

Vindolanda Tablets Online

As we reported in Newsletter 9, an online edition of the Vindolanda tablets is currently under development at the Centre for the Study of Ancient Documents as part of the Andrew W.Mellon Foundation funded programme, ‘Script, Image and the Culture of Writing in the Ancient World’.

The website (<http://vindolanda.csad.ox.ac.uk>) is being built in collaboration with the University's Academic Computing Development Team. It will include the texts published in *Tabulae Vindolandenses II* by Alan Bowman and David Thomas, together with new digital images of all the texts. The opportunity to search and browse the texts, translations, commentaries and metadata will make the online edition of the tablets a more flexible research tool. An introduction to the context and content of the tablets, in the form of a virtual exhibition, will facilitate access to the tablets for students, schools and those with a general interest in the ancient world. As well as cross-references to the tablets themselves, the exhibition will be supported with other documents and archaeological material, including many illustrations of sites and artefacts. A ‘reference’ section will assist with more specific aspects of the tablets’ content, for example Roman nomenclature and currency.

Substantial progress has been made on the different elements of the site. The overall framework has been designed and built. Digital versions of all the resources for the online edition, together with metadata, have been compiled. The material is organised and stored in a 4th Dimension database, which will also store other groups of documents included in the Mellon programme. A trial version of the exhibition and reference sections is now ready.



View over the site of Vindolanda, looking north-west from Barcombe Hill

The web publication of epigraphic and papyrological documents offers several challenges. We want to offer large images of tablets without incurring prohibitive download times. This will be realised through a zoom facility: a frame moved over a smaller tablet image calls up from the server a larger version of the area of the image within the frame. The layout of individual tablets must accommodate a large number of entities, including images, text, translation, notes and metadata. Our solution offers a customisable framework: some resources can be switched on or off whilst others may be explored through pop-up windows, for example the larger ‘zoomable’ images, notes and bibliography.

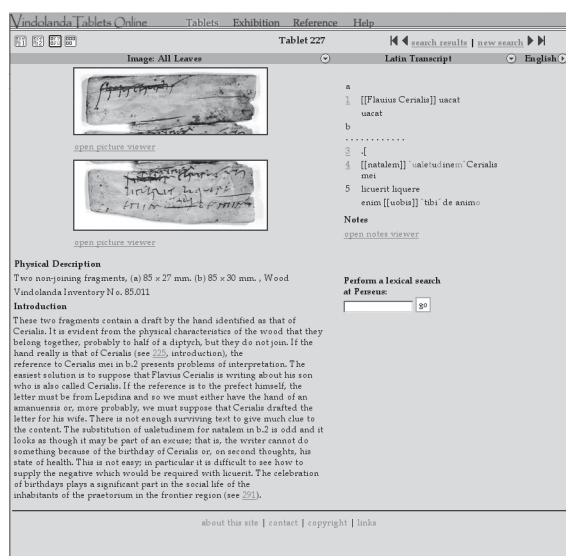
Perhaps the most acute challenge is posed by the mark-up and display of the texts themselves. Some of the typographic conventions used to mark-up printed texts cannot be consistently reproduced on the web. New, more digital-friendly conventions must therefore be adopted. In choosing these conventions, we have followed three guidelines: they must contain all the functionality of the print conventions, they must be compatible with other digital epigraphic corpora, and they must be based on common and current technology.

Given these concerns, we think that the Epidoc DTD—an XML standard for the encoding of ancient documents under preparation by an international consortium of which the CSAD is a member (<http://www.unc.edu/awmc/epidoc/>)—is the best option presently available, although it is still under development. The Vindolanda texts will therefore be marked up in XML following the Epidoc conventions. For display on the website, these XML texts will be converted into HTML by XSL stylesheets. Using style-

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sheets allows us to be flexible about the final appearance of the documents and to customize the display for each individual project.



Screen-shot showing sample page from the website:
Tabulae Vindolandenses II 227

Perhaps the thorniest display issue is the representation of uncertain readings, indicated in print by a subscript dot. It is not yet possible to present this in a stable form across different browsers. Our currently favoured alternative is to differentiate such readings by faded text. This and the rest of the website will be evaluated by testers in late summer and early autumn 2002. If readers would like to view the prototype site and take part in the evaluation, please contact us. The site will be launched in the winter of 2002.

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New Evidence for the Early Written Transmission of Greek Hexameters

From before the Macedonian conquest of Egypt and the consequent preservation of Greek papyri there, we have almost no direct written evidence for the handing down of Greek poetry. The large exception has been the “Orphic” gold tablets, with their instructions to the dead, from South Italy and elsewhere, the oldest of which, from Hipponion, dates *ca* 400 BC; it is only from this cluster of inscriptions that we can get any clear example of the pre-Ptolemaic filiations of any Greek poetic text. Since the publication of the Hipponian text in 1974, there have come to light several early witnesses on lead tablets, one dating in the 5th century BC, of another set of hexameters; these inscriptions, which Roy D. Kotansky and I are preparing for publication,

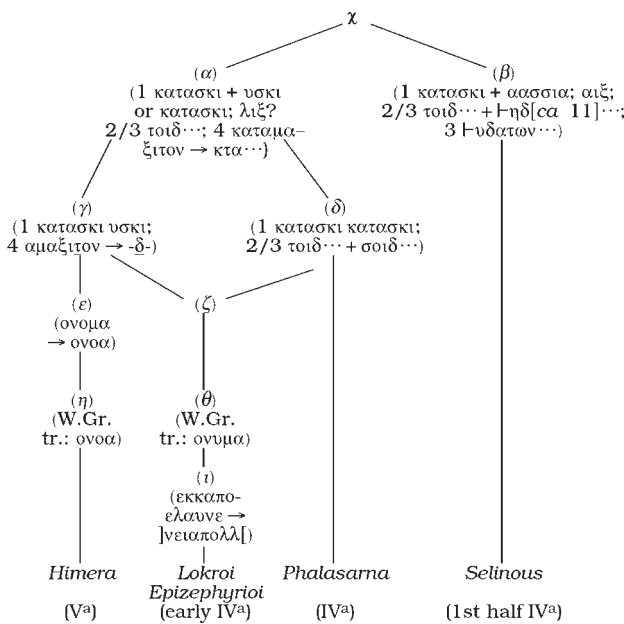
were the subject of a talk given on 30 January to the CSAD.

The Getty Museum houses a lead tablet from Selinous in Sicily, with 3 columns of hexameters in epic-Ionic whose letter-forms suggest the earlier 4th century BC and whose mistakes point to a model in an earlier alphabet. Its Col. A begins with a promise of overall protection to whoever will inscribe certain “holy verses” on a tin tablet and will hide them in a “house of stone”, and then it gives the verses themselves, which tell of shadowy mountains, a goat to be brought from the garden of Persephone and milked, Demeter, torches, Hekate, frightening shouts, and a god, the pronouncement of Demeter’s “god-spoken words”—all redolent of the *legomena* of mystery rites. Col. B, fragmentary at the top, opens with phrases about the protection of cities, etc., evidently a prelude to its main incantation, which begins with the so-called *Ephesia Grammata* and refers to a mystic shout along the highway of the blessed. Col. C, more fragmentary, seems to end with a macarism. The general arrangement of each of the 3 columns seems to be instructions followed by a core incantation (I call their archetypes ϕ , χ , and ψ), rather like the Hipponian and other early gold “Orphic” texts, their own cores being the words that the initiate is to speak to judges in the Underworld.

The “core” verses of the “Orphic” texts had a wider circulation than the longer forms, and this turns out to be true of the Selinuntine verses as well. ϕ turns up on two other metal tablets: embedded within the prose of an erotic spell on a lead curse tablet of the 2nd or 3rd century AD from Egypt, and garbled, in a puzzling mixture of Greek and Latin letters, on a silver tablet of the mid-3rd century AD from Rome. Of χ we have a copy on a lead tablet of the 4th century B.C. from Phalasarna on Crete and also, in apparently two separate West Greek translations, on lead tablets of the 5th century BC from Himera and of the early 4th from Lokroi Epizephyrioi. The Lokrian text also contains part of the macarism ψ .

The tablet from Phalasarna ψ was published in 1899 and has long been discussed as being the earliest epigraphical witness of the so-called *Ephesia Grammata*. The tablets from Himera and Lokroi now show earlier occurrences still but bring us no closer to understanding these *grammata*, as enigmatic today as they were in Antiquity: *aski kataski lix tetrax*, etc. Dr Kotansky and I ask ourselves whether they may have originated as corruptions of the opening of ϕ , with its phrase *kata skierôn oreôn* “down from shadowy mountains”. One of our literary sources for these *grammata* is Hesychios, who quotes from his own source: a complaint about charlatans who offer ignorant explanations of these words. The passage invites comparison with the Derveni papyrus; with its similar complaints about the misinterpretations of ‘holy writ’: can it be that Hesychios’

source was a commentary on χ ?



Tentative stemma showing the degenerations of χ into the 4 forms in which we have it today. The archetype was old enough to have undergone translation and several distortions by the time its earliest extant witness was inscribed in the 5th century.

Those who heard my paper will remember that it took a long 90 minutes. I thank the audience for their patience. There was no time to include what I add here, about my favorite part of the accumulation of these several texts. I first stumbled onto the hexameters of ϕ as I was reading a late erotic spell. Shortly afterwards, Roy Kotansky sent me his transcription of the Selinuntine hexameters. From Duke University, where the strange silver tablet is housed, Kent Rigsby sent me a photograph. A few years ago, Felice Costabile sent me a copy of his publication of the Lokrian tablet. I was preparing a note about this last for *ZPE* when Jaime Curbera sent me a photocopy of Maria-Teresa Manni Piraino's publication of a fragmentary and discouraging lead tablet from Himera—a drawing only, with no attempt at a transcription. My wife and I were about to drive to the Peloponnese, to the source of the Styx. I took along the Lokrian notes and the photocopy of the drawing from Himera. When we arrived, she, being a more enthusiastic hiker than I, headed off, and I sat in the car with my notes, eventually looking at the photocopy. It was a glorious summer day. Some time later, she came back, with a thermos of Stygian water. "Drink this," she said. "No. Look at this," I said: the lead tablet from Himera, of the 5th century BC, to judge from its letters, was the earliest witness of the *Ephesia Grammata* of the second column of the hexameters!

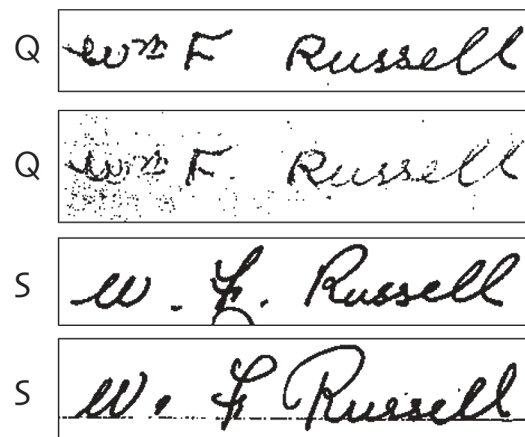
David Jordan

Handwriting Identification: Practice and Theory

There is a belief that there are perceptible characteristics in the handwritten trace that can be used to identify the author of that handwriting. This is an extremely widespread belief: signatures, for instance, are used commonly and casually to authenticate all kinds of transactions, and this authentication quite often guarantees a great deal of money. There is also a belief that there are people called handwriting experts who can recognise these characteristics and, with reasonable reliability (often, in fact, beyond reasonable doubt), make identifications on the basis of that recognition.

I think I can reasonably claim to be one of those people, since my evidence has been accepted in court doing just that, speaking with the privileged voice of an expert witness, many many times over nearly thirty years, and I have worked on some thousands of forensic handwriting cases. So, speaking if I may with that voice, I would say that in my opinion that skill exists, that it is learnable by those who have a reasonable potential competence at it, and that it is also rule-bound, though a number of the rules are not fully understood, and dependent on a body of solid information, though much of that information has not been fully explored or rendered explicit. What I will do in this brief paper is give just two examples of the practice of handwriting identification, and attempt to sketch some of the principles behind this practice.

Here is my first case. Mr Russell was denying that he signed the document, and someone else was saying that he did. At the top is the questioned signature, Q; underneath it is the same signature photographed under ultra violet light.



One can just see that there are some faint pencil markings around the initials—W.F.R—of the signature, which seem roughly to repeat the form of the initials.

Now, it is known that rather incompetent forgers might make a trial attempt in pencil, trace over it in ink, then erase the pencil (usually leaving behind easily realisable evidence of what they have been up to). Is that what happened here? I was shown a report by another handwriting expert that says it was.

Here is another oddity. S are just two of the sample signatures that I used. If you compare them with the questioned signature, you can see that the 'ussell' part of the Q signature resembles rather exactly the equivalent bit of the sample signatures; while the initials, WFR, do not resemble the sample at all. Why is this?

Let me inject a little theory at this point. Handwriting experts can be said to work on three levels of analysis of handwriting: letter formation, detail of the letter, and line quality. Letter formation is the basic way in which the letter is formed: so, the letter formation of the /F/ differs between the questioned and sample signatures in the case of Mr Russell. Detail of the letter deals with the differences or similarities between writings whose letter formation is more or less the same: so, for instance, the detail of the 'ussell' is extremely similar in the case of Russell Q and S, and the detail of the /W/ is different, though in that letter the letter formation is more or less the same. Line quality is the most delicate of the three, and refers to the way the writer's skill, competence, and other factors are shown in the written trace of the handwriting movement. It is an indication of speed and fluency: the better the line quality, the more skilled, or at least fast, the movement of the pen. Poor line quality can indicate forgery. In forensic handwriting identification, line quality is extremely important. And in the Russell case, the line quality of the capitals in Q is poor, but that of the rest of the signature is reasonably good, and matches well that of the S signatures.

Therefore, in the case of Mr Russell, either we have a very skilled 'ussell' forger who was unfortunately bad at doing capital letters, which seems unlikely; or else the 'ussell' is genuine, and the capitals are forged, which also does not seem very likely. However, that second possibility is in fact what happened. Lawyers sometimes have the habit of writing initials in pencil where they intend the signator to sign. Mr Russell took this as an instruction as to how to write, and obediently traced the initials, effectively forging part of his own name, and then wrote his normal fluent 'ussell' after that. Here are some principles we can deduce from this rather odd case. One is, handwriting is produced by human beings. It leaves a written trace, and this is fairly stable and predictable once written; but it

is the trace of a human behaviour, and people are unpredictable and peculiar.

In particular, handwriting is subject to conscious intention, and is modifiable: at that level, it is not stable, and hard to predict. Second, in general, letter formation is not a reliable indicator of identity. Letter formation is very much under the control of the writer. For example, most competent writers have at their disposal a number of fairly distinct alphabets: lower case cursive, upper case cursive, block capitals, and that style which most people learn before they learn the joined-up writing, which is called print script. So, as a general rule, handwriting identification is not interested in gross areas of similarity or difference, but in those areas of fine detail that are less likely to be in the writer's conscious control: in other words, we work principally at the level of letter detail and line quality.

Here is a signature, S; followed by a forgery of that signature, Q.

The forgery is quite good, both in line quality and letter formation, and at first sight looks very convincing. But look at it closely. Firstly, the laudable concentration on line quality has led to a gross error, in that an extra minim has been inserted. Then close examinations reveals that the line quality also lets it down. The capital /E/ of the surname is slightly less assured, with a tendency for curves to turn into straight lines and corners, which is very characteristic of forgery. And, in the case of the lower loop of the final /y/, the reverse has occurred: a curve in place of an angle and straight line. Then look at the end of that stroke: in the genuine signature the pressure of the pen reduces gradually, while in Q the line ends abruptly, no doubt with relief; a failure of concentration at the end of the job. Forgery is really very very difficult, and handwriting identification itself is not easy; but it can be done, and, I would maintain, it can be theorised, taught, and tested.

Tom Davis, University of Birmingham

The 11th-Century Novgorod “Codex” on Waxed Wooden Tablets

On 13 July 2000 archaeologists in Novgorod, working under the direction of V. L. Ianin, unearthed a triptych of waxed limewood tablets with the incised texts still largely intact. This “codex” can be dated to the first quarter of the 11th century. It is the only early medieval object of its type from the entire Slavonic world, and one of very few from Europe as a whole. The text was written in the Cyrillic alphabet. The language was Church Slavonic, but the presence of specifically East Slav (early Russian) “mistakes” indicates that the scribe was an East Slav. The handwriting is the same throughout. The text preserved in the wax is from the Psalter (Psalms 75 and 76 in the Septuagint numeration). Apart from a few damaged letters it can be read easily and reliably.



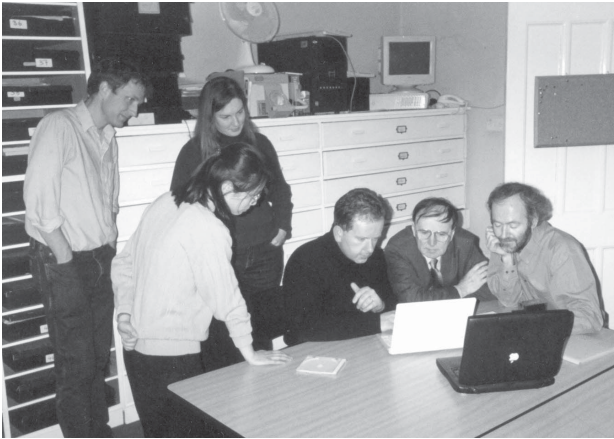
Waxed limewood tablet from Novgorod

However, when the wax was removed for conservation, it was revealed that, apart from the main text, the codex preserves—in the wood under the wax—faint traces of earlier lettering. These “hidden” texts are comprised of psalms and an assortment of religious works. Taken together they are many times longer than the main text in the wax. The “hidden” texts are of exceptional interest both for their literary content and for their historical implications. They include, for example, a series of hitherto unknown Slavonic compositions, apparently of native origin (i.e., not derived from Byzantine Greek), written by a single author and—as is clear from their contents—reflecting a non-canonical brand of Orthodoxy (which may explain why they have not been preserved in any other form).

The study of the “hidden” texts poses exceptionally complex challenges, both technically and philologically. The texts have been examined in four states: (i) in the original, *in situ* on the tablets; (ii) from photographs; (iii) on computer, from scans of photographic negatives, and (iv) on computer, from scans of the original wooden tablets (after conservation). The first mode of study was possible only for a brief period before the tablets were handed over for conservation work on the wood. Thus far the best information has been obtained from study in the third mode.

The prime obstacle to the decipherment of the “hidden” texts is the fact that the traces of letters in the wood are barely discernible, so faint as to be almost indistinguishable from natural cracks, grain and other surface irregularities. A second and still greater difficulty arises from the fact that the wax was re-used many times, and each time the new text left a new layer of traces on the wood beneath. The Novgorod triptych codex is therefore a palimpsest. However, by contrast with “normal” palimpsests (which typically consist of two—or at any rate of very few—layers of superimposed text), the Novgorod codex consists of multiple layers. We can term it a “hyperpalimpsest”. Trying to disentangle all the layers and decipher the texts on the Novgorod codex is therefore rather like trying to disentangle and decipher the layers of text on an old and much-used sheet of carbon paper. Nor can one separate the layers on the basis of handwriting (as one might in the case of a classical palimpsest) since the handwriting on all layers is identical.

I am unaware of any precedents for the reading of such a “hyperpalimpsest”. It has therefore not been possible to make use of existing techniques (there are none). Appropriate methods have had to be devised in the course of the work itself. Indeed, one cannot properly speak of “reading” any of these texts as one might speak of “reading” a text in a normal manuscript. With a normal manuscript we might first decipher the letters in sequence, and then grapple with their interpretation as text. In the case of the Novgorod tablets such a procedure would be wholly unrealistic. Any given line, or any given point in a line, consists of several layers of letters—or of the faint impression of possible letters—with no obvious indication of which is primary, of which goes in sequence with which to form words or phrases. Thus in order to find the sequence one must at each stage select the next letter or letters from a range of initially equal possibilities. This can only be done if one is constantly forming and testing hypotheses as to the possible shape and continuation of the emerging text. With a normal manuscript, reading mostly precedes interpretation; here it is an integral part of textual reconstruction at every stage, prior to any possible sequential reading.



The Novgorod and CSAD Teams examining a scanned image of the limewood tablets

To give an example, from the fifth line of the first “page”. At one point we can discern (among other possibilities) letters which might go together to form the word AZ” (‘I). We examine the area following this word, and find traces of a large number of different letters. Checking for possible groupings to go with AZ”, we find that some of the letters might be combined as ESM”, or ES”M” (“am”). Further checks show that ES”M” fits: all five letters can indeed be found where they “ought” to be if we follow the spacing of the earlier word. So, the hypothesis—that here we have words meaning “I am” is locally adequate. Whether it is in fact correct one can only tell from the continuation of the phrase. The longer a phrase which fits all the relevant phonetic, grammatical and stylistic criteria, the lower the chances that it is composed of purely accidental juxtapositions of letters originally belonging to quite separate notations. In the present example, after several stages of similar hypothesis and verification, we arrive at the sentence AZ” ES”M” ISTINA I ZAKON” I PROROTSI (“I am the Truth and the Law and the Prophets”), which fits all the relevant criteria. This reconstruction is further confirmed by the fact that the same phrase is apparently repeated several times.

The reconstruction of the texts of the Novgorod codex is slow and labour-intensive. We have received valuable assistance from the Centre for the Study of Ancient Documents, during our visit to Oxford in February 2002. Although it was not possible on this occasion to bring the object itself to be re-scanned, our scans were processed so as to remove the effects of the grain and improve the visibility of incisions, and to an extent this made it easier to recognize and identify traces of lettering. It is to be hoped that further cooperation may lead to the development of improved techniques for dealing with hyperpalimpsests.

Andrei Zalizniak (tr. Simon Franklin)

The Miletus Decree: is dating by three-bar sigma true or illusory?

In a paper given at a seminar on 6th February 2002, I challenged the accepted date of the Miletus decree (IG I³ 21) and argued that it was passed in 426/5 BC, probably at the last prytany of 426/5 BC. The following is a summary of my presentation.

Traditionally, the Miletus decree has been dated to 450/49 BC. Although the archon’s name Euthynus appears twice in the decree and we know that Euthynus was an archon in 426/5 BC, until recently this was not considered to be the date of the decree because the consistent use of a three-barred form of sigma in the inscription seemed to indicate an earlier date. Consistency has been maintained by following Kirchoff’s tentative suggestion that the name of the archon of 450/49 at Diodorus 12.3.1 could be emended from Euthydemus to Euthynus. Until Harold Mattingly first questioned its validity no one doubted this change. In spite of Mattingly’s continuing work and the demonstration by Chambers et al. in 1990 that the three-bar sigma was used in the Egesta Decree of 418/7 BC, dating by style changes in letterform, particularly of sigma, continues to be favoured over other types of evidence.

For this reason I have re-examined the letterforms used in 5th (and early 4th) century Attic inscriptions, paying particular attention to the letterforms of numeral signs for staters and the flexibility of usage of some letterforms such as nu and upsilon with a stem, where clear changes are hard to recognize. Examination of stater signs shows that some anomalies did exist. At least 6 masons continued to use three-bar sigma for engraving staters in the later 5th and early 4th centuries. This seems to suggest that each mason could use his own preferred letterforms (see my forthcoming paper in *ZPE*). As for nu and upsilon, it was not easy to find any particular year when older letterforms changed to the modern ones: from sloping nu to the more classical shape of nu; from two-stroke upsilon to three-stroke, and so on. They are used rather changeably and a variety of letterforms can often be found in one inscription.

I conclude, accordingly, that there were no rules for using particular letterforms and that letterforms can only be used to establish general trends. It is not strange if there were exceptions.

To turn to the Miletus decree itself, there are certain irregularities in its stoichedon style and orthography which have been considered to reflect the translation into Attic of an originally Ionian text. Careful examination of inscribing style and orthography in the Miletus Decree, however, leads to a different conclusion. In the decree, ξ and ψ are consistently inscribed in Attic style, that is, κ+σ and φ+σ respectively. This suggests that the original text was

prepared by an Athenian secretary but contained some Ionicisms. Mixed usages of Ionic and Attic orthography in a decree are seen in Attic inscriptions throughout the fifth century BC— particularly from around 430 BC. The date of the decree seems to fit better in the 420s BC when Ionic orthography began to be common in Athens.

The draft of this decree was made by the *syngrapheis*. The only parallel phrase is seen in *IG I³ 78*, 3-4, the so-called First Fruits decree. Why were the *syngrapheis* asked to make a draft for the Milesians? In 428 and 427 BC, several major revolts and *staseis* took place. Although we do not know exactly what procedures were followed after the suppression of the revolts, decisions seem to have been made at the Assembly in the normal way. However, the Milesian case seems to have been a somewhat irregular one, perhaps because of the Athenians' considerable confusion at this time.

In studying *IG I³ 21*, I have found a number of problems with reading and restorations. I also have great doubts about line length in the inscription—each line should have had 59 stoichoi (instead of 58). When these problems have been resolved, we will have a more vivid picture both of the relations between the Athenians and the Milesians, and of the Athenian Empire.

Akiko Moroo - Chiba University of Commerce

Epigraphic Sources for Early Greek Writing

As reported in Newsletter no. 9, this project aims to create a new online resource for the study of Archaic Greek inscriptions using an archive of papers and photographs bequeathed by Anne Jeffery.

The archive comprises 100 foolscap folders of papers, two scribbling diaries and one large tin of photographic negatives. Preliminary sifting has shown that, as expected, most of the material—amounting to some 5000 pages of notes and drawings in 77 of the folders—relates directly to the production of *The Local Scripts of Archaic Greece (LSAG)*. Of the remainder of the archive almost all the folders concern epigraphically related lectures, papers or topics, including five on non-Greek scripts of Asia Minor. The non-epigraphical photographs are more diverse in subject matter, reflecting Jeffery's travels in the 1950s and 1960s and the path of her academic career from Cambridge to Oxford via Princeton. Some of the photographs of Aegean scenes record landscapes, townscapes and agricultural practices which have now changed or disappeared. On a different theme, about 40 photographs concern the excavations at Bayraklı (Old Smyrna), where Jeffery supervised the excavation of the Bronze Age levels of "Trench B" during the 1949 season.

One unwelcome discovery was that the archive is deteriorating. Anne Jeffery did not plan for the preservation of her papers in perpetuity and so was frugal and imaginative in her choice of stationery and generous in the application of sellotape, paper clips and staples. In view of this the project has been initiated not a moment too soon.

The detailed cataloguing and digitisation of the archive is now well underway, beginning with the LSAG-related material. Other work over the past year has focussed on developing a range of analytical and illustrative tools to complement and enhance the use of Jeffery's book, including a searchable database and concordance, and mapping tools with a facility to generate customised maps. Ultimately it will be possible to sort and plot the diffusion of scripts according to a wide variety of criteria.

A poster containing details of the project and a selection of sample images was taken to the XII Epigraphical Congress in Barcelona in September, 2002. The poster attracted much interest and helpful suggestions about the shape and direction of the project, as well as a number of requests to visit and consult the resource. Comments and enquiries of this nature are most welcome.

Further details of the project and sample images drawn from all the categories of material in the archive can be viewed at <http://www.csad.ox.ac.uk/LSAG>.

David Lewis Lecture

The Seventh David Lewis Lecture was delivered by Mrs. Charlotte Roueché of King's College London on 22 May, 2002 in the Garden Quad Auditorium, St. John's College. The title of Mrs. Roueché's lecture was "Epigraphy and the New World Order".

Mrs. Roueché took the history of the *Monumenta Asiae Minoris (MAMA)* project, one of whose founders was her grandfather W.H. Buckler, as a paradigm for the spirit of international cooperation between scholars which is now reflected in the Epidoc initiative to create standards for the encoding of epigraphical texts. The lecture traced the history of *MAMA* through its creation in the aftermath of the 1919 Paris Peace Conference, the favourable reception of its early volumes, and the projected collaboration of Louis Robert with Buckler on a volume for Aphrodisias which was frustrated by permit difficulties in 1935. The lecture drew on a fascinating range of archive photographs and private correspondence to illuminate the history of epigraphical scholarship in Asia Minor during the 20th century.

The Lewis Lecturer for 2003 will be Prof. John Davies of Liverpool University. The title and further details of his lecture will be announced in the next issue of the Newsletter.

CSAD News and Events

CSAD Director

We are pleased to announce that the Centre's Director, Dr. Alan Bowman, is to succeed Professor Fergus Millar as Camden Professor of Ancient History. Since 1977 Dr. Bowman has been a Lecturer in Ancient History and Official Student (tutorial fellow) of Christ Church. He will be taking up his new post at Brasenose College on October 1st 2002.

Other Seminars

During the spring and summer of 2002, a series of occasional seminars at the Centre included papers by Jacques Oulhen ("The Theorodokoi List from Delphi"), Charles Crowther ("A New Sacred Law of Antiochus of Commagene from Zeugma"), Peter Haarer ("Archaic Obelomaniacs and their Dedicatory Bases"), Anne Biemann ("Women in Hellenistic and Imperial Inscriptions of the Cyclades"), and Prof. A.P. Christidis ("New Questions for the Oracle at Dodona").

The Epigraphy of the Greek Theatre: a Colloquium *Oxford, 7 - 8 July 2003 (dates to be confirmed)*

In July 2003, Dr. Peter Wilson is planning a colloquium, to be held under the auspices of the Centre. The aim of the event will be to gather together those working on the subject for an exchange of news and ideas, with particular emphasis on new and neglected material and the light that new methods and questions may throw on the familiar. Although the focus will be epigraphic, Dr. Wilson is hoping to include presentations from those who are working on other kinds of documentary evidence related to the Greek theatre (painted or plastic iconography, for instance)—particularly if discussion of these could be considered alongside the nature and potential of epigraphic evidence. The chronological scope will stretch from 'Beginnings' to the Hellenistic period with no geographical limits.

A two-day programme, with up to nine speakers, is planned, with a participating audience of around twenty-five scholars and graduates. More details will be announced in the Spring CSAD Newsletter, as plans take firmer shape.

Epigraphy Summer School 2004

Preliminary Announcement

Dates for the next Oxford based Epigraphy Summer School have been fixed at July 5th to 15th 2004. The academic programme will be directed by Dr. Graham Oliver and Prof. John Bodel. Further details and a programme will be published next year.

Visitors to CSAD

The Centre is able to provide a base for a limited number of visiting scholars working in fields related to its activities. Enquiries concerning admission as Visiting Research Fellow (established scholars) or as Visiting Research Associate should be addressed to the Director. Association with the Centre in either capacity carries with it membership of the University's Classics Centre.

Visiting members of CSAD during the Academic year 2001/2002 were Dr. P.J. Stylianou, Prof. A. Avram (Visiting Research Fellows), and Ms. Mónica Elías Pérez (Visiting Research Associate)

Circulation and Contributions

This is the tenth issue of the Centre's Newsletter, which is circulated in Autumn and Spring.

We invite contributions to the Newsletter of news, reports and discussion items from and of interest to scholars working in the fields of the Centre's activities—epigraphy and papyrology understood in the widest sense. The Newsletter is circulated to individual scholars on the Centre's mailing list and is also available online in HTML and pdf formats (<http://www.csad.ox.ac.uk/CSAD/Newsletters>). Contributions, together with other enquiries and requests to be placed on the Centre's mailing list, should be addressed to the Centre's Administrator, Maggy Sasanow, at the address below.

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