the broad features of the site more or less mapped and understood, the process of interpretation continues into the present. It is hardly surprising, therefore, that a reading of Golson's writings over time reveals significant shifts in emphasis and understanding. As the list in the Appendix shows, most of Golson's Kuk papers were written after 1977, and it is on this latter body of work, all of it drawing largely on the same material, that I want to focus. A more recent phase of excavation, conducted jointly by Golson and his student, Tim Denham, has yielded further data and interpretation, but this is still very much work in progress (see Denham, this issue), and I focus here on the earlier period of interpretation, from the late 1960s until the early 1990s. An adequate intellectual biography of the work of Golson and his team at Kuk, which this brief discussion does not pretend to attempt, would have to engage with the ongoing flow of ideas published since the early 1990s.

The most striking feature of Kuk, as a site, is the lengthy series of cycles of swamp reclamation and then abandonment of the ditch systems, a process that yields the distinct phases of use and disuse listed in Table 1. For approximately 2300 of the last 6000 years, the swamp has been abandoned, and much of Golson's writing centres on the reasons for this. As Modjeska has noted, '[T]he major problems [at Kuk] have to do with the delineation of phases and their interpretation as a meaningful sequence of stages' (1977:79, my stress). If we regard the moments or processes of reclamation and abandonment at Kuk as events, then the chronologically ordered explanations for these events represent Golson's narrative constructions, his attempts to give the sequence meaning.

Table 1 summarizes Golson's interpretations of reclamation and abandonment for each phase. It should be noted that the complex inter-layering of the chronologies of paper production and publication does not allow a genealogy of his thoughts on Kuk to be read simply from the sequence of publication. However, some common themes can be traced through Golson's explanations, particularly during the 1970s and 1980s. Swamp reclamation was generally interpreted by Golson as a response to stress in dryland agricultural systems induced by land degradation, and swamp abandonment was held by him to be effected as soon as technological innovations permitted a return to dryland systems: 'The agricultural system is thus seen to be in continuous adjustment to the effects of the transformation of the environment for which its operations are responsible' (Golson 1977d:17, see Appendix). Swamp reclamation, under these terms, was treated by Golson as a labour-intensive solution to problems posed by pressure on dryland resources. As discussed earlier, narrative structures which rely heavily upon sequences of problems and consequent solutions tend to reflect a teleological logic, in which the sequence of events is naturalized and rendered seemingly

inevitable, given certain environmental constraints or innovations. The dominant trope in Golson's various narratives for Kuk appears to be that of progress, such that each episode of wetland abandonment could be attributed most readily to a dryland innovation that released communities from the burdens of drainage and allowed them to progress to a new level of productivity.

Golson's interpretations have become increasingly sophisticated over time, incorporating new insights from agronomic, palynological and geomorphological research, but a major watershed in his writing is marked by two papers prepared between 1978 and 1980 (1981c and 1982b, see Appendix), in which he first acknowledged and attempted to address the criticisms of Modjeska. Modjeska's 1977 thesis ushered in a return to the Ipomoean revolution debate and revived many of Watson's original proposals. Modjeska's objections to the tale that Golson was telling for Kuk concerned the role conceived for human agents. Golson, in working rigorously from the archaeological data (as he saw them), had located his explanations largely at the level of environmental change and technological innovation, invoking a determinism that accorded the human agents in the process of swamp exploitation what Golson subsequently recognised to be 'an essentially negative role, acting only in reaction to environmental changes' (Golson 1982b:123, see Appendix). The image of swamps as a fall-back resource during periods of dryland stress had obscured the fact that swamp soils are highly productive. Rather than being forced back on to swamp-land, communities may have been making conscious decisions to produce a surplus of food and fodder for a range of subsistence and non-subsistence demands.

Golson's (1982b, see Appendix) response to the issues raised by Modjeska was his most complex treatment thus far of the questions surrounding the social contexts for production from wetlands; in addition to responding to Modjeska, Golson was also engaging with observations on the critical role in agricultural intensification of pig domestication and production (Dornstreich 1977, Morren 1977). Golson's argument for Kuk now ranged widely across the ethnographic literature, and reintroduced to his accounts a range of narrative possibility that the self-containing universe of technological and environmental explanation could not have allowed. The details of this and other more recent and more sophisticated arguments, most notably his 1990 paper with Don Gardner (Golson and Gardner 1990, see Appendix; see also Bayliss-Smith and Golson 1992b, see Appendix, 1999), need not detain us. The simple point to be made is that a fairly constant body of archaeological data has been made to speak in a variety of ways.

The view still being advanced by Peter White in 1982 (immediately prior to Golson's 1982 reformulation), that 'the archaeological evidence [at Kuk] is a more reliable guide to prehistoric events' (White with O'Connell 1982:187) than Watson's conjectures, fails to account for the fact that the advances in explanation at Kuk have profited as much from changes at the level of narrative

construction as they have from changes in the intrinsic value of the data. Golson's experiments with different narratives at Kuk cannot be seen, as they might under the terms of a naïve empiricism, as an embarrassing failure to "read" the data correctly. Rather the Kuk narratives represent Golson's ongoing engagement with interlocutors from a wide range of disciplines, a dialogue that has spared Kuk from any form of narrative closure and that continues to stimulate fresh questions for research.

Case 3 — Huli historicity and an oral history of wetland drainage

The third and final case study concerns a set of narratives drawn from my doctoral fieldwork with Huli communities in the Tari area of the Papua New Guinea Highlands. This research, conducted under Golson's supervision, was an attempt to explore the social circumstances for the most recent phase of swamp reclamation and abandonment in the Haeapugua basin (Ballard 1995b). At Kuk, where the wetlands had been abandoned well prior to European contact, ethnographic enquiry at this level of detail had not been possible, though Paul Gorecki's enquiries (1979, 1982) offer significant insights. My principal reason for selecting Haeapugua had to do with the reputation of the Huli as historians, with genealogies of exceptional depth — often greater than 20 generations — and a vast corpus of oral narratives about the past; indeed, Watson had originally drawn on the evidence of Huli oral history to advance his claims about the recency of adoption of sweet potato in the Highlands (1965b:300). From the inception of my project, I was interested to understand the relationship between Huli historicity and the nature of their relationship with the landscape. How have the culturally specific forms of Huli conceptions of time and history influenced the ways in which Huli actually engage with their material world? If historical narratives 'contain within them a core of assumptions and analyses of the way that history works, that life is experienced' (Harkin 1988:102), then we need some understanding of culturally different assumptions and analyses of history if we are to appreciate how people in other cultures create identities through narrative, and then create history as they enact those identities in practice.

Huli historicity adopts a particular moral cast, which invokes a specific trajectory for human lives, in the past as in the future. Huli men and women express a meta-narrative of moral decline that is similar in many respects to Biblical notions of history, while also reflecting significant differences (Ballard 1998, Frankel 1986, Goldman 1983). Huli cosmology conceives of a universe in which an originally complete form of knowledge, or *mana*, is ruptured and rendered incomplete through human transgression. From this rupture flow all of the ills of the world, and Huli history is thus a catalogue of inexorable, entropic decline in the condition of the universe and in the morality of humans, culminating in a

cataclysm — the time of darkness, or *mbingi* — which destroys the old world (Ballard 2000). However, this telos or moral closure to Huli history can be forestalled through appropriate ritual interventions. Further, the character of this cataclysm — which can feature either as the total devastation of all life, or as a replenishing, refertilizing renewal of the earth — is subject to the influence of humans, both through ritual performance and through the strict observance by both men and women of moral codes in their daily lives. Previous failures to induce an earth-renewing form of apocalypse are held to have resulted in the eradication of several earlier epochs of human and non-human beings.

A sense of this meta-narrative of entropic decline pervades the vast majority of Huli narratives. It is commonly invoked through cursory references to the drying of the land, or the failure of young men and women to conduct themselves in an appropriate manner, and also consciously reflected in daily practices and in the constant scrutiny of people and the environment for signs of decline. Formerly, ritual performances were a prime site for this interplay between past and future. An awareness of the past, evident in the careful repetition of established ceremonies, was matched by equally crucial innovations, which anticipated a future of arrested decline by attempting to reinstate lost elements of knowledge through a process of experiment.

If Huli historicity is concerned on one hand with the spectre of future decline, on the other it exhibits what amounts to a cultural obsession with the identification of origins and priority in the past (Goldman 1983:78, 93). Huli narratives about the past hinge upon the naming and identification of ancestors who were the first to claim a block of land by digging a ditch around it, the first to hold sweet potato vines, or the first to provoke a war (thus bearing responsibility for the ensuing compensation payments). Intersecting with narratives about specific events, such as a war, are the genealogies recounted by descendants of the participants, in which historical individuals and their relationships are linked, through descent, to the modern narrators and protagonists. A third layer of evidence is the material proof for the claims advanced in either narratives or genealogies. Skulls held in ossuaries, named ditches and drains, and planted trees are amongst the many forms of material evidence that narrators and disputants marshal in support of their claims. It is possible, then, to draw on these narratives, genealogies and material proofs to generate a dense account of Huli history over at least the past two centuries. But what I want to draw attention to here is the fact that narratives about the past, whatever their historical veracity, play a vital role in structuring the present-day actions of Huli people, in activities such as forming alliances, waging wars, and draining swamps. Narrative and action are thus engaged in an endlessly recursive process of inscription and reinscription, and the material landscape bears their trace equally.

Huli oral historical narratives offer the means to generate a detailed account of the history of local wetland

drainage and agricultural production at the Haeapugua Swamp near Tari (Ballard 1995b:172–9). A chronological sequence of events can be charted, opening with the extension and redirection of major natural channels, to be followed by the drainage of most of the swamp area by a network of ditches. Once this relative sequence of drainage is matched against the extensive genealogies for named ditch-diggers, and correlated with determinations for the dates of other events, such as wars, it can then be dated with some degree of confidence. In brief, it appears that drainage of the wetland centre of Haeapugua, which is the largest swamp in the Tari region, was only initiated during the 1860s. Archaeological evidence exists for drainage and agricultural use of the swamp margins at Haeapugua from at least 2500 BP, but the oral narrative accounts remain our only evidence for the shorter history of exploitation of the wetland centre.

From an archaeological perspective, the Huli account is by no means 'complete', being mute on the historical depth of wetland margin use. Geomorphological evidence from the wider region also supplies a broader context within which to understand the history of Haeapugua, suggesting that there were powerful hydrological constraints on wetland drainage prior to the stabilisation of the levees along the course of the large Tagali River, which winds through the centre of the swamp at Haeapugua. Intriguingly, it may have been feasible to drain the wetland centre only after the movement of a substantial population onto poor-soil environments in the Tagali catchment — a move made possible by the adoption of sweet potato — had resulted in land degradation and the release of massive quantities of soil that formed the levees along the Tagali River at Haeapugua (Ballard 2001). Establishing the environmental constraints on wetland drainage, however, brings us no closer to appreciating why drainage should have been initiated at all, even after it became possible.

It is at this point that historicity plays a pivotal role in structuring the nature of Huli engagement with their landscape. Wetland swamps, as centres or repositories of moisture and fertility in a world threatened by desiccation, were probably excluded from use in the pre-*Ipomoean* past. However, Huli accounts of a dramatic increase in the numbers of people and of pigs after the adoption of sweet potato coincide with a perceived acceleration in the pace of the world's decline, gauged against such measures as declining crop yields, lowering water-tables in the swamp, and increasing social conflict. In the process of experimentation with ritual forms in order to restore this declining fertility, Huli appear to have opted for an increasing use of pigs in ceremonies of sacrifice and exchange (see Wiessner and Tumu 1998 for a very detailed account, based on oral historical narratives, of parallel processes amongst the neighbouring Enga). The principal impetus for wetland drainage was thus the increasing demand for pig fodder for these new and more frequently performed rituals, and also as a

means of meeting the requirements of compensation for larger and costlier wars with expanding neighbours (Ballard 2001).

Our ability to furnish a complex account of the recent history of wetland drainage at Haeapugua depends upon a productive and complementary interplay between archaeological / geomorphological narratives and those of the Huli. At one level of explanation, Golson's account of drainage at Kuk as the result of pressure on dryland resources holds true also for Haeapugua. But the richness of the Huli account, particularly once it is set within our understanding of the hydrological constraints on drainage and geomorphological consequences of dryland degradation, provides an altogether more complex, and hence more satisfying, reconstruction. As history it is no closer to being complete than Golson's various tales of Kuk, but it is instructive in furnishing us with a sense of the limits to completeness in any archaeological account of the past. Archaeological reconstruction is not solely a question of what can be explained but also, importantly, a matter of retaining a sense of what cannot be accounted for.

Against closure (some conclusions)

Through these case studies I hope to have established at least some basis for contending that narrative requires serious consideration by archaeologists: as an important, if culturally variable influence on the actions of historical agents and thus on the nature of their material presence; as an integral feature of the very nature of archaeological explanation; and as a form of argument in its own right, with its own, often unspoken powers of persuasion and creation of consensus.

Narratives are similar in many respects to models, in terms of their predictive value in guiding and redirecting research. Yet they are also a particular type of model (for not all models are narrative in structure), with a peculiar logic and structure which is necessarily sequential, or evocative of sequence, and convincing by virtue of being familiar to the audience. If the capacity of narrative to persuade is a power, it is also a trap, and one which requires us to interrogate those forms of narrative, the enabling tropes (De Bolla 1986:57), that are so familiar to archaeology that they suffuse our very conception of what archaeology should be. A narrative is also more than a model, in that narratives influence both the actions of agents in past worlds and our reconstructions of those past actions.

Like paradigms, narratives stand or fall on the basis of their flexible capacity to accommodate and illuminate new insights and new evidence (Roth 1989:468–9). The search for unifying narratives, for a single logic that might underpin archaeological explanation universally, is a misplaced venture. Rather than grading different narratives for some form of absolute truth content, we should be asking which alternative we find the most useful relative to the immediate question at hand. One law to be left

in cleaning out the legislative cupboard must be the defence against the closure that is effected by totalizing discourses — the notion that there is a single truth, a single narrative, a single past out there to be found, and that there is but one way of telling it.

Hayden White observes that none of 'the great classics of historiography... has ever "wrapped up" a historical problem definitively, but has always rather "opened up" a prospect on the past that inspires more study' (1989:25). Under these terms, I would suggest that Watson's 1965 papers, which served to open up the history of Highland societies to a host of new questions, have earned the status of classics. While work on narrative forms in archaeology can never be a substitute for excavation, it has the potential to awaken us to a kind of enquiry and writing which, 'instead of pacifying our will to know, stimulates us ever more to research, ever more discourse, ever more writing' (White 1989:25). To paraphrase Landau (1987:122): 'If we need more sites, we also need more stories. We need larger plots and more complicated ones'. Jack Golson's corpus of tales for Kuk charts his own intellectual progress together with the process of steadily deepening engagement between archaeologists and a complex past. As Golson continues to generate new narratives of his own, and to assist in the provision of a forum for other voices on Kuk, many of them indigenous (see Strathern and Stewart eds. 1998, Ketan and Muke 2001), Kuk begins to assume an additional significance for the very practice of archaeology in the New Guinea Highlands.

Acknowledgments

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Appendix

An Annotated Register of Writings by Jack Golson on the Kuk Swamp Site, 1966-1993

This chronological list of writings by Jack Golson and co-authors includes only those papers that make substantial reference to the archaeological research at Kuk and related sites, and that were published between 1966 and 1993. Notes in square brackets refer to the year in which the paper was originally drafted, and the circumstances of its genesis and production, where these details are known. These notes were prepared on the basis of interviews with Golson during 1993.

- Golson, J. 1966. 'Prehistoric research in Melanesia.' Unpublished paper, 52 pp. [Written in 1966, never submitted for publication.]
- Golson, J., R. J. Lampert, J. M. Wheeler and W. R. Ambrose 1967. 'A note on carbon dates for horticulture in the New Guinea Highlands.' *Journal of the Polynesian Society* 76(3):369-71. [Drafted by Golson in Port Moresby in 1967, with subsequent modifications by others in Canberra.]
- Golson, J. 1968. 'Archaeological prospects for Melanesia.' In I. Yawata and Y. K. Sinoto (eds) *Prehistoric Culture in Oceania: a symposium*, pp.3-14. Honolulu: Bernice P. Bishop Museum. [Written in 1966 for the 10th Pacific Science Congress, Tokyo, in the same year, with the addendum (pp.11-12) added to the galley proofs close to publication in 1967.]
- Golson, J. 1970. 'A hydraulic civilisation in the Wahgi Valley.' Unpublished seminar paper, presented at the Australian National University, 17 pp.
- Powell, J.M., A. Kulunga, R. Moge, C. Pono, F. Zimike and J. Golson 1975. *Agricultural Traditions of the Mount Hagen Area*. Department of Geography, University of Papua New Guinea Occasional Paper No.12. [Last section on archaeology written by Golson in 1974, after the May 1974 Australian Institute of Aboriginal Studies conference to which he refers in 1977b.]
- Golson, J. 1976a. 'Aspects of the agricultural history of the New Guinea Highlands.' In K. Wilson and R. M. Bourke (eds) *1975 Papua New Guinea Food Crops Conference Proceedings*, pp.79-80. Port Moresby: Department of Primary Industry. [Published abstract of a paper delivered at the 1975 conference.]
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- Golson, J. 1976c. 'Archaeological investigations at Kuk Tea Research Station, Mount Hagen: an enquiry into the agricultural history of the New Guinea Highlands.' Unpublished report, 4 pp.
- Golson, J. 1976d. 'The history of the sweet potato in the New Guinea Highlands.' Unpublished paper, 34 pp. [Written for Harry Maude's festschrift but withdrawn because of the considerations set out in Golson 1977c:54-5.]
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