

ca 1318 + Patrice

Quarterly No. 98, January 2000

FoMRHI Quarterly

BULLETIN 98

Bulletin Supplement

Membership List Supplement

J. Montagu 2

E. Segerman 6

J. Montagu 63

ANNOUNCEMENTS

Michaelstein Symposium on the History, Construction and Playing of Large Bowed Instruments, 17-20 November, 00

9

Edinburgh Symposium on Bowed String Musical Instruments, 2-3 June, 00

10

COMMUNICATIONS

1677 Review: *Larigot* 24, Décembre 1999, ACIMV

J. Montagu 12

1678 Review: *Makers of the Piano 1820-1860* Vol. 2, by M. N. Clinkscale

M. Cole 13

1679 Review: *Two Upon a Ground*, CD by Charivari Agréable

S. Heavens 15

1680 Review: *Bouwerskontakt Bouwbrief* 95

C. Stroom 16

1681 Review: *The British Violin*, ed J. Milnes

E. Segerman 17

1682 Review: 'Problems of Authenticity of 16th Century Stringed Instruments', by K. Moens, pp. 41-49, *CIMCIM Newsletter* XIV, 1989

E. Segerman 19

1683 Review: 'Italian violin strings', by M. Peruffo, pp. 105-203, *Recercare* IX, 1997

E. Segerman 26

1684 Review: *The Trombone: An Annotated Bibliography*, by G. B. Lane

A. Myers 34

1685 Musical millenium memoirs

A. V. Loretto 35

1686 I'm not sure that you ought to read this

J. Montagu 36

1687 News from the MFA

D. Kuronen 37

1688 Molto stuccato

G. Lyndon-Jones 38

1689 Postscript to Comm. 1673 on "The Sistrum"

J. Little 40

1690 On Western travelers who described and drew inspiration from 'eastern' instruments and music, ca 1830s-50s

J. Little 41

1691 A Roman mouthpiece in England

R. Chiverton 46

1692 Making Italian roses

D. Wraight 48

1693 Ways of treating wood

D. Owen 51

1694 About an unusual method for stabilizing wood

M. Tiella 52

1695 A theologian's specific for happy musicology

J. R. Catch 52

1696 A question of evidence

J. R. Catch 53

1697 Further to Comm. 1655, the 'sisters' and Irish double harps

J. Downing 53

1698 Modern lute stringing and beliefs about gut

E. Segerman 54

1699 Tinctoris and depictions of 15th century fiddles

E. Segerman 62

FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS

Honorary Secretary: Jeremy Montagu, 171 Iffley Road, Oxford OX4 1EL, U. K.

FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS

Bulletin 98

January, 2000

A **happy new year** to a good many of you – renewals seem to have come in rather better than some years – turning over a new millennium, no doubt. The rest of you will receive the greetings in April and I trust that the remaining months will also be happy.

Elections: As one might expect, there has been a certain amount of reaction to Eph's and my bombshell, but as yet there have been no positive nominations for either of the posts. A few people to whom I was gossiping at the Early Instrument Exhibition (nice to see so many of you there and very good to re-recruit a number of past members who have returned to us) have expressed some interest, but as yet nothing definite. **There's not much time!** We shall want to send out ballot papers with the April Quarterly – it would be very expensive, though of course possible, to send a separate mailing.

It is probable that you will all have a vote. I sent out a Fellows Ballot last month with the proposition that all members should be entitled to vote on this occasion (and, as a discussion point for a further vote, that this should become the routine). Of the 21 people who may at present be Fellows (I put it like that because when somebody who has been elected a Fellow rejoins after a lapse, they are automatically again a Fellow, but if anyone who voted has not renewed their subscription by the time we count the votes on January 14, their vote will not be valid) eleven have so far voted 'Yes' and two 'No' (but neither has yet renewed, nor have several of the 'yes' votes), which means that even though we don't officially count until the due date, we do already have a good idea of the answer!

So please can I have nominations, either of yourself or someone else (but if someone else do please send with it their signed willingness to serve).

And yes, Eph and I are very pleased and flattered by the reactions of many of you, but we are not, we hope, irreplaceable. We did start something, but that was twenty-five years ago, and it should be obvious to all of you that people are somewhat more efficient and a good deal more energetic at rising 48 (I speak for myself but Eph is around my age) than at 72. It is time for the next generation to take over and, if I may put it like this, put back some of the benefits received.

Maybe what I should do is type up a nomination form which you can fill in and bung back (or send the answer by email). I have. See herewith.

Fellows in general: One or two people have suggested that it is time that we abolished the distinction between Fellows and the rest. I'd be very reluctant to see this happen. The original idea was that people were elected Fellow because they had published in our field and the other Fellows had found their work reliable and good. The nominations have often been by self, and perhaps this has been a mistake; people may have been shy of putting themselves forward, and this may be why there have been no new elections for a long time (some of it is laziness on my part and on that of anyone else who might have nominated somebody – looking down the Members' List there is a fair number of names who are more than eligible for Fellow). But I think that Eph was right in his initial suggestions as above when we started FoMRHI – there should be a distinction between those who are willing to take some trouble to help their fellow members and those who simply read the Q (and who are very welcome to do so). Your views on this would be welcome (though whenever I say this there follows a deafening silence).

In Memoriam: One of our earliest members (no.31 – he joined on the first day when I was walking round the Early Instrument Exhibition in the RCM drumming up members), Philip Bate,

has died. As most of you will know, I owe him much, for my first ever regular job (after a long career as a rogue and vagabond as musicians used to be called, and maybe still should be) was as the curator of the collection he gave to the University of Oxford. I knew him first, of course, as a member of the Galpin Society and although we got off on the wrong foot to begin with, that didn't last long (it was then my job to persuade people to give instruments to what was intended to become the Galpin Society's Permanent Collection in Edinburgh before someone who'd better remain nameless botched the whole thing – Philip, Brian Galpin and I had a hell of a time trying to sort the mess out without it winding up as a court case but it eventually became EUCHMI and lost the Galpin connexion – and Philip was already negotiating with Oxford, which I didn't know, so we got at cross purposes). As the years went by we did a good deal of swapping instruments, on my part because Philip's was a systematic collection whereas mine was random and general, so when I found something that might fill a gap I offered it to him, and he usually had something duplicate or spare to produce in exchange. On his part because he was always very generous in that way, both to me and to the Bate Collection in Oxford – he would often ring up saying "I've got something for you" and it was always something interesting. He had the advantage of Camden Passage on his doorstep, and he kept a continual eye on all the antique and less antique shops and stalls for anything musical.

He was always also very generous with loans and many musicians borrowed instruments from him, and this of course was something that he wanted the Collection to continue – he gave the instruments to Oxford so that they would be used, as indeed they are. When he died, his widow Yvonne asked that instead of sending flowers people should send donations to the Bate Collection (Faculty of Music, St Aldate's, Oxford OX1 1DB) so that they might buy one or more instruments in his memory. I hope that many people will do so or perhaps even give instruments. I have given them my Delusse traversa because Reginald Morley-Pegge gave it to me to help me start my own collection and because Robert Bigio charged me no more than the cost of the ivory for the end-button and ferrule when he made me a new head to replace the original which was cracked. Robert did that out of kindness to me and to the Bate because it has been on loan there ever since I became curator and could have been, and occasionally was used, and now it's their's. Will any of you follow my example in Philip's memory?

Further to: Comm.1650: I asked Graham whether he knew the wall decorations in Christ Church Library here in Oxford (Eric Halfpenny called them the Christ Church Trophies when he published them in *Galpin Society Journal* 28 and 29). He hadn't met them and has now written them up for this Q. He would be very glad to hear of any more examples.

I sent a copy of that Q to my friend, the Town Piper (the post still exists) of Bilbao, Sabin Bikandi, who is very happy to correspond with and help, and/or learn from, anyone interested in the pipe and tabor as it still played today **as part of a continuous tradition since the Middle Ages**. His address is Zuberoa 12-4.esk, E-48960 Galdakao, via Spain; t/fx +34-944560137; sbikandi@euskalnet.net

Comm.1588: Roy Chiverton writes: "Some time ago you aired the thought that recorder makers might once have bought in ready-made parts to finish off – perhaps like past front-line painters, who added the master's touch to apprentice (or better) work. I don't have ready access to enough information to make a decent Comm. but I recall GSI articles saying how much one of the Stanesbys put into Consols, and how inventories for wills listed houses apparently stuffed with harpsichords, and I wonder whether rates of productivity might be an interesting line of investigation. After all, John Hale was apparently a specialist/mass producer of keys, while I remember being told of an old (?18th c) book on making horse collars which gave details of making and a time of three days to produce one where a modern maker would need two weeks."

Since he wrote this to me about a Comm of mine, I hope I can be allowed to add a couple of points. I have spoken on several occasions of the basic fallacy of so many of our members and other 'early instrument' makers in the slow and careful production of instruments, compared with those of the original period and of the makers, from whom we can learn much, of similar instruments in other cultures today, such as the 'ud makers of North Africa and the Near East, who will knock out several instruments while one of us is only beginning to cut a couple of ribs. John Paul had some interesting material on this in Mark Stevenson's article (John Paul, *Modern Harpsichord Makers*, Gollancz, 1981). We also need a thorough study of firms like Goulding and Longman which almost certainly never made the instruments with their name on - there must have been a whole industry of 'woodwind makers to the trade' in the late eighteenth and early nineteenth centuries of which, as yet, we know nothing.

Lost member: Anyone know what has happened to Eckhard Bohringer? He's no longer in Strasskirchen. If you have a new address for him, please let me know.

Subscribing to FoMRHI: While he was renewing, Richard Abel asked whether we could use VISA or MasterCard, the answer to which is 'No' - too much hassle, costs too much (we'd lose half the sub or more) and anyway I think we are too small to interest them. He also asked whether, if not, we could have someone in the US who would take subscriptions and pass them on in one lump, which usually costs no more than any one individual payment. I've often suggested this in the past and the answer is yes, of course we can. Just so long as we get all the names, so that everybody gets the Q, we don't mind how the money comes. If any of you are interested, please either let me have your name if you are volunteering, or else get together and twist Richard's arm to do the job next year (it was his idea!).

Aubade: Piers Burton-Page asked me if I'd seen the correspondence in *The Independent* about electric shavers in E♭ (I hadn't - I read *The Times* and *Gwen The Guardian*). Apparently someone - surely he ought to be one of our members - has one in D which he claims to be an authentic Baroque razor, shaving at A=413 and therefore in the E♭ of the period.

Changes of telephone number: After solemnly assuring us that the provision of two different London numbers, 0171 and 0181, would provide an ample stock of numbers for many years to come, only a short while later they are changing again, and by the time you read this the old numbers will no longer function. Our main List in April will of course have all the new numbers in it, but I can't face the idea of putting all the London members in the Supplement herewith (there are some new ones, which will serve as samples of how it works), so if I may I will just give you the formula. All 0171 numbers have now become 0207 and all the 0181 numbers have become 0208. This is not the official way of doing it, which is that all London is now 020 and instead of having xxx xxxx as the number pattern it will now be either 020 7xxx xxxx or 020 8xxx xxxx, with the 'xxx xxxx' the present number. This of course allows them to shove in a very large number of new numbers as time goes on, with 03 to 06 as spares (what's the betting that some of those wind up in London?) and 07 for mobiles and pagers, 08 for free or other special rate calls, and 09 for the rip-off ones. They have also changed the number for a few other cities. The only ones which affect our members are Cardiff (Museum of Welsh Life, 029-2056-9441), Portsmouth (Bernard Taylor, 023-9275-1982), and Belfast (David Crookes, 028-9040-2612); the others are Coventry, Southampton, and elsewhere in Northern Ireland where we don't at present have any members. For incoming international calls, we are still +44 and don't use the initial 0 (nor of course the hyphens), so you dial 4420 for London. The change is going to cost a fortune and make big money for printers, with all the new letter-heads, business cards and so forth.

Free offer: Roy Chiverton has a root of *Arundo donax* growing vigorously in his garden up to 10-11 feet tall. Some of it is drying in his work shed and if any FoMRHI member likes to send him an SAE of suitable size, with an indication of desired diameter (up to 21 mm, though he can't guarantee to have all sizes), he'll be happy to send some.

Also he has been using Sibelius (the music writing programme, not the gloomy Finn), running it on an Acorn for some years and now on a PC, and if he can help anyone with that, get in touch.

Code of Practice: I wrote about one of these in the last Bull and now another has come (our present government is very much into this sort of thing) 'for small firms' (which we aren't) saying 'Equal Opportunities is your Business too'. That's true enough and if anyone feels discriminated against, do please let us know. **You each have an equal opportunity to become Hon Sec or Hon Ed – see above.** Yes, I do know that it's a serious matter, too, and we would take it seriously if there were any problem.

A Request: Roy Chiverton asks if there's any chance of reprinting Eph's String Guide, saying that we've been very generous with supplements lately and the Guide was a great idea when it first happened.

Other journals: There are two summaries of *Bouwbrief* elsewhere here. The *American Recorder* for November '99 has an interesting article by Anthony Rowland-Jones on 'The First Recorder: How? Why? When?... and Where?' which is essential reading for anyone interested in the history of that instrument.

Events: There are four **Parley of Instruments Baroque courses** this year: 4-6 February, orchestral weekend for players of violin, viola, cello, bass viol, double bass, flute, recorder, oboe, bassoon, theorbo/archlute and keyboard; 24-30 April Baroque opera (Rameau's *Adonis*); 30 July-5 Aug. Cambridge Summer School, instruments as above, concentrating on Louis XIV; October, chamber music for pre-formed groups. For further information in general contact 01462-459446.

Magnano courses are running as usual, 17-27 August, for playing all keyboards, building organs, choir and organology (bbrauchli@worldcom.ch or Via Roma 48, I-13887 Magnano, Italy).

Stiftung Kloster Michaelstein has a conference on history, construction and playing of 'cello and double bass, November 17-20. They sent me a sheet about it in German (which I'll send Eph in case he wants to include it) – I offered to include one in English but they've not sent it. Information from Kloster.Michaelstein@t-online.de or PF 24, D-38881 Blankenburg, Germany.

The British Library Saul Seminars continue with 22 February 'Whose Record is it anyway' (producers v artists) with Christopher Bishop from EMI and Michael Haas from Decca, and 21 March 'In search of the true Bach' with Martin Elste, each at 6.15 in Meeting Room 4, British Library 96 Euston Road, admission free; information from boxoffice@bl.uk or 020-7412-7222.

Deadline for next Bull and nominations for Hon.Sec. and Hon.Ed: All Fools Day would be appropriate but I might be at a conference in Paris, so let's say Monday April 3.

Quick Work: Just as I was about to print this off, a short Comm arrives in the post from Alec Loretto. Written and posted January 1 in New Zealand, received in Oxford and sent to the editor January 10, printed January ?? And received by you February ?? Not many other Quarterlies can compete on a quick turn round like that.

Have a good spring, Jeremy Montagu, Hon Sec FoMRHI, 171 Ifley Road, Oxford OX4 1EL; jeremy.montagu@music.oxford.ac.uk or jeremy@jmontagu.freeserve.co.uk

BULLETIN SUPPLEMENT

Ephraim Segerman

On Gill's Comm. 1667 on wood stabilisation

It is only the hemicellulose content of the wood that absorbs moisture and so affects dimensional stability. It is also the most unstable chemically, and will readily break down to carbon dioxide and water with higher temperatures (at room temperature it breaks down dry wood at the rate of 1% per century). On prolonged exposure to water, it will break down into sugars by hydrolysis, aided by the natural acidity of wood, and this is speeded up if there is added acidity. The effects of temperature, moisture and acidity are additive. Any way of degrading the hemicellulose will stabilise wood, and known methods include baking, stewing, ponding steaming and smoking. Salting more permanently replaces some water in the hemicellulose with ions, and so also stabilises. Soaking in bovine urine should stabilise wood both by acid-aided hydrolysis and by salting.

On Smith's Comm. 1674 on neck tilts

This tilt affect is so small that it could be the result of distortion of the instrument resulting from the string tension being higher on the treble than on the bass side. Distortion of violins due to string tension is usually not noticed unless one sets out carefully to measure it, when it becomes obvious. Distortion has two parts, an elastic component which springs back when the string tension is released, and an inelastic component that doesn't spring back. The settling-down of a new instrument, which is speeded up by playing-in, is the development of the inelastic distortion accomplished by creep in the wood. Creep absorbs sound vibrations, dulling the sound.

The string tension tends to make the soundboard shorter, making the arching more convex (increasing the arching height), complicated by the pressure of the bridge which flattens the arching in that region, resulting in even more convexity in the arching away from the bridge.

The soundpost often needs refitting (and sometimes the bridge) after the instrument has settled in. The creep is much faster when the string tension is first applied (most happens in the first week), and gets slower as time goes on. We consider an instrument is settled-in when the creep has got to be so slow that its absorption of acoustic vibrations is no more noticeable. If the string tension is later released for some time, some of the creep can recover (humidity cycling helps recovery), so tuning it up again may involve a new settling-in time that would be shortened by playing-in.

Determining whether the distortion due to string tension is enough to explain the tilt observed by Smith would require long-term measurements of a few instruments from new.

Reviews of influential papers in other publications

Reviewing books is traditional, but reviewing papers in other publications is not. It is also traditional to dispute published papers in other publications as well as the one in which the paper was published. Reviews of books not only judge the book's contributions to the field, but they also attempt to tell enough about the contents of the book for readers to be able to make some independent judgements. I suggest that a review of a paper would give a full summary of the new ideas, evidence and conclusions presented in the paper, as well as the criticisms of them. In this Q, I have attempted to write such reviews of two papers that have become quite influential (one amongst the museum community about the authenticity of their instruments, and the other amongst the early musicians about the early properties and uses of gut on fiddles), showing their deficiencies in objectivity.

Fairness, non-judgmentalism, objectivity and understanding

Post-modernist morality involves being fair to, and non-judgmental about, different points of view, and meeting these criteria is considered sufficient for objectivity. No civilised person will argue against the morality of fairness and being non-judgmental about different beliefs people have. But what is missing in post-modernism is being fair to the evidence about reality. In that philosophy, there is no reality or truth beyond our personal and communal beliefs about it. So the 'quality' media, like the BBC, proud of its objectivity, will make sure that spokesmen for opposite views on a topic of current interest will be given their say, but if there is evidence that is basic to the topic, it is often given very poor coverage, and no independent evaluation is given of how fair each of the different views is to that evidence. Public perception is what seems to matter, not truth.

Theories postulate mechanisms which explain how the evidence became what it is. They are

generalisations that predict what the evidence would most likely be where there isn't any, and that offer an understanding of what is going on. Some theories are obvious conclusions from the evidence. What I am concerned with here are theories that go beyond these obvious conclusions.

In the scientific fields of scholarship there are theoreticians who specialise in formulating theories about what reality could be, and experimentalists who specialise in collecting the evidence that invites new theories and judgmentally rejects theories that can't explain it. The ingenuity of the experimental methods may be interesting, but the main interest is in the understanding offered by the theory that can most comprehensibly explain the evidence, not in the evidence itself. Non-judgmentalism becomes appropriate only when the evidence cannot choose between different theories.

In historical arts scholarship, the evidence itself has high interest. Most, including the field leaders, only collect, sort and write up the evidence and its context, coming to whatever conclusions are obvious. They often feel that this is all that real scholarship is about. Scholarship is for expanding knowledge, and they want to be certain about knowledge. Theories can't be that certain. Theories acceptable for scholarship (that can reasonably explain all of the known relevant evidence) can change when new evidence is discovered. Other theories (that present attractive ideas with no attempt to explain all of the evidence) can change as new attractive ideas are dreamed up.

Since professional careers in scholarship can very successfully be pursued without new theories, the practitioners can afford to insist on the certainty of evidence and the apparent certainty of old theories, that are unchallenged and have become generally accepted, which they consider to be facts. They are content with adding to the body of evidence, accepting as mysteries those areas of history about which there is either no direct evidence or where the evidence is not understood. All theories when the evidence is 'not conclusive' are considered to be speculations. Some are dismissively non-judgmental about all theories, but most will tentatively accept those which seem reasonable to build on apparent current knowledge, but judgmentally reject those that question any of that knowledge and ask for it to be reexamined. They are aware that judging the truth of theories is very subjective for the individual, but when there appears to be a consensus developing, they will join the crowd.

But the purpose of historical scholarship is much more than to uncover and present historical evidence, and just to speculate about the rest. It is to use the evidence fully to tell as complete a story as possible of what probably happened, and how it developed - it is about not respecting any mysteries and trying to understand as much as possible. This is too serious a purpose to leave it to mere speculation, and it necessarily involves accepting the risks of uncertain knowledge and taking non-obvious theories seriously enough to check out how well they explain the evidence. Understanding the development of history can't do without them. Theories acceptable in scholarship provide the objectivity that minimises the risks of being wrong in the long run because they make full use of the evidence. Dealing in these theories requires creative imagination and deductive skills, more than just the skills with languages, collecting, sorting and communicating otherwise needed.

Objectivity is about being fair to all of the evidence, and it judgmentally rejects views that are not fair. For our understanding of history to advance, to be objective and fair requires being judgmental - not about the truth of theories (which cannot be objective), but about how well they respect and explain the evidence.

Praetorius's pitch

You might be aware of my paper entitled 'Praetorius's Cammerthon Pitch Standard' in *GSJL* (1997). That standard is quite pivotal in any theory of pitch standards in the 16th to the 18th centuries. Determinations of that pitch from duplicating pipes of the *Pfeiffin* diagram of a set of pitch pipes were done by Ellis (1880) and Bunjes (1966), leading to $a' = 424$ and 430 Hz respectively, and calculated from the pipe dimensions on the diagram by Thomas & Rhodes (1971) which was modified by Gwynn (Comm.342, 1981), leading to $a' = 426$ and 433 Hz respectively.

Surviving Nuremberg trombones of very similar length as Praetorius's trombone (Comms 1327,8 and 1371,2), when blown nowadays, imply that his pitch was a semitone higher. This is evidence supporting the hypothesis initiated by A. Baines (1962), and accepted by many wind-instrument specialists, including Myers and Haynes, that Praetorius's pitch was $a' = 460$ Hz or higher. The followers of this hypothesis have so far either ignored or tried to discredit the evidence from the pitch-pipe diagram.

THE NEWS is that John Koster, in an appendix to a forthcoming book, has claimed that with a different mouth opening and higher wind pressure, the pipes in the 1558 Hofkirche organ in Innsbruck, can be interpreted to result in that pitch. This will be debated. If it stands up as a reasonably probable historical possibility (which I doubt), Haynes and/or Myers have yet to reasonably explain the rest of the interlocking pitch evidence (like Silbermann's), for that hypothesis to compete as an acceptable scholarly theory.

My explanation of the trombone evidence is that modern players blow their instruments to give the richest sound (highest harmonic content), which is as high as one can safely pitch without breaking into the next harmonic, while players in Praetorius's time apparently preferred most of the time to blow at a semitone lower pitch (with the same slide position). He wrote (Crookes translation): "A skilled player...can go beyond the compass in either direction...by practised control of embouchure and wind-pressure, without using the slide at all." It is not clear whether going higher than the compass was within the same harmonic (as going lower than the compass was) or up to the next higher one. In my hypothesis, it was both.

The Lute Quiver

Mary Anne Alburger's chapter in *The British Violin*, reviewed in this Q, quotes an advert for instruments made by Ralph Agutter. John Talbot had consulted him for information on various types of fretted plucked instruments, but used other informants for information on bowed instruments, yet the advert mainly mentioned bowed instruments. It seems then that the instruments included in the advert much more reflected what new instruments were in demand in Edinburgh at the time that he could make, than his primary expertise. The only example of this primary expertise on the list was 'the Lute Quiver'.

I had not come across that term before, and asked Ian Harwood, who hadn't either, but he recalled Psalm 127 (vv5, 6), which says 'Like as the arrows in the hand of the giant : even so are the young children. Happy is the man that hath his quiver full of them: they shall not be ashamed when they speak with their enemies in the gate.' The OED lists 'quiver full' as one of the meanings for 'quiver'. Then a possible meaning of 'the Lute Quiver' as 'a selection of lutes' emerges. Hypothetically, if there had still been a small market for ensemble viols, which included treble, tenor and consort bass viols, but listing each would seem to dwell too much on the unfashionable, he could have just included 'the Chest of Viols' in his list. Both a chest and a quiver are containers, but the terms could also apply to the contents of the containers. Since a chest of viols was usually stored in a chest, it is possible that a selection of lutes could well have been stored in a box that, in musician's slang, could have been called a quiver. If one were going to have a rectangular box made to store a theorbo, it needn't be made much bigger to include several other fretted plucked instruments as well. Any other ideas on this issue would be very welcome.

Mary Rose Fiddles

Mary Ann Alburger (see above) gave a paper on her examination of the two Mary Rose fiddles at last summer's Galpin Society conference, and it will be published in next year's *GSJ*. The soundboard of the one with outward-facing C holes is about 34 cm long and 15 cm wide, and of the other, with inward-facing Σ holes, is about a cm shorter and narrower. They appear to have been flat, with the edges flush with the sides, of pine about 3 mm thick, with no signs of any bass bar or soundpost. In each fiddle the sides and back was carved from a single piece of maple. The sides were about 6 mm thick at the back, tapering to 2 mm at the surface the soundboard is glued to. The sides in the tail and neck regions have mostly disappeared, but they are clearly much thicker there. Side height is about 3 cm, and back thickness about 4.5 mm.

Alburger found features in both fiddles that can associated with tailpiece attachment. There is evidence on one that could indicate the bridge position, and on the other that the fingerboard extended over the soundboard by 5 cm. In her opinion, the piece of ash about 36 x 5.6 x 2.2 cm, found near one fiddle, is far too large and heavy to have been its neck.

Bate Memorial event

Since he wrote his Bulletin, Helene La Rue has told Jeremy that she's planning a Philip Bate Memorial concert/gathering/party/what-you-will on Sunday March 19th at 3.00 pm in the Holywell Music Room and tea afterwards in Wadham - everybody welcome, no charge, but collection in his memory.

21. Musikinstrumentenbau-Symposium
Michaelstein, 17. bis 20. November 2000

Geschichte, Bauweise und Spieltechnik der tiefen Streichinstrumente

Ein Beitrag zum 250. Geburtstag von Johann Matthias Sperger (1750–1812)

Anreise: Donnerstag, 16. November 2000
Eröffnung: Freitag, 17. November 2000, 10.00 Uhr
Abschluß: Sonntag, 19. November 2000, ca. 18.00 Uhr
 Konzerte: 17. November 2000, 19.30 Uhr
 18. November 2000, 19.30 Uhr

Die Baßinstrumente der Streichinstrumentenfamilie haben sich hinsichtlich ihrer Bauform, Größe, Stimmung, Saitenzahl, Spieltechnik oder Klangfarbe in einer erstaunlichen Vielfalt ausgeprägt und erfuhren dabei eine geringere sowie spätere Normierung als kleine Streichinstrumente.

Ausgehend von den überlieferten Instrumenten sollen im Rahmen dieses Symposiums die Geschichte, Bauweise und der musikalische Einsatz der tiefen Streichinstrumente untersucht werden. Des weiteren werden aber auch die akustischen Besonderheiten sowie Fragen des Nachbaus und der Restaurierung zur Sprache kommen. Die nach wie vor schwierige Terminologie erfordert eine exakte regionale Differenzierung, auch unter Berücksichtigung der unterschiedlichen Aufführungspraktiken.

Einen bedeutenden Beitrag zur Herausbildung konzertanter Kontrabaß-Literatur leistete der Kontrabassist Johann Matthias Sperger (1750–1812), dessen 250. Geburtstag im Rahmen dieses Symposiums zugleich gewürdigt werden soll. Das solistische Konzertspiel Spergers ist einerseits geprägt von der solistischen Wiener Kontrabaßpraxis, andererseits in seine Umgebung eingebettet, etwa in die Musiziergepflogenheiten der Hofkapellen von Dresden, Berlin und Ludwigslust oder in die der großen Bürgerstädte wie Leipzig oder Prag. Wichtige Hinweise auf regionale Eigenheiten des Instrumentenbaus und der Spielpraxis sind besonders von den Beständen der reichen mitteldeutschen Museumslandschaft zu erwarten.

Anfragen und Anmeldungen richten Sie bitte an folgende Adresse:
 Stiftung Kloster Michaelstein
 PF 24, D-38881 Blankenburg
 Tel.: +49-(0)3944-903012
 Fax: +49-(0)3944-903030
 e-Mail: Kloster.Michaelstein@t-online.de
 Internet: <http://www.kloster-michaelstein.de>

SYMPOSIUM ON BOWED STRING MUSICAL INSTRUMENTS

Meeting organised by the Edinburgh University Collection of Historic Musical Instruments with the Early Music Forum of Scotland and the Viola da Gamba Society of Great Britain

2-3 June 2000

Call for Papers

The Edinburgh University Collection of Historic Musical Instruments in conjunction with the Early Music Forum of Scotland and the Viola da Gamba Society of Great Britain, is organising a symposium on bowed string musical instruments, to be held in Edinburgh, 2-3 June, 2000.

The Symposium will start with papers reporting recent research, and finish with papers of more general interest to players. The second day (Saturday) will be combined with a meeting of the Viola da Gamba Society and will include visits to the Edinburgh University Collection of Historic Musical Instruments, the manuscripts in the National Library of Scotland, the National Gallery of Scotland, and the Russell Collection of Early Keyboard Instruments.

Keynote papers will be given by Thomas Munck (University of Glasgow) on music manuscripts in the National Library of Scotland, J. Patricia Campbell (University of Edinburgh) on the iconography of viols, and D. Murray Campbell (University of Edinburgh) on the viol string acoustics.

On Saturday June 3 (early evening) there will be a recital by Alison Crum (viols) and John Kitchen (keyboards).

On Sunday June 4 there will be a viol playing workshop with Alison Crum coaching viol consorts which will appeal to symposium participants and also to a wider membership; it is hoped that many will wish to attend both.

The concert and the workshop are being organised as part of the Viola da Gamba Society Meetings Series.

Offers of full-length (up to 40-minute) papers or a short (10-minute) contributions are invited. Papers should be based on original research and discoveries, and may be on any topic relevant to the study of historical bowed string instrument design, making, or use. The viola da gamba will be a focus of the meeting, but papers on other bowed string instruments, and on bows, will be welcome.

It will not be necessary to submit the full text of papers. The language of the abstracts and presentations will be English. Papers should be delivered in person at the Symposium by one of the named authors. It is intended that there will be no parallel sessions. There will be a small fee for participation in the symposium.

The organisers, Arnold Myers and Patsy Campbell, request that abstracts (150 words maximum) of full-length papers and titles of short contributions should be sent (preferably electronically) to Arnold Myers by 15th February, 2000. Acceptance of submissions will be notified by 15th March, 2000. Accepted abstracts will be placed on the Edinburgh University Collection of Historic Musical Instruments website. A full programme, and information for participants, will be sent to all members of the Early Music Forum of Scotland and the Viola da Gamba Society of Great Britain.

Please notify Arnold Myers or Patsy Campbell as soon as possible (preferably by e-mail) if you expect to attend the Symposium – whether or not you intend to give a paper – to help with planning, and so that you will receive further information about the meeting.

Information about the Symposium will be maintained on the website:

<http://www.music.ed.ac.uk/euchmi/fbp.html>

Further information: **Arnold Myers**, Collection of Historic Musical Instruments, Faculty of Music, University of Edinburgh, Reid Concert Hall, Bristo Square, EDINBURGH EH8 9AG, U.K. Tel: +44 (0) 131 650 2423, Fax: +44 (0) 131 650 2425.

E-mail communications to: Arnold.Myers@ed.ac.uk

And J. Patricia Campbell, Department of Fine Art, University of Edinburgh, 19 George Square, EDINBURGH EH8 9LD, U.K. Tel: +44 (0) 131 650 4123 Fax: +44 (0) 131 650 6638

E-mail communications to: J.P.Campbell@ed.ac.uk

Further Report, 4 January 2000

The initial response to the call for papers has been very encouraging: preliminary offers of presentations have come from a number of scholars and makers in the field. The programme be very interesting, and since it will probably be fuller than originally envisaged, provision has been made for the Symposium to open on Thursday June 1st.

Preliminary offers of presentations have come from:-

Mary Anne Alburger (University of Aberdeen)

Mia Awouters (MusTe des Instruments de Musique, Brussels)

J. Patricia Campbell (University of Edinburgh)

D. Murray Campbell (University of Edinburgh)

Michael Fleming

Ian Harwood

Ben Hebbert (London Guildhall University)

Rudolf Hopfner (Sammlung alter Musikinstrumente, Kunsthistorisches Museum, Vienna)

Darryl Martin (University of Edinburgh)

Ton Moonen, Maastricht

Thomas Munck (University of Glasgow)

Brenda Neece (University of Oxford)

Annette Otterstedt (SIMP, Berlin)

Frances Palmer (Royal Academy of Music)

Terence M. Pamplin (London Guildhall University)

Hans Reiners

Roger Rose (West Dean College, Chichester)

Marjorie E. Rycroft (University of Glasgow)

All subject to confirmation. Offers of full-length (up to 40-minute) papers or short (10-minute) contributions are invited. The deadline for submission of abstracts is 15th February.

Review of: *Larigot 24, Décembre 1999, ACIMV, 136 Boulevard de Magenta, F-75010 Paris.*

And as soon as I write this, up rears a guilty conscience. Here is *Larigot 24*, in July we had *Larigot 22* – where is *Larigot 23*? This is one of the reasons why I think you need a new Honorary Secretary – what with trying to get two books finished and also spending too much time on clearing up my late mother's flat, I've not been doing the job as well as I should.

Anyway, after half an hour or so of delving through piles of papers which should have been dealt with long since, I have *Larigot 23*, for Août 1999, to hand, so let's start there.

It begins with a list of makers and dealers in Toulouse, from 1800 to 1914. A useful survey, for while makers get into the usual lists (Langwill, Waterhouse, etc) dealers seldom do so and, as all collectors know, they do stamp instruments as though they'd made them. Not only is this a descriptive list but, as so often with *Larigot*, it is well illustrated with photos of instruments and reproductions of makers' (and dealers') marks and trade cards.

Bruno Kampmann contributes a very interesting short article on the use of the sarrusophone in the orchestra. He says that the contra bassoon was never used in France before this century (odd, because there is a number of surviving french-system instruments from the nineteenth century – damn it, I was just about to write 'from the last century' – it's going to take a while to get used to this!) and that the contrabass sarrusophone was used instead, up to about the 1920s. A few people are taking note of this and Bruno has been lending suitable instruments for performances. Interesting that the need for the revival of an instrument for authentic performance should come so close to our own time.

The following article is by the unfortunate contrabassoonist of the Lyons Opera Orchestra who had only ten days before the first rehearsal of Ravel's *L'Heure Espagnol* to learn to play the thing! Not only did he have to learn

a completely different fingering system, but he had to persuade an instrument built at *diapason normal* (A=435) to produce A=442 at the same time as transposing on an instrument built in B♭! An entertaining article.

As always there is an instrument maker's catalogue, in this case L'Association générale des Ouvriers, Maître, Fonclouse & Cie, a Parisian firm of which I'd not heard, from around 1897. Some interesting instruments include a B♭/A cornet and a C/B♭ double tuba, Adolphe Sax shell-less timpani (I had no idea these were still made and used so late in the century) and a large selection of bulb-operated horns for '*Marine, Chemins de fer, Tramways, Vélocipèdes, Voitures automobiles et autres*'.

Larigot 24 begins with an article on trombones with forward (normal) or backward facing bells – instruments were made to face either way and as well as other illustrations there's a nice photo of Arnold Myers playing one, and a list of known examples, with a couple that face only backwards and 1 valve and 14 slide that face either way, including 1 alto and 3 basses. Bruno Kampmann (who wrote that article) also has one on a weird American horn system by George McCracken which uses double rotary valves much as the Vienna uses double pistons, one valve to let the air in and the other to let it out from the auxiliary tubing. It was, with eight rotors, somewhat heavy but otherwise worked well.

The instrument maker's catalogue is that of August Reichel jr of Markneukirchen. No date given but prices are in RM, which I take to be *ReichMark*, and therefore later 1930s.

Jacques Cools, who is now the editor, has a very interesting article on brass mouthpieces, with the French terms for all the parts of a mouthpiece and a useful list of makers and inventors with some illustrations of their efforts.

As always, many good things for anyone working with wind instruments.

REVIEW: *Makers of the Piano 1820-1860, Volume 2, Martha Novak Clinkscale, Clarendon Press, Oxford, 1999. With action drawings by John R. Watson. ISBN 0 19 8166257. 487 pages. Publisher's price: £80.00*

Collectors and museum curators who found Clinkscale's earlier book to be a useful reference tool will be delighted to have this second volume. The period covered by this new book, 1820 to 1860, was arguably the greatest age in piano history – the era of Chopin, Thalberg and Liszt, the Great Exhibition, and the foundation of the house of Steinway in New York. This was the period when the grand piano became a powerful and dominant force on the concert platform – and the upright piano became the ubiquitous source of home entertainment for the newly prosperous middle classes. Hundreds of thousands of these instruments survive, and Martha Clinkscale has here made a pioneering attempt to catalogue them, and to add biographical material about their makers.

Two thousand four hundred individuals are listed as piano makers, sometimes just with a single sentence giving an idea of when and where they worked, sometimes with two whole columns chronicling an important dynasty in detail. Following directly, about 30% of the entries have an extract from Clinkscale's database, providing information about extant pianos, and where they may be found.

Anyone who has ever taken apart a piano from the 1850s will not need to be reminded how intricate the mechanism can be; how well designed it was; and how accurately their repetitive production work was done. Pianos surviving from this period are among the most complex artefacts of their time, and remain a most impressive testimony to nineteenth-century advances in manufacturing skill. So it is an area well worthy of study, not just as a manifestation of musical culture, but as a record of technology and commerce. To what extent Martha Clinkscale's books will aid that study is still debatable. Accurate and consistent description is not her forte

To sample the best of *Volume II* you could turn to 'Albrecht' – a well-constructed and informative biography, properly annotated at numerous points to show from which of her four sources Clinkscale has drawn the information. This is a very welcome improvement on *Volume I*. The method adopted here, rather belatedly, is that used by Donald Boalch in *Makers of the Harpsichord and Clavichord*. Cryptic citations are inserted within square brackets, from which the reader is directed to a twenty-five-page bibliography, to find a more complete reference to the sources used.

Another innovation is that the author has undertaken some original research, revealing the names of numerous piano makers who were not listed in Harding or any previous compilation. This is mostly the result of combing through old newspapers and trade directories from London. A little caveat has to be placed on this, however. Few of these lesser-known names are represented by surviving pianos in the database and the reasons could be that few if any pianos were made by these people. As others have noted, some of these men were nothing more than purveyors or merchants, and many others were only makers of component parts. There is of course no easy way to distinguish them. The problem goes right back to the eighteenth century when music sellers like Longman & Lukey or John Bland sold pianos misleadingly inscribed with their own names. This became common practice in the nineteenth century when all sorts of provincial and colonial traders bought in unmarked, ready-made instruments and then applied their own label. It would take a great detective to trace some of these products to their rightful origins. And this could only be done by an observant and careful researcher, well practised in recording technical details. Yet technical precision is the feature that is conspicuously absent from Clinkscale's work,

and this remains its most serious limitation. Since about half the space in *Volume 2* is devoted to descriptions of surviving instruments, this must be a major fault.

As long ago as 1933 Rosamond Harding emphasised that the action design is a vitally important component of any piano. She carefully drew and named dozens of them. It is therefore very disappointing to see that Clinkscale is unable to build on this. There is absolutely no consistency in her terminology. So, for example, successive square pianos from Clementi are listed as having *English* action, *English Double* action, and *Grasshopper* action. They are variously said to have *Over dampers*, *Mophead dampers*, *Crank dampers*, and *Dolly dampers*. In fact, hidden behind this needless obfuscation there is only **one** type of action and **one** type of damper! Clinkscale's readers are very badly served by this sort of thing. German terminology, presumably transcribed from museum catalogues, produces some bizarre translations. Among them Clinkscale explains *Stiefeldämpfung* as 'flat overdampers'. (*Stiefeldämpfung* is in fact the stirrup damper system used in Stein's pianos. How they come to be rendered as flat overdampers is incomprehensible.)

Another problem is inconsistency. Trawling only through the letters A and B, I find that there is a very thorough listing of extant pianos from Alpheus Babcock – which I attribute to the research undertaken by Darcy Kuronen. This quality of treatment is what is needed with every maker. By contrast Brinsmead is only represented by one piano. Clearly this is not consistent or representative. Of course, if Broadwoods' pianos were to be tracked down and reported as thoroughly as Babcock's the book could not bear the overwhelming amount of data. Some sort of selection criteria ought to be in place. But, so far as I can see, the inclusion of any given instrument depends only on the serendipity principle. Turn to page 409 for example. Among thirty-odd pianos by Wornum you find an entry for a grand piano, tentatively dated *c.1859* [i.e. just within this book's remit]. Apparently it was seen by Peggy F. Baird in 1992 at an antiques dealer's shop,

whose location is not given. There is no worthwhile description. We do not know its present ownership, location, or anything else that will identify this instrument if and when it surfaces again. So why is it here?

Given the importance of the pianoforte in social and musical life in the nineteenth century, *Makers of the Piano* will be used as an important reference book. Collectors and curators, who will constitute the main readership, cannot afford to be without it. And indeed, for anyone who needs to locate pianos by a named maker, it will be invaluable.

On the other hand, you may come with a different sort of question. For example, 'Who was making pianos in St Petersburg?' [or Milan, or Budapest]. But you will not find an answer. The reason is that, despite the ease with which information can be sorted and rearranged in a database, Clinkscale declines to provide a geographical conspectus. Can anyone understand why? Boalch had one.

But there are good things too. Dip into this book with no particular aim in view and you may find some interesting and surprising scraps of information. I used to think that *Prellmechanik without escapement* was more or less obsolete around 1800. Clinkscale's book has informed me otherwise. Apparently Geronimo Bordas, working in Barcelona as late as 'c.1830', was still using this action in a six octave square piano that is now in the Shrine to Music Museum. Armed with this information I was able to E-mail keyboard conservator John Koster to find out more. From him I learned that the Bordas piano is surprisingly equipped with brass *Kapseln* that look identical to those used in Vienna. Maybe this proves how useful Clinkscale could really be – and may yet become, if the problems can be frankly addressed and its shortcomings rectified.

A praiseworthy addition to *Volume 2* is the inclusion of seven action drawings, provided by John Watson. Their precision and clarity are exemplary. You will see none better.

**CD Review: Two Upon a Ground. 17th century English instrumental music
performed by Charivari Agréable. Signum Records SIGCD007, 1998**

No price indicated

This disc arrived for review some time ago, but I was not sure at the time how it would be of particular interest to readers of FOMRHIQ. It is unusual to see a record review in these pages; but it would seem natural for instrument technologists to want to hear the end-product of their labours from time to time. In addition to its main feature - divisions for two bass viols by Simpson and Jenkins - the disc contains solos and duets by Lawes, Locke, Hume, Tomkins, Purcell and Ennemond Gautier (the latter included as a display piece for double-strung English theorbo in G). Continuo instruments used also include guitar, chamber organ, spinet and harpsichord, the last two being original 17th century instruments (Ruckers and Keene) apparently in excellent condition.

The sleeve notes quote various sources by Christopher Simpson and his contemporaries, which provide a useful background for working out what this music is all about. There is however one interesting passage from *The Division Viol*, that is not quoted:

"A Viol for Division, should be of something a lesser size than a Consort Bass.....The Sound should be quick and sprightly, like a Violin; and Viols of that Shape (the Bellies being digged out of the Plank) do commonly render such a Sound."

Viol makers have often debated the merits or otherwise of the phrase in parentheses but have overlooked the significance of the phrase 'viols of that shape', referring to violins. On the facing page the first instrument shown by Simpson is indeed violin-shaped. Michael Fleming, who wrote some of the sleeve notes, was too modest to mention that he had successfully reconstructed a division viol on the strength of this information alone. I know this because I commissioned him to do it. Division playing on this instrument perhaps requires the arms of an orang-utan and more athleticism than that of your average viol player, and stratospheric (treble clef) passagework can be quite difficult to reach, but the responsiveness and resonant quality of the instrument are undoubtedly what Simpson had in mind.

So how do the instruments used in this recording fare with the repertoire? The continuo instruments - superfluous in the bass viol duets, of course - are evidently intended to create some variety of texture but this is hardly noticeable since the style of playing is unchanged throughout. Since the continuo realisations and the unaccompanied pieces were basically unremarkable - except for some quirky harpsichord solos - I focussed attention on the viol duets and solos. On the disc Susanne Heinrich plays bass and treble viols by Merion Attwood, and Susanna Pell bass and treble viols by Jane Julier. We are told only that the instruments are 6-string, not the sources or models on which they are based. One of the players produces a consistently bright and incisive sound which is ideal for a soloistic repertoire, while the other's is the lack-lustre and soporific sound typical of most present-day English viol ensemble playing. In any case the performance of the divisions by both players has a somewhat dated feel to it. It reminded me of lute *passaggi* playing during the 1980s - fast, metronomic (no hint of notes *inégaes* of course - except in one set of divisions in F which as a result are considerably more lively than the rest), and lacking grace (except for the occasional inverted mordent which Eph has persuasively argued is not now used the way that it was historically). I do not know whether lute playing has moved on since then, because I haven't heard any; perhaps both players and audiences have become bored to death. The viola da gamba has perhaps fared slightly better, but we do not seem to have moved on in the sense of developing a genuinely extemporised style of division playing, which in the 17th century was, as the sleeve notes remind us, "received with greater applause than those Divisions which had been most studiously composed". The recording is pleasant enough to listen to, and those who are used to and expect the mellifluous and unadventurous style of playing that has become a modern convention will not be disappointed. Those searching for individuality, inventiveness and inspiration will be.

Bouwerskontakt Bouwbrief 95, My Summary

I already informed my readers last time (are there actually readers of these summaries?) that the Bouwbrief is very much organ oriented, and again #95 is no exception. A bit of a problem as my knowledge of English organ terms is limited.

The first article is by Jan Burema, who describes the clavecitherium built by Matthijs Mijnders. Burema has been intrigued for a long time by the clavecitherium, but never build one himself. The description includes a schematic of the workings, but no working drawings.

The first organ article describes in detail the organ 'toetstractuur', or the keyboard traction(?). The article is especially concerned with the calculation of the dynamic key pressure required to open the valve and to sustain that open position. Although I found the notation of the formulae somewhat confusing, using old units, such as the gf (gram-force), and mixing 'pressure' (force per unit area) and 'force', it is well presented (5+ pages), with good illustrations. (As an aside, because the Bb is retyped by the editor, I have a feeling that the way formulae are presented is limited by the word processor used and the interpretation of the editor and sometimes leads to strange results.)

A short article describes a poor man's bending apparatus (a metal tube heated internally by a gas burner) is followed by a review of the book *Klavecitherium – zelf gebouwd*, but I suppose it is really called *Das Clavecitherium – selbst gebaut*, as it is by the German author Konrad Nagel. The book contains a detailed guide and a novel method to get a stable hinge for the jacks. The main point of criticism of the reviewer Burema is that the author leaves little room for alternatives. The ISBN is 3-87537-229-8.

The second organ article is by Peter Hoogerheide and concerns the design of the place of the spring loading the valves ('ventielen'). Some calculations are presented to obtain a good balance between reaction speed of the valve and the air pressure. The author has verified his calculation with a prototype and found the results confirmed.

Bb 95 contains two more book reviews, the first a short review of a Dutch book: *Akoestiek van de instrumenten van de violenfamilie tussen wetenschap en kunst*, by Jan James. The reviewer, Gijs van Ulsen, considers it to be a basic introduction and believes an older book *The violin explained*, by Beament, as a better acquisition.

The other review is a rather long one by Jan Bouterse on a German book: *Vergleichende Untersuchung historischer Blockflöten des Barock*, by Thomas Lerch (ISBN 3-922378-14-5). It must be a rather voluminous book as already chapter 9 alone takes a 110 pages (and it has 13 chapters in total). The study has investigated many (~60) historical instruments and contains bore diameters for all of them, which may make it an attractive acquisition, certainly because the price is only DM 78. However, Bouterse retains some reservations about the results which concentrated on the sound quality aspects, rather than sound frequencies.

The Organ Working Group and the String Working Group report on their regular meetings. The 'Strings' (Wim Tol) includes a short summary of an older book *Geigenbau in neuer Sicht*, by Rödning, which he has translated into Dutch.

A two-page article on 'Tools' is not worth mentioning, while another short article by the same author provides some information about the English company Kirkpatrick, which makes old-fashioned hinges and handles in cast iron. Apart from further reviews of 3 German music magazines and the FoMRHI (up to C-1609, so they have not yet arrived to the first of my reviews when we would get a dog-bite-tail situation), there was not much more in Bb 95.

Review: *The British Violin - Historical aspects of violin & bow making in the British Isles*, ed J. Milnes (British Violin Making Association, Oxford, 1999)

This little book (51 pages) is made up of papers given at a one-day (4 April, 1998) symposium associated with the exhibition *The British Violin: 400 years of Violin Making in the British Isles* at the Royal Academy of Music, London.

Introduction, by Charles Beare (1 page)

The introduction outlines the ups and downs of quality in British bowed-instrument making during this period. The first peak in quality was in the 17th century viol-making tradition. The next peak was in the late 18th century and the first half of the 19th century, and the final peak has been in the last 30 years. No doubt, the criteria for judgement of quality are modern.

The Violin in Tudor and Stuart England, by Peter Holman (8 pages)

This chapter is well written and full of good information, as we would expect from this author. It is essentially derived from his book *24 Fiddlers*. There are several points in his exposition of the the 16th century history of fiddles that differ from the theories that I have proposed in recent Comms, and these need debating. They include his calling all fiddles 'violins' (ignoring the change from the original names 'violen' or 'violan' to 'violin'), and that they derived from the medieval fiddle, rather than the Spanish vihuelas that the viols derived from. One danger of being a professional early music performer while doing historical scholarship is that traditional assumptions of the early music movement can be confused with instrument history. Thus his statement that the medieval fiddle was an alto-range instrument is not consistent with the ranges of medieval fiddles derived from the properties of the gut used then and the estimated string stops seen in the pictures.

I was somewhat surprised by the statement: "In sixteenth-century England the viol and the violin were played almost exclusively by professionals. A class distinction between viol players and violinists only began to develop in the early seventeenth century, when there was a sudden boom in viol playing among gentlemen amateurs just as the violin was beginning to be played by humbler classes of professional musician."

There is much evidence that 16th century professional musicians, that were not just lute specialists, usually played more than one type of instrument. Because of social conventions, only children and noble adults could play instruments publicly while sitting. So professional musicians generally played standing, while smaller viols had to be played kneeling. Servants to the aristocracy would have played both fiddles and viols, and probably taught music to children. The Eglantine Table is evidence of fiddles probably played by smaller children. The Mary Rose fiddles are clear evidence for a humbler class of 16th century fiddlers. Since fiddle playing was primarily for dancing (an activity widespread across the population), we can expect that there was much humbler fiddle playing for this purpose. The types of fiddles played by the more humbler players apparently changed when the violin became popular.

Holman expects fiddle bands to include only fiddles. So when the royal fiddle band was called a 'consort', and consorts then usually played with continuo, he is still convinced that it played without continuo. It would be surprising if a fiddle player couldn't play a lute continuo part if appropriate, as most modern orchestral players could play a simple piano continuo part if appropriate. He quotes Anthony Wood that from the Restoration onwards, 'viols began to be out of fashion and only violins used', yet this seems to be at some variance with his illustration showing 'members of the Twenty-four Violins', where a number of instruments depicted look remarkably like viols.

Tree Ring Analysis Applied to English Instruments, by John Topham (11 pages)

An excellent explanation is given here of the basics of dendrochronological dating of soundboards. Each grain line in the soundboard is a ring in the log it came from, and represents a year of growth of the tree. A year of good growth makes a wider ring, and a year of poor growth a narrower ring. If the pattern of ring widths is almost identical between two pieces of wood, one may conclude, as Topham does in a few cases, that they came from the same tree. The patterns in most of the trees of the same species grown in the same region are usually close enough that, using samples of that wood of varying age from freshly felled to as old as one can get, one may establish a 'reference chronology' which assigns a particular year to each ring in the pattern. Then, matching the pattern on a soundboard to that in the reference chronology, the year of the youngest ring seen on the soundboard establishes the earliest date at which it could have possibly been made. Comparison is both by graphs (logarithm of ring width vs date) and by statistical calculation or correlation.

Topham had measured 104 instruments, and was able to date 66 of them. Success was with about half of the 38 violins, about 2/3 of the 20 violas and 6 viols, and about 3/4 of the 40 cellos. The greater success with cellos is attributed to the larger number of rings available to measure. Most were 18th or 19th century instruments. Around 16 of them, mostly violas, had one-piece soundboards, and of the others with two-piece soundboards, only around half used fully matched pairs. The others were either statistically correlated unmatched pieces or completely unmatched pieces.

A major objective of Topham at this stage in his work has been to explore how well different reference chronologies correlated with his instrument measurements. He used various spruce reference chronologies, as well as those of larch, fir and pine. Some of the instruments matched only with fir chronologies, implying that this was the wood used. All of these chronologies were established by other specialists for particular regions. He also set up his own reference chronology for English instruments that cross-matched particularly well. That chronology is remarkably similar to the one established by Dr. Klein from instruments made in Nuremberg from wood that came from the Erz Gebirge region north east of Nuremberg, substantially north of the Alps, implying that this may be the source of much of the soundboard wood English makers used.

Scottish Violin Making: Myth and Reality, by Mary Anne Alburger (12 pages)

The earliest reference to a violin maker in Scotland that she found is an advert (*Edinburgh Courant*, edited by Daniel Defoe, 13 May, 1707) for Ralph Agutter, whose information on the cittern, guitar, lutes and theorboes was included in James Talbot's (c. 1694) compilation of instrument information. "Ralph Agutter of London, lately come to Edinburgh, Musical Instrument maker, is to be found at Widow Pool's, perfumer of gloves, at her house in Stonelaw's Close, a little below the Steps; makes the Violin, Bass Violin, Tenor Violin, the Viol de gambo, the Lute Quiver, the Trumpet Marine, the Harp; and mendeth and putteih in order and stringeth all those instruments as fine as any man whatsoever in the three kingdoms, or elsewhere, and mendeth the Virginal, Spinnet, and Harpsichord, all at reasonable rates."

Other 18th century makers working in Scotland included John Grace (also originally from London), Joseph Ruddiman and Matthew Hardie. Hardie, sometimes referred to as the 'Scottish Stradivari' is the maker that the myths were about. He worked into the 19th century, and the rest of this history concerns makers of the 'Hardy school', working till the middle of the century.

British Bow Makers, by Philip J. Kass (15 pages)

The only identified English bow makers who worked before the middle of the 18th century (when the author states that bows with screws first started to appear) were Peter Wamsley and his successor Thomas Smith. Their bows had the swan head design. The hatchet head design

associated with the violinist J. B. Cramer apparently came to England with him in 1772. The modern style of head associated with the violinist G. B. Viotti apparently came to England with him in 1792.

English bow making has been dominated by the Dodd family, which worked for a century from about 1765, the Tubbs family, which worked from about 1800 to about 1920, and the Hill workshop, which worked for almost a century till 1983. The appendixes show the family trees of the Dodd and Tubbs families, lists the makers and their identifying marks in the Hill workshop, explains the significance of the different brands on Hill bows, and charts the working dates of the members of the Dodd and Tubbs families plus Louis Panormo, James Brown II, William Acton, George Darby and Samuel Acton.

The National Register & Database of Musical Instruments: A Pilot Study of the Cello, by Brenda Neece (3 pages)

An editorial note preceding this paper states that "Academic restrictions relating to her research prevent publication of the full text of Brenda Neece's paper to the symposium. What follows is a summary, with illustrations of preliminary results." This has resulted in a paper that offers only an anecdotal sampling.

Neece reports the earliest surviving English 'cello' found, one by Jacob Rayman in London dated 1646. Of course it was called a bass violin at the time, not a cello. A cello by Robert Duncan survives in essentially its original 1736 state. DIY cellos made of sheet iron, packing crates and petrol cans are mentioned, as well as cellos with innovative designs (one with detachable back and neck, and one with a built-in hollow chamber from the middle of the back ending in a horn instead of the scroll).

General Comment

The quality of the contributions to this book is generally high. My only complaint is that there aren't enough of them.

FoMRHI Comm. 1682

Ephraim Segerman

Review: Problems of Authenticity of Sixteenth Century Stringed Instruments by Karel Moens pp. 41-49 from CIMCIM Newsletter No. XIV, 1989

This paper was given 11 years ago at a conference in Berlin. It can be seen without the illustrations in the CIMCIM web site: www.music.ed.ac.uk/euchmi/cimcim/index.html. I have been told that most of the museum community appears to have been convinced that its thesis is true. That thesis is that, despite appropriately early dating of the belly by dendrochronology, a considerable number of surviving 16th century bowed instruments in museums can well be relatively recent fakes, presumably put together for antiquarian purposes. That is unfortunate, for this paper, with its open bias, was obviously never intended to be a piece of historical scholarship. As a solicitor does in legal proceedings, it interprets all of the evidence that could possibly be so interpreted as supporting his faking case, and ignores other interpretations and evidence. The alternative theory would be that each instrument is essentially what it appears to be, subject to the normal repairs and maintenance expected since then. An objective scholarly presentation of the faking theory would ignore the largely subjective impressions of wood age, and concentrate on whatever critical evidence there is that can be more readily explained by the faking theory than by the alternative theory.

Below are given explanations of his evidence according to that alternative theory. If we discount the apparent bias in his unsupported impressions of wood age, the case of Moens is very weak indeed.

In the same CIMCIM Newsletter, Peter Klein reported tree-ring dendrochronological determinations of the earliest dates of the bellies of four 16th century instruments in European museums. They were the Ventura di Francesco Linarolo violin label-dated 1581 (Vienna C96), the Giovanni d'Andrea da Verona lira da braccio label-dated 1511 (Vienna C94), the Hanns Vogel bass viol label-dated 1563 (Nuremberg Mi 5) and the undated Heinrich Ebert treble viol (Brussels 1402). The dendrochronological dates are consistent with the label dates for the bass viol and the lira. The method gives a not-unexpected date of 1580 for the treble viol, and an unexpected date of after 1640 for the violin.

The Moens paper appears to be a companion to the above Klein paper as each refers to the other, and this association has contributed to its perceived authority. Klein's work involves a valid scientific method, and some non-specialist readers have mistakenly thought that the Moens paper did as well.

The author presents detailed observations on each of these instruments plus the Wendelinus Tieffenbrucker lira da gamba label-dated 1590 (Vienna C 95). He also presents a few observations on eight Antonio Ciciliano viols (Vienna C75, C76 and C77, Brussels 1424, 1425 and 1426, Museo Civico Bologna 1761 and Academia filarmonica Bologna).

Linarolo violin

Let us first consider the violin soundboard, which Moens does not discuss because the later dendro date than the label date already discredits it. Its inside surface is covered with gauge marks, so that a bass bar could not be fitted to it. That is not the way that soundboards of violins were made at its dendro date. If the rest of the instrument comes from the label date (which Moens doubts) the belly most probably was a replacement copy of the original (if it were modernised, it would have had a bass bar). There has always been respect for relics of the past, and modernisation only becomes necessary if a musician owner wants to exploit that respect in performance. If, as Moens believes, the rest of the instrument is later, then the soundboard represents the faker's concept of what a violin of the label date was like, including a soundboard without a bass bar. Where could the faker get that idea from? The faker, even if he was from the 19th century, was much closer to the label date than we are, having access to early instruments and information about them that we don't have. All of the other 16th century violins we know of have been modernised. Therefore his faked soundboard is likely to have more historical validity than any of our purely intuitive guesses about what such soundboards were like.

Historical scholarship is about objectively finding the most probable picture of what was in the past from the surviving evidence. It can never be completely certain, and there cannot be an objective measure of that certainty. The popular criterion for how much probability is enough to convince is consensus amongst the experts. But this is a matter of fashion, and is not objective about the past, as historical scholarship should be. Uncertain history based on uncertain evidence is much more useful than pure speculation or the non-history of a 'mystery'. It is good scholarship because it makes the maximum objective use of the evidence.

Concerning the rest of the instrument, Moens writes: "First superficial examination reveals that the pegbox, the neck, the ribs, the bottom, the fingerboard and the tailpiece are extremely well conserved and that the belly is more damaged and seems to be older." The judgement of age here is probably based on cracks in the soundboard wood and the condition of the surface (dirt and the state of the varnish). Age contraction, especially if there is not the pressure of tuned-up strings to spread it, will easily pull cracks on a spruce soundboard without similarly affecting the hardwood body. As for the surface, the observation of Moens, that the varnish on the rest of the instrument is newer and over the soundboard varnish, could well indicate that the rest of the instrument has been cleaned up and revarnished at a late date. This seems to have happened to all of the instruments in that collection.

Moens tries to induce distrust in the originality of the wood of the rest of the instrument by

writing that it is "a wood very similar to walnut, but probably it is more likely to be a kind of mahogany. ... An exotic wood like mahogany would be unlikely for a sixteenth century instrument." His judgement of the probability of it being a kind of mahogany is given no support, and probably comes from his expecting it not to be from the 16th century. The Diaz guitar in the RCM London was made of a wood very similar to mahogany (kingwood) that was available to instrument makers in the 16th century.

He next writes that "the inner construction is highly individualistic and cannot be compared to any other violin." It is not necessarily individualistic, and could easily be quite standard for a violin made in Venice around 1600. We just have no other evidence about such instruments.

Following is "The spruce upper block is very thin, very wide and has horizontal growth rings. The neck connection isn't strengthened with nails." This could be evidence that violins at this early stage in their development were made that way and were not strung as heavily as later.

Then: "The connection of ribs and bottom is strengthened by a piece of parchment. On the inside of the bottom too, seven horizontal and four vertical strips of parchment are glued. Such parchment or paper reinforcements are known from bellies of late seventeenth century instruments from southern Germany and Austria. However, we have never seen then fitted in this way in bottoms, and certainly not on such early instruments. These parchment strips, which are glued in illogical, but visible, places, give the impression of having been applied with the intention of giving the instrument an archaic appearance." This impression is at variance with the relevant history. Violins c. 1600 were made by high-class makers who normally made lutes and viols. Parchment reinforcement of the joints between ribs and bottom were common on viols then, and vertical and horizontal parchment strips 'in illogical and visible' places was standard over the insides of lute backs. This was at the beginning of violin making, before the specific traditions of baroque violin making had properly developed. Another example of this is the parchment platform on the back for the soundpost to rest on, which Moens does not mention.

The final comment by Moens on this instrument is "The label doesn't seem old, either, and the handwriting and text are quite different from other Ventura Linarol labels." Impressions of label age come from darkening by dirt and yellowing of the paper. With the variability amongst different papers in tendency towards yellowing, it is unlikely that anyone can reliably judge the difference between an age of four centuries and, say, two. It is easy to clean dirt from a label (as it is easy to dirty it to make it look older). The point about handwriting and text would be worrying only if Moens could report that there was a consistency amongst other labels of this maker that this one was different from.

On the evidence presented here, a persuasive case for the instrument (except for the soundboard) being from other than the label date has not been made.

Giovanni d'Andrea da Verona lira da braccio

Moens claims: "Here too, a whole series of details point to a belly that is much older than the other parts. Except on the edge, which shows the same cracked varnish as on the other parts, the belly has an older varnish." This instrument, like the violin, appears to have had all except the soundboard cleaned up and revarnished at a much later date.

He continues: "The dendrochronological dating of the belly corresponds with the date on the label. This label, however, consists of two pieces. The upper piece with the name seems to be older than the lower one with the date. Both parts have been written in a different handwriting and with different ink." The last point probably indicates that half of the label is not original. If it was done with the intention to deceive, the whole label would have been faked. It most probably was an attempt of a later repairer to add the information that he honestly thought should have been there. This has not been uncommon.

Following is: "Almost all worm holes in bottom and ribs have been cut open. These parts have

probably been worked and assembled when the wood had already been damaged by woodworm." A good piece of well seasoned wood has always been very highly valued by makers, and a few inactive worm holes that present no threat to structural integrity need not be a deterrent to its use in making a new instrument. Even modern violin makers, who are fussier in many ways than early ones, will sometimes use such wood.

Next, he writes: "The construction of the instrument is unusual. The ribs aren't bent, but sawed and glued on belly and bottom. The neck has never been replaced, but is nevertheless very oblique. It is glued without an upper block or any other reinforcement between the two ends of the ribs. This connection is not very stable and is highly unusual. Up till now we have only seen such a neck connection in another instrument from the same Catajo collection [Vienna C70], whose authenticity is also problematic. These and many other findings justify some of the doubts concerning the age of the instrument in its actual composition and shape. Despite the age of the belly, it is very dangerous to draw conclusions from this instrument concerning the construction of the lira around 1500."

This construction is just what we should expect from an instrument that is intermediate in construction technology between medieval and Renaissance fiddles. Medieval construction had the back, sides, neck and pegholder all carved out of one piece of wood. The rebec type of fiddle and the Italian Renaissance cittern were late survivals of this construction method. The Renaissance/baroque construction had the back and sides separate pieces attached to each other and to the separate neck and pegbox. The intermediate construction method had the sides integral with the neck and pegholder, with the back glued on, as the soundboard was. As seen in the late 15th century iconography, this construction was advertised by concave sides or by overhanging soundboard and back, or both.

The violation of this intermediate construction style by this lira is that the neck is not integral with the sides. It was probably sawn off later to be reset at a more oblique angle to accommodate a higher bridge, and then reglued. The stability of this construction style depends on the thickness left in the sides near the neck, and that should reflect the string tension the instrument was expected to bear. As it was primarily a chamber instrument, it is unlikely that the tension was high. The lira retained many aspects of the medieval fiddle in playing style as well as construction. An instrument of this design could have been made at any time in the first half of the 16th century.

Tieffenbrucker lira da gamba

On this instrument, Moens writes: "Sometimes traces of older constructions make it possible to reconstruct precisely what happened to an instrument. This is the case with [this instrument]. ... It has not been possible to determine the age of the belly until recently. There are two signatures on the instrument whose authenticity is very doubtful: a label and an inscription on the pegbox. The belly and the back have a completely different arching. The belly is almost flat and the back is highly arched. Once again, only the belly shows traces of an old varnish. The other parts have a transparent varnish. Some parts of the edge of the belly have been recut, replaned and revarnished with the same varnish as was used on the back. All this could point to an alteration in the outline."

Moens does not "reconstruct precisely what happened to" this instrument. We can presume that the dendro date is consistent with the label date because if it were later, he would surely have said so. What is doubtful about the label and inscription is not stated. Tieffenbrucker was primarily a lute maker, and lutes have flat bellies and very rounded backs. Thus, much lower arching on the belly than on the back would not violate the aesthetic sense of a lute maker, like it might a modern person steeped in violin history. As with the other Vienna instruments discussed here, all except the soundboard has been cleaned up and revarnished at a more recent time. There appears to have been some repairs to the edges of the belly. None of this points to an alteration in the outline.

He continues: "Similar traces of alteration of the outline were found on the back. Originally the

ribs were glued in a groove in the back. The centre parts of the ribs are still glued in this groove. Where the outline of the back has been recut the groove leaves the edge. The direction of the groove corresponds with the normal outline of a cello. Presumably because of the constriction of the outline, three new rib pieces had to be mounted. The inside of the back shows scorch marks at the bottom which make it likely that the back has been rebent at this place. This might have been necessary because the vaulting of the back was incised which recutting the outline. By bending down the edge the whole outline was again brought into one plane."

Lire da gamba were instruments that were never made in quantity. Since each one was probably unique for the maker at that time, this one could well have wanted to save work by adapting a spare back from another instrument. In this case, for a cello-like outline, it would have been a 5-string basso da braccio.

Next, he writes: "The woodworm holes in the neck and the pegbox have almost all been cut open. Probably those parts were cut from a piece of wood which already had woodworm. The fingerboard is old but it has been recut and it probably originally belonged to another instrument. The inlay is different from that in the belly." As mentioned before, it was not uncommon to use wood with some worm damage in new instruments. Recycling a good piece of fingerboard wood is also not unexpected. As for matching the inlay on the back with that in the soundboard, we should not project the types of fussiness of modern makers onto early ones.

He concludes: "From these and from other findings it can be concluded that the body has probably been cut from the parts of one, or more, cellos. It would be wrong to draw conclusions from this instrument about the construction of the lirone around 1600." The maker thought that it was appropriate to incorporate a back that was originally made for another instrument into his concept of what a lira da gamba was (I don't know of any evidence for calling this instrument a lirone). Moens is here saying that we should not accept that concept because he believes that the maker was much later than Tieffenbrucker. Moens has not in any way established that.

Vogel bass viol

Of this instrument, Moens writes: "much evidence points to the fact that the neck, belly, ribs and back do not belong together, and that parts of the body are not original viol parts but parts of one or more bass violins." Modern making traditions involve exclusively using fresh wood. We should not project our modern expectations on early makers, who were much less inhibited about recycling wood from other instruments. Parts might not belong together as an aesthetic creation or perhaps in terms of first use in an instrument, but they can belong together if they succeed in providing a working tool to make music with. The only issue that is historically interesting is whether this combination of parts was assembled in the 16th century.

He continues: "Belly and back have completely different arching. The arching of the back is very high and looks quite complete, with a channel at the edge. The belly is much flatter and has no channel. This may point to a recutting of the belly. Other indications of this are the extremely thin edges of the belly and the bottom, these being the result of the replaning of the edge. Also the varnish looks different. In both day and UV light an old varnish can be seen in the middle of the belly and on parts of the ribs. This varnish is completely absent on the back and along the edge of the belly and its purfling. Also, along the edge there are visible traces of a lot of recutting. The whole instrument has been revarnished with a coat of the same lacquer which is only used alone on the back."

I don't know of any reason to expect similar arching in the belly and back in the lute-dominated 16th century. Early English viols in the 17th century were made with shape control applied to the back, with the belly fitted to whatever shape the tops of the ribs offered, so a groove to control rib shape only on the back in this earlier instrument would not be unexpected. Very thin edges can improve bass response, especially on instruments with relatively low string

tension (as Mersenne's bass viol had). It is essential for good sound on lutes. What is surprising concerning the varnish is that any of the original is left at all. Instrument varnish has always been rather ephemeral, like tuning pegs, to be replaced when not up to current requirements.

He writes further: "The neck foot is much lower than the ribs, and it has been fixed to the upper block in a very unusual way, which was, however, commonly used for repairs and transformations. On the basis of the wooden pins with which the ribs were fixed in the upper, lower and corner blocks, two different earlier stages - probably as a bass violin - can be reconstructed." Not having examined this instrument myself, and being not enough of a linguist to read Moens's previous publication in *Musica Antiqua I* (1987), pp. 3-11 (where he presumably has gone into more detail), I cannot begin to appreciate how his reconstructions might fit the evidence (rather than his preconceptions) better than the hypothesis that the instrument was made to be a viol. I expect that these details could easily result from difficulties in making the convoluted shape or from later repairs.

Following is: "In spite of the early dating of the belly, in its actual state this instrument doesn't tell us anything at all about early southern German viol making." This is nonsense. There is no evidence (and no historical reason to expect it) that such large bowed instruments in south Germany at this time were made according to different traditions according to whether the strings were to be tuned in fifths or fourths. Whether or not parts were recycled, as long as they are from that time, we can learn much about the making of such instruments from this one.

Ebert treble viol

On this viol, Moens writes: "According to dendrochronological evidence this belly originated from 1580 at the earliest. The outline and arching of the belly fits exactly between the soundholes of a small doublebass. Probably it was a belly with a central bar integral with the wood of the plate instead of a bass bar, which points to the great age of the belly. The back too has been cut from a larger whole. Evidence is to be found in the open worm holes and in an interrupted text fragment in the inside. Worm holes that are cut and that don't continue are also to be seen in the joint of ribs and bottom, this points to recutting."

The belly is clearly a late 16th century replacement for a cross-barred belly that was original a generation earlier on that instrument (see below). Cross-barred bellies give a different kind of sound than other types, and the replacement could easily be because their sound went out of fashion. His speculation that it came from a small double bass is curious, since such instruments did not yet exist at the time. As stated before, cut wormholes are not evidence of recutting of a component of an instrument.

He continues: "The ribs may have been rebent. They could have been planed on the inside and bent without heating. Hence the cracks in the middle parts of the ribs. Next to the four corner blocks one can see the remains of small blocks that would have supported crossbars under the belly. These small blocks might, therefore, have belonged to the earliest stage of the instrument. They are also made from the same piece of wood as the corner blocks. However, one of the remains of these blocks is situated on a part of the ribs that isn't old. These fragments of wood were possibly fitted to simulate an original condition. If the actual belly is original, which is possible on account of its age, the support blocks for cross bars under the belly don't make sense. The belly has indeed a carved arching. Finally it should be mentioned that the fingerboard was actually too large for this neck and it has been narrowed and shortened. It may, therefore, be a reused fingerboard from another instrument."

Such cracks in the ribs can easily be the result of problems in original bending, and need not be associated with rebending. The observation that a cross-bar supporting block was glued to a part of a rib that is supposed not to be old could be an indication that the criteria used to judge wood age could be faulty. If it was a repaired part of the rib, that repair could have happened when the cross-barred belly was still in place. Moens doesn't mention the observation by

Edmunds (*GSI XXXIII* (1980), p.81) that the neck has been shortened about 6 cm, explaining the alterations to the fingerboard.

On this instrument, Moens concludes: "All these and many other element could be interpreted as indications that the actual design of the instrument is not original, in spite of the age of the belly. As a result it is dangerous to draw conclusions from the construction and the design of this instrument as to Venetian viol making in the sixteenth century." Whatever danger there is in drawing conclusions about design from this instrument are further reduced by the similarities with the Francesco Linarol treble viol (Vienna C71).

Ciciliano viols

Moens writes: "I presume that similar things happened to several violins [viols] attributed to Antonio Ciciliano. Instruments from this maker are in the Kunsthistorisches Museum in Vienna, the Academia Filarmonica and Museo Civico in Bologna and in the Museum of Musical Instruments in Brussels. These instruments are all very different from each other in form, construction, signature, etc. There are, however, in one collection instruments do have similarities. The Brussels Museum of Musical Instruments has two viols attributed to Battista, Antonio's son, which are identical to the viol attributed to Antonio. As far as the Viennese instruments are concerned, tenor and bass are very similar, but the treble is completely different. The earliest possible origin of the bellies of the three Viennese instruments has been put at between 1580 and 1607. This last date (for the bass viol) is very late bearing in mind that Antonio Ciciliano is mainly mentioned in accounts from the sixteen sixties." The last sentence here is misleading since according to Edmunds (1980), Antonio "is mentioned in Venetian documents in 1566, 1569 and 1581". And it is not unknown for a son to inherit a number of unfinished instruments when his father dies, which then get finished later when needed.

He continues: "Most of the bellies of the instruments show evidence of having been recut: they show plane traces, thinning of the edges, bending of the corners, etc. The back and the ribs of the Viennese treble are cut from old parts, this is demonstrated by the state of the worm holes. The back of the tenor in the same collection is probably much younger, and the back and the ribs of the bass are also younger. It is probable that the necks of the three Viennese instruments are also not old." As mentioned before, these kinds of evidence can just as easily be explained by the non-faking theory.

His final comment is: "Despite the early dating of the bellies the authenticity of the dating of the actual shape remains an open question." An historical question can be closed by a consensus amongst the experts, but it can never be closed in historical scholarship. New evidence or a new theory that explains the evidence better can always change the conclusion.

It is possible that any old instrument in a museum is a fake. But there is no reason to take this seriously unless there is conclusive evidence for it. Moens has not offered such evidence.

On first reading this paper, with the completely open bias and the lack of objective supporting evidence, I first suspected that this paper was written as a practical joke, feeding on the sensitivity of instrument keepers to the revelations that many of the instruments they knew had been through the hands of Franciolini, the notorious instrument faker. It is only when I became aware of how seriously the museum people I know seem to have taken it, and that Moens, in all this time, appears not to have admitted that it was a practical joke, did I suspect that the paper was probably written as a provocative worst-scenario statement intending to start a debate that didn't develop. Well here it is. Better late than never.

**Review: "Italian violin strings...", *Recercare* IX (1997), pp.155-203
by Mimmo Peruffo**

Histories and dictionaries of musical instruments have never dealt seriously with the history of their strings. It appears that the new edition of the New Grove is continuing this tradition. To the best of my knowledge, my three-part article in *The Strad* in 1988 was the first major survey and interpretation of the surviving evidence (a large part of which is on violin stringing in the 18th and 19th centuries) on the subject. The paper by Peruffo reviewed here, despite of its more restrictive title, covers the same ground. It is about time for the subject to be brought up to date, elaborated, and possible alternative historical conclusions explored that explain the evidence as well as, and preferably better than, my previous ones. Indeed, Peruffo introduces much new evidence, mostly Italian, which is very welcome. Also welcome are the many quotes, most of which have English translations.

In a few cases, Peruffo has come up with new hypotheses that explain the evidence well, but for the most part, he appears not to have taken full account of all of the evidence in coming to his conclusions. I will discuss these shortcomings, as well as the positive contributions, in the course of this review. For ease of reference, I will discuss Peruffo's ten sections in order

1. The four ages of gut strings

The first era was from ancient times until gut string making became a professional trade, late in the 15th century. There Peruffo states: "Due to its wide availability, silk was the material mainly used in the Western and Mediterranean civilisations." This is one of many places in the paper where Peruffo presents an opinion that makes sense to him, but is contradicted by the surviving evidence. The major collections of evidence on strings in this period are in Bachman's *The Origins of Bowing* (1969) and Page's *Voices & Instruments of the Middle Ages* (1987). Both indicate that though silk was mentioned (especially in the context of luxury), gut and tendons were much more commonly used. Tendons (also called sinews and sometimes called nerves because nerves look like sinews) are composed of a material very similar to gut in chemistry (collagen) and in its physical properties.

The second era, according to Peruffo, was from the second half of the 15th century to the first half of the 16th century. I would have the end somewhat later, since the evidence for the the next era starts in the final quarter of the 16th century. In this second era, the maximum open-string range of instruments increased by a fourth downwards compared with the first age. He states: "the main centres of string making were also important for the dyeing and spinning of silk and cotton: Florence, Venice, Nuremberg and Lyon". This list does not correspond with the string-making centres that the string names used in this age apparently referred to, which are Brussels, Munich and Barcelona.

The third era, characterised by another fourth downward expansion of maximum range, was due to a newly available type of bass string. Of this, he writes "Recent studies have tended to show..." that these were loaded gut strings. In a footnote, he mentions the 'claims' by Abbot-Segerman that they were gut with rope construction. Here again, Peruffo states his opinion without taking the care to make sure that it is supported by all the evidence. Visual clarity or transparency was reported as a general property of all gut strings, including the thick bass ones, by Dowland, Burwell and Mace. This is evidence that no loading material that Peruffo has suggested can meet. It needs to be a material with the unusual properties of very high density and low index of refraction. If such a rare loading material can be found, and shown to be available then, there is still the question of why it was preferred to much more common loading materials in all of the bass-string making centres then. The technological and historical probability of all of this is so low that loaded strings cannot be a serious historical option.

The fourth era was that of metal-overspun strings. He quotes from the "Ephemerides"

manuscript by Samuel Hartlib, dated 1659, the first evidence of such strings: "Goretsky hath an invention of lute strings covered with silver wyer, or strings which make a most admirable musick. Mr. Boyle. [...] Strings of guts done about with silver wyer makes a very sweet musick, being of Goretsky's invention". This antedates by a few years the well-known Playford advertisement in *An Introduction to the Skill of Musick* (1664).

The statement that, as a result of the availability of overspun strings, 'the instrument makers systematically shortened the vibrating lengths of the *da fondamento* instruments so as to make them more manageable', is contrary to the evidence except for the bass viol. The earlier larger bass viol used an overspun 6th in Germany to become a small violone. The violoncello grew in popularity, often replacing the violone, but neither seriously changed its vibrating length. The normal violone (of the size of the modern double bass), and the considerably-larger contrabasso violone continued, sometimes (and only sometimes) using an overspun 6th for a lower tuning. In the 19th century, the 4-string violone/double bass had an overspun *EE*, but the very popular 3-string double bass (played by Bottesini and Dragonetti) used no overspun strings.

2. Gut string manufacturing technologies in the 18th and 19th centuries

Mace's designation of strings with different names to treble, mean and bass ranges is mentioned, with the strong implication that Dowland used the same designations. Actually, Dowland divided the different types of strings into just two categories, with his Great strings including Mace's means and basses. Peruffo suggests that the manufacturing criteria for each of Mace's three types were 'maximum resistance to wear and breakage for treble strings, maximum elasticity for the strings of the middle register, and an increase in specific weight [this means loading] and elasticity for the bass strings'. Mace's criteria for 'Goodness' for all ranges were 'the Clearness of the string to the Eye, the Smoothness, and Stiffness to the Finger', with the Lyon bass strings not fulfilling the criteria as well as the others (Pistoy basses, being smooth and well twisted, but usually unavailable, were better). All three of Mace's criteria are signs of compact uniformity (necessary for trueness), and clearness was also a sign of freshness (since old strings decayed, reducing strength). I don't know of any evidence of what string makers then did to fulfil any of Peruffo's criteria.

Then he writes that with the introduction of overspun strings 'the ancient secret techniques of making all-gut bass strings declined rapidly and were soon forgotten by the new generation of string makers'. The making of catlins was a secret of the Munich/Barcelona string makers through the first two-thirds of the 16th century. The third era only happened when makers from Bologna, Lyon, Nuremberg and Strassburg were making such strings, all competing in the bass-string trade with rather similar products. This could only have happened if there was no more secret in how they were made. And there was no cessation of demand for their products (for secrets in their making to have been forgotten) since these products continued in use, especially for violones. Only after the middle of the 20th century, when the popular use of gut strings died, was much of its traditional technology forgotten, and new technology replaced it in its remaining specialist market.

Peruffo then writes: 'Though at first glance the procedures for making gut strings look remarkably like those in use today, there were substantial difference. And what these differences unquestionably suggest is that the earlier strings (right up to the end of the nineteenth century) were more elastic, and hence better, than those available today.' The 18th/19th century treatment of gut that differs from today, and that an early author (Griselini (1769)) considered to affect elasticity, is sulphurisation. Sulphurisation bleaches and preserves gut (it is still used to preserve dried fruit), and when it was introduced into string making, we read no more about the need to buy fresh gut to forestall decay, with which 17th century instructions on gut choice were very concerned.

Peruffo suggests that the effect on elasticity of sulphurisation is due to the formation of sulphide links between collagen molecular chains, as in vulcanisation: New bonds between

The statement that, as a result of the availability of overspun strings, 'the instrument makers systematically shortened the vibrating lengths of the da fondamento instruments so as to make them more manageable', is contrary to the evidence except for the bass viol.

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chains would increase stiffness, increasing the returning force when a string is bent a given amount, and this appears to be what Griselini meant by 'elasticity'. This 'springiness' interpretation of 'elasticity' is clear when Savarèse (in Maigne et Maigne, 1869) wrote "when you squeeze a packet of chanterelles they must feel elastic and return promptly as a steel spring would do". Similarly, Galeazzi (1791) wrote that a good string should be 'supremely elastic and strong and not limp and yielding'. There thus seems to have been no change from the 'stiffness' criterion expressed in the 17th century, and so no essential change in innate elasticity since then.

Peruffo seems to be writing about an elasticity that is limp and yielding, contrary to what Galeazzi wrote. He cites coils of string sticking out of pegboxes in 17th century pictures as evidence of flexibility of gut then. On the contrary, a considerable amount of stiffness in the string material would be necessary for the strings to maintain the depicted shapes in mid air without support.

Peruffo nevertheless does provide evidence of a new quality criterion in 18th/19th century string making. Savarèse wrote: "The bigger strings, the second or third ... must be transparent and very white. Moreover, they should be very soft when the packet is pressed, but they must not change colour and must return promptly to their cylindrical state". The softness could well result from swelling by the tartar and rock-alum used early in the process, and olive oil late in the process, as Peruffo suggests. This sponginess appears to be what Peruffo is calling 'elasticity'. The earliest evidence of this variant in the string-making process is from the middle of the 17th century in Padua.

The more twist that is built into a gut string, the easier it is to stretch it and the more susceptible it is to breaking. Peruffo writes 'As a rule, modern strings are less twisted than the strings of the past'. This is true if we don't consider first strings and consider 'modern' as being the second half of the 20th century, when gut manufacture was mainly for harp and tennis-racquet stringing and surgical sutures. Then the musical value of high-twist strings was forgotten by the makers. Before then, this was not the case.

Peruffo continues: 'This is shown not only by the historical documents, but also by the examination of the surviving samples of old strings. Old strings were always made with a high twist, with perhaps the exception of lute chanterelles, the strings that received the severest treatment'. This just doesn't make sense either technologically or historically. The historical documents cited indicate particular numbers of turns given to the wheel in the string twisting. Any string maker should know that a constant number of turns gives a low-twist string if there are fewer guts in the string, and it gives a greater amount of twist when larger number of guts are twisted up. There is a maximum amount of twist a gut string will take, beyond which it will adopt a corkscrew shape. So there is a maximum number of guts that can be twisted up with a given number of turns, and any string with fewer guts has less than the maximum amount of twist. Low twist is necessary for enough strength for E's on violins at pitch standards close to modern, and even at the lowest pitch standards used, high twist E's would have a short life. This will be discussed later with respect to Peruffo's evidence of high-twist violin E's.

At the end of this section, Peruffo writes: 'The logical conclusion would appear to be that the strings made in the past were superior from the points of view of acoustical performance and durability'. I can see no logic behind this conclusion. No evidence has been presented that shows that a softer swollen string has any better (or different) acoustic properties than a harder compact one, or that durability is greater in a string that has been sulphurised rather than treated with peroxide (which is current practice), or if it is made of whole guts rather than split guts. Strings made of lamb guts could well last longer than those made from guts of older sheep, but mutton is rather out of gastronomic fashion nowadays, and many gut makers can still use lamb (though competition from Japanese sausage makers limits the supply and raises the price).

An unpolished violin E should last a bit longer than a polished one, but the very high standards demanded of modern players would probably lead most of them to trade a somewhat shorter E-

string life for the trueness of a polished one. At the modern 'baroque' pitch standard of $a' = 415$ Hz, the string life is much longer than at the pitch standards, up to almost a tone higher, that mid-19th century violinists had to contend with.

There are two minor points in Peruffo's description of the twisting process that need correction. One is that the material used for the string, after everything else in the intestine has been removed should not be called a 'muscular membrane'. ~~Intestinal muscles are amongst the various materials discarded.~~ The other is his statement: 'The strips were then attached to a special wheel used for twisting the string while the other end was fixed to a peg at the side of a drying frame (figure 1).' This may be what Angelucci said, but is not what is going on in figure 1. The drying frame in the background is not involved with the string twisting being shown. In the twisting frame in the foreground, the string is tied to a hook on a cog that is turned by the wheel, goes across the twisting frame to go around a peg on the other side of the frame, and then back again to another hook on a cog next to the first one, and which is turned in the same direction by the wheel. In this way a string of length that is double the distance between the wheel and peg can be twisted at one time, and with the same number of turns of the wheel.

3. The centres of production

This is a good collection of references on 18th/19th century centres of string making, mostly Italian, and their relative reputations.

4. Criteria for judging gut strings

This is another collection of references, most of which have already been discussed above.

5. String types

Two categories of 18th/19th century string types are defined, 'oiled all-gut strings with a high twist for the medium-to-high registers, [and] overspun strings for the basses'. I've mentioned above that Peruffo's documentary evidence implies that the first category would have had the string twist variable from high for the thickest to low for the thinnest. There is no need to assume (as he does) that any 17th-century string types specifically made for middle registers disappeared to explain the apparent problem of balance between the all-gut higher-pitched strings and the overspun strings that led to the use of open-wound (also called half-wound or *demifilé*) strings at the transition, most apparent in French stringings. The difference in type of sound is strong enough to want to soften the transition by using strings having an intermediate tone quality.

He writes that the set of three all-gut strings and an overspun 4th was 'already from the late 17th century, a typical violin stringing in Italy - but also in England and the German speaking countries'. It is not good history to say that something was 'typical' before there is any evidence for it, which is the case for England and Germany. There is a single piece of possibly supporting Italian evidence, a light coloured 4th string in one picture, which can possibly also be an early example of sulphurisation. There is contrary evidence from Stradivari, discussed below.

In a footnote, Peruffo rejects my suggestion that since Leopold Mozart could have used an all-gut 4th because he advocated strict equal tension and mentioned that the strings were uniformly thicker with lower pitch. Without offering an alternative explanation for this statement, Peruffo writes 'In our opinion, Mozart, who constantly referred to the Italian tradition, used an overspun fourth string like all the other German violinists.' Here again, he expresses an opinion without caring about whether it explains the evidence. He will readily reject evidence that does not fit his picture of history without feeling any need for either showing how that evidence could have become wrong, or for interpreting it in a way that is consistent with his opinion. Another example of this is his response ('We feel that assessments of this kind are

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completely untrustworthy.') to my estimate of the diameter of a violin 4th from the width of a short line drawn by Stradivari labelled "*una quarta da violino*", when that short line is one of 7 of graduated widths illustrating the thicknesses of the gut strings required for a theorboed guitar on a drawing of its neck. No matter how inaccurate this evidence is, the thickness implies an all-gut fourth. It is contrary to Peruffo's conclusion that 'As far as we can tell, the fourth string was always overspun in the Italian tradition'.

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When Peruffo mentions (in another footnote) Brossard's specification of an overspun 4th and an open-wound 3rd, he neglects to mention that all-gut stringing of all strings was given by Brossard as an alternative. Peruffo does not consider the likelihood that all-gut stringing was a respectable alternative during most of the 18th and 19th centuries. Curt Sachs (in his *History of Musical Instruments*, 1940) mentioned H. W. von Guntershausen (Frankfurt, 1855), who 'wrote that some violinists imitated the example of the three lower strings of the guitar by overspinning the G string with silver, but that most players preferred gut strings'. All-gut stringing was a respectable alternative in some places and times. ok

6. The string gauges

Before getting into the question of string diameters, Peruffo discusses polishing and string trueness, where he surprisingly underestimates what the skills of early string makers could accomplish. The traditional test for trueness of a particular length of string is to observe the vibration pattern produced by plucking while stretching it between the two hands located at the two ends of the length being tested. He seems not to appreciate that using this test, regions causing untrueness can be localised and polished away. With this method, we can often improve the trueness of strings polished by the centreless grinder (which Peruffo calls 'mechanical rectification'). Rotating the string rapidly during polishing preserves roundness. Polishing violin E strings demands greater skill than thicker strings. They were frequently unpolished to increase life when they were true enough without it, not because polishing reduced trueness, as claimed by Peruffo. 77

Peruffo presents many references indicating that a violin first was made of 3 or 4 lengths of whole gut, or sometimes with an equivalent number of lengths of split gut, and that the thickness contribution of each gut can vary considerably. The evidence of a few surviving violin E strings that have high twist is presented. A high-twist E would probably sound marginally richer than a low-twist E, but would have a much shorter life. There are always some musicians who will go to any lengths to sound as good as they possibly can. We have a few such customers who insist on our making catline seconds for their violins. It is worthwhile for them to use the best-sounding string possible for an important concert or recording session as long as it lasts long enough for these events. Most players follow a different balance between tone quality and string longevity. 20

I am glad to be corrected about Ruffini, who was a highly respected Neapolitan string maker rather than a violinist, as I had guessed. Other interesting reference are given, including string diameters given by Hart and the number of guts per string given by Savarese, which I missed in my study.

Given in a footnote is the statement by Hart (1875): "Vast improvements have been effected in the stringings of violins within the last thirty years. Strings of immense size were used alike on violins, violoncellos, tenors and double basses. Robert Lindley, the king of English violoncellists, used a string for his first nearly equal to the second of the present time." Also given is the statement by Fétis (1856) that Vuillaume reported that twenty years earlier, the total of the string tensions was 80 pounds (39 Kg). The total in the second half of the century was rarely over 30Kg. Peruffo dismisses these pieces of evidence of heavier stringing around 1840 as 'mainly the result of variations in pitch standards'. We know much about variations in pitch standards in that period, and they are not great enough to come close to explaining this evidence.

peruffo's evidence is not reliable
see the above

Then Peruffo dismisses my interpretation of the markings for the different strings on Spohr's illustration of a string gauge. Spohr's book was written in this period, and he wrote "Generally speaking in order to obtain a rich and powerful tone, a violin should be furnished with the largest set of strings it will bear". I found a unit of measurement, 'grades of millimetres' that was used for strings in the 19th century (where a 'grade' is a twentieth of a mm) that if applied to Spohr's numbers, made sense in terms of higher than normal 19th century tensions. The metric system of measurement was established in Germany by that time.

Peruffo considers that my 'interesting hypothesis is inconsistent with Spohr's writings'. His first point is that Spohr recommended Italian strings over those made in Germany. We know little about the heaviness of Italian strings in this period, but no matter what they were, we can expect that Italian string makers would provide whatever strings were wanted for whatever market they exported to. The second point is that Spohr suggested a light stringing. I don't have a copy handy, so can't tell the context, but Spohr's 'light' would surely be relative to his normal stringing, and not relative to general stringing history. We should expect that the "largest set of strings" was for advanced players, and lighter strings were for the less experienced. The final point, which is not a point against my hypothesis at all, is that if one assumed that the gauge mouth opening was 2 mm, the E string marking would correspond to the diameter of 0.70 mm that is acceptable to him. No measurement units for Spohr's gauge markings that could result in that mouth opening are offered.

7. Working tensions and "feel"

Peruffo is quite correct in pointing out that equal tension between different strings does not correspond with equal feel to the fingers, but he assumes that it is different in a way opposite to the way I do. In my view, the feel of a string pressed against a finger with a given force depends on how deeply the string depresses the skin, which is deeper with a thinner than with a thicker string. So on an instrument strung in equal tension (like the viol or modern lute), the thin strings feel stiffer than the thick strings. Consequently, for true equal-feel stringing, thicker strings should have higher tension than thinner ones. This has never been the usual stringing on an instrument, and statements about even feel should be interpreted as a smooth progression in feel.

In his view, in equal tension, a thinner string feels slacker than a thicker one, so in equal-feel stringing, thinner strings have higher tension than thicker ones. Since the historical evidence does not define 'feel' when it is mentioned, there is no basis to choose between these views other than one's own personal experience. But when Peruffo defines equal feel in physical terms, he runs into trouble because he apparently does not understand the basic physics and geometry of the deflection of a string by a perpendicular deflecting force.

He writes 'if an elastic band and an "unextendible" steel string were both stretched to 10 kg, nobody would be in any doubt that the elastic band would be more yielding than the metal string. Yet the number of kilos is constant'. On the contrary, in musically relevant contexts, the physics indicates that the deflection by a deflecting force at the same place would be the same regardless of elasticity. For musical usefulness, the number of kilos of tension should remain relatively constant with and without the deflecting force that causes the yielding. This is because if that force is strong enough to significantly affect the string tension, the pitch of the string would rise with stronger playing. The less-extensible metal string will have a much lower maximum deflecting force (to keep playing in tune) than a more extensible material.

If the deflecting force is within the required limits for both (so that tension does not significantly increase with the deflecting force), the forces of tension on both sides of the position of the deflecting force are in slightly different directions because the deflecting force bends the string. The two tension forces and the deflecting force are all forces on the point on the string where the deflecting force is applied. Since there is no movement, according to Newton, the three forces acting on that point have to cancel each other out. The geometry of how these forces relate is the same as the geometry that relates the shape of the string as a result

of the deflecting force. Consequently the amount of deflection caused by the deflecting force is completely determined by the tension, the deflecting force and the point on the string that it applies. It does not depend on the elastic properties of the string. The elastic band and the steel string at the same tension would yield the same amount of deflection with the same deflecting force.

Peruffo tries to convince his readers that the most usual stringing with an overspun 4th, where the tension progressively increases from bass to treble 'genuinely embodies the traditional concept of equal feel under the fingers'. He defines equal feel as having equal deflection by the same deflecting force acting at the same place along the string. According to the physics mentioned above, that can only happen if the different strings have equal tension. Yet he writes that his theory can be verified experimentally, indicating that one should take a set of strings like Ruffini's (presumably tuned to the proper pitches) and attach the same weight 3 cm from the bridge to each, and measure the deflection with a ruler.

He does not report the results of the described experiment, say, comparing deflections in a Ruffini set with an equal-tension set. Perhaps he never tried it, but most probably it did not show what he wanted it to show when he measured it. As seen elsewhere in the paper, Peruffo believes in his conclusions so strongly, that he assumes that any evidence contrary to them (in this case, his own measurements) must be wrong, even though he can't think of how they could have become wrong. This is not what one would expect from a person trained as a scientist. Instead of this experiment, he shows the results of a very different experiment in Graph 1.

The conditions of Graph 1 that are similar to the experiment he described are that he used a deflecting force of a 500 gm weight acting on the string 10 cm from the bridge, and varied tension to get the same deflection (the amount of this deflection is not given). In the experiment described in the text, one measures for different string diameters at the same length, but in the experiment of the graph, he measures for the same string diameter and different string lengths. Most of the variation of tension with length in this graph can simply be explained by the changing fraction of the total string length represented by the 10 cm distance of the weight from the bridge. This experiment has no relevance to Peruffo's theory, and one must seriously question why it was done and included. The claim at the end of this section that his theory is confirmed experimentally cannot be accepted.

Graph 2 plots the tensions of the three highest strings of a number of 18th and 19th century stringings with progressively increasing tension. Graph 3 plots the amount of stretch caused by varying tension for a half-dozen different diameters of low-twist strings. Graph 4 compares the stretch vs tension of three strings of the same diameter with different twists: low twist, high twist and catline. The slopes reflect the different elasticities. This is where the greater elasticity of an early type of string (with tartar-rock alum pretreatment and olive-oil final treatment) having the same twist as one of those shown should be demonstrated, but it is not. Graph 5 makes the same comparison as in Graph 4 between a high-twist and a catline string when both have a larger diameter.

There are well over a half-dozen 18th and 19th century sources (the latest, which Peruffo does not mention, is Hepworth at c. 1900) specifying equal tension stringing. It must have often been used then, yet Peruffo goes to great lengths (with no success, as far as I can see) to try to discredit the evidence he does mention.

8. The fourth string

There are interesting references here about windings of silver, silver-plated copper, plain copper, mixed wire and even iron, with some indication of their reputations as to how the material affected the sound. The sources report that the core could be a first, a thin second, a thick second or just 1.0 mm.

CAP 12

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It is claimed that 'As the tension of the A string was close to that of the top string (though nevertheless "scaled"), it is easy to understand why Galeazzi called the fourth string the "cordone". It also offers us an explanation for a progressive distribution of Mozart's four diameters, and allows us to rule out the possibility of an anachronistic all-gut stringing.' The logic of this, if there is any, completely escapes me.

Figure 2 shows the illustration in the *Encyclopédie* of a machine for making overspun strings. Peruffo has 'observed experimentally that this type of machine is unable to impart a strong tension to the gut string and to the metal wire at the manufacturing stage: since it rotates the string from one end only, rotation is not uniform along the string's whole length (the opposite end tends to turn at a slower speed).' If he ever tried to use such a machine, he obviously never got it to work properly. If the two ends turned at a different speed, the string in between would steadily change in twist and will soon corkscrew up. To work, the friction at the weight end needs to be low enough and the weight heavy enough for the rotating string to have high enough tension to inhibit corkscrewing. The operator's right hand controls the closeness of winding of the wire on the core, and the left hand controls the wire tension by friction between the wire and the fingers.

8. (Peruffo has two sections 8) Conclusions

A table of E, A and D string diameters is given from Peruffo's deductions from 18th and 19th century sources. All sources indicating equal tension have been omitted. It is observed that modern "baroque" stringing is much lighter than indicated by early evidence. **This is true except for Lullist France and Restoration England, which he ignores. He repeats his claim that all early strings were 'much more elastic' than modern strings, and that string types and gauges were quite standardised throughout the period, with enough range of variation to allow for differences due pitch standards or individual preferences.**

Appendix

What is given here is the history of some Italian string-making family firms, quoted from an article by Natale Cionini (Modena, 1902).

This reviewer's conclusions

Peruffo must be thanked for finding much new evidence that enriches the field of string history. He is a very good researcher, digging out the references and reporting them. It is a pity that he falls short on the scholarship that produces the conclusions that should objectively best explain all of the evidence. It seems that his conclusions came first, with non-contrary evidence claimed as confirmation and contrary evidence either ignored or rejected as unreliable.

The deviations from respecting the evidence in the simplified history of Peruffo's conclusions fall into a clear pattern. By eliminating consideration of evidence for equal-tension stringing at any time, all-gut stringing after 1660, and changes of tension at different times and places, Peruffo's 'history' allows the early-music violinist to have only one set of gut strings for all periods earlier than modern. Peruffo's definition of "equal feel" allows the same set of unequal tensions to apply to before 1660, so for this earlier period, one has only to replace the overspun 4th with one of his loaded gut ones. It is probably not a coincidence that this is just what modern early musicians would like most to believe is true, for they prefer to have a minimum amount of different historically-correct instrumental equipment to cover the various repertoires that they play.

I wonder how many of Peruffo's readers will be astute enough see through the very impressive scholarly apparatus of lots of footnotes, references, quotes, translations and graphs, and notice that this paper does not meet the standards of respect for the evidence and objectivity expected from historical scholarship.

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Review of: George B. LANE, *The Trombone: An Annotated Bibliography*, Lanham, Maryland and London: The Scarecrow Press, 1999. ISBN 0-8109-3465-0. xvi, 425pp. Price \$65.00, £58.50.

Lane's previous venture between hard covers was a book, *The trombone in the Middle Ages and the Renaissance*, based on his 1976 doctoral dissertation – published in 1982 by Indiana University Press, badly received by reviewers, and quickly withdrawn by the publishers. The bibliography in that book, however, was quite impressive, and the 1999 bibliography is not only larger but is also annotated. Much of the expansion is coverage of literature dealing with the trombone in the post-Renaissance period, and most of that concerns modern practice, particularly in North America.

In a compilation of this size (1305 entries), even the best-read scholar with an interest in the trombone or the slide trumpet will come across something new and potentially interesting, conversely (as in any bibliography) anyone working in the field could point out a handful of lacunae. The annotations are a good idea, since many article titles give little idea of content. A pity therefore, that in many cases Lane frankly admits he has not seen the original publication, and that in too many others this can be deduced from the inaccuracy of the comments.

There is no doubt that a bibliography published on paper has its place, but for many purposes it would be better to access this kind of information electronically, especially as a number of the sources cited are themselves websites. A regularly updated searchable database with a web interface would offer much to bibliography users, whose need is probably very occasional, and probably be more satisfying for the compiler as well. As it is, I suspect that this bibliography will be more useful to scholars of our times in the future than to those studying the historical trombone today.

FoMRHI Communication Number: 1685

Alec V Loretto

Musical Millenium Memoirs

The end of one millenium and the arrival of a new one will almost certainly produce claims concerning who was the first to do this, that, or something else. My own experience illustrates what I mean.

As midnight approached on Friday December 31 1999, I was in my workshop putting the finishing touches to a boxwood [*Buxus sempervrens*] alto recorder in G at a-415Hz. It occurred to me that if I delayed the final operation for a few minutes until midnight struck, I could possibly be *completing* the first recorder of the new millenium. This I did. It immediately occurred to me that if I straight away started on a new instrument it could possibly be the first recorder completely *made* in the new millenium. In a little under ninety eight minutes, I completed a one piece Ganassi soprano in c at a-466HZ made from New Zealand kauri - *Agathis australis*.

For very obvious reasons I mention that these recorders are *possibly* the first of the new millenium. Not *definitely*. Although New Zealand lies close to the International Date Line's western side, there are numerous places also to the western side which lie much closer and are therefore in earlier time zones. It is not out of the question that an instrument maker in an earlier time zone was in fact the new millenium's first to finish and/or make recorder[s].

If readers know of such a maker, particularly if he/she lives on a winterless golden-beached atoll, might I ask that I be provided with details. I would hope to make regular visits during Auckland's wet and, at times, waterlogged winters.

Alec V Loretto
Auckland
New Zealand

January 1 2000

I'm not sure that you ought to read this

This may be a wholly unwarranted attack but I am beginning to feel, and I don't think I am alone in this, that *Early Music* is becoming excessively self-indulgent.

Journals differ in their intent, in their purpose, and, perhaps more important, in their subscribers' expectations. There are a few funnies like us, informal, quickly and cheaply produced, where almost anything goes and where the reader, while always aware that opinions in one issue may be vituperatively shot down in the next, can nevertheless hope to find nuggets of pure gold among the dross. Then there are the specialist journals of high repute, for example in organology the *Galpin Society Journal*, the *Journal of the American Musical Instrument Society*, somewhat more specialised the *Historical Brass Society Journal*, and more specialised still the *Lute Society Journal*. Every subscriber to these knows what to expect and receives it. Then there are the frankly commercial journals, several in this country published by Rhinegold, which are addressed to the general public and are intended to inform, entertain, advertise, and publicise in more or less equal proportions. In contrast with all these are the journals which are addressed almost exclusively to the denizens of university music departments and which delve into the *minutiae* of musical historiography, hagiography, and epistolography.

Early Music, when John Thomson (the first adumbrations of whose obituary appear in the latest issue) started it, was different. Somehow it managed to straddle almost all the above categories, but it wasn't so learned as to put off the general reader, nor so superficial that it put off the specialist. In almost every issue there were at least some things which would interest almost anybody with the slightest inclination towards early music, whether as a player, editor, writer, or listener. Not only that, but it was produced to the highest commercial standard. That indeed was the root of much of the argument between John and OUP

which eventually drove him back to New Zealand and, to our great regret, out of our ken. The Press had some logic on their side – they had to balance their budget and John always wanted bigger and better issues, and so of course did we. We all loved to write for John, too, for unlike all other periodicals in the field, the publication of an article was followed by the arrival of a cheque, and John positively encouraged the use of photographs and was always willing to pay for the cost of the prints.

These policies have continued and we are all grateful for them, though of course one inevitable result of some of the more attractive of these policies has been the inexorable rise in the cost of a year's subscription.

So what is the reason for my title and my opening paragraph?

The answer is the latest issue, which arrived yesterday morning.

This is devoted to the Italian composer of madrigals, Luca Marenzio. Now he is a gentleman with whom and with whose music and the literary sources thereof you may be enthralled, and obviously nobody could complain that space in an early music journal was devoted to his work. The trouble is that if by any mischance you are not particularly interested in him or his work, there is practically nothing in this issue to make it worth even tearing open the polythene envelope in which it comes. There are four, and only four, book reviews, three, and only three, music reviews, five, and only five, record reviews, plus three brief reports of conferences, one of them on Marenzio again.

Early Music has always had some special editions, some themed issues, but it seems to me that their number has increased of recent years and, much more seriously, that the theme has pervaded a greater proportion of the relevant issue. Once upon a time (the traditional accompanying phrase is 'and long, long, ago'), a theme took some third to half an issue. Then it gradually increased to three-quarters, and

now, as you will have seen when this issue thumped on your doormat, it has taken almost all of it.

I do not believe that this is what *Early Music* is for. I believe that each issue should interest as many people as possible, that things should be reasonably spread in every issue so that every regular reader will find something of interest and, for we wish *Early Music* to thrive, that the casual reader will be encouraged to look for the next issue and, next year we hope, take out a subscription.

I have been writing for *Early Music* off and on since its second issue, and while some of my articles have been pretty specialised (if you're not interested in early percussion, forget the first one I wrote, and if mediæval ico-

nography bores you, forget some others) but I have always tried to remember, as I do in my Comms here, that at least some readers may have glanced at a page by accident and their eye been caught.

I don't believe that this is true today of *Early Music*. I wish it were, and I believe, as I said at the beginning, that I am not alone in this.

So I have written this Comm in the hope that it will be seen by people who have some influence in that quarter, who perhaps may sympathise, and who perhaps may be able to persuade *Early Music* to think again about the directions in which they are heading.

And yes, I have seen the beam in our eye as well as the mote in theirs.

FoMRHI Comm. 1687

Darcy Kuronen

News from the MFA

The Museum of Fine Arts, Boston, is preparing a major exhibition of over 120 guitars that provides a revealing look at the most interesting changes in the instrument's visual design from the late sixteenth century to the present day. Curated by Darcy Kuronen, *Dangerous Curves: The Art of the Guitar* will examine how the forces of fashion, technology, and society have influenced the appearance and construction of the world's most popular musical instrument. Planned for display in the Museum's 10,000-square foot Gund Gallery, the show will open to the public November 5, 2000, and be on view for fifteen weeks until February 25, 2001.

Several institutions, performers, and private collectors are lending guitars for the exhibition, including The Metropolitan Museum of Art, The Smithsonian Institution, America's Shrine to Music Museum, Edinburgh University, The Gene Autry Museum of Western Heritage, Les Paul, Chet Atkins, and Steve Howe of the rock group Yes. There are plans for a profusely illustrated catalog and a state-of-the-art audio guide to lead visitors through the displays. An ambitious series of concerts is planned to showcase the broad range of guitar music from classical to jazz, and there will be lectures and demonstrations presented by leading performers, guitar makers, and collectors.

The MFA also recently announced the reorganization of certain curatorial departments. The Museum's instrument collection has now been instituted as the Department of Musical Instruments, and is no longer a sub-division of Decorative Arts. Darcy Kuronen has been promoted to Curator of Musical Instruments (*email* DKuronen@mfa.org), and the former title 'Keeper of Musical Instruments' has been eliminated. In the coming years the Museum is planning to construct a major addition to the current building, and it appears that new and additional space will be allotted for the display of musical instruments.

Molto Stuccato

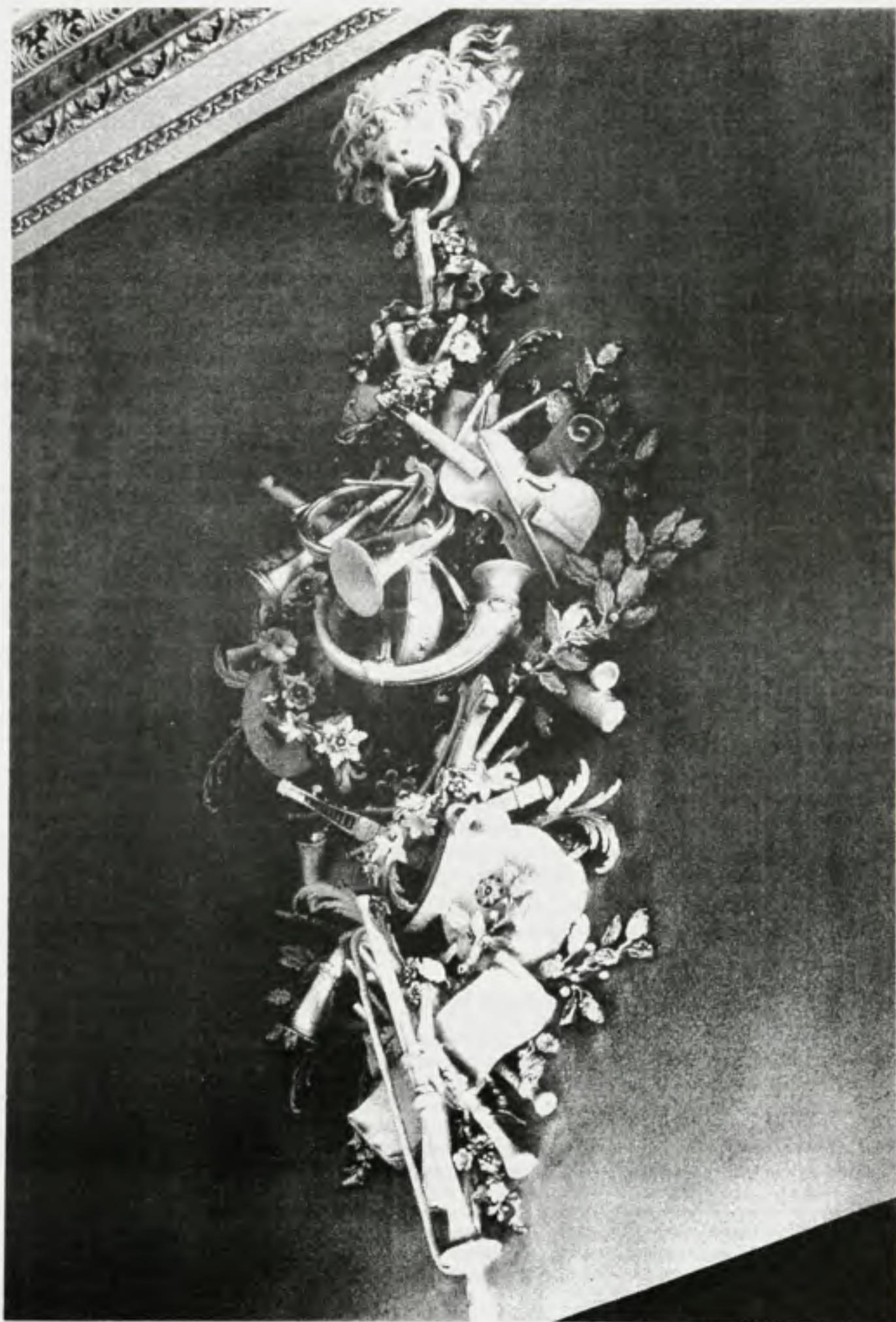
After he'd seen my "Silent Whistle" Comm (1650), Jeremy drew my attention to an item by Eric Halfpenny in the Galpin Society Journal, 1974, "The Christchurch Trophies". I had long forgotten about this, perhaps not being interested in this subject at the time. So almost immediately I went along. In the upper library at Christchurch College, are a number of stuccoed decorations all along one wall, the central two of which are bundles or "drops" of musical instruments. They are made to look as if they are suspended from a ring in a lion's mouth. Whereas at Highnam Court, three or four instruments were considered about right for each of the five clusters, here there are about 25 in each. Eric Halfpenny did a very good job listing them all, though I find his use of the word "trophies" rather quaint, as there is nothing of the spoils of victory about them. He had them photographed from the gallery opposite with the aid of theatrical lighting. I took the accompanying photograph from floor level, with just the available light, which is not good, as the instruments are between windows, and the electric lights don't help much. Visitors to and users of the library therefore are only intended to get a general effect.

There is no great difference in the two arrangements, which include some real instruments, and some fanciful freaks, such as the short square cornetts. The librarian told me that some expert visitors have said that many actual instruments are present, but close inspection is well nigh impossible, as the ten-foot tall clusters are very high up.

For this form of three-dimensional art, each component has to be fairly rigid. The overlength shawms are clearly not, as they are all slightly bent. They are also very unlike any known shawm.

Other instruments are the same as those at Highnam: English guitars (though 10-string here), recorders, horns, pipe and tabor, violins etc.

These wall-decorations get no specific mention in Pevsner. The other drops to the left and right all represent mathematics, and contain actual tools, dividers, set-squares and the like, plus some rather arbitrary looking fictitious geometrical shapes which contribute to the general effect. I am still not greatly enthusiastic about this form of decoration, but am prepared to concede that the wall was meant to be taken in at a sweep of the eye, and not for studious examination, which was after all, the purpose of the books kept in the library.





Isis.

† Engraving of Isis shown in Alexander Murray's *Manual of Mythology*, 2nd edn. (London, 1874), pl.XLV, facing p.347.

Postscript to FoMRHI Comm. 1673 on "THE SISTRUM"

One further interesting reference on the subject of the sistrum may be found in a section entitled "The History of Egyptian Music", within Dr. Charles Burney's *A General History of Music*, Vol.I (London, 1776), pp.198-232. In this section, Burney remarks on the perceived religious importance and ubiquity of the sistrum in Ancient Egypt. He speaks of it as

an instrument of sacrifice, which was so multiplied by the priests in religious ceremonies, and in such great favour with the Egyptians in general, that Egypt was often called, in derision, the *country of sistrums*; as Greece has been said to be governed by the lyre. (pp.202-203.)

A little later, Burney quotes an extensive letter (headed "Kinnaid, Oct.20, 1774") from James Bruce, an "intrepid and intelligent traveller" - in this age when to travel widely and far afield could often, on one's return, turn one into a minor celebrity overnight. Burney values Bruce highly for his "extensive knowledge of Eastern countries", which is evinced both in Bruce's writings, and in the detailed and "exquisite drawings" of objects which complement them. Most pertinently, Burney regards Bruce as "an excellent judge of the subject of music".

In his letter to Burney, Bruce discourses mainly upon "the state of music in Abyssinia" (i.e., in the mid eighteenth century). Bruce speaks of the sistrum as remaining, since ancient times, "dedicated to the service of the church", and being

used in the quick measure, or in allegros, in singing psalms of thanksgiving. Each priest has a sistrum, which he shakes in a very threatening manner at his neighbour, dancing, leaping, and turning round, with such an indecent violence, that he resembles rather a priest of paganism, from whence this instrument was derived, than a Christian. ...

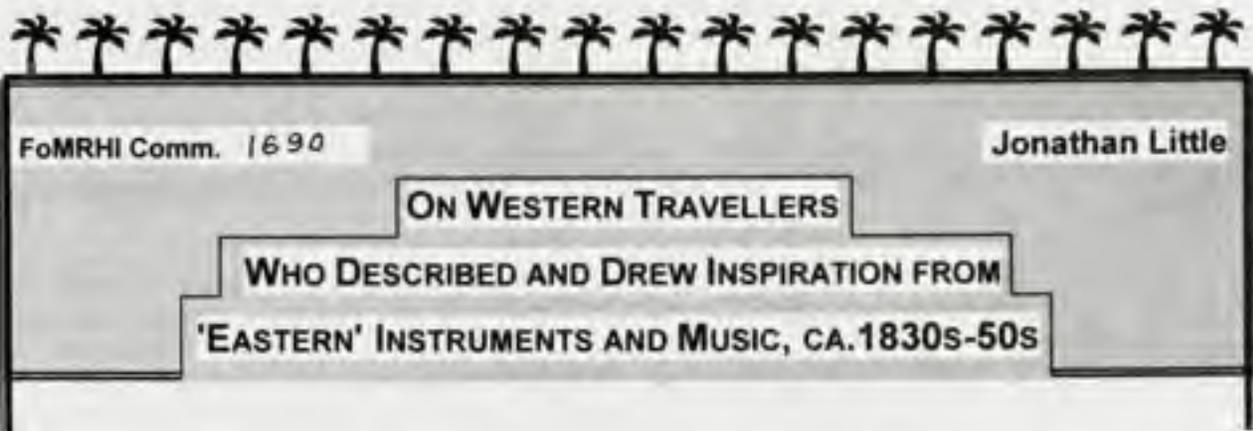
The Abyssinians have a tradition, that the sistrum, lyre, and tambourine were brought from Egypt into Ethiopia, by Thot, in the very first ages of the world. (pp.214-15, 217-18.)



Bronze sistrum found at Pompeii (from ca. end 2nd century B.C.). A cat reclines atop with head protruding. Roman sistra could be thicker, heavier and less delicate than their Egyptian fore-runners.

This threatening use of the sistrum recalls Cleopatra's massed priestesses of Isis fiercely shaking their sistra in a vain attempt to ward off their Roman invaders at the battle of Actium.¹

¹ M. Oldfield Howey's obscure tome, *The Cat in the Mysteries of Religion and Magic* (London: Rider & Co., n.d. [ca.1930]) devotes its entire third chapter to "The Sistrum", explaining the role and importance of this "mystical musical instrument" in the worship of the Egyptian goddess, Isis.



IN his classic *Account of the Manners and Customs of the Modern Egyptians Written in Egypt During the Years 1833-5*, Edward William Lane observes how 'the music of the Egyptians is of a style very difficult for foreigners to acquire or imitate' (rpt.1989: 352). As this may indeed be said to be the case, Lane here supplies the principal reason for the stylization of Eastern music by Western composers.

Within a chapter devoted to an examination of Egyptian music as observed in the first half of the nineteenth century, Lane remarks upon the wide variety of musical instruments employed by the Egyptians. He goes on to document and illustrate each one in detail, initially listing those instruments generally used at 'private concerts': the 'kemengeh' - 'a kind of viol'; the 'kánoon' - 'a kind of dulcimer' (which Lane thinks extremely pleasing as a solo instrument); the 'ood' - that well-known 'lute-shaped' instrument played with a plectrum which for many centuries has been 'the instrument most commonly used by the best Arab musicians and is celebrated by numerous poets'; and the 'náy' - 'a kind of flute' which 'yields fine, mellow tones', the several kinds of which differ from each other 'in dimensions, but in little else'. These four instruments together 'represent an ordinary Egyptian band, such as is generally seen at a private entertainment'. But sometimes, as Lane has himself observed, there may be more musicians, and often two singers. He draws attention to the fact that one other instrument in particular, 'a small tambourine called "rikk"', is often used at private concerts (pp.355-60).

Hereafter, Lane concentrates mainly on describing religious and ceremonial instruments, many of which are for outdoor use, and belong to the percussion family. He writes:

The instruments used in wedding-processions, and the processions of darweeshes, etc., are chiefly a hautboy, called "zembr," and several kinds of drums, of which the most common kinds are the "tabl beleedee" (or country drum—that is, Egyptian drum) and the "tabl Shámee" (or Syrian drum). The former is of a similar kind to our common military drum, but not so deep. It is hung obliquely. The latter is a kind of kettle-drum of tin-copper, with a parchment face. It is generally about sixteen inches in diameter, and not more than four in depth in the centre, and is beaten with two slender sticks. (p.361.)

Lane adds that a 'pair of large kettle-drums, called "nakákeer" (in the singular, nakkárah), are generally seen in most of the great religious processions connected with the pilgrimage, etc., in Cairo'. Both are made of copper, and played in spectacular fashion by a rider mounted upon a camel (pp.361-2). He soon introduces the metallic percussion instruments of the Egyptians as well:

Darweeshes, in religious processions, etc, and in begging, often make use of a little tabl or kettle-drum, called "báz," six or seven inches in diameter ... They also use cymbals, which are called "kás," on similar occasions. The báz is used by the Musahhir, to attract attention to his cry in the nights of Ramadán. Castanets of brass, called "ságát," are used by the public female and male dancers. Each dancer has two pairs of these instruments. They are attached, each by a loop of string, to the thumb and second finger, and have a more pleasing sound than castanets of wood or ivory. (p.362.)

Lane's subsequent description of the physical appearance of the 'târ' and 'darabukkeh', and their method of performance, is worth quoting at length. It was the sound of these two instruments (along with other types of tambourines and drums), as well as the aforementioned sâgât (brass castanets), nây (a relative of the flute) and zemr (a relative of the oboe), which were all imitated by Orientalist composers (Fig.1). French composers became especially fond of 're-creating' the sound of these instruments, from the date of the première of Félicien David's 'Desert' Symphony, in 1844, onwards. Lane states:

There are two instruments which are generally found in the hareem of a person of moderate wealth, and which the women often use for their diversion. One of these is a tambourine, called "târ," ... It is eleven inches in diameter. The hoop is overlaid with mother-of-pearl, tortoise-shell, and white bone or ivory, both without and within, and has ten double circular plates of brass attached to it, each two pairs having a wire passing through their centres. The târ is held by the left or right hand, and beaten with the fingers of that hand and by the other hand. The fingers of the hand which holds the instrument, striking only near the hoop, produce higher sounds than the other hand, which strikes in the centre. A tambourine of a larger and more simple kind than that here described, without the metal plates, is often used by the lower orders. --The other instrument alluded to at the commencement of this paragraph is a kind of drum, called "darabukkeh." The best kind is made of wood, covered with mother-of-pearl and tortoise-shell, etc. It is fifteen inches in length, covered with a piece of fishes' skin at the larger extremity, and open at the smaller. It is placed under the left arm, generally suspended by a string that passes over the left shoulder, and is beaten with both hands. Like the târ, it yields different sounds when beaten near the edge and in the middle. A more common kind of darabukkeh is made of earth, and differs a little in form from that just described. ...

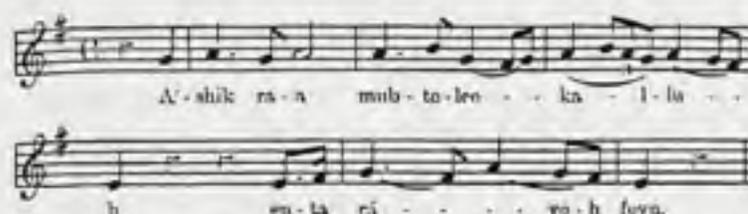
The boatmen of the Nile very often use an earthen darabukkeh, but of a larger size than that used in hareems, generally from a foot and a half to two feet in length. This is also used by some low story-tellers and others. The boatmen employ, as an accompaniment to their earthen drum, a double reed pipe, called "zummarah." There is also another kind of double reed pipe, called "arghool," of which one of the reeds is much longer than the other, and serves as a drone or continuous bass. This, likewise, is used by boatmen, and sometimes it is employed instead of the nây at zikrs [religious song and dance festivals which climax in wild, ecstatic frenzy]. Both of these reed pipes produce harsh sounds, and those of the latter much resemble the sounds of the bagpipe. (pp.362-3.)

As well as that of the darabukkeh, Berlioz was one of several French composers who approximated the sound of the 'arghool' - complete with drone - by the use of three oboes (the lower two playing the drone) in his 'antique Middle-Eastern' opera, *Les Troyens*. Alphonse Daudet felt that such instruments produced the most strident and characteristic of sounds to be heard in the 'East'. He particularly remarked upon 'the Oriental pomp, discordant music, reed-flutes and little harsh-sounding drums' of the musicians of Algiers (trans. n.d.: 58).



Fig.1. Engraving of sâgât (above left), a târ (below left), wooden darabukkeh (centre), and earthen darabukkeh (right), as shown in E. W. Lane's *Manners and Customs of the Modern Egyptians* (1836) (rpt. 1989: 362-3).

Lane even provides four notated specimens of Egyptian music, but cautions that they lack 'the embellishments which are added to them by the A'láteeyeh.' Regarding their method of performance, he reports that 'distinct enunciation and a quavering voice are characteristic of the Egyptian mode of singing' (p.364). Each of the simple, repetitive 'folk' or popular songs which he cites abounds in metaphors of intoxicating sensuality (**Ex.1**).



Ex.1. The last of the four examples of Egyptian popular songs, as cited by E. W. Lane in his *Manners and Customs of the Modern Egyptians* (1836) (rpt. 1989: 369).

Lane is one of the few Western commentators to appreciate the true meaning of the term, "A'l'meh". He points out that the "Awálim" (in the singular "A'l'meh" or "A'limeh") are extremely accomplished female singers, learned in literature, whose music he has enjoyed more than any other he has ever heard. He stresses that they are not the 'common dancing girls' to whom 'travellers have often misapplied the name of "almé"' (pp.354-5). Eugène Fromentin made precisely this error when, in *Un été dans le Sahara* (1857), he provided a description of an Oriental dancing girl of the sort that fascinated Westerners.¹ When Fromentin's North African caravan stopped in the evening near Boghari, dancers and musicians were called out from the town to entertain the travellers. An *almée* stepped forth whom Fromentin determined to be a 'pretty woman of easy virtue'. Against a backdrop of blazing fires and a golden 'Oriental' sunset, he recalled a scene in which a large circle of Arab spectators was formed. To the deafening sound of half a dozen unseen musicians 'armed with drums and flutes', the dancing girl commenced her performance. Beginning with slow bodily undulations, she soon graduated to swooning movements and stamping of the feet, then stretched out her arms as if to entice an invisible lover who seemed to be speaking to her 'in the voice of the flutes' (see Hagan 1985: 74-5). The seductive 'Oriental' dance became almost a cliché through its imitation in innumerable European musical compositions of Eastern flavour, beginning in the early nineteenth century, and continuing for a period of approximately one hundred years afterwards.

Lane recognizes Egypt to have long been celebrated for its 'public dancing girls', the most famous of whom he states in no way resemble the 'A'l'mehs, but belong to 'a distinct tribe called "Ghawázee". Lane finds little elegance in their dancing, and warns that while they may commence with some decorum, it inevitably follows that by their heightened animation and 'by a more rapid collision of their castanets of brass, and by increased energy in every motion', the Ghawázee soon present an outrageously lascivious and immoral spectacle. Lane observes that the dancing of the Ghawázee is generally accompanied by musicians 'whose instruments are the kemengeh, or the rabáb [a 'curious kind of viol'], and the tár, or the darabukkeh and zummárah [a geminate single-reed-pipe], or the zembr'. The status of these dancers is far below that of the honoured 'A'l'mehs (pp.372-3, 361 and 376).

¹ The French writer and painter Eugène Fromentin first visited Algeria in 1846 (3rd March-18th April), and returned for a second and longer sojourn in 1847-8. On his third and final trip to the 'East' (Algeria) in 1852-3, he studied and painted several 'Oriental' instruments, including a 'darabukka' (shown as Plate 9 in Kemp ed. 1988: 206-7).

IN 1833, the same year that E. W. Lane travelled to Egypt for the second time to observe and document modern Egyptian customs, a young, idealistic French musician, named Félicien David, embarked at Marseilles for the East with several other 'brothers' of the (socialist) Saint-Simonian Order. David was only twenty-three when on the 23rd March, 1833, he left on a journey of over two years' duration, which took him first to Constantinople, 'the hoped-for city of light', then Smyrna, Egypt and the Holy Land (Hagan 1985: 45). The charismatic leader of the Saint-Simonian Order, 'Père' Enfantin, dreamt of digging a canal through Suez as part of his aim to further world peace and productivity through trade. While in Egypt making preparations, he observed how players of the 'darabukka, the fife, and the ivory flute' would march at the head of ten thousand peasants as they went off to their day's work (Locke 1986: 182).

David returned to France in 1835 with a suitcase full of sketches of the East, and a 'heart full of new musical impressions'. Besides notating countless 'airs arabes' (many of which subsequently found their way into his 'Oriental' works), David also took particular note of native drum rhythms (Hagan 1985: 53). In his *Méodies orientales* for pianoforte, David proudly attests to the authenticity of these drum rhythms by inserting, where appropriate, the superscription 'le tarabouka' (Locke 1986: 193). On the homeward voyage, David wrote to a childhood friend: 'I am coming back to sing in France of the Orient'. And that is precisely what he spent the rest of his life doing.

Like E. W. Lane, David spent much time in the cafés of Cairo, listening to popular song and dance music 'flowering with throat trills and scales, played and sung in unison or heterophonically, sometimes dreamy, sometimes nasal and monotonous' (Hagan 1985: 51). As David held success to stem from originality in artistic creation, he concentrated on delivering to his public exotic works of a kind unprecedented.

We may safely assume that many of the 'Oriental' instruments which Lane described in Cairo at the same time that David was there (there is no record they ever met), were also heard by the composer. Many of these instruments had changed little since ancient times. Indeed, in the seventh century, the prophet Mohammed is said to have bestowed his blessing upon several of them, including the tambourine, and an Arabian tambour with gut snares called 'bendair' (Blades 1975: 184). These instruments and their unique sounds left a lasting impression on David. Berlioz, who had in the year prior to the première of David's *Le Désert* only just published his influential *Traité d'instrumentation* (1843), enthused over the 'exquisite choice of timbres' which David had created as a testimony to his Eastern travels (Hagan 1985: 84). David's orchestration was certainly far more innovative than his generally unadventurous harmonies, which remained largely unaffected by his contact with the 'East'.

Of the particular sounds which have been identified as being imitated by David in his 'Oriental' works, are those produced by the ubiquitous 'derbouka' drum, the 'bendeyr' (bendair), paired drums called 'tobilets', 'dnoutsch' or finger cymbals, the 'deff' (duff) - a square-shaped tambourine - and 'the short, breathy melodies' of flutes called 'djouwak' and 'gsbah'. Hagan further argues that a 'variety of such instruments in combinations applied to unison melodies are still to be heard in cafés, and they may have suggested the characteristic trceries of David's later orchestrations' (pp.51-2). It should be noted, however, that just as David represented the East in his music with Western instruments and compositional devices, so he and his friends were always aware of the fact that the Orient which they had visited was something quite apart from the one they set about fashioning at home.

David's 'Desert' Symphony was the first major orchestral composition to be inspired by his 'Oriental' pilgrimage. In this work, with text by the Saint-Simonian poet, Auguste Colin, there is an episode entitled 'Hymn to the Night', which includes an account of the dance of an 'Almeh' at a caravan halt, much as Fromentin later described it. Colin writes of how the Almeh's musical accompaniment is provided by the 'tarabouka' drum, now that 'the whole caravan rests, dreaming, / on the moving sand'. But it is the 'Dance of the Almées' collectively (and the vigorously percussive 'Arabian fantasy' which precedes it), rather than the somnolent 'Hymn to the Night', for which David saves the full resources of his 'exotic' orchestration. To an accompaniment provided by short, syncopated figures in the strings, the sinuous lines of the 'Almées' emerge in the form of prominent

woodwind solos (oboe and clarinet especially), although these melodies always manage to remain as distant, mysterious and beguiling as those of a snake charmer. A great tutti flourish completes the dance and also serves to lead into 'Liberty in the Desert' - a hymn of praise to the virtues of the unspoiled East, much glorified by the Saint-Simonians in comparison with the over-sophisticated West.

Four years after *Le Désert*, David wrote an oratorio entitled *L'Eden* (1848), depicting the Golden Age in the earliest days of the world. The following year (1849), he began an apocalyptic melodrama entitled *La fin du monde*, with a finale depicting the Last Judgment. This work was adapted to focus attention on the Babylonian Queen Olympia's attempts to stop the spread of Christianity, and eventually became his four-act opera, *Herculanum* (1859). The volcanic explosion at Vesuvius which destroyed, in 79 A.D., this ancient town dedicated to the supposedly invincible Hercules, was David's allegorical warning to a morally culpable and spiritually denuded West, which had upset the balance of nature. Reviewing this opera with its undertone of 'nostalgic complaints', one critic placed David 'in the tradition of Meyerbeer and Halévy' - two of the most notable French orchestrators of the first half of the nineteenth century. The critic spoke of how 'charm and colour emerge unceasingly from the fecund palette of this Léopold Robert of music'² (in Hagan 1985: 160).

Berlioz, who in 1859 had just begun work on his own epic opera, *Les Troyens*, aimed to improve upon David's orchestration, disappointed that his young disciple had never quite fulfilled his early promise in this direction, after the unprecedented 'Oriental' timbres employed in *Le Désert*. Indeed, *Le Désert* became the exemplar of musical Orientalism for years to come. In the 1840s, the critic J. Maurel found it to be a work of such originality as to bear 'no resemblance to any other music' he had ever heard - and it was recognized as being entirely due to the fact that David had journeyed to the 'Orient' that he was able to create such extraordinary music (pp.80, 82). This view was shared by Berlioz in a review he wrote for the *Journal des débats* on 15th December, 1844. As late as 1872, David's 'Desert' Symphony still seemed refreshingly 'new' to yet another music critic, Gustave Bertrand. Moreover, thanks to improved travelling conditions and the recent opening of the Suez Canal, Western travellers were now properly able to 'recognise the subtle *bizareries* of Félicien David's tone-paintings of Oriental scenes' (trans. in Gradenwitz 1976: 505). By this date, the instruments and sounds of the exotic East had well and truly begun to impact on Western orchestral timbres.



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² Léopold Robert was an important early nineteenth-century painter and vivid colourist.

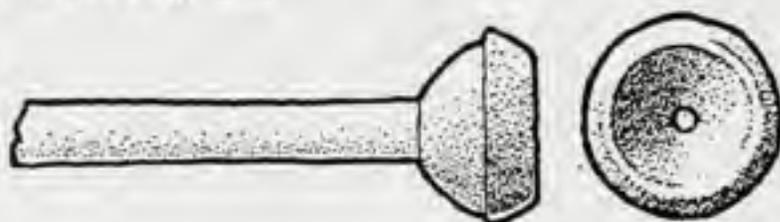
Comm. 1691

Roy Chiverton

A ROMAN MOUTHPIECE IN ENGLAND

I learned by chance that a Roman mouthpiece had been discovered in excavations at Coppice Corner, Gloucester, in 1983, and that it is now on permanent display in Gloucester City Museum and Art Gallery. I have since been allowed to examine it and to put together this basic description of it for the FoMRHI Quarterly. I am very grateful to the Gloucester Archaeology Unit and to the City Museum and Art Gallery and in particular to Mrs Sue Byrne, Archaeology Officer (Collections), for making this possible.

The sketch of the mouthpiece is after a drawing by Philip Moss, formerly of the Gloucester Archaeology Unit.



The mouthpiece is at present 65.4 mm long overall. The unevenness of the bottom edge of the shank makes me wonder whether it might not have been broken off a longer tube. There is, however, no sign of the crimping which might have accompanied such an event. The bowl externally is about 15mm at most deep and 25 mm wide. Internally, it is bee-hive (oldstyle) in shape, and is 16.9 mm wide and 10 mm deep. The rim is quite sharp, with the sharp edge at the inner face. The throat appears to be about 3.8 - 4 mm in diameter, and the shoulder to the throat is fairly sharp.

The shank of the mouthpiece is 53.5 mm long and about 7.5 mm in outer diameter. There is a little external corrosion, but it appears to be cylindrical externally. The internal diameter appears to be about 5.8 mm. Here corrosion is more noticeable.

The depth of the mouthpiece cup and the external measurements of cup and stem suggest that the inner cup penetrates a little below the outer cup/shank transition. Were the mouthpiece cup to have been cast, this would suggest a separate shank to me. Looking in both directions through the mouthpiece, I could persuade myself that I saw a line round the shank which might support this, and a gentle probe with a slender wooden rod suggested a shoulder, but I would not assert this very positively. I could see no turning marks, nor could I see a seam down the shank.

The museum has suggested that this might be the mouthpiece of a cornu, but those books of mine which say something about Roman mouthpieces ("Brass Instruments", Anthony Baines, 1978, and "The Trumpet and Trombone", Philip Bate, 1978) describe the cornu mouthpiece as fitting over the end of the resonating tube. If this was the case here, the bore of the main tube must have been extremely narrow.

It would seem to me to be more likely that the mouthpiece fitted inside the resonating tube. A cylindrical shank would provide the further advantage of a tuning

capability, unlike the cornu shank, which apparently expanded from the cup (Bates, p104). I cannot, however, find any discussion of variations in tuning pitch of Roman musical instruments in my texts, and I saw no indication of a circular mark on the outside of the shank which might suggest a regular depth of insertion. The unevenness at the end of the shank leads, too, to the alternative possibility, as I suggested above, that the shank was originally part of something longer or even of a complete instrument, and that the mouthpiece had been permanently fixed in it. At some point the instrument might have been broken and, perhaps, thrown away. This would imply, given the apparent absence of a shank seam, a sophisticated construction technique (cf the lur).

I can't suggest positively what instrument this mouthpiece might have belonged to, but it seems, in terms of modern brass instruments, basically of trumpet/tenor horn size, hence for an instrument from 4 to 6 feet long.

Memory (essentially fallible) tells me that there is a reference in Ennius:-

"Lituus stridens tantaradantu dixit"

and the Oxford Dictionary of Quotations quotes Ennius as saying:-

"At tuba terribili sonitu taratantara dixit"

If both are quoted correctly, (the second one is), this doesn't really help to distinguish tuba from lituus, but both suggest a trumpet-like sound. Baines also says that the cast bronze mouthpiece of the cornu fitted over the end of the instrument tube, and quotes cornu mouthpiece dimensions of 25-26 mm cup width, throat 5.5-6 mm and depth 5.5 (small)-11 (large) mm., a possible tuba mouthpiece is given as cup 21, throat 5, depth 14, and a Gallo-Roman is cup 16 throat 3 and depth 5. This suggests that the Gloucester mouthpiece did not belong to a cornu.

I also consulted Comm. 542 by Peter Barton in FoMRHIQ 36 of July 1984, which was also the basis for an article in "Roman Military Equipment and the Accoutrements of War" ed. M Dawson, BHR Series 336, 1987. At the end of the BHR 336 article, there is an appendix on "trumpet mouthpieces probably for use on the cornu" by N. P. Wickenden, which includes drawings of mouthpieces from Colchester, Verulamium, Lydney Park and Wickford. That from Colchester is, I infer, that on which Peter Barton based his mouthpiece. It is apparently one-piece, on a conically expanding tube (cf Bate, p. 104), as is the Lydney Park mouthpiece. The Verulamium mouthpiece and that from Wickford both have a shank which tapers externally, presumably to fit in, and an expanding backbore, much like modern brass mouthpieces. Lacking scales for these drawings, I cannot comment on sizes relative to the Gloucester mouthpiece, but it seems at least clear that none of them could have been used for fine tuning without packing the end of the instrument with which they were used. Peter Barton's comm shows a two-piece mouthpiece, but he tells me that this was a maker's facility. Could it then be that the Gloucester mouthpiece is unusual in its parallel shank and potential tuning ability?

There may be more information in "Die Musik im Römischen Heer", Mainzer Zeitschrift 7, 1912 by F. Behn, but I have not been able to consult this.

Making Italian roses

As a belated reply to William Hendry's enquiry (Bull. 86, p. 3) I would like to add some more detail to John Rawson's Comm. 1524, and E. van Weerd's Comm. 1515.

Glue: I have used liquid fish glue (Croid), or, which is now more easily obtainable, Franklin's Liquid Hide glue. Normal warm hide glue, fish glue in sheets, or kitchen gelatine can also be used in the way described below. The advantage of Liquid Hide glue is only one of time saving.

Before applying glue to parchment I have rubbed the top surface with 0 grade wire wool to remove some of the waxy surface finish (whatever it is...) and **lightly** sanded the underside with 180 grit abrasive paper. The underside of the first layer of the rose should be sized with glue (I have used half strength glue, i.e. diluted with 50% water) **before** the pattern is cut out. It helps if the top surface of the second layer is sized with an even more diluted version (say 4:1 water:glue); if there is too much glue on the upper surface it will appear obviously shiny. Of course, the underside of the second layer also requires preparation with glue **before** it is cut out.

When the first layer has been cut it suffices to dampen the glue with a wet rag and dampen the second layer (even if it has been sized) then place the two layers in a press. I have found that (non-historical) expanded polystyrene is excellent for ensuring even pressure over the rose, but it is essential to use a barrier film (e.g. kitchen "cling film") to prevent the glue sticking to the polystyrene. The rose and polystyrene can be cramped between blocks of wood. This technique works extremely reliably. It is quite impractical to spread glue on the delicate structure of the rose design **after** it has been cut, and the lack of glue beads bulging out of historical roses suggests to me that this technique must have been the one used by the old makers.

Materials: Parchment has already been described, but it was only recently that I became aware that there really is a distinction between vellum and parchment¹. Parchment is a split skin, and therefore tends to be thinner than vellum. William Cowley may now be the only UK supplier of skins². Many old roses were probably made of sheep skins, which gives an even, light colour, but I have obtained a calf skin vellum with similar colouring from Carl Wildbrett³. Goat skins are usually too variegated in colour to be suitable for roses. It is preferable to select material which

¹See The Calligrapher's Handbook (Faber & Faber, London, undated) ch. IV. My thanks to Barbara Shaw for a copy of this source!

²William Cowley, 94 Caldecott Street, Newport Pagnell, Tel 01908 610038.

³Carl Wildbrett, Waldstr. 20, D-86399 Bobingen bei Augsburg, FAX 0049 8234 3533. Mention my name in order that they are aware of the type of skin you require for roses.

is slightly thinner (e.g. about 0.2-0.3 mm) for the lowest layer (i.e. usually the third layer) in order that it can be punched cleanly. The top two layers may be slightly thicker (e.g. up to 0.5 mm) in order that the appearance of relief is created. If the vellum is too thin, or is parchment, then a poor sense of relief will be obtained. A vellum which is crisp and hard is preferable to one which is soft or has layers that tend to separate.

Most 16th-century Italian roses (i.e. principally from Venice) were made of thin cypress (0.2 mm) reinforced on the underside with very thin parchment (0.1 mm). If the total thickness exceeds 0.3 mm it will be found that it is difficult to cut this material and I recommend a total thickness not exceeding 0.25 mm. It is tempting to try to start with knife-cut veneers as an alternative, but these lack the stiffness across the grain of thin (solid) wood. Unless one has special sanding facilities for achieving this thickness, it will be necessary to plane by hand and then scrape and sand cypress to achieve the requisite 0.2 mm. The reinforcing parchment is glued in the normal way with hide glue. Nevertheless one must expect several breakages when working with cypress veneer; in this respect vellum is much more forgiving.

Tools: Scalpels have already been recommended. When I last examined the quality of blades available here, Swann Morton were the best. The no. 11 blades (in a no. 3 handle) used to have a slight hook to the end which was extremely useful for cutting roses. In recent supplies I have found that the factory no longer achieves this shape so I now recommend 10A. Although the blades are thin (0.4 mm) it will be found advantageous on no. 11 blades to grind the tip of the blade so that it is only about 0.25 mm thick; this helps prevent the blade acting as a wedge and splitting thin cypress and also makes it easier to cut through the material in one cut, which reduces the amount of work involved. This is unnecessary with 10A blades. Don't be mean with the blades and use only the sharpest ones; this may mean a new blade for each layer. Forget about trying to sharpen them; unless you have special jigs and very fine grits you will not achieve the sharpness of a newly-ground blade.

In cutting the rose one also needs to pay attention to the direction of cut and where the cut will end, in order that the blade does not stray into a part which should not be cut through. An elementary point is always to turn the work so as to be able to see the guiding line and not hide it behind the blade. It is also advantageous to cut some parts of a design before others in order that the layer is still strong enough to be handled. In cypress roses the top layer is particularly difficult; here it is desirable to leave as much "wood" in the rose as possible, cutting out sections but leaving them held in place by a few narrow (e.g. 1 mm) bridges until the last moment. Although the cypress rose is slightly reinforced with parchment, this should not lull you into any false sense of security: one must always pay attention to the grain and decide how the wood could split at any point along the cutting line.

John Rawson drew attention to the use of a hard, end-grain wood (box) for punching holes. Beech or maple may be adequate, but box

is better, especially on soft vellum. It is also helpful to cut the rose on a block of endgrain wood; here the hardness of box is not helpful.

For some roses one also needs a small chisel-like cutter which can be ground from a 1 mm (or larger, as required) drill. Although a wood handle can be made for each such tool, a pin chuck also holds such a cutter securely and enables accurate work. All my used HSS drills land in the box containing my rose punches since I occasionally need to make a special hole or other shaped cutter. One source of tubular stock from which a range of cutters can be made (about 2.5 mm-8 mm diameter) is a three section tapered antenna (approx. 3 m long intended for military vehicles) made of a slightly tempered steel. Although I cannot give any present source of such antennas, military/electronics surplus supply companies are the places to ask. A $\times 3$ jeweller's eyepiece for close in work is useful but one is so close to the work that it is not the best solution. Binocular magnification at about $\times 2.5$ - $\times 3$ is better and I have a lightweight spectacle (Labomed model no. 1644) which takes interchangeable plastic lenses of good optical quality⁴.

The design: Some old cypress roses show signs of scribed construction lines. I usually use HB pencil for layout on vellum, but sharply pointed dividers not only show the place where the cut should come, they can also guide the blade to some extent (especially in cypress). Mostly one has to discover the correct opening for the compasses and where they have to be set in an empirical reconstruction on paper before attempting the layout on parchment or cypress.

⁴Made by Eschenbach of Nuremburg. Order no. 164453 for $\times 3$ magnification, order no. 164452 for $\times 2.5$.

FoMRHI Comm. 1693

David Owen

Ways of Treating Wood

In his FoMRHI Comm.1667, "An unusual method of stabilising wood", Donald Gill ends by asking the question, "...have I come across something that is already well known?"

Whilst looking for historical information concerning early woodwind manufacturing methods and acid staining, I came across several exotic ways of treating wood.

Plumier in "L'Art de Tourner" (1701) Part XII Chap.III gives many recipes for hardening, softening and dyeing/colouring woods and other materials.

Here follow three extracts to set the scene:-

To Harden Wood The workpiece completed,boil it for half a quarter of an hour in olive oil and it will become as hard as brass.

To harden and petrify wood Take rock salt, rock alum, white vinegar,slaked lime and powdered de-crusted flint in equal quantities; mix together. It will effervesce, and when this has ceased, the liquor produced will petrify whatever porous material is put into it, such as wood etc., when leaving it immersed for four or five days.It is necessary that the quantity of the liquor should be more than double that which is to be petrified.

This next recipe seems to have something in common with the Swiss wooden clock makers!

To give wood whatever colour one wishes Gather the freshest, most moist horse dung (here the original French was more robust!) that you can find. Wrap it in a linen cloth and express the juice into a glass vessel.....(summarising now, the recipe continues with the addition of various quantities - stipulated in long-obsolete units of measure - of rock alum, gum arabic and whatever dye.....)

Some of these recipes are repeated, more or less word for word, by Bergeron in "Manuel du Tourneur" (1815)

Later, Holzapffel in "Turning & Mechanical Manipulation" Vol I pp 113-116 "Memoir on the preservation of woods"....various experiments described including an allusion to hardening of woods.

All this is probably just the tip of the iceberg?

Comm 1694

Marco Tiella

About an unusual method for stabilizing wood

With regard to Comm. 1667, 'An Unusual Method for Stabilizing Wood', Professor Elio Corona of the University della Tuscia, Viterbo, Italy, wrote to me to say that the practice of using of dung to "stabilise" has been known for a long time. According to Aristotele, Polit. VI, 8 (5), VII, 12 (11), Greek land surveyors lined the insides of new wooden casks with hay and dung (A. Di Berenger, *Studii di Archeologia Forestale*, 1865, Reprinted 1965, pages 628-629): Fulton (1991) suggests treating "green wood with ammonia followed by arching and bending of violin plates". There are records of urine-rich fresh dung having been used in a few alpine valleys. The practice has also been recorded in the Trentino region. Urine and not dung was used in the Primiero as well as in Friuli. Both these areas belong to the alpine region where very fine fir trees grow. Finished pieces of wood were placed into vats containing the urine. The most common types of wood used were from broadleaf trees i.e. chestnut and oak. Beech was not used, as it tended to simmer, or "cook". Fir and silver-fir were never used. In Professor Corona's opinion, during construction, treatment with urine on the instrument's resonant areas should be avoided. Urochrom, stercobilin, chororubine, pyrocatechol – which are all phenols – and the components of bilirubin e.g. meso-leucobilin and meso-bilificoin should not be used to treatment the "resonant" fir. Phenols and components of bilirubin should not be used in the treatment of the "resonant" fir. These chemical components will also cause a change in the tree rings, especially the early ones. The cellulose crystallographic structure, and consequently sound dissipation, may also be altered and interference may occur in the medium-high frequency range.

Comm. 1695

A Theologian's Specific for Happy Musicology

John Catch

"Let us maintain, before we have proved. This seeming paradox is the secret of happiness". (John Henry, later Cardinal Newman, 'Tracts for the Times', no. 85, p. 85).

In Comm. 1675 Eph Segerman, referring to conventionally-tuned small 6-stringed bass viols in England in the 1690's, makes the following uncompromising assertion about the low D string: 'That was the only overspun string'.

How does he know that?

If FoMRHIQ aspires to a place on scholarly bookshelves such a blunt *ex cathedra* assertion (literally so, from the editorial chair) needs to be supported by evidence of fact and logic of the kind which scholarly literature demands. None is offered. I know of none. I recall none in our editor's voluminous past publications. Hypothesising based on 'considerations of string technology' will not serve. An overspun G fifth was as practicable three hundred years ago as it is now. That there is no positive evidence for more than one - the fallacy of deriving a positive conclusion from negative evidence - cannot justify such an oracular pronouncement. If my memory serves, that reasoning was at one time adduced by Segerman for asserting with equal confidence that Talbot knew nothing of overspun bass strings, before I found, in 1988, Talbot's reference to them. In a case such as this a careful writer acknowledges that he does not know, and will be cautious even in expressing an opinion. T.H. Huxley has somewhere remarked on the unwillingness of many human beings to admit that they do not know.

But there is in fact a scrap of positive evidence for the use at that time of more than one overspun string on one instrument - in fact, of no less than three - in Merk's Augsburg 'Compendium...' of 1695. That was a rather unusual case; but remembering that overspun strings are (on present knowledge) first noticed in England thirty-six years before that date, it is perfectly possible that more than one was used on a small bass viol of the 1690's. I don't know that there were more than one. Neither (I submit) does Segerman know that they were not.

Further to Comm. 1655. The 'Sisters' and Irish Double Harps.

Harper Derek Bell of Bangor, N. Ireland, recently wrote to me about the 'Sisters' in Irish harp tuning. He says:

" I believe that harp accompaniments based on pedal points in the form of a continuous bass, decorated drones were very common and when the 'Sisters' are tuned to a unison then tremolos, trills, mordents and trills are possible without the interference of any dampening of the active strings.

The 'Sisters' are fine when they clearly differentiate the tenor and bass registers of the harp, as they most often do, but if the 'Sisters' were to occur - as they would on a very small harp - in the middle of the bass register, then for octaves, thirds and sixths intervals, the 'Sisters' would throw the left or right hand out of alignment for stretching such chords or in passage work based on such chords. The 'Sisters' allow such strings to continuously ring on."

He then goes on to say:

" According to the Kilroys who make baroque harps, we are told that 30 string harps were tuned in two tiers, one using sharps and another using flats and naturals. They list the Book of Ballymote as their source and have written a lot about this!"

Modern lute stringing and beliefs about gut

This Comm. has been prompted by an exchange of articles in *Lute News* (the newsletter of the Lute Society) between myself and Martin Shepherd. He had used gut strings on his lute for some years, and then converted to modern stringing. His dissatisfaction with gut stringing is both for practical reasons and because he has not been convinced that the modern gut available has the properties that early gut had. In each of a pair of exchanges, I explained how the evidence offered is consistent with the properties of modern gut, and then he presented new evidence that he expected me to be unable to explain in this way. There was editorial concern about this continuing indefinitely, so I offered, with Martin's agreement, to continue our exchanges privately, after which we would present the final results. We did this, but after several rounds, Martin offered no more evidence, and suggested that we ended the exercise. Since he had no position he wanted to take, he asked me to write, as the final statement of the debate, my explanations of the evidence he raised during those rounds. This final statement was rejected for publication in *Lute News*, as stated in the third paragraph of Comm. 1647.

Most modern lute players¹ have a strings credibility problem, and historical scholarship on lute stringing has not offered solutions acceptable to them. So perhaps Goodwin was right in Comm. 1647, and FoMRHIQ is the right place for it rather than any of the Lute Society's publications.

The early music audience knows that lute stringing was originally with gut. It knows too that the early bowed instruments are now usually strung with gut (and that interest in original stringing is growing), and asks why most current lutes are still strung with plastic strings. The answer often given is that modern gut is not as good as early gut, so plastic strings are the only ones available today that can do the job that early gut did.

There is a convincing philosophical reason for the lute players to believe this explanation. The early-music movement was founded as a logical extension of an unprecedented philosophy developed in the 20th century classical-music field: that fidelity to the composer's intentions is more important than more modern traditions of performing their works. Much of this was a reaction against what was perceived as 'excesses' of the 19th century. There was then the belief that music always gets better, so musicians felt that their up-to-date interpretations of previous music were the best ever, and that there was plenty of room for improvement of that music. In reaction against this, musicians became modest in response to acclaim, saying that they were only doing what the composer wished. Also, musicologist did research into 'urtext' editions, which critics promoted, holding out the promise of 'definitive' performances for collectors of recordings.

The early-music movement extended this philosophy by attempting to recreate the sounds that the original composers expected in performances of their works. Implicit in this philosophy is the expectation that the closer one gets to the composers intentions in performing their works, the more attractive we will find the result. This prediction had become an article of faith amongst many in the movement. Early-music audiences have enthusiastically embraced it, enjoying the idea of hearing precisely what was heard centuries ago whenever they listen to performers who claim that they are being authentic.

The early-music bowed-instrument players claim that gut stringing sounds better than modern alternatives (though the gut strings used tend to be of types historically appropriate for later than the music played). When lute players hear lutes strung with gut, they are not impressed by any better sound than from lutes strung with the plastic strings they use. That is a very serious problem. Historical accuracy should enhance beauty (according to the philosophy), but using modern gut strings doesn't (according to modern aesthetic perception). Therefore, the argument goes, modern gut cannot be historically accurate, and the properties of modern plastic strings must be closer to those of early gut than modern gut.

¹ A single word would be less cumbersome than 'lute player'. The word 'lutenist' was used by Dowland, and recently in the Stainer and Bell song publications. It was suggested some time ago in a Lute Society publication that 'lutunist', another old term, was preferable. I find the other old term 'luter' more attractive than 'lutunist' (an alternative spelling is the more quaint 'lewter'), but it is subject to unfortunate punning. Since there is discomfort with all of these, I will stick with the awkward 'lute player'.

Modern aesthetic perceptions cannot be admissible as evidence in historical scholarship. We know historically that perceptions of what is beautiful have always changed with time, as other fashions change. So the basis of the philosophy, that whatever was perceived as beautiful centuries ago would be perceived as more beautiful than any alternative version of it today, is historically quite untenable.

← *Comms 1545, 1593 + 1657 + 1658 + 1659*

In order to support the deduction about the inferior properties of modern gut compared to early gut, certain lute players have been citing evidence that they claim indicates properties of early gut that cannot be met by modern gut. The most important properties claimed are greater tensile strength for the highest string and a richer sound in the lowest strings. Before examining such claims, let us review the modern history of lute stringing:

Treble strings and tensile strength

Before WWII (nylon was invented in 1939), the only options for treble strings were gut, silk and metal. Metal was not acceptable for lutes, and the properties of gut and silk are very similar. Ian Harwood informs me that Diana Poulton found that silk first strings made by Arnold Dolmetsch lasted longer than gut. When players then tried using gut treble strings on lutes that were typical of original ones in size (from surviving instruments, early pictures and Praetorius's depicted lute with a 62 cm string stop), with the highest string tuned to g' , they broke much more often than would be expected from the breakage rate of gut strings that still were used on some violins (Fritz Kreisler used a gut e'' string throughout his career). Praetorius's pitch standard had been deduced to be close to modern, so the pitch standard seemed not to be a problem. Thus a modern gut first string on a lute seemed rather impractical, stimulating conjecture that modern gut is not as strong as early gut was. Very few people played lutes then.

When nylon became available, it solved the problem in a practical way. Nylon treble strings don't sound quite as good as gut because nylon has lower density, but its sound was acceptable. Gut sounds a bit richer since there are more higher harmonics in the sound. The great advantage of nylon is that it can be tuned higher than gut without breaking. That is because it has greater tensile strength. Since the problem of top string breakage disappeared, lutes became practical to acquire, and lute playing blossomed.

Recent research has increased our understanding of how the problem arose. The centres of lute making in Europe (mainly Italy and south Germany), where the traditions of the relationships between sizes and string pitches developed, were in regions where the pitch standard was over a tone below modern. This pitch standard was called Chorthon in southern Germany, and though Praetorius discussed his own northern German Chorthon (which was a tone higher) with respect to organs, whenever he was discussing other instruments, he used the term 'Chorthon' only to refer to the tone-lower south-German Chorthon, which he preferred. A survey of the highest pitches and string stops of all of the gut-strung instruments depicted by Praetorius² shows that his lute and mandora were very exceptional, with the highest pitch for the string stop a tone higher than the highest of all of the others. He wrote that the mandora was played in France, a place which followed the tone-lower pitch standard, and the lute name he gave was 'ChorLaute'. There appears to be no reason for the 'Chor' in that name other than referring to a lute that played at his preferred Chorthon pitch standard.

Thus, the string-breakage problem is solved historically by the realisation that lutes of originally normal size (called 'mean lutes' in England) played originally at over a tone lower than modern pitch. This is not a practical solution for today's lute players because the early-music movement has adopted the pitch of a semitone lower than modern as a standard (and some have adopted a standard of a semitone above modern for Renaissance music), and many singers have perfect pitch, requiring the accompanying lute to play at modern pitch. The lute players are resistant to using a lute in a less typical original smaller size to play at modern pitches, and they don't want to play their own lutes at a lower pitch which would isolate them from the rest of the movement.

Recent developments in plastic technology have created new materials with a tensile strength as high as nylon, but with higher density, making treble strings that sound as good as, or even better than

² Comms 1545, 1593 and 1657

gut². These include polyester and PVF (polyvinylidene fluoride). These developments removed the remaining motivation of improving sound by changing to gut treble strings, and many lute players have adopted these new plastic strings.

Bass strings and inharmonicity

Attempts at stringing lutes early in this century tried to follow the indications given by Dowland and Mace that all of the strings were made of gut, with the bass strings apparently just thicker versions of the treble strings. Gut of varying thickness was available for harps, so these were mounted on the lutes. The result was that the thick gut bass strings sounded very dull indeed.

Technologically, the dullness is the result of a dearth of harmonics in the sound, caused by inharmonicity. The number of harmonics in the sound is just less than the cube root of half the 'inharmonicity factor'. The inharmonicity factor of a string with uniform cross-section is proportional to a materials component (the elastic modulus divided by the density) multiplied by an instrument-usage component (the square of the diameter divided by the product of the vibrating length to the 4th power multiplied by the square of the frequency). On a metal-wound string, the inharmonicity is only that of the core, with the relevant frequency being that which it would vibrate at without the metal. So, to increase the number of harmonics, one can change to a material which is either more elastic (stretchier) or of higher density, or to a string that is either thinner or tuned higher (metal-wound strings have both) or having a longer vibrating length.

These strings also sounded sharp on fretting. There is no historical evidence that frets were set at an angle to compensate for such an effect, so this stringing with thick harp strings was neither practical nor historically correct.

To be practical then, the early modern lute pioneers had to use bass strings that were metal-wound on gut, though some used guitar strings which were metal wound on silk. Historically, such strings were first used on English Guitars in the second half of the 18th century. With the invention of nylon, the guitarists quickly replaced their bass strings wound on silk by strings wound on nylon floss, which lasted very much longer. This latter type of string soon became available in a variety of weights, and were enthusiastically adopted by the rapidly growing band of lute players. Lutes had now become practical to play in modern concert conditions.

Several decades later, in the late 1970's, NRI began producing all-gut bass strings of rope construction, called 'catlines'³. These were more elastic than equivalent harp strings. This increased elasticity solved the problem of fretting sharp. It also reduces inharmonicity, giving the sound much more focus and resonance, though still much less than is given by metal windings. The sound of the harp strings had only the fundamental and barely hints of any harmonics. The sound of low bass catlines was still dominated by the fundamental, but it included a few clear harmonics to give it some focus. The sound of metal-wound strings is dominated by harmonics, with a small fraction of the sound energy in the fundamental.

Thick all-gut strings used on lutes were called 'catlines' by Dowland, and 'catlins' by most others in the late 16th and 17th centuries. There is evidence that some thick all-gut strings had rope construction⁴. Though there is no direct evidence that the strings usually called 'catlins' also had roped construction, it is absolutely clear that roped construction is one valid solution to the historical question of what catlins were. There has been much interest in other possible solutions, but none other has been shown to be valid both technologically and historically. It is quite unlikely that one will be found.

Thick all-gut basses were preferred to metal-wound basses on double basses in the 19th century.

² The criterion for sounding 'good' is a modern one. With the help of a leading guitar maker, we conducted a survey of professional guitarists by providing polyester treble strings of varying elasticity, and they consistently reported that the more elastic strings sounded better. More elastic strings (i.e. with a lower elastic modulus) have lower inharmonicity, which allows more higher harmonics in the sound, making the sound richer. The material-properties factor in the inharmonicity depends on the elastic modulus divided by the density. Gut sounds richer than nylon (without metal) because its density is higher.

³ Comm. 138. The name choice was unfortunate because catlines were most probably a version of roped gut that was polished smooth, while the version produced then only had the normal bumpy surface of ropes.

⁴ Comm. 1318

Relevant here is what Quantz⁶ wrote with respect to low-pitched large double basses: 'what is lost in clarity is made up in gravity'. Twentieth-century aesthetics puts clarity above all, and modern double bassists want as much richness and focus as they can get from their metal-wound strings. This illustrates how judgement of what constitutes good bass sound does change with the fashion of the time.

There is evidence that the sound of metal-wound bass strings (with much less fundamental and much more higher harmonic content) was considered inferior to rope-construction strings for lutes after 1660, when they were first reported. Mace in 1676 wrote about lute stringing in such detail that we can be sure that he would have mentioned wound strings if they were in any way acceptable, and he didn't mention them. The Talbot ms. (c. 1694) mentioned that wound strings could be used for the lowest string on the bass viol or the bass violin, but he didn't mention their use on lutes, which he discussed extensively. *CARRATE*

When catline strings became available again late in the 1970's, a small minority of lute players switched to all-gut stringing. They apparently felt themselves to be pioneers, leading the way towards the authentic lute sound. To their disappointment, not many other lute players followed suit. Just as the original early lute players had loved their instruments with the sound they were used to (catline basses) in their ears, and did not appreciate the sound of wound bass strings when these became available, so the modern lute players loved their instrument with the sound they were used to (wound basses) in their ears, and did not appreciate the sound of catline basses when they became available. As a concession to the historical sound demonstrated by catlines (and to avoid the accusation of preferring a guitar-like sound), some players partially dulled the sound of their wound basses by rubbing beeswax onto them. Others accomplished this by using only very old wound bass strings.

At that time, early musicians who played publicly expected an advantage in obtaining performing opportunities by using more authentic equipment than others. This did not happen because the leading players did not convert to gut, and those who offered performance opportunities were more interested in the musical reputations of lute players than authenticity in their equipment.

In the early 1990's, loaded gut strings, where powdered metal and metal salts are folded in when gut strings are twisted up, became available⁷. These were offered to lute players as an historically acceptable interpretation of what was meant by 'catlin'. With the increased effective density (or specific weight), and the metal distributed evenly throughout the gut rather than wound on top, these strings provided much of the richer lower-inharmonicity sound that most lute players prefer. Some have adopted loaded gut for their bass strings (often retaining plastic trebles), but most lute players have kept the kind of stringing they started with in playing the lute, with which most are well satisfied.

The historical claim for loaded strings is that the technology was possible at the time, and that early catlines must have been loaded since the increased effective density is necessary to explain the diameters of bass-string holes in many bridges on museum lutes. These holes are too small for ordinary and roped gut strings to be in equal-tension. The historical evidence that loaded gut has not met, and would have great difficulty in ever meeting, is that fresh gut bass strings appeared 'clear' (Dowland, Burwell and Mace), and even 'transparent' (Burwell). At the very least, translucency seems to be implied here.

For the translucent optical effect, the index of refraction of the loading material would have to be close to the index of refraction of gut. The difficulty here is that the loading material would need to have a very high density, and most high-density materials have particularly high indexes of refraction, much higher than that of gut. If a loading material that satisfies the requirement of high density and low index of refraction happens to be found, a claim that this was used for most bass strings played on would need to explain why this particular rare material was used in all the centres of bass string making rather than other more common loading materials. That is a tall order, and unless and until these criteria are met, we must consider that the normal bass strings used in the 17th century were historically not loaded. Roped-gut basses (catlines) are the only alternative that fits all of the evidence.

⁶ J. J. Quantz, *Versuch...* (1752), Chapter XVII, Section V, §3

⁷ M. Peruffo, *Recercare* V (1993), pp. 115-151; see also Comm. 1021

Old gut characteristics presumed to have been lost by modern gut

The lute players claim that plastic stringing (with metal added in bass strings) has more authentic properties than modern gut since early gut was superior to modern (unloaded) gut in the ways that plastic strings are. They try to show that their theory explains some of the evidence better than the theory that gut has not changed significantly. If all of this evidence can be equally well explained by the theory that there has been no essential change, then that theory would be preferred in historical scholarship because gut strings have been continuously manufactured professionally from the 15th century to today, and there is no evidence (as there is, for example, in the case of Meuler's steel⁸) of any secret process from one workshop producing superior strings that dominated the European string market and was then lost, or that the physical properties of sheep intestines have changed in the last half-millennium.

The explanations that follow may well be reasonable from an objective historical perspective, but perceptions of reasonableness can be strongly influenced by vested interests. Establishing which theory is historically most probable will not convince believers in another theory. Such believers will see confirmation of their beliefs in any evidence that can possibly be interpreted their way, will either ignore or try to discredit any contrary evidence, and will demand an impossibly high level of 'proof' before taking any notice of any other theory.

The complaints about the alleged inadequacy of modern gut fall into four categories: the bass sound is too dull, the gut feels too stiff, tuning is too unstable to changing humidity, and treble strings break too quickly.

Bass strings - the evidence thought to support equal tension

It has been pointed out above that, because of the historical requirement of translucency, loaded bass strings are very unlikely to have been the type that became standard in the 17th century. So the small bass bridge holes in museum lutes (and the thinner bass strings in paintings of lutes than of bowed instruments) needs to be reasonably explained assuming the alternative: roped-gut bass strings. With only the density of gut in all strings, the tension in the lute bass strings would be less than in the treble strings. This is contrary to equal-tension stringing, which works very well on modern lutes and viols, and has much historical support. Support for it on the lute is in Mersenne's discussion of lute stringing, printed in 1636.

Mersenne (Bk. 2, Prop. II) stated that strings would be correctly proportioned amongst themselves if their ratios of diameters followed the ratios of frequencies of the intervals of the notes. Thus, if the largest or 11th course on the theorbo (actually an archlute) or lute (*BBb* in both) has a diameter of one line (2.28 mm), it follows that the diameter of the 7th (*F*), a fifth higher, is $\frac{2}{3}$ of a line, that of the 4th (*f*), a 12th higher, is $\frac{1}{3}$ of a line, and that of the 2nd (*d*), a 17th higher, is $\frac{1}{5}$ of a line. He was here giving an example of how to calculate string diameters according to his theory, which implies equal tension. That these diameters were not intended to be fully realistic is illustrated by the calculated diameter of the 2nd, which is somewhat less than the thinnest usually available then.

Mersenne was interested in formulating theories when no theories existed before, each of which he arrived at after making some measurements. He was interested in general trends and did not attempt to refine these theories or explore their limitations. His theory of equal tension was a good one, apparently working well for the stringing of viols and violins, and most of the strings on a lute.

What is curious here is that Mersenne lumped the archlute and lute together, implying that the lute and archlute string had the same diameter. The archlute 11th course is, of course, on the extended neck where the string stop is 1.5 times that on the fingered neck, so the proportional relationship of diameter with frequency will not result in equal tension. If that relationship is nevertheless applied, the tension in the strings on the extended neck would be greater than twice that on the strings on the fingered neck. For the 11th to have the same diameter on the archlute as on the lute, the tension on the lute 11th would be much less. These inconsistencies illustrate that Mersenne was not concerned with deviations in tension in the bass on these instruments, and by lumping them together, he presented an ambiguity that would ensure that there would be no reliance on calculations in the bass, where they differ. As a consequence, we can conclude that reduced tension in the low basses of a lute is not inconsistent with what Mersenne wrote about lute stringing.

⁸ Comms 866, and 1593

Instructions from Burwell and Mace state that one should string the lute so that it feels even from string to string. This has been considered to indicate that lute stringing was with equal tension. But what one actually feels when plucking a string is the kinesthetic sensation in the muscles involved in plucking (which sense force applied and distance moved), and the touch sensation in the skin (which senses the depth of depression). When we have theoretically-correct equal-tension stringing, the same plucking force will produce the same string displacement, no matter which string it is. Nevertheless, we will feel that the treble strings are stiffer than the basses. This indicates that the depth of depression in the touch (which is deeper with a thin string than a thick one with the same depressing force) is more important than the combined kinesthetic senses of distance moved and plucking force to give us the impression of string tension. Thus the statements of evenness in these sources refers almost certainly to smooth variation rather than equality of tension. Mace implied this in his comparison of playing divisions on a lute that is not 'equally strung' with running 'over a piece of uneven ground, with hard and soft places mix'd together'. Equal tension stringing can be arrived at either by calculation, as Mersenne did, or by hanging the instrument and putting equal weights to provide the tension on each string, as Leopold Mozart did, but not by equal feel to the fingers.

Bass strings - 'big sound' and low-tension low basses

It has been thought that all-gut lute bass strings must have had considerable power because the Burwell tutor stated that one of the reasons why the lute masters had abandoned the thickest string (the low octave of the 11th course) was 'because the sound of it is too big and smothers the sound of the others'. What is odd about this statement is that musicians in this period were used to varying dynamics in their performances, and contrary to the situation with the harpsichord, they could pluck as softly as they wanted. So there would be no reason to reject the use of a string if it had too much power. It is thus likely that another aspect of the sound was referred to here.

Beyond a maximum plucking force on a string at a particular plucking point, plucking harder does not increase the loudness of the sound produced. The higher the tension in the string, the higher the maximum plucking force, and the greater the loudness of sound it can produce. If the plucking point is closer to the bridge, one can pluck harder for more loudness, but with more plucking noise at the beginning of a note, and a more astringent sound afterwards.

Modern lute players avoid plucking near the bridge because the astringent sound is not as sweet as they like the lute to sound, and a strong initial transient noise in producing notes is avoided in all modern music. Now as well as then, instrumental sound has been expected to be similar to vocal sound, but vocal sound has changed, with modern suppression of the noises of consonants that were originally strong in the rhetorical style of performance. Mersenne wrote (4th Bk Prop V) that the viol 'imitates the voice in all its modulations', and he wrote (3rd Bk Prop I) that the viols 'have a percussive and resonant sound like the spinet'. Compare this with the *messa di voce* normal note production on viols today. In Purcell's famous 1692 Ode, the phrase was 'strike the viol', not 'stroke the viol'.

With low-tension basses on the lute, the normal thumb plucking position would be used for playing softly, and when one wanted to play loudly, one would pluck harder and closer to the bridge. The more astringent sound could well have been associated with musical tension. When Mary Burwell wrote 'if you chance to raise the little finger, it must be to reach a bass the better', it seems that this was an unusual circumstance, and not every time a low bass was played with a small hand. If this was so, this shift of hand position could have been to play close to the bridge.

From the bridge-hole diameters, the lowest string would be at the lowest tension, and so would have the softest sound, making playing close to the bridge more common. It is likely that the loud initial plucking noise was the 'big sound' that the early masters (mentioned by Burwell) objected to.

In the 16th and 17th centuries, instruments with octave-pair lowest bass courses had maximum open-string ranges that were a tone greater than those with unison-pair or single lowest bass courses. Earlier in the 16th century, the maximum-range octave-paired instruments were the lute and the lira da braccio. Compensating for the longer bourdon string stops on the lira, the range of both was two octaves and a tone. The maximum-range single-strung instrument was the viol, and that range was two octaves. When catlines became available, the maximum-range instruments were the octave-pair

⁷ discussed further in Comm. 862

lute, with two octaves and a fifth, and the single-bass viola bastarda and/or lyra viol with two octaves and a fourth.

How could an octave pair lowest string lead to the extra tone of range? If the low-octave string of the lowest course was thick enough to have a similar projection to that of higher strings, it would have had more inharmonicity than was normally tolerable. Otherwise, it could have been thinner, and the lowest-string inharmonicity would be at the normal maximum, but it would sound weaker. In each case, the sound of the high-octave string includes harmonics missing from the low-octave string, giving a fuller combined sound. The bridge holes in museum lutes indicate that the second was the case. It seems that the maximum inharmonicity tolerable in that musical culture applied to all strings, and the high-octave string in the pair made a weaker, rather than a greater-inharmonicity, low-octave string acceptable.

A closely related issue is what was considered good balance between treble and bass at the time. Ganassi wrote that the advice of Gombert on setting the pitch for a vocal performance applied to assembling a set of viols. In that advice, the pitch level should be set low enough to avoid strain in the trebles, and the balance is acceptable if the lowest bass notes are just audible. This bottom-light attitude towards balance is supported by the normal range of bass singers given by Praetorius, which goes lower than it does in modern choirs. We nowadays require a stronger bass contribution than in those days for good balance.

Gut flexibility

Gut stringing has a reputation of sounding warmer than modern substitutes. It thus makes emotional sense (but not necessarily objective sense) to expect that one can feel a physical property of gut that can somehow be associated with that warmth of sound. The finding that a more elastic type of gut string (achieved by more twist) is needed for good sound as gut gets thicker, has encouraged musicians to assume that good early gut was more flexible than modern gut.

What they find very convincing of this idea is that many paintings show spare string length curling out of the scroll, or even looped into a figure-of-eight. They are misled into believing that these effects are evidence of extreme flexibility. The flexibility of a sewing thread falling on a flat surface can produce curling shapes like these, but to maintain such shapes unsupported in three dimensions requires considerable stiffness. Strings that have previously been wound around a tuning peg can exhibit such curling. That part of a used string has been less stretched than the rest, and so would be the strongest part of it to keep as a spare length to splice onto a string that has just broken in order to get more use out of it. Spare length is needed for the treble string, and the modern gut that is that thin can easily form figure-eight shapes as seen. Both Dowland and Mace made it clear that stiffness was a desirable characteristic when choosing strings.

There is evidence that some Italian-style baroque medium- and high-twist gut was made with a tartar-rock alum pretreatment and a final treatment of soaking in olive oil. This swells the gut somewhat, making it softer and spongier. Some string makers are making such gut now, but they have not yet produced evidence that any other use properties are different from those of gut without such treatment. Some people today are promoting the idea that all early gut was of this spongier type, misleadingly calling it 'more elastic'. Many people, including Mimmo Peruffo, use the word 'elastic' colloquially to mean 'flexible'.

Mace mentioned that a string can stretch an inch or two when mounted and tuned up. It has been claimed that this shows that early gut was much more elastic than modern gut. If one makes a mark on a modern gut lute 1st string, between the first and second fret before tightening, one can observe a similar amount of stretch.

Tuning stability

In modern concert conditions, the tuning of an instrument is not acceptable except before the beginning and during the interval (intermission). Gut-strung lutes have the reputation of not being able to meet these conditions, but gut-strung concert harps seem to be able to do so. One reason for the difference is that the harps use varnished strings, and lute players (who use gut) tend to use unvarnished gut, believing this to be more historically accurate. Oil enhances tuning stability as well as varnish, and Mersenne mentioned that oil was used in making gut strings. But Dowland wrote that oil was used to make old strings appear fresh, so it has been believed that oil should be avoided.

If we assume that Mersenne's oil was a drying oil and Dowland's oil was non-drying, this can explain the evidence that fresh strings looked clear but when old they lost clarity which could be restored by oiling. We have found that linseed oil (a drying oil) is more effective as a barrier to moisture vapour than any other natural material tested (including non-drying oils, waxes and varnishes). Tuning stability needs not to be a problem with gut-strung lutes.

Tensile strength

Aside from cost, the main deterrent to using gut strings on lutes is the short life of gut 1st strings at the normal string stops that lute players use and the pitches they tune these strings to. As shown above, this life is much shorter than on early lutes. It is understandable that lute players have been seeking evidence that early gut was stronger. They have held out hope for this in the use of whole guts (rather than split guts that are often used today)¹⁰ and sinews or tendons (mentioned as an alternative to gut in some early sources)¹¹, but no measurements have been reported to fulfil that hope. The desire for maximum tensile strength for highest strings has been prevalent throughout the history of gut use, and the way of making a widely available stronger variant is unlikely to have been lost or forgotten.

Conclusion

Musicians in the early music movement need to be able to play music that their audience enjoys, which is what being a musician has always been about. They also feel the need to give their audience some semblance of the authenticity it expects. Both of these needs can usually be met if they make sure that what they do appears to be historically possible. The goal of historical scholarship is different. It is to find the most historically probable conclusion, which is the one that objectively best explains all of the evidence. It is not to show beyond reasonable doubt that its conclusion is true (the evidence for this is very often not available). The musicians naturally are unhappy when the scholar's historically-probable differs from their own historically-possible. When there is that difference, they will usually ignore or reject the scholar's conclusion because the case for it is not 'proven conclusively'. They can thus justify continuing to do what they had been doing. One cannot expect musicians to keep to the standards of objectivity demanded in scholarship, but when they claim also to be scholars as well, one should.

As shown above, there is no support in historical scholarship for the claim that there is any property of early gut strings that cannot be met by modern gut. This will not stop many lute players from believing otherwise. The problem of 1st string breakage (at the unrealistic pitch for the string stop usually used) will continue to make gut stringing rather impractical for lutes.

I would like to thank Ian Harwood for his very helpful comments on a draft of this Comm., although the opinions expressed are entirely my own.

¹⁰ D. van Edwards, *The Lute* XXV (1985), pp. 17-28

¹¹ J. Downing Comms. 1441,2

Tinctoris and Depictions of 15th Century Fiddles

In Comm.1633, I mentioned Tinctoris's (1487) discussion of contemporary instruments. It appears to have been an attempt to be comprehensive in terms of instruments presumably surviving from antiquity that were still played. Only included were bowed instruments that could be played one string at a time - purely melodic bowing rather than multiple-string bowing. All of the instruments he included could, he stated, play at least one part in a polyphonic composition. His prejudice of considering melodic bowing to be the only use of bowed instruments worth considering survives to the present day.

The bowed instruments Tinctoris included are two types of fiddle (his term was viola) and a rebec. The fiddle is distinguished from the lute by being smaller and flat, rather than tortoise shaped. The rebec is very small, and is tortoise-shaped, like the lute. The fiddles include the 3-string type tuned in fifths and the 5-string type tuned unevenly in fifths and unisons. The rebec is 'strung for bowing', with no tuning given.

The purpose of this Comm. is to broaden our view of what Tinctoris wrote by looking at 15th century bowed instruments in the pictorial evidence. The first problem is to distinguish between fiddles and rebecs. Tinctoris distinguished only according to size and whether the backs were flat or dome-shaped, and not according to body outline or whether the shape had a clear delineation between the body and neck. In the modern literature, any bowed instrument with a tear-drop body outline is called a rebec. Large ones cannot be rebecs according to Tinctoris's criterion of being very small. I can't imagine how his rebec could be wider than the width of the player's face.

Sculptures show most of the backs of the larger ones as shallow and probably flat enough to identify them as fiddles. The few other larger ones had lute-like backs. With that width, they could only be played with multiple-string bowing, and being of no interest to Tinctoris, need not have any place in his classification. I suggest that they be called bowed lutes. With 5 strings or courses, a tuning in fourths with a third, like the lute, is possible. They may have been equivalents of the Spanish vihuelas of the time. The paintings usually show instruments face-on, giving no information about the back, and so these larger instruments with tear-drop outlines would be identified as either fiddles or bowed lutes, with the fiddle chosen if it had less than 5 strings.

Tinctoris mentioned that the fiddles could play only one string at a time because their bridges were curved. This would only work if the strings were bowed relatively close to the bridge, say, at rather less than a quarter of the way from the bridge to the nut. Farther from the bridge than that, the softness of the strings to the bow pressure would put more than one string in contact with the bow hair. The bow position in only a small minority of the 15th century pictures I've looked at would fulfil this criterion. Possible reasons for this are 1) artists were particularly careless as to where the bow was placed on the string, 2) curved bridge fiddles of the type Tinctoris mentioned were rare, or 3) we note that Tinctoris wrote that 'over the greater part of the world the viola with a bow is used not only in this way, but also in the recitation of epics', and that this second way of using the fiddle, with multiple-string bowing, is what we usually see in the pictures. Since the theory that accepts the evidence most fully is to be preferred, that one is 3). Some fiddles could only play with multiple-string bowing, but others could play either with single-string or multiple-string bowing.

Very many depicted fiddles had four strings. Since Tinctoris did not mention this number, we would suspect that these had flat bridges. I don't remember seeing one with a curved bridge. Occasionally we see them clearly as two pairs, which could be either unison or octave pairs, most probably tuned a fifth apart. If the top two were a unison pair and the others an octave pair, this would be Jerome of Moravia's first tuning without the bourdon. Substitute the bourdon for the lowest, and one gets a unison and two fifths. A violin tuning of three fifths is much less likely than any of these.

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in left hand margin = change of address or other change

- # Giuseppe Alfonso, via Faltugnano 27, I-59028, La Briglia (PO), Italy; *g.alfonso@lycosmail.com*.
Andrew M Atkinson, 147 Newport Road, Leyton, London E10 6PG, UK; 020-8588-5336 (gtar, M,P,C).
- # Göran Arnoldsson, Älvans väg 248, SE - 907 50 Umeå, Sweden.
- # Dmitry Badiarov, Avenue Victor Rousseau 211, B-1190 Brussels, Belgium; *dbadia@attglobal.net*.
Alexander Batov, Flat 2, 18 Devonshire Place, Brighton BN2 1QA, UK; 01273-684781 (lutes all types, gmba all types, bar vln; M).
- Carey Beebe, Factory 35, 17 Lorraine Street, Peakhurst, NSW 2210, Australia; (02) 9534-2002; *fx (02) 9534-2531; Carey_Beebe@clubmac.org.au* (hpschd, pft; M, R).
Bibliotec Berenson, Villa I Tatti, Via di Vincigliata 26, I-50135 Florence, Italy.
- Richard Blake, 3 Melton Street, Melton Constable, Norfolk NR24 2OB, UK; 01263-860110 (bagpp, trav; M,P,C).
- # Roderick Blocksidge (vla, 'ud, med lute, cittns; P, res).
- # Philippe Bolton; *philippe_bolton@compuserve.com*.
- # Wesley Brandt, 1508 SE 43rd Ave, Portland, OR 97215, USA.
Christopher Burgon, 20 Lambert Court, Bushey Grove Road, Watford WD2 2HF, UK; 01923-277128; *Christopher.Burgon@bl.uk* (plckd str).
- Stephen H Cadney, 12 Springdale Gardens, Didsbury, Manchester M20 2GX; 0161-434-0079 (gtar, vih; M, P).
- # John Cousen; 01484-427870.
- # Andrew De Witt, 51A Station Road, New Barnet, Herts EN5 1PR, UK; 020-8364-8090; *KAD_INFO@compuserve.com* (6-hole wstl; M, P).
- # Reg Dines, Santhill House, Rissington Road, Bourton-on-the-Water, Glos GL54 2DZ, UK; 01451-820319; *rwvdines@lineone.net*.
Atelier Marc Ducornet, 24 rue Jean Jacques Rousseau, F-93100 Montreuil-sous-Bois, France; 48.51.93.96; *fx 48.51.66.72; 101447.1765@compuserve.com* (hpschd; M, R).
- # Michael DuPree, PMB 534, 2118 Wilshire Blvd, Santa Monica, CA 90403, USA.
- # Bernard Edwards (after Indonesian add Japanese).
- # Stefan Ehricht,[as before], Große Nikolaistr. 516, D-06108 Halle (Saale), Germany.
Christopher Elmes, (1F2) 25 Haddington Place, Edinburgh EH7 4AF, UK; 0131-557-2397 (med fidl, 'ud, perc; M, P, R).
- Dana Emery, Box 664, East Quogue, NY 11942 USA; (631) 653-5120; *demery@ieaccess.net* (ren winds, lute; P).
- # Anders Emmerfors, Jakob Pers Vag 17, S-23231 Arlov, Sweden.
Richard & Anita Evans, Fairhaven, Lingfoot, Southwaite, Carlisle, Cumbria CA4 0EP, UK; 016974-73799; *fx 0870-052-6933; evansbagpipes@lingfoot.demon.co.uk* (Nhumb & Scot smallpp; M).
- # Michael Fleming; *1/fx 01865-512807*.
- # Michael Foulds (gmba, other bowd & plckd str; M, R).
- # David Freeman; +420-2-2824-2357.
- # Donald S Gill; *dong@easicom.com*
- F Stephen Gottlieb, 9 Collingwood Avenue, London N10 3EH, UK; 020-8444-5367 (lute, gmba, cittn; M).

- Malcolm Greenhalgh, 48 Lordship Park, London N16 5UD, UK; 020-8800-6176; m 0385-572446 (hpschd, fpno, clavchd, orgn; M, R).
- # Dietrich Hakelberg, Wendelingässle 5, D-79111 Freiburg i.Br., Germany; t/fx +49-761-4787887; *hakelber@ruf.uni-freiling.de*
- Craig Hartley, 3 Hillside Cottages, High Street, Fowlmere, Royston, Herts SG8 7SJ, UK; 01763-208615; *vellum@msn.com* (lute; P).
- Roland Henschel, ...[as before], Große Nikolaistr. 516, D-06108 Halle (Saale), Germany.
- Myrna Herzog, R.Hayovel 15/18, Ra'anana 43400, Israel; +972-9-741-8669; fx +972-9-746-0326; *herzom@mail.biu.ac.il* (gmba, vcl, vln, med str; P, res).
- Peter Holmes, 21 Colin Drive, London NW9 6ES, UK; t/fx 020-8205-4529; *drpholmes@compuserve.com* (tpt etc; M, P, coll).
- Peter Holtslag, Munzstraße 4, D-21335 Lüneberg, Germany; +49-4131-390920; fx +49-4131-390930 (recrdr, trav).
- # Istituto Nazionale Tostiano, Palazzo Corvo, I-66026 Ortona, Italy.
- # Roderick Jenkins; *rod-sally@lineone.net*
- Sean Jones, 63 West Street, Tadley, Hants RG26 3SY, UK; 0118-981-4600 (varius wind; M, P).
- # Peter B Koval, 508 Valley View Drive, Vermillion, SD 57069-3539, USA; (605) 624-6594; *pkoval@usd.edu*
- # Daniel Larson, *dan@daniellarson.com*
- Helen Leaf, 87 Mountview Rd, Crouch End, London, N4 4JA, UK; 020-8374-9250; *helenhutileaf@hotmail.com*
- Santiago Muñoz Barbosa, 10 John Parry Court, Hare Walk, Kingsland Road, London N1 6RN, UK; 020-7739-0394 (keybds; M, C, R).
- # Padraig ó Dubhlaoidh, 67 Somers Road, Malvern, Worcs WR14 1JA, UK.
- # David Rachor; *rachor@uni.edu*
- Hugo Reyne, 1 impasse Damesme, F-75013 Paris, France; 01.45.89.68.53; fx 01.45.89.69.84 (trav, recrdr, flag, czkn, whstl, etc; P, C).
- # RILM Abstracts, City University of New York, 365 Fifth Avenue, New York, NY 10016-4309, USA.
- Aiden Scanlon, 62 Oaktree Road, Stillorgan, Blackrock, Co. Dublin, Éire; +353-1-288-0931; *aideaux@hotmail.co.uk* (orgn, regls, hpschd; M, C, R, P, coll).
- # Derek Skinner; *derek.skinner@virgin.net*
- # Frank Tomes; fx 020-8287-9528; *ftomes@wimbledon.ac.uk*
Bar Ilan University Library, 52900 Ramat-Gan, Israel
- # Charlie Wells; *cdwells@kawells.fsnet.co.uk*
- # Kenneth de Witt – now Andrew De Witt, listed under De.