

The following invited lecture was delivered at College Year in Athens, 2 April 2014

Meandering through Late Minoan III Crete, Proust, Pottery, and Palaces

Halford Haskell
Southwestern University

I am delighted to have the opportunity to speak here this evening about some of my work. What better place to meander through some broad approaches to an historical issue than here at College Year in Athens (CYA), a program where we can think and assimilate and learn the other side of the ropes – as we have with our wonderful CYA students at, for example, the Stoa Basileios just last week, also the Propylaia and Parthenon.

In 2008, Jonah Lehrer published a book with the startling title “Proust was a Neuroscientist.” The fundamental thesis of this controversial book is that artists often anticipate discoveries of the sciences, or to put it another way, scientists *rediscover* truths first discovered by artists. Science is able to gather and quantify data, but this alone does not yield sense or meaning. Through intuition, artists have been able to imagine how the human mind constructs sense or meaning. As Proust, Cézanne, and others so presciently observed, the process of constructing sense is something far more complex than can be readily quantified. Proust for example, ruminating in his cork-lined room, anticipated memory theory by remembering a cookie. Proust realized that human beings creatively construct memory. Memory, then, is not like a digital tape recording, but rather a created sense of reality, a concept now understood by neuroscience.

Paul Cézanne too developed a vision later confirmed by neuroscience. Just as with memory, Cézanne understood that what we “see” is necessarily a creation of the human mind. The mind, rather than relying on photons as if uninterpreted pixels of a digital image, develops a sense of the object based partly on photons but more critically on a very complex mental creative process. Our minds construct the sense of an object. In his Pont de Maincy painting, Cézanne gives only the *essential* information necessary for the mind to create sense. By stripping the scene down to its fundamental essence in this highly intellectualized work, he carefully replicates what the eye actually first sees – shapes, contrasts rather than sharp lines – before the mind takes over and creates the sense. This painting represents the beginning of vision, after which the mind creates intelligible meaning. The bridge as painted becomes profoundly more vivid and intelligible than is the case of a digital image of a similar bridge near Maincy. Sight perception is subjective, something now confirmed of course by neuroscience.

Lehrer writes that many have argued “If something can't be quantified or calculated, then it can't be true. Because this strict scientific approach has explained so much, we assume that it can explain everything. But every method, even the experimental method, has limits.” Data provide only part of the answer, a necessary part of the answer. Quantification of data may help organize those data, but quantification in and of itself does not produce meaning. Only through human interpretation can we develop the meaning or the sense of the data.

This all sets one to thinking about how we learn, how we approach a particular problem, how we process data to create meaning or sense, and ultimately how we articulate that sense. Our students here at CYA, for example, can rehearse for you how various approaches were integrated to cut through the chaff and crack the riddle of the dating of Pnyx III.

This evening I use as a setting another dating riddle. We will look at how various kinds of data - archaeological, scientific, and epigraphic - derived from pottery studies have been brought to bear on an historical problem of Late Bronze Age Crete, that is, the date and nature of the Greek administrative center at Knossos, or, to use the conventional term for Late Bronze Age Greeks, the Mycenaean administrative center at Knossos. The issue of the evening is: how are we to approach the evidence, that is to say the data, to create meaning or sense.

Beginning in the 1960's, impressionistic assessments of scientific data relating to pottery production yielded certain results with fascinating historical implications. Soon these impressionistic assessments were seriously challenged by statisticians, who insisted upon rigid application of rational statistical assessment of those data. Although these strict statistical assessments did not yield results that were meaningful in an historical sense, the very "rationality" of the process comforted many. More recently, additional analytical techniques have confirmed results of the original assessments, i.e. they have rediscovered initial results. Science tempered by human reason proves to be most persuasive.

So, let us meander. First, I will define the historical problem with which these pottery studies have been associated. I then will turn to the history of these analyses from the early 1960's until the present day - this is a project through which I have been meandering in my professional career. Finally, we will finish up with how our various analyses, not only scientific but also our typological and epigraphic work, have been integrated to illuminate the historical question.

First then the historical problem at Knossos. It all revolves around the date of the Mycenaean, or Greek-speaking, administration at Knossos. This has been of considerable interest ever since Sir Arthur Evans first conducted excavations here beginning in 1900. Over the course of a very few excavation seasons Evans uncovered a vast administrative complex. By 1904, he had cleared the bulk of the site. Evans often employed large teams of workers and dug quite quickly - here you see workmen removing deep fill from the area of the so-called Unexplored Mansion. He needed only two seasons to reveal the west wing with its magazines, here seen in a photo taken in 1901.

Knossos of course is the home of the legendary King Minos and the Minotaur, the half man half bull monstrosity eventually dispatched, we are told, by Theseus (with a little help from his new friend Ariadne and her ball of thread), as depicted here on an Attic black figure band cup in Munich.

From our lofty perch in the 21st century we may, and perhaps should, decry Evans' early 20th century excavation technique. His rapid excavation style might well be described as treasure hunting, and his interpretations often were quite imaginative, to put a positive spin on it. Certainly considerable evidence was lost during these early excavations, evidence that would bear directly on tonight's historical question regarding Mycenaeans at Knossos. Before we judge Evans too harshly, we should remember, however, that Evans, like Heinrich Schliemann, was inventing a discipline as he was going along, and it is a little unrealistic to apply contemporary expectations to such early work. Turning again to our students in Athenian Democracy here at CYA this semester, they have tackled this very issue of applying

contemporary expectations to early archaeological work. They have engaged in lively, informed, and often very passionate debate regarding 19th century style “excavation” specifically in relation to the excavation (or perhaps better robbery) in 1871 of a tomb just up the hill here in Pangrati near the church of Profitis Elias. In the tomb was a 4th century *pinakion*, that is an Athenian juror identification document, surviving today only as a drawing. The *pinakion* reveals the name Theodoros of the deme Ptelea, and may provide evidence that this deme was located here in the Pangrati area, a nice topical tidbit for a CYA Democracy class. To give you an idea of how Athens has changed, compare this view of Athens, taken about a month ago, to the same view taken 117 years ago. – you see here the same view toward Pangrati taken in 1887, not too long after this early excavation.

OK, back to Late Minoan Crete. It was Evans who developed and applied a chronological scheme that continues to serve today (Figure 1). For the Bronze Age, Evans divided the history of Crete into phases, for lack of a better word, and named them after King Minos himself. Early Minoan, Middle Minoan, and, of particular relevance tonight, Late Minoan, each of these subdivided into I, II, III, and so forth, with many further subdivisions upon subdivisions imposed by later scholarship. One particular horizon is known, for example, as Late Minoan IIIA:2 early, perhaps an over-refinement referring to a period that may have lasted about 28 ½ seconds.

1600-1450	Late Minoan I	
1450-1400	Late Minoan II	
1400-1370	Late Minoan IIIA1	
1370-1320	Late Minoan IIIA2	
1320-1200	Late Minoan IIIB	

Figure 1
Sir Arthur Evans and his chronological scheme

According to Evans, in the Late Minoan I period, Knossos, along with similar complexes at Malia, Phaistos, and elsewhere as we now know, for example Kato Zakro, and various town sites thrived until the end of the this period, when they suffered some sort of destruction. Thereafter during the Late Minoan II period, Knossos was occupied for some 50 years, continuing as an administrative center. This was the only such center on Crete during this period. At the end of Late Minoan II, this administrative complex was destroyed for the last time, afterwards reoccupied only by “squatters,” although there was a considerable amount of storage-type pottery associated with the latest level.

Evans associated the Late Minoan II phase with mainlanders – our Mycenaeans -, based on finds from within the complex and surrounding area. He uncovered clay tablets such as you see here, with signs in the script known as Linear B. Although he could not read the characters, he noticed that this script was not known elsewhere on Crete (at least up until his

time) but was seen on the Greek mainland. Some of the tablets include depictions of equipment suitable for war, for example chariots. Furthermore, in contemporary tombs in the Knossos area, interments were accompanied by weaponry, suggestive of a warrior class. Evans had developed quite an affinity for his Minoans, believing them to be peaceable folk, and he found it much easier to think of these warriors as outsiders, mainlanders. One thinks of similar iconography appearing in mainland contexts, such as on rings found in Grave Circle A at Mycenae.

It turns out that, despite Evans' imaginative and overly romantic notion about peaceable Minoans, his assumption about the mainland character of his Linear B tablet phase of Knossos proved, despite himself, correct. In 1952, Linear B was finally deciphered and the characters were found to represent, as we now know well, an early form of Greek. Evans' inference of Mycenaean control seemed confirmed.

We should remind ourselves here that Linear B tablets record economic details of interest to administrative authorities, for example inventories of manufactured goods such as chariots, allocations of raw materials, assessments and collection of goods such as wool and textiles, olive oil being of particular interest, etc. We learn from the Knossian Linear B tablets themselves that administrative oversight at Knossos encompassed, to varying degrees, much of central and western Crete – this geographical area – central to western Crete, particularly western Crete – is an important part of the story this evening.

So, according to Evans, a Late Minoan II or 15th century date for Linear B, or now we might say, Mycenaean Knossos. The destruction horizon was subsequently downdated from Late Minoan II into Late Minoan IIIA - perhaps a little after 1370 might be a consensus date, but in substance remained *the* reconstruction of events in force for quite some time, ... until, that is, shortly after the middle of the 20th century.

Oxford philologist Leonard Palmer was the first to lead the charge against this chronology for Mycenaean Knossos. He fervently believed on philological grounds, that is, on sign forms and various other language factors, that the Linear B tablets at Knossos, and the administrative center that they represent, could be no earlier than the Linear B archives at Pylos on the mainland. The tablets at Pylos belong to the end of the 13th century, corresponding to the end of Late Minoan IIIB, that is, some 150 years after the date first proposed by Evans. This change of date is of no small significance, as the tablets reflect the political and economic history of either early 14th century or late 13th century Crete, and has considerable implications for relations between Crete and the Greek mainland over this century and a half.

Once Palmer had established his thesis for a late 13th century date for the Knossos tablets, remember, established on philological grounds, that is on the basis of the tablets themselves, he then went after Evans and his excavation records. Palmer's attack style upon Evans's is fascinating. While much of what Palmer questioned archaeologically at Knossos was well worth questioning, certain of his own tactics were questionable themselves. Keep in mind that, in attacking Evans, Palmer was taking on a man who had become an absolute legend in his own time, Sir Arthur Evans, the man who had brought to the eyes of a fascinated public the brilliance of Minoan civilization. During Evans's lifetime, it was not helpful to one's professional career to disagree with him. Alan Wace, the very distinguished British excavator at Mycenae, was basically exiled from Greece for a while for daring to disagree with Evans on various matters of interpretation. By the time that Palmer launched his attacks, Evans had passed away and it was open season

In the 1960's Palmer accused Evans not only of gross incompetence – and by modern standards this charge has validity -, but also, and more significantly, of deliberate falsification of the evidence. Evans' defenders sprang to his defense with as much energy as Palmer had shown in attacking him. Palmer and John Boardman agreed to work the problem together and author the definitive book, but they failed to agree on virtually anything and published two separate accounts under a single cover (1963). Matters really got out of hand when scholars engaged in this debate turned from Evans and Knossos themselves to attacks on each other's personal and professional integrity and competence, all of this in print, especially in the British journal *Antiquity*. The editors of *Antiquity* noted that this dispute reflected "a splendid 18th century feeling for the place of invective and abuse in scholarship." This certainly was a highly emotional, very personal debate. Again, I turn to our Athenian Democracy students, who no doubt recall the invective and abuse in some of the scholarship over the so-called Themistocles Decree, even though in this case its initial publication by Michael Jameson was exemplary.



Figure 2
Inscribed Transport Stirrup Jar at Thebes

Palmer cited as supporting evidence for his later dating of Mycenaean Knossos Linear B inscribed transport stirrup jars (Figure 2) – we *are* getting to the science part of the talk, I promise. Many such pieces had been found on the Greek mainland, the deposits then known including the largest known surviving deposit at Thebes, with other pieces at Mycenae, Tiryns, Orchomenos, and Eleusis. The longer inscriptions consist of three words, as on the inscribed jar found at Eleusis, two of which are personal names and one of which is a place name. Sometimes the adjective *wa-na-ka-te-ro*, that is, the adjective of the word *wanax*, king, i.e. "royal," is substituted for one personal name, here shortened to *wa*. The place names probably are of the places of manufacture of the jars and/or of the contents. Place names on stirrup jars can be matched up with place names in **west** Crete known through the Knossos Linear B tablets. The west Cretan place names were a prime reason that Palmer associated the inscribed stirrup jars with Linear B Knossos. As I suppose you can guess, the inscribed stirrup jar phenomenon dates for the most part to the 13th century, corresponding to Palmer's Late Minoan IIIB date for Linear B Knossos.

Palmer noted the significance of these jars early on. His ASSUMPTION – one of MANY assumptions this evening - was that Linear B on Cretan jars indicates "palatial" in the Mycenaean *Knossian* sense of the word. If I may reduce the issue to its simplest, Palmer

reasoned that, if these jars could be shown to be of Cretan manufacture, then they must have been connected with a *Knossian* Linear B administration. Simple enough, right? Surely I jest, more on this later, but to anticipate, this was a bad assumption! This is NOT how one addresses such an issue.

Something on transport stirrup jars in general. Most transport stirrup jars were not inscribed – inscribed transport jars have received the bulk of attention due to their intrinsic interest. Stirrup jars were used for the transport of liquid commodities in bulk, and they were carefully stoppered, the clay caps over the stoppers sealed to guarantee the integrity of the contents (Figure 3). They held some 12-14 liters. These jars moved in relatively large numbers within the Aegean and beyond through the Late Bronze IIIB period.



Figure 3
Sealed Stirrup Jar
Mycenae, House of the Oil Merchant

They most likely contained olive oil, although other commodities such as wine are possible. Olive oil was of considerable importance in the Bronze Age, as we learn from Linear B tablets on the mainland and at Knossos. Oil could be used for cooking, lighting, textiles, cleaning, and as a base for aromatics – perfumed oil. Given the intense administrative interest in olive oil as reflected in Linear B tablets at Knossos and other Linear B centers, Palmer made the assumption that, since several inscribed pieces bear place names associated with west Crete and recorded on Knossian tablets, then Knossian administrators must have been managing this industry as represented by these transport stirrup jars.

This assumption from the outset was not warranted, but that in and of itself did not prove that it was wrong. One can stumble blindly upon the right answer. Scholarship is rife with poorly grounded assumptions that later prove to be true on entirely different grounds – witness Evans' own association of Linear B Knossos with mainlanders. Although I have spent much of my own professional career trying to debunk any NECESSARY connection between the transport stirrup jar phenomenon and the Knossian Linear B administration, sometimes people do stumble upon the right answer for the wrong reasons.

But let us move forward to PART II. Up to this point, Cretan origins for inscribed jars found on the Greek mainland were based on place names alone. Surely this was an instance where, trumpets please, SCIENCE could save the day. Transport stirrup jars are perhaps uniquely suited for a study of origins, for several reasons. As utilitarian vases, they are not likely to have been carefully imitated, as was the case with fine ware pottery, and therefore one can more easily trace origins and movements. The use of Linear B on certain pieces suggests a high level of administrative involvement. Furthermore, the large scale of the production and movement of these vases is suggestive of more than sporadic, elite gift exchange.

IF the jars could be shown to be of Cretan manufacture, and IF one accepts Palmer's assumption of Knossian control of such jars, then we would have something. Now we are getting to a major thesis of this evening's paper, that is, how can one, or how should one, meaningfully interpret data from scientific analyses. In 2011, my colleagues in Glasgow, Sheffield, and Cambridge and I published a comprehensive analysis study of transport stirrup jars, and we are fairly confident that we have it all pretty well figured out, but there are always (and there should be) skeptics out there – just this past October at conference in Brussels I was delightfully besieged by a two brilliant and highly accomplished Minoan pottery experts (Athanasia Kanta and Birgitta Hallager), who remain not entirely convinced by our results. In the future it may prove to be the case that some of our assumptions and results need to be revised.

To place our specific results in context, it is very important, however, for us to be aware first of the history of scholarship prior to our publication. The evolution of this study reveals that a number of assumptions made by scholars in the past must be revised. An understanding of the development of archaeological and scientific perspectives that form the context of whatever it is that we are reading written 15, 20, 25, 50 years ago is essential for a reasonable evaluation of that work – earlier work does remain of considerable value, but only within context. An understanding of past work is essential also for a reasonable evaluation of our own most recent work. Furthermore, through the history of scholarship, we learn much about ourselves and our approach to our own work, which is, after all, a humanistic endeavor.

So, what I will do now is discuss the history of transport stirrup jar study, as the type has received so much scholarly attention, and describe the evolution culminating our own publication. Much has been made of the value, or lack thereof, of scientific analyses, and continues to be the case, as I was reminded in Brussels. We must place such work into its proper context.

The scene first turns to the Oxford archaeometry lab (Oxford Laboratory for Archaeology and the History of Art) and their project to develop a chemical "pottery map" of the Bronze Age Aegean world. Their initial work was published in 1961-1963. The attempt was made to establish by chemical analysis, specifically optical emission spectroscopy, characteristic chemical signatures for the pottery supposed to have been made at major Aegean centers.

The resulting, initial pottery map was applied to the inscribed stirrup jar issue as a test case. Keep in mind that this was a test case, with the limited objective to explore the validity of chemical analysis. Some 25 transport stirrup jars were selected at Thebes on the mainland and analyzed, this study published by Hector Catling and Ann Millett in 1965. While outside of the lab Evans-bashing was popular, inside of the lab there was hope that a scientific technique would provide useful answers.

The results of the analyses of the Thebes pieces were both intriguing and immediately controversial, and instantly taken entirely out of the context of this initial test case. 18 of the 25 jars were assigned to Crete, specifically *east* Crete (2 shops), 5 were assigned "local" Theban manufacture (we will return to the assumption, oh that word again, of "local" in a bit), and 2 to Peloponnesian.

As often happens, too much store was put into the specific assignments of origin. Linear B scholars lamented that the analyses indicated *east* Crete, while place names on jars indicated *west* Crete. Furthermore, the supposed Theban origins for five jars were problematic. On the basis of the elements chosen for analysis, the investigators actually could not distinguish

chemically between local Theban and what they considered to be local Knossian, but it seemed easiest to assign the jars to their findspot, Thebes. This became known as the "Thebes/Knossos effect." This is not especially helpful. But at least the Palmer crowd could take some comfort in the Cretan origins of the 18 inscribed jars.

Indeed, Palmer's assumption of administrative control over this industry specifically at Knossos and *nowhere* else on Crete remained unchallenged. East Crete, west Crete, north Crete, south Crete, wherever, some folks really didn't care. In any case, this is an instance of shoe-horning stirrup jar evidence into a predetermined conclusion - Knossos later would prove to be not the only possibility for such administrative control.

In fact, later came very quickly, although still too late for the Oxford investigators, but at about the same time that their study appeared in print, excavators at Chania in west Crete began uncovering inscribed stirrup jars. Up to this point little had been excavated in west Crete, meaning that the Oxford lab's pottery map was obviously inadequate in its coverage of west Crete. What then was the role of Chania? So it was back to the drawing board.

Incidentally, during this period, in 1974, the locus of analysis changed from Oxford to Athens and the newly established Fitch Laboratory at the British School at Athens, moving us into the next phase of early stirrup jar studies. In light of the continuing Chania finds, the Fitch Lab, under the direction now of Richard Jones, established a control group for Chania, analyzed certain Chania found inscribed jars, and reanalyzed the 18 jars at Thebes thought originally to be east Cretan. Almost predictably, the 18 jars at Thebes were found to have a somewhat better match with west Crete, and so they were reassigned. The late Knossos daters were delighted - vindication, it seemed. Chania itself in west Crete was ignored.

There were, however, fundamental methodological objections. Critics were unhappy that the data were assessed visually, i.e. by human, or subjective, assessment rather than by recourse to objective statistical work. Furthermore, the obvious shortcomings of control groups became quite visible: the ease with which east Cretan jars were reassigned to west Crete shows how dependent projects like this are upon identifying and properly characterizing local control groups, and indeed this came up yet again at the Brussels encounter of which I spoke of earlier. Not only was there the "Thebes-Knossos" effect, but now the chemical overlap had emerged between east and west Crete. Nevertheless, this was the state of the art at this time, and in many ways, as we shall see, those who assessed the data at this stage were prescient. By visual assessment, west Crete seemed to be an appropriate correction for the formerly "east" Cretan jars.

In terms of historical implications, any necessary connection between the stirrup jar industry and a Linear B center at Knossos remained unproven, yet on the authority of Palmer's assumption it was now nearly universally accepted as fact.

The time was ripe for a broader study. Keep in mind that original Thebes project back in 1965 was conducted quite openly as a pilot study, to assess the viability of the technique. This is an important point, often forgotten. The investigators were experimenting with, at least in an Aegean context, a new and unproven chemical analysis technique. Scholarly criticism was to be expected. Over-interpretation of the results was an unfortunate consequence.

Taking into account the masses of responses to the Thebes studies (1965, 1977), it was indeed time to move forward. The Fitch Lab decided expand the project and sample *all* extant inscribed transport stirrup jars from throughout the Aegean; a few uninscribed pieces at

Mycenae and Tiryns were thrown in for good measure. This takes us into the final phase of early stirrup jar work (1980). In terms of research design, this project marked a significant advancement. The authors of this 1980 study (Hector Catling, Richard Jones, John Cherry, and John Killen) for the first time consciously brought together three lines of inquiry: analytical, archaeological, and philological. That is, the authors advanced the line of approach to attempt to integrate various avenues of inquiry. The authors also had access to some preliminary typological work that I had done with pieces at Mycenae from the House of the Wine Merchant and the House of the Oil Merchant, plus some results of petrographic work on these same jars done by John Riley of Southampton University; although the typological and petrographic assessments were not a formal part of the 1980 inscribed stirrup jar analysis project, the researchers could take comfort that our work for the most part corroborated theirs.

A key ingredient of the 1980 study was the tentative application of statistical assessment to the data derived from chemistry, although still tempered to a degree by the human eye. The underlying theme at this point in the history of archaeological science generally was "objectivity." This sort of thing was very much in vogue, very trendy, very fashionable and was thought to be absolutely necessary for there to be any legitimate results from chemical analysis work. While earlier work had relied on visual assessments of the data to group jars and assign provenances, thereby introducing a human or a subjective element, the use of statistical assessments was thought to provide the necessary distance and objectivity from the data. This was the end all, and it was going to provide hard *answers!*

The hard-core statisticians were becoming quite adamant and rather intimidating, and were rising up. Those who dared question the hegemonic status of the statisticians were, let us say, becoming marginalized.

Nevertheless, some results of the 1980 study confirmed what had already been suspected from earlier, more narrowly focused studies: most inscribed stirrup jars were assigned to west Crete, as were most of the uninscribed pieces at Mycenae, for example the jar on the right of the screen.

The remaining jars were designated "local," i.e. most likely manufactured at or near their findspots. This inscribed jar, found at Thebes, was classified as of Theban manufacture (the old Thebes/Knossos effect). Its inscription is of considerable interest. The "sign" forms are unique and in some ways seem to be more a part of the decoration than actual sources of information. A couple of other jars at Thebes bear the same characters, such as you see here. John Killen, who wrote about these pieces in the 1980 study, stated under some duress (that is, he intuited that something was not right here, as he indicated at the time) he stated under some duress that these inscriptions were in effect poor imitations of the good stuff coming in from Crete; indeed, the inscriptions on imported west Cretan inscribed jars are far more competently done, and it seemed to be the case that Cretans could inscribe jars and mainlanders could not. To quote Killen, "In contrast to these rather unconvincing local [= Theban] inscriptions, many of those on the imported vessels [meaning west Crete] are quite elegantly written."

Also classified as local were some inscribed stirrup jars at Mycenae. Despite the advance in data interpretation, that is, statistical analyses, the chemical overlaps between what were considered local control groups at various sites were not resolved, and in fact things became considerably worse! The most significant and therefore the most troublesome were the Thebes/Knossos, first identified in the 1965 study, east Crete / west Crete (1977 study), but

now also Mycenae/Knossos, and Mycenae / west Crete (Figure 4). This visualization does not inspire confidence. So, on strictly chemical terms, with objective assessments, we seemed to have virtually nothing of archaeological or historical significance. We have a flat, uninformative digital image.

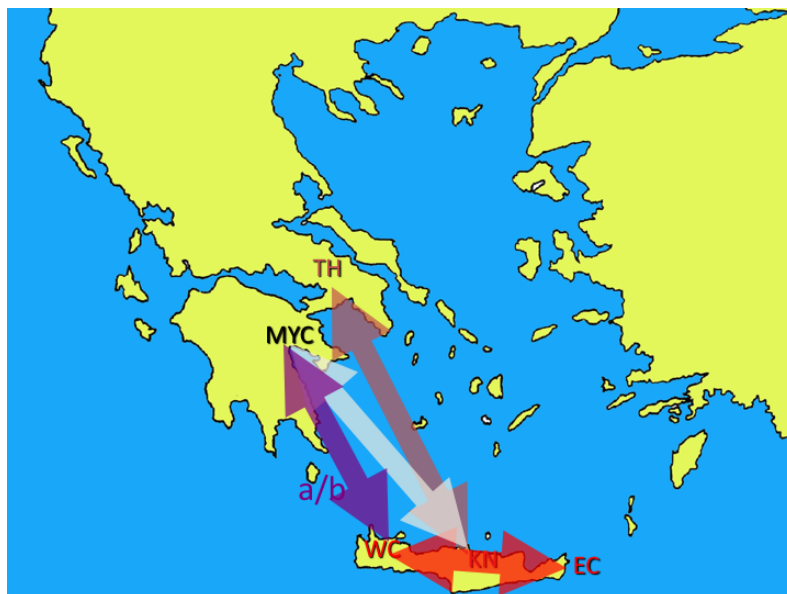


Figure 4

Chemical overlap zones: Thebes(TH) / Knossos(KN), East Crete (EC) / West Crete(WC), Mycenae(MYC) / Knossos(KN), Mycenae(MYC) / West Crete(EC)

But, now back to human assumptions – you may be getting the idea that “assumption” can be a bad word here. There was a need to put individual jars *somewhere*. The ASSUMPTION was made that if a jar fits squarely within local parameters, the most economical interpretation is that the jar is local – that is, local was the default interpretation. Only those pieces whose compositions stretched toward other end of the chemical overlap range were classed as an imports. This was a matter of statistical probability, a matter of numbers.

This sense or meaning derived from the data assessment was criticized immediately, however, and from the time of the publication of the 1980 study Richard Jones and I together enjoyed a wonderful meandering through various assumptions and approaches. This certainly was eye-opening for me. Statistical probability determined the divide between local and imported, with a built in bias toward local. In *typological* terms, I had reason to suspect that certain jars especially at Mycenae came from central Crete, others from west Crete, although the pieces in question fell near the middle of the overlap zones between Mycenae and Knossos and Mycenae and west Crete respectively. They were assigned chemically, therefore, to the local group for the reasons that I have just stated. This period of extremely positive discussion that Jones and I had marked the beginning of our extremely fruitful and ongoing collaboration, and our work together was from the beginning mutually beneficial. For me it was a time of learning about and appreciating the strengths and pitfalls of chemical work, for Jones the strengths and pitfalls of traditional typological work, and for both of us a growing realization of the value of true, long term, integrative collaborative work. Jones’ gut instinct, or shall we say his **art** of reading data, was being trumped by the cold, hard numbers. My admittedly subjective sense of origins on typological grounds also was being trumped by the numbers. It is also the case that I was hoping for more from science than science could deliver at this point.

So much for the early period of scientific analysis. We now move to a period that saw significant methodological changes and also a major retrenchment, fully the **dark ages** of stirrup jar study. Methodologically, the Fitch Lab switched from optical emission spectroscopy to atomic absorption spectroscopy. The technical differences between the two techniques are summarized by their names. In the first, OES, the sample is volatilized in a flame or arc; the thermal energy excites the outer electrons of the atoms, and when they return to ground state they release energy, visible as near ultraviolet and visible light; a quartz prism in a spectrograph disperses the light into wavelengths characteristic of particular elements, observed as spectral lines. The intensity of a given line reflects the concentration of the element. Atomic absorption is basically the opposite, in which the amount of light absorbed at given wavelengths is proportional to the appropriate element. AAS has proved to be more reliable with minor elements, in this case, the 9 elements originally chosen by the Oxford Lab back in the early 60's.

Work continued at the Fitch Lab in the 1980's, but as time went on things became bleaker and bleaker and bleaker for chemistry. It truly was the Dark Ages – just look at this slide!. It became a period of retrenchment. The problem was multi-faceted. There was pressure for complete reliance on statistical objectivity in data assessment, which demanded that, in the case of a jar falling within an overlap zone, just as the assumption of local manufacture is problematic, the assignment of imported status really is just as insecure. Now the statisticians completely dominated the field, intimidating and stifling further work. This jar at Mycenae illustrates the problem. In the 1980 study, i.e. just prior to our Dark Ages, since chemically the jar fell near enough to the west Cretan end of the west Crete/Mycenae overlap, it was considered to be a west Cretan import. This was, however, a subjective assessment, but least we had something. Now, in our dark ages of the 1980s, genuine statistical objectivity dictated, on the other hand, neither here nor there nor really anywhere!

And, just to make it even more perplexing, the fabric of this jar is absolutely typical of many many sherds found at Mycenae and therefore jars of this fabric were generally **assumed** – oh there's that word again - by the excavators to be part of their local repertoire at Mycenae. Here history of scholarship again is relevant for our understanding of the issue. Excavators were working within a traditional assumption expressed quite directly by Hector Catling back in 1961: "It is a reasonable assumption that sites such as Knossos, Phaistos and Mallia on the one hand, Pylos, Mycenae and Volos on the other, manufactured virtually all their own wares for themselves." Given the frequency of the fabric represented by jars such as you see on the screen in the local sherd assemblage at Mycenae, Richard Jones and team were under enormous pressure now from both excavators and statisticians to rethink any attribution of "imported." On the other hand, typologically this jar belongs in west Crete – this is where I stood -, and impressionistic assessment of the chemical data earlier had suggested a west Cretan origin.

What this meant was that chemistry was betwixt and between. While in chemical terms this particular jar may tend toward the west Cretan end of the chemical overlap zone, on a purely objective basis it could also very easily be local Mycenaean. The general implication is something like this: jar X on strictly objective assessment of the chemical data comes from either place a or place b, here Mycenae or west Crete. So the period of retrenchment: Richard was driven to conclude that such a piece merely belongs someplace within this overlap zone. This of course does not get us very far. There was also the enormous pressure exerted by various excavators of mainland sites to think local, seriously broadening parameters well beyond the assumption of local in the 1980 study. To leap ahead for just a second here, this jar will prove in the end to be of west Cretan manufacture.

But back to our story of the 1980's dark ages. Obviously there was a real problem here. Results of chemical analyses as assessed by highly sophisticated statistical work were getting us exactly nowhere. But I would like to quote Prudence Rice, who has written a basic pottery analysis text *Pottery Analysis. A Sourcebook* (1987): "The more complex and sophisticated the method of analysis, the more difficult it is to interpret the results in behavioral terms because of the problems of translating mineral and chemical data into human decisions about pottery production and trade." We are in fact dealing here with human choice, human decisions. Humans are not entirely rational beings and human behavior often defies and confounds objective assessment.

Chemistry was in the dumps, unable to tell us anything. Years of work seemed in jeopardy, as Jones and others struggled to justify chemical analysis programs. Yet luckily it was during this period that petrography first came to the Fitch Lab and perhaps there could be some hope. Petrography, or thin section analysis, promised to deliver more helpful results. And the question of the hour was: would petrography supplant chemistry, or would it complement chemistry. It was becoming fashionable to reject *all* chemical work, and I note that the same tendency occurred with respect to lead isotope analyses. The later 80's was a time of great doubt.

Ok, now, at long last, the light at the end of the proverbial tunnel? Was this to be one of those moments that we have all had, when something becomes clear for the first time, an "ahah" moment? We move now from the Dark Ages to into the later or most recent phase of stirrup jar work.

The study that we fairly recently completed had several objectives, some of which are in response to problems of the early and dark ages of study. One is coverage of the type. Inscribed stirrup jars, the focus of previous analytical work, represent only a very small proportion of the overall corpus of the transport type. Furthermore, Linear B inscribed jars are confined to where one would expect, that is, the Aegean. Therefore, we expanded the focus to include a large number of uninscribed pieces, and we expanded our coverage map to include pieces found beyond the Aegean, that is Cyprus, the Levant, and Sardinia and Sicily.

We studied these jars from a variety of methodological angles - typological, chemical, petrographic, and epigraphic. My own work involved typological classification and grouping - analyses of shape, decoration, and specific ceramic features. Richard Jones did chemical work. And at long last we had petrographic work, this done by Peter Day. John Killen filled in with fresh observations on the inscriptions. Initially, Jones, Day, and I independently established our various chemical, petrographic, and typological groups respectively, although we always had been in very close contact and critiqued each other's work. That is, the very positive meandering through various paths that Richard Jones and I had followed from early on continued with Peter Day. We often played Devil's Advocate with one another, questioning each other's work sometimes just for the sake of it, and I think that our work has benefitted as a result of this.

Before I get to specific results, and I will very quickly, let me say very briefly three things about methodological issues. First and foremost, our work has demonstrated conclusively that most transport stirrup jars intended for movement were manufactured on Crete, i.e. most jars found in the mainland (and beyond the Aegean) come from Crete. We thereby have exploded the myth of the preëminence of local manufacture. At sites such as Mycenae, west and central Cretan fabrics comprised such a large proportion of the local assemblage that they had skewed the sense of "local."

Secondly, we have also exploded the myth of the preeminence of objective assessment. After all, subjective decisions go into formulating questions and techniques, and it is very easy to manipulate data unconsciously so that they produce a desired pattern. As we have seen, the great hope through the 1980's was in "objective" statistical assessments. But these have been proved to be methodologically flawed, especially with regard to how one "objectively" separates "local" from "imported." Statistics and probability may warm the hearts of purists, but as we have seen as applied to this particular archaeological issue they gotten us precisely nowhere. We are dealing with human history, human decision making, that is to say, activity which can, and does, defy and defeat objective assessments.

Richard Jones now has cut through the chaff, assessing the data in a variety of ways, by univariate assessment of what he demonstrated are the most diagnostic elements (calcium, magnesium, chromium, and nickel), and multivariate analysis, both cluster analysis and principle component analysis. Basically, Richard Jones combined "objective" statistical work with subjective but reasoned evaluation of what the data are telling us – that is, he blended the **art** of data assessment with objective statistical assessment. Through focusing on the essentials, Jones has developed meaning - just as Cézanne enables the viewer's mind to construct meaning in the *Woman with a Coffee Pot*, another work that struck us forcefully a couple of weeks ago in Paris (you can tell – the Paris trip really was fully a “research” trip!). By reducing this composition to the essentials – look at the bolt upright spoon, for example – Cézanne conveys far more about this woman than any high-resolution digital image could.

Third, there remains to an extent the problem of certain chemical overlaps. Chemistry was not able to find certainty with various pieces. This was partly solved by overcoming a poor understanding of what "local" wares are, based originally on the frequency of certain fabrics within an a site's assemblage. Ultimately it was petrography, coming to the fore, that was able to supplement and clarify chemical work. Peter Day can take a jar like this, which chemically is in the Argolid / west Crete chemical overlap, and tell us definitively that it comes from west Crete – an assignment that, ah, some of us had maintained all along!

And in fact, in most cases, petrography is confirming what chemistry suggested to the human eye for origins before the Dark Ages, before the Period of Doubt, i.e. before chemistry was intimidated by excavators into denying import status to jars and by statisticians from assigning any origin a jar. Jars assigned to west Crete in the 1980 study and consequently reassigned at the very least to uncertain are now back in west Crete. Chemistry does after all still have much to offer.

Those who sought to reject all chemical work now have the opportunity to rethink their positions. Some of our results confirm earlier work, others provide some very different and exciting information. Chemistry is still valuable in that it does not have to stand alone. We have petrographic work, plus typological work. In fact, although we did initially establish our typological, chemical, and petrographic groups independently, when we have compared our individual results, we saw that we had a *remarkable*, almost embarrassing, degree of agreement.

Ok, what are the results more broadly? So much for theory and the history of scholarship. How does all of this relate to Late Minoan IIIB Crete and more specifically to Knossos? Part III.

As already noted nearly all transport stirrup jars that travelled significant distances from their places of manufacture were made on Crete. Some two thirds were made in west Crete and one third in broadly in central Crete, both north and south central Crete.

Looking at the corpus of Linear B inscribed transport stirrup jars, the vast majority was manufactured in west Crete, a very few in central Crete (including the well known inscribed piece from the Unexplored Mansion at Knossos), and probably none the mainland. At Thebes, where we have by far the largest and most representative deposit of inscribed stirrup jars, the ratio in favor of west Crete is very high, with some 94% of the inscribed stirrup jars from west Crete, and only 6% from central Crete. The 6% are represented by just three nearly identical jars, to which we already have made reference to this evening, assigned in the 1980 study as most likely local – back when “local” was the default mode. We should note here that these pieces at Thebes, as well as various other pieces found in Aegean and eastern Mediterranean contexts, were manufactured in *south* central Crete, some distance from Knossos.

What then of the implications for the economic and political history of Crete, since that is where these vessels were manufactured? We meander here into the always tricky waters of political history, often thought of in prehistoric work as creative or imaginative history, since we have no real historical records upon which to rely.

We see first off the *regional* nature of production, or to put it another way, the *decentralized* pattern of production on Crete. Vigorous workshops existed in disparate areas of Crete, producing distinctive pottery. Regionalism was the case also in the production of fine ware pottery. In the case of transport stirrup jars, this distinctiveness applies both typologically and palaeographically. This comparatively elegant jar (ok, we are talking domestic wares here – it is “relatively” elegant) is typical of west Cretan shops. It is relatively slender, with its maximum diameter somewhat above the median. There is considerable uniformity in the west Cretan group in body proportions, as well as in decoration. Most west Cretan jars are decorated with single bands, and can be dark on light or light on dark, as this west Cretan jar at Mycenae. Sign forms on west Cretan pieces occur either on the shoulder or body, are of fairly large size, and are close in form to signs seen on Linear B tablets.

In contrast, this comparatively heavy shape at Mycenae demonstrates features typical of central Cretan jars. Central Cretan jars can be nearly as broad as tall, with the maximum diameter nearer the median, but there are considerable variations in body proportions and shape within the central Cretan group. This banded jar is at Knossos. Decoration can be octopus or octopus derivative (deep wavy line), or bands. Linear B signs tend either to be very neatly rendered in small characters, as is the case with the Unexplored Mansion jar, or extremely poorly, even incompetently, rendered, as with certain pieces of the south central Crete workshop tradition.

What does this tell us? Certainly at Chania in west Crete there must have been some version of a Mycenaean style administration, to judge not only from the deposits of transport jars there, including inscribed ones, but also from the tablets that have turned up. Palmer of course had operated under the assumption of a single Linear B center on Crete, i.e. at Knossos, contemporary with the inscribed stirrup jars, as no Linear B had as yet been found elsewhere on Crete. While the Chania tablets really do little to undermine Palmer’s main thesis of a 13th century date for Linear B Knossos, they illustrate the problem of arguing definitively from negative evidence. In any case, we cannot on the basis of stirrup jars alone determine whether Chania was operating on its own as a first order center, or as a second order center perhaps under Knossos.

Of course, we now know that west Crete was not the only area producing stirrup jars for export, including inscribed pieces. Surprisingly, with Knossos located on the *north* coast of

central Crete, *south* central Crete produced very many of the central Cretan transport jars intended for export. This south central Cretan fabric is seen even at Knossos, indicating that Knossos was more a consumer of transport stirrup jars than an exporter. It is difficult to interpret the poorly inscribed pieces from south central Crete. Does this argue against the existence of a Linear B center at Knossos and thereby push Linear B Knossos back to the period proposed by Evans?

This brings us full circle and we return to the specific historical or political issue with which transport stirrup jars have been so often associated, that is, the date of Linear B Knossos. All the action seems to have occurred away from the immediate region of Knossos. West Crete was the biggest player, *south* central Crete coming in second, with its seemingly illiterate inscriptions. Leonard Palmer of course was vehement about the intimate connection between Cretan inscribed stirrup jars and the Knossian Linear B administration. We have succeeded in taking these jars far from Knossos itself and have demonstrated the decentralized nature of their production. There is no direct *archaeological* connection between movement of transport stirrup jars and Knossos.

Does this mean that Knossos was not the great administrative center implied by the tablets? After all, Knossos itself *seems* not to have been a player itself in this industry. Scholars who supported a connection between the inscribed stirrup jar business and Linear B Knossos bent over backwards to explain away this distance or physical separation. Some went through remarkable contortions to downplay the importance of various sites outside of Knossos in order for relative importance of Knossos to increase.

At first sight it has appeared that the relative lack of locally produced transport stirrup jars at or near Knossos, as well as existence of distinctive pottery workshops on Crete in LM IIIA2/B1, argue against the notion of the strong central authority at Knossos implied by the extant archives. But John Bennet pointed out some time ago and quite rightly that with the rise in prosperity that would accompany the Mycenaean administration at Knossos and with an improvement in the techniques of mass production, one might expect to see local pottery shops flourishing.

But the fact of the matter is that various assumptions have gone by the board. Not only must we NOT assume that the transport stirrup jar phenomenon was necessarily connected to a Linear B center at Knossos, no longer can Crete be seen as a single Linear B center island.

>>>>>>And - drum roll - the date of Mycenaean Knossos? After all of this, it turns out that most scholarship today, following on the ground-breaking work of Jan Driessen, favors some sort of modification of the scheme first proposed by... Sir Arthur Evans! What, then, of the vast quantities of storage and transport jars found in what we can now call "postpalatial" Knossos, i.e. in Late Minoan IIIB? As Oliver Dickinson put it so eloquently at a conference here in Athens back in 2005, "Knossos is a nowhere - it's a dump!"

We have seen how emotion and lack of common sense have muddled and muddied paths of inquiry and prolonged the debate over of the date of Linear B Knossos. The archaeological evidence provided by transport stirrup jars demonstrates what I think is a central issue here. Many scholars were misled and distracted by the highly emotional debate over Linear B Knossos. This led to mis-assessments, misuse, and misunderstanding of evidence. Basically the approach has been to set the hypothesis for a historical problem, and then go out in search of supporting evidence. The American expression of "an axe to grind" is apt here: once a thesis has been proposed, one can always find evidence to support it. Our work with

fundamental material has shown that there are extremely exciting things going on Crete that can be appreciated *without* reference to goings on at Knossos. Crete was in the swing of things, participating at a very large scale in overseas commerce. The scale of that commerce, in terms of actual numbers of transport vessels and in terms of their wide distribution, certainly indicates something beyond sporadic gift exchange.

Data, hard evidence, are necessary to address an issue in an informed manner – you can't just make it up based on nothing – hmmm, as we like to remind our students of this concept all the time (and they have shown themselves to be quite sophisticated here in this regard). But Cézanne's paintings resulted from **intense intellectual** reflection. A data set is just information without meaning. Chemical data, typological data, petrographic data are devoid of sense. We must inquire, interrogate, thoughtfully consider the data set, as Robert and Sam here are doing just last week at the Stoa Basileios, right before Sam had to wear a hat every day for a week. Proust, Cézanne, and others have so forcefully demonstrated that our minds **can** make sense of what we see – the data set. We have far more sophisticated and productive ways to create meaningful sense than through reliance on objective scientific assessment alone. There is, after all, a place for the human mind to meander in the morass of data.