FoMRHI Quarterly

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The next issue, Quarterly III, will appear in February 2009. Please send in Comms and announcements to the address below, to arrive by February 1st.

Fellowship of Makers and Researchers of Historical Instruments


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Readers will be gratified to hear that the initial call for papers (including the call for waifs and strays never published when the Quarterly went into abeyance six years ago) has yielded enough more than enough material for another Quarterly, so much in fact that some material has been held back for next February's issue. In deciding what to print and what to hold back, I have been swayed by the considerations that, (1) at least something by each author who submitted multiple papers should appear, (2) the range of subject matter should be as broad as possible, (3) responses to previous Comms, including those printed a long time ago, should appear straight away.

Nonetheless, concerns remain. I am a bit surprised to have received no responses as yet to Chris Coakley's papers, not least to his exciting and provocative suggestion about the reason for angled bridges on old lutes. Surely someone has a view? Perhaps readers are still digesting this extremely substantial piece of work. Also, Comms submitted seem to be dominated by issues pertaining to stringed instruments: come on, you wind, brass and percussion scholars! Surely you have thoughts to share? The next deadline for Comms will be the 1st February 2009. It only remains to remind readers that the revival of FoMRHI will only succeed if YOU, YES YOU, send in contributions whenever it occurs to you that you have something to say. Comms which pose a question are just as welcome as those which present an answer.

Members' announcements are also welcome – if using a computer, please send these as plain text emails, rather than attachments.

A stall at the Greenwich Exhibition: volunteer and get in free! I have now booked a FoMRHI stall at the Greenwich International Early Music Exhibition, on 14th-16th November. One again it will be held in the Old Royal Naval College, now home to Trinity College of Music. Our stall will be (God Willing) next to the Lute Society stall in the magnificent Painted Hall, so that I can oversee both stalls(!) In return for helping for an hour or so, you can get in free. Just phone or email and say when you would like to help out. The opening hours are: Friday, 10.30-5.30, Saturday 10-6, and Sunday, 10.5-30. Better still could anyone help out with a bit proselytising? We really want to go round and give a complimentary copy of a recent Quarterly and call for papers to all the instrument makers present. Let me know if you can help.

Where are they now? Over six years our address database has got a bit out of date. Does anyone know the whereabouts of Jonathan Little, or Anthony Elmsly? They paid their subscriptions along with everyone else, and are entitled to receive FoMRHI Quarterly correspondence has been returned by the Post Office. Many thanks to all of you who have given information so far, including news of those who have gone to mix their music with that of the angels.

Email addresses, please! If you haven't received any emails from us this year, that means we don't have your email address. It makes communication so much easier if we have it. We promise not to send out any spam, or pass it on to anyone else. Please send a brief message to Lutesoc@aol.com, and we can add you to our list.

STANDING CALL FOR PAPERS

The Fellowship of Makers and Researchers of Historical Instruments welcomes papers on all aspects of the history and making of historical musical instruments. Communications or 'Comms' as they are called, appeared unedited (please don't be libellous or insulting to other contributors!), so please send them EXACTLY as you wish them to appear – in 12 point type,
on A4 paper with a 25mm or 1 inch border all round, or to put it another way, if you are using non-European paper sizes, then the text area must be 160 x 246 mm (or at least no wider or longer than this). Our printers usually make a reasonably good job of scanning photos.

You can send contributions EITHER on paper, OR as a Word-compatible or PDF attachment. If you really do not have access to a word processor of any kind, we may be able to retype typed or handwritten submissions.

NOTE OUR NEW ADDRESS:

FoMRHI
 c/o Chris Goodwin
 Southside Cottage
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and the email address for Comms sent as attachments (and other email correspondence) is Lutesoc@aol.com

Non-members will be given a year's free subscription if they send in a Communication to the Quarterly.

If you ever sent in a paper (in the last 6 years) for the Quarterly, and it never appeared, please re-send it, to the new address.

There are plans to scan back issues of the Quarterly and make them downloadable from a website, to be set up; in the meantime you can obtain back issues for the princely sum of £3 per issue, including postage; send a cheque payable to FoMRHI, at the above address, or write with your credit card details.

If your interests have changed, and you don't now want to be a member of FoMRHI, please let us know, to save postage costs.

MEMBERS' ANNOUNCEMENTS

Classical handmade guitars and one steel string. At cost price. Spruce fronts, indian rosewood backs and sides, ebony fingerboards, oiled finish. Made with great enjoyment by me. Call Mike, on 01702 556892 (Southend area).

Jan Bouterse's dissertation about Dutch woodwind instruments and their makers, 1660-1760, has been translated and published in English language at: http://home.hetnet.nl/~mcjbouterse/inhoud&samenvatting.htm; also http://www.kvnm.nl/current/03Catalogus/BN 9.htm, with English summary. He is on the point of finishing a (thick) manual on making woodwind instruments, incorporating his experience gained from research into historical instruments, though this is currently in Dutch only.

Contact: Jan Bouterse, Sandenburg 69, 2402 RJ Alphen a/d Rijn, Netherlands tel. + 31 172 445957 e-mail: mcjbouterse@hetnet.nl
NEW DATES: after communication of conflicting dates for an important meeting organised by the Galpin Society, AMIS, CIMCIM and the Historic Brass Society:

16 – 19 September 2009

CALL FOR PAPERS

The planning committee is now accepting proposals for papers with a preference for topics on:

- HAYDN and the Clavichord
- From Clavichord to Fortepiano

History, inventory and characteristics of known extant instruments, iconography, social and musical role, present state of instrument building

Concerts will be held in the evening and shorter recitals will complement the morning lectures.

A display of instruments, original or copies, will be held in the Chiesa di Santa Marta.

Proposals should be addressed to the ICCS Committee no later than 15 January 2009.

Proposals for performances should include a programme of thirty minutes of music.

Proposals for the exhibition of instruments should include all pertinent information on the copy or original to be displayed.
On frets and barring; some useful ideas

The purpose of this article is to take a closer look at two problems, that of frets wearing out, and that of internal barring becoming loose. As a theorist rather than a luthier, I cannot say whether the latter is still problematic at the present day, but it certainly was in historical times, and even in the early days of the current modern renascence of the lute. In copying historical models, some lute makers apparently failed to allow for the fact that wood thins out over time, resulting in splits from tension or barring becoming loose. Solutions to both problems have been offered, the former by Stanley Buetens "On Fretting a Lute" (JLSA III, 1970, pp. 53-63), and the latter by Kurt Rottman "Historical Lute Bellies from the Standpoint of Statics and Acoustics" (GSJ XXVI, 1973 pp. 25-28).

FRETS

Gut frets were used on lutes and all other gut strung fretted instruments because, if a harder material were used, the strings would wear out too quickly. By the same token, metal frets were used on wire strung instruments because metal strings would wear through gut all too quickly. This may seem obvious to some, but others may not have realised it. When nylon stringing is used (as it is by those who are more concerned with durability and reliability than with historical accuracy), the frets must also be nylon as, again, gut would wear out too quickly. With tied frets, some makers and players would say that they were used in order to have the option to move the frets about in order to play in meantone temperament whereas, with fixed frets, only equal temperament is possible. The moving of frets to obtain tonal accuracy in various keys is, I believe, both a restriction and a largely unnecessary nuisance. I seriously doubt whether lutenists ever did, or ever do, much of this. Can we have some readership feedback on this? Nevertheless, I believe it much more sensible to use fixed metal frets for nylon strings, and there
and there is both practical and historical evidence that refer the reader to the article by S. Buetens, noted above where, on p. 53, he says, "However, many modern lutenists already employ inlaid metal frets, such as those used on the classical guitar. As far as I know they have caused no complaints from players or listeners. I have several lutes, some of which have fixed frets and some of which have tied-on frets. I can testify to the fact that from a player's point of view, it does not make much difference. If the metal frets are correctly set in by an expert, they will last almost forever and are always in tune." Further, on p. 54, "As early as 1555, Juan Bermudo states, "I am persuaded that if the frets were of metal or ivory, they would cause better music. The dampness of the fret, especially in humid weather, causes great imperfection in music...". As far as I know, Juan Bermudo did not actually use metal frets for gut strung instruments but, if nylon had been available, then he most certainly would have done so. Then, on p. 55, S. Buetens further states, "I think, after many years of experience and experimentation, that expertly set in metal frets are probably the best for general reliability." As for the problem of buzzing, which probably comes to mind, on p. 57, he says, "Fixed frets are usually the same thickness throughout, but are normally adjusted to eliminate buzzing." INTERNAL BARRING

The problem of internal barring becoming loose has been both researched, and largely solved, by Kurt Rottman (in the article referred to above). The reason WHY internal barring became loose was that Mace and earlier makers, "Probably to increase the volume they weakened the ends of the bars ... facilitating the vibration but producing fatal consequences for the resistance of the belly and especially the first bar (above the bridge). We learn well enough from Mace ... and can observe today in similar instruments, how the two thin ends of the first bar
after some time come loose from the belly, which then alone has to support the bending stresses in these parts, and the belly splits above the bar. Thus Mace understandably recommends repair to the belly annually or at least every two years.”. Whether these problems are still current I do not know; perhaps some readership feedback may shed light on the matter.

The answer to this dilemma, if it still exists, may be (i) to start thinking in terms of slightly thicker soundboards, perhaps 2.0 mm instead of 1.2 - 1.8 mm (some surviving lutes have a soundboard of 2.4 - 2.5 mm), and/or adopt the barring pattern used by Sebastian Schelle, given with this article. For this, I refer the reader to Kurt Rottman’s article where, on pp. 27-8, he says, “It is astonishing that the first bar (above the bridge) was generally not the strongest, but the two bars at the upper and lower end of the rose were so. The strength of these last is justified because they had to protect the weak part of the rose; but it is questionable if many weak bars under the rose, already mentioned, are the best solution for such protection. Also the manner in earlier times of strutting the space under the bridge and towards the bottom edge does not show much comprehension of the real force effects. The solution in later times, with the system of radial bars, was more adequate, better protecting the most stressed part of the belly. As long as the lute had only eleven strings the old system was statically useful, but when the number of strings increased, and with it the total string tension, control of the forces became more and more difficult.”

David van Edwards tells me that he makes his soundboards thicker than most modern makers and comments that whilst the system of radial bars used by Sebastian Schelle would strengthen the soundboard at its point of greatest stress, this would strengthen the bass tone at the expense of some treble, ideal perhaps for a chitarrone, but not for a higher pitched instrument. It certainly seems to me that, at the very least, thicker soundboards are the way to go.

(1) Additional Bibliography:

FIG. 6 Belly of a chitarrone by Sebastian Schelle, Nuremberg 1728.
(Author's property.)
Modification of recorder blocks to improve sound production

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Abstract

For the musical instruments known as recorders, those notes rising from, and including the sixth note of the second octave, present problems for all but the most expert of players. To correct this and associated problems, a modification is presented that consists of providing a recess within the sound-producing area of the instrument. Such recess may be formed by removing material from the block face in one of several ways. It can be applied to all recorders in use and to those in manufacture. The modification allows for top notes to speak clearly. All notes speak with less effort and more strength; have greater dynamic range, and a faster response. It also reduces the amount of moisture which collects in the windway.

I. INTRODUCTION.

Recorders belong to the class of wind instruments known as "fipple flutes." The fipple is a plug or block, partially stopping the top end of a pipe. Figure 1 shows the common names of parts and features of the recorder, its structure, and function, relevant to this discussion.

In essence, air is blown into the mouthpiece 1, passes through the windway 2, and is directed towards a sharp edge, the lip 3, which causes the column of air in the bore 4, to vibrate. The vibrating column of air also interacts with the jet of air from the windway, producing a secondary vibration. When these two vibrations are in phase, a sound is produced.

From the sixth note of the second octave the top notes of recorders' present problems for all but the most expert of players using first-class instruments; a phenomenon which has been observed by a multitude of recorder players. Commonly, those notes which prove difficult to play are frequently unstable when they do sound, and may not even sound at all. Another problem for recorder players is that the pitch of a note is affected by the force with which it is blown, so that the dynamic range of the instrument is severely restricted. The purpose of this paper is to describe a way to modify recorders so that the first difficulty is overcome and the second is considerably reduced.

The account is likely to be of interest not only to recorder players and recorder makers, but also physicists with an interest in the acoustics of wind instruments. Makers and repairers also, if they are convinced by this account, will want to know
exactly what to do to achieve these results. Physicists may be interested to give a far more sophisticated explanation than outlined here, of the science underlying the apparent success of the technique. The writer is cognizant of the recorder’s functions, the relationship with bore shapes and dimensions, placement of finger holes etc., thus discussions in these areas has been omitted, emphasis being on the modification only.

II. OBSERVATIONS.

Recorder makers and others have made various attempts to overcome the problem of the high notes, but none has been entirely successful. For example, reducing the height of the windway 2, and lowering the lip 3, helps the high notes and slightly improves the recorder’s dynamic capabilities, but it also weakens the low notes and is associated with a greater build up of moisture, which obstructs the flow of air at the windway exit 5. In the past, the author too has tried a variety of other means to overcome these problems, but again, none proved entirely successful.

When a recorder is being played, an astute listener will hear that the high notes are not clear. This may be likened to the sound made by an untrained singer attempting to sing high notes. The notes are there, but incomplete in their quality, unlike a trained singer whose notes are full, effortless and have no suggestion of being strained.

Students who engage in singing lessons learn that to produce full, rich sounds, it is necessary to open the throat in a manner similar to what happens when one is yawning. This analogy was considered when examining the sound-producing area of the recorder.

In the 17th century, experiments were carried out on transverse flutes and it was established that the placing of a space to the left of the embouchure hole i.e. within the head joint, made a difference, broadly speaking, to the flexibility of the tone. Benade.2

In considering Benade’s discussion, his results, and the generally accepted observation made by players that, when compared with large recorders, there is an ease in playing the top notes of small recorders; during trials, an interesting relationship was observed between the space from the face of the block to the lip, and the frequencies of the high notes.

On examination of those high notes which presented difficulties in the instruments tested, all appeared to occur from the sixth to the eighth notes of the second octave in all sizes of recorder - a vital point when considering the modification. And the few notes of the third octave were less of a problem.

When an attempt is made at playing the high notes in question, the standing wave has little freedom or room to extend towards the head of the recorder, and so breaks or collapses at or near the lip 4. The writer believes this is due to disturbances caused by breath pressure against this standing wave. It seemed that if more free space,
(volume) could be provided in this area, it should give the standing wave room to shape, remove eddies or kinks, and thus provide clearer high and, in all probability, low notes. Therefore the necessary harmonics within these notes would have the potential to develop without being forced.

It appeared then, in order to produce an unrestrained sound (like that of the singer); it ought to be only a matter of providing this space towards and into the block.

III. EXPERIMENTS.

After many years and countless experiments by the author, a modification that does produce the desired result, and which has other advantages, became evident. It necessitated providing a precisely located recess 6, in the block face 7.

It should be noted that all experiments were carried out in conditions suited to normal playing. That is comfortable temperature and room conditions, and after sufficient time had lapsed for the instruments to reach body temperature.

While experimenting, the author carried out tests on several different timbers and shapes of wind-ways and blocks 8, and results indicated that there are “ideal” materials, shapes and forms that blocks and wind-ways must have for any recorder to function properly. However, for the purposes of voicing, ideal will vary between makers.

Employing this ideal situation, the required space was calculated by ascertaining the wavelength produced by the top notes, window size etc., and placed at the face of the block 6.

Depending on the size of the recorder block 8, there were at least three ways in which the desired space could be provided: Fig. 2 (a), (b), and (c). The first was to cut from top to bottom at an angle or curve removing the material, the second was to drill a parallel-sided hole, and the third and most satisfactory for all recorders, was to form an elliptical or conical recess.

All these methods functioned satisfactorily, and though the elliptical recess gave marginally better results than the conical one, the latter became the accepted modification as block removal was straightforward regardless of the size of recorder.

By applying the factors affecting the calculations, namely, frequency of note(s), speed of sound, size of the window 9, and height windway at the exit 5, a guide to the required depths was given by the empirical equation:-

\[ R = \frac{dc}{4fw} \]

Where \( R \) = required depth of recess, \( d \) = windway exit height, \( c \) = speed of sound in mm/sec, \( f \) = frequency of the note, and \( w \) = window opening, block face to lip.

Considering an alto recorder pitched at \( A_4 = 440\text{Hz} \), the most troublesome notes are the topmost of the second octave and commence at \( D_6 = 1176\text{Hz} \); \( F_6 = 1396 \text{was} \)}
not chosen as the starting point, for it is evident that as the frequency \( f \) is increased, the required distance becomes less and is thus already accommodated.

IV. RESULTS.

For the alto recorder used during testing, the depth of recess required was 14 - 15mm. It was found in trials, that if the recess became too deep, the tuning of some of the lower notes was adversely affected. The rate of taper used for this preferred conical recess was 1 in 9, leaving a material thickness of 0.7mm at the block face. Some recorders have a step in the bore at this point; therefore the block wall thickness at its face requires special attention. Vertical placement of the recess should be such that sufficient material is left so the chamfer 10 is substantial and is therefore not damaged.

It is common to find that window distances do not increase in proportion to the increase in size of recorder. For example, some tenor recorders were found to have smaller windows than altos; also, there were variations in window sizes of up to 8% in the same size of the recorders tested, and results from the equation accommodate this tolerance. After several trials, it became reasonable to accept that the depth of recess would range from 11mm for a sopranino, increasing to 30mm or so, for a contra bass. Given that all well-made recorders require “fine tuning” by hand, the final depth of each recess will need similar attention to produce an acceptable result.

V. CONCLUSIONS.

The author has carried out this modification on numerous recorders of all sizes, and found, that without exception, it allowed the top notes to speak clearly and all notes played with less effort. There was more strength to the notes, greater dynamic range, and a faster response. Additionally there was not the same propensity for moisture to collect around the exit. This last observation leads to the possible conclusion that there is now much less turbulence around the window which would otherwise prevent the escape of moisture.

With the modification in place, it allows for the jet stream from the windway 2, to be centered on the lip 3, thus increasing the intensity of the low register, and, at the same time, allowing the top notes to speak clearly.

Earlier in this article, it was noted that in the smaller instruments, the higher notes sounded more readily. This is to be expected because there is not as much space required to produce the high notes clearly in these sizes, and sufficient space already exists in them. Nevertheless, improvements were present in all sizes of recorders once the modification was applied. In the larger recorders, before applying the modification, there is quite some difficulty in making top notes speak well. This is understandable since the relative lower frequencies of the top (second octave) notes of the larger instruments require more space. It was also noted that, after the modification had been
applied to an existing recorder, the pitch of that instrument became slightly lower, thus a tuning adjustment was necessary to return the instrument to its original pitch.

There is little doubt that the best improvements are made on good-quality recorders, and it comes as no surprise to find that it is possible to improve even the poorest of instruments be they made from wood or composite materials.

The shape and materials used in making the block are significant, and what might be considered ideal by the writer may not be seen the same way by others, thus my opinion regarding these variables, has been omitted. It is suggest however, so that the numerous benefits of this modification are fully realized, it will be necessary also to modify the wind-ways of some brands of recorders.

In order to present the results pictorially, the writer positioned two microphones 300mm away from the instrument (in this case an alto recorder), fed the sound into a computer and recorded the sound of the note produced. These results were suitably labeled then cut and pasted to form part of this paper.

For figures 3 & 4, the note F₆ was played, first with a standard block then with a modified block replacing the original. Under the heading 'Observations' it was noted that to an astute listener the top notes are not clear. When one compares figures 3 & 4 of the accompanying spectra, this lack of clarity becomes evident.

Figure 5 was produced in the same manner as before; this time an arpeggio in G major was played. Here the unmodified recorder shows clearly the very high frequencies caused by the sound of the strong jet of air passing over the lip; amounting to nothing more than hissing. After the recorder blocks were exchanged, the figure shows that those high frequencies have been removed.

Figure 6 is similar to figure 5 but here the difference shows a change in the amplitude of the notes for the same arpeggio.

It is important to note that in order to preserve the history of the recorder, rather than modify the original block; (which could be seen as vandalism of an historical instrument), it may be more prudent to remove it for preservation, and replace it with a new modified one.

Having said that, since first “discovered,” the recorder has suffered many changes before arriving at its present state. So, it is hoped the modification as described, will be accepted as another one of those changes necessary to improve the function and status of the recorder.
VI. ACKNOWLEDGEMENTS.
The writer wishes to acknowledge the assistance provided by Dr. Gordon Troup, (Hon. Physicist Monash University, Melbourne) for the reviewing of this article and carrying out tests; for his helpful discussions about data presentation, suggestions regarding the production of the spectra, and his general interpretation.
Also to Dr. (Technology) Bjorn G. Svardson. KTH (Royal Institute of Technology, Stockholm, Sweden) now residing in Melbourne, for his review, supportive comments and suggestions.
And to those recorder players who have provided testimonials supporting the benefits the modification has made to their instruments.

1 Although this paper focuses on the so-called “Baroque” form of recorder, with its inverted conical bore, most of what is said applies equally to the “Renaissance” recorder with its parallel bore. For this latter instrument, however, the range is less than that of the Baroque, and the problem is more severe.


Modification to block

Figure 1

Blocks with various methods of modification

Figure 2
Figure 3
Figure 4.
SPECTRUM ANALYSIS – ARPEGGIO IN G (G₂, B₂, D₃, G₅, B₅)

UNMODIFIED RECORDER

MODIFIED RECORDER

Figure 5.
Figure 6

UNMODIFIED RECORDER

MODIFIED RECORDER
What is wrong with Vermeer's guitar?

An analysis of the guitar shown in Vermeer's 'Young Woman with Guitar' of c. 1672, Iveagh Bequest, Kenwood, London.

(This paper is developed from part of an unpublished talk on the uses of iconography in musical instrument making given to the Association of Art Historians at the Tate Gallery, London, in 1993).

Many of Vermeer's paintings contain musical instruments, particularly keyboards and two different citterns. These seem accurately observed and depicted, probably aided by the use of a camera obscura. However the guitar of the 'Young Woman with Guitar' has several features which do not occur in other paintings of the period nor on extant instruments. The picture has been x-rayed confirming that it has not been altered or repainted. A copy, almost identical except for the hairstyle of the young woman, is in the Philadelphia Museum of Art. A list of anomalies follows:

1. 17th c. guitar roses were constructed from several layers of parchment, usually gilded, as seen for example in paintings by Sir Peter Lely: 'Two paintings of the Lake family' c. 1660, Tate Britain, and Gonzales Coques: 'A Gentleman with his Daughters', Wallace Collection, etc., and in extant instruments (photos 2 and 3, after Alexandre and Jean Voboam). Alternatively a single layer of wood, sometimes laminated with, and backed by parchment, usually (always?) not gilded was used. Extant instruments include guitars by Stradivari, Ashmolean, Oxford; Sellas, Edinburgh; Choco, Paris (photo 7). Paintings by Caspar Netscher: 'Lady playing the Guitar' 1669, Wallace Collection, etc. Both types always used a radial pattern.

Harpsichord roses north of the Alps were figurative, often an angel with harp and incorporating the maker's initials (photos 4). They were cast from lead or similar metal and gilded.

The rose in the Kenwood guitar appears to be that of a harpsichord, although the voids in the design are not shown. Several of the painted shapes included echo those in the photos 4, and 5. There is one extant 17th c. guitar with a harpsichord rose, in the Brussels museum (photo 5). This is by Matteo Sellas, converted during the 18th c. to a chitarra battente, by shortening the neck and fitting wire strings etc. The rose, probably added more recently to enhance the guitar's commercial value, is that of Henri Hemsch, an 18th c. harpsichord maker.

A modern reproduction of either type of guitar rose weighs 5 - 7 grams. A metal harpsichord rose weighs 200 - 250 grams. As the total weight of a 17th c. guitar is usually rather less than 1000 grams, the adverse acoustical effect of fitting a harpsichord rose can be imagined, making it highly unlikely that this would have been done to a working instrument during the 17th c.

2. On those extant guitars which use a diagonal black and white pattern for the binding, the orientation of the pattern is governed by the junction between neck and body. To make good carpentry this angle needs to be bisected (photo 6, Alexandre Voboam, Paris). In the painting
the pattern is wrongly orientated. Additionally the change of direction which should occur centrally at the bottom of the guitar, here appears to be behind the player’s right wrist. The very acute angle at which the painted inlay meets the edge of the guitar would cause easily broken points to catch on clothing, etc.

These two features of the painting are very obvious. Those following are less so, but add corroborating evidence.

3. At the neck/body joint ‘points’ or ‘stings’ are shown. These occasionally occur, especially on Italian guitars (photo 7), but I cannot recall seeing them in association with the apparently French binding depicted. With the patterned binding shown they would make both awkward design and woodwork. They are usual on lutes.

4. Very few 17th c. guitars remain in their original condition. To understand the alterations which have occurred it is helpful to also look at depictions. 17th c. guitars had smaller bodies but generally a longer string length than modern. Most alterations are one of two kinds. Firstly, during the 18th c. when some were converted from gut to wire strings. This involved shortening the string length by 10 - 12 cms, and often angling that part of the belly below the bridge, as on a mandolin. Secondly, during the 19th c. when the original string length was reduced by around 5 cms., and the original five pairs replaced by six single strings. There are of course also re-conversions, repairs, etc.

It is difficult to decide whether the guitar is parallel to the picture plane or at an angle. If at an angle, as implied by the partial view of the bottom of the guitar and also by the player’s right hand seeming larger than her left, then the neck of the guitar tapers in the wrong direction.

If parallel to the picture plane, as implied by the shape of the guitar body and the rose outline, and by the apparent orientation of the peg-head, then the neck is short - implying one of the two alterations described above.

5. The peg-head has ten dark spots representing the ends of the 17th c. pegs for five pairs of strings - or perhaps their holes. There are also six light-coloured spots. These could represent decoration, but are also reminiscent of the peg-ends required for the six single strings of a 19th c. conversion.

Considering the amount of attention that Vermeer’s works have received, it is unlikely that the painting is a fake despite Van Meegeren, his most famous forger, also working in Brussels. Could he have seen the Sellas/Hemsch guitar? It seems more likely that the painting was left unfinished and completed without the guitar being available as a model. This could have been by the painter of the Philadelphia copy. (It has been suggested that the different hairstyle dates from around twenty years after the Kenwood painting). Or more recently, or by Vermeer himself - he had keyboard roses in his house to copy, and could have incorrectly remembered the binding pattern.

I have had several requests for copies of this guitar, as I expect have other makers - Don’t!
A New Addition to the Instruments of the Mary Rose

Most of you will have seen various articles about the musical instruments found when Henry VIII’s warship the Mary Rose was raised from the seabed of the Solent twenty-six years ago. Frances Palmer wrote the initial one in *Early Music* 11:1 (1983). This was followed by one from Herbert Myers (*EM* 11:2, 1983) identifying the shawm as a doucaine, dulcina, or still shawm, as described by Tinctoris (Antony Baines, *GS* 3, 1950). Charles Foster produced a preliminary copy of it in the early 1980s that worked well.

The problem was that the Mary Rose people went quiet about the instruments for a number of years and it wasn’t until ten years ago that a group of us, Charles, Mary Anne Alburger, and I, were asked to go down and have a real look at the instruments. I reported on that visit in *Comm.* 1597 in Q 93, October, 1998. We knew that one of the two long tabor pipes had a maker’s name on it, E. Legros, and that the shorter and more finely made of the three pipes had a splodge that might be a mark. One of our major discoveries was that when I looked at that splodge through a magnifying class, it was clearly the double plume mark (!!) that may or may not be associated with the Bassano family of makers who were working at Henry VIII’s court at that time. Inside one of the pipes was a stick, too short to be a cleaning stick, or a support stick such as one finds with Balkan kavals. Both Charles and I are sure that it is a tabor beater, and though much shorter and thinner than the beaters ones sees in the pictures, the copy of it that Charles made for me works well. Charles has made copies of all the woodwind, the three tabor pipes and the shawm, and Mary Anne has had copies made of the two fiddles. The tabor is too fragmentary to be worth copying.

Since then, we have all three been talking about the instruments on various occasions, and there was one rather daft BBC broadcast about them, with the ‘presenter’, Lucy Skeaping, asking the usual silly questions typical of such BBC productions.

It was, if I remember rightly, on that occasion that Mary Anne’s maker was taken into the store and came out with a shaped piece of wood, simply cut to shape from a plank rather than bent, that was clearly a bow for one of the two fiddles.

We were all three asked to write the relevant section of the catalogue of finds, published as *Before the Mast: Life and Death aboard the Mary Rose*, edited by Julie Gardiner, published in 2005 by the Mary Rose Trust in Portsmouth. This is a huge, and quite expensive tome, but in it are photographs of all the finds, as well as the original drawings made by the archaeologists, some of which Frances Palmer had published 22 years earlier.

Last year was the 25th Anniversary of the raising of the ship, and because Charles was unwell, I was asked to go down and talk about the instruments at the celebratory conference. While I was there I was shown a very small photograph of a stick and asked if it might be another bow. There was no way of telling from the photo, so next morning we went into the store and fished it out.

Undoubtedly it is a bow. This time it is a straight stick with what seems to be an integral horn-shape frog, though because there is twine around the stick immediately above and below the frog, and a hollow beneath it, it does look to me as though it must be a separate piece of wood attached to the stick. The top of the frog is grooved with a channel to carry the hair, and at the back of the frog there is a hole drilled through it, right through the stick, with what looks like the remains of a peg in the hole to retain the hair.

The point end of the stick is broken off so there is no way to tell how long it was nor how the hair was attached at that end. Nor can we be sure that the stick was always straight, for 450 plus years under the sea doesn’t do any bit of wood much good.
Oud or Lute? – a Study

This early print seems to be a rather quaint depiction of a five course fretted oud. However, a more detailed examination indicates that the image may be a more precise geometrical representation than at first appears.

The print may have been made in two parts - a metal plate engraving (intaglio print) of the outline of the oud overlaid with a wood-block print of the Arabic script including, possibly, the peg-box and bridge.

The title script confirms that the engraving represents an oud (1), but from what period of history and how might it relate to the European lute?

The original source of the print is uncertain. It has been attributed to the early Arabic theoretical music treatise, the Kitab al-Adwar by Safi al-din Al Urmawi (1216-1294) and is supposed to be from a manuscript copy dated c1334 (2). A number of copies of the manuscript survive yet identification of the specific manuscript copy and its location remains unconfirmed.

Intaglio printing was first practiced in Europe around the middle of the 15th C. However, as many technologies in Europe came from the Arabs, this print may have been made at an earlier date in the Middle East, North Africa or Moorish ‘Spain’ perhaps.

The oud has two small sound-holes – unlike modern ouds that have either a single sound-hole (like a lute) or one large sound-hole together with two small sound-holes underneath. The star design of the rosettes may have some significance, suggesting either that the luthier may have
been Jewish (Moroccan?) or that the print plate may have been cut by a Jewish goldsmith skilled in engraving metal.

The pegbox is sickle shaped – unlike the ‘S’ shaped pegboxes found on modern ouds. The representation of the pegbox, although crude, confirms that the oud represented is intended to have five double courses.

The engraved profile of the oud is, detailed and, apparently, well proportioned – going so far as to include the sound-board edge banding - although the engraver made a bit of a mess of cutting the profile of the bottom left hand quadrant of the sound board. The engraver also left faint scribed markings, visible at the bottom of the oud profile, which may be layout marks.

**Geometrical Analysis**

The geometrical construction, determined by trial and error using dividers, is shown in Fig. 1. 

A line drawn through the rosette centers intersects the central axis AL of the soundboard at G. An arc of radius R1 with its center at G describes the lower profile of the soundboard. An arc of radius R2 describes the upper profile.

This construction is identical to that of a lute design given by Henri Arnault de Zwolle in the mid 15th C. (3)

The distance of the front edge of the bridge from the bottom edge of the soundboard JL is 1/6 of the overall length of the body BL.

The faint scribed layout lines, referred to above, appear to touch the line representing soundboard banding at point K. These lines are on an arc, described by radius R3 with center at F, which intersects the central axis at C. Here, point C is taken to define the inner edge of the neck block and K the inner edge of an end plate or thin bottom block (4). This being the case, the scribed layout lines define the free vibrating length of the oud soundboard CK.

**Soundboard Barring**

A line, perpendicular to the central axis and drawn through point F just clears the upper edges of the rosettes. If this can be assumed to be a bar position, another bar may be placed at equal distance below the rosette centers at H. (Modern ouds always have bars on either side of the small sound holes – never through the centers)

To speculate further, if the oud geometry was to include a large sound hole with a diameter 1/3 of the sound board width (as given by Arnault de Zwolle), the center E of this imaginary sound hole, which touches the bar at F, may be readily established, by trial and error, using dividers. This is not to suggest that the oud should have a large sound hole but this assumption makes it possible to estimate the theoretical location of additional bars. With the sound hole diameter and position determined, another bar may then be located at D as well as on the sound hole center E. An arc of radius R4, with center at D, intersects the central axis at B - defining and confirming the neck joint position.

**Mersenne**

Marin Mersenne writing in the early 17th C (5), gives a basic barring layout for a lute of his time. He instructs that the soundboard be divided into eight equal parts, with bars glued upon parts 2, 3, 4, 5, 6 and 7 - the neck beginning at part 8. The center of the rose is on the middle of the fifth part. The bridge is located by dividing the first two parts into three - and is found upon the second part.
Fig 1 includes the barring layout given by Mersenne, assuming that the division of the soundboard is between K and C – the free vibrating length. There is close correlation except for the single bar at H, midway between the two bars at parts 2 and 3 given by Mersenne. This difference may be explained by the more elongated body length of Mersenne’s lute – requiring an additional bar for support for the soundboard.

Arnault de Zwolle
The Arnault lute has a single sound hole, a very long neck (of unknown dimensions), and a massive bottom block. In addition, the method given for the geometrical layout differs from that of the oud in this study. The Arnault lute has three bars in practically identical relative locations – i.e. at points, D, G and H in Fig 1. Furthermore, the relative locations of the neck joint, inner face of the neck block, center of the sound hole, and front edge of the bridge closely correlates. The geometry of the soundboard profiles of the oud and lute are identical.

Other Observations
The length of the neck of the oud (including the neck block) AC is 1/3 of the overall length AL. The neck length is more oud than lute like. The fingerboard length (measured from the front edge of the nut to the neck joint) on a modern oud is generally 1/3 of the overall string length. Here the fingerboard length is a little too long but if the width of the fingerboard at the neck joint had been narrower – as it would be on an oud of today – that proportion might apply. Modern ouds usually have a bar below the bridge, located about midway between the bottom of the soundboard and the front edge of the bridge.

The Frets
The scale over the fingerboard of the adjacent image represent, approximately, the fret positions, as they would be on a European lute of the 16/17th C. With this scale, there is just sufficient space on the fingerboard for seven frets (6). Surviving early Arabic and Persian theoretical works document the musical scale systems applicable to the oud and other musical instruments of their era. In his paper “The Lute Scale of Avicenna” (7), Dr H.G. Farmer refers the Kitab al-Shifa of Ibn Sina (died in 1037) where instructions are given for the placement of oud frets. The frets known as the first finger fret, the Old Persian second finger fret, third finger fret and fourth finger fret coincide with frets 2, 3, 4 and 5 on the lute. The fourth (or little finger) fret is the lowest on the fingerboard placed at 1/4 of the string length from the nut. Additional frets were also placed between these basic four fret positions – although oud players did not always use all of the fret positions dictated by theory. The lowest fret in the engraving, which coincides approximately with fret 5 on the lute scale, might represent the fourth finger fret. As there are no corresponding matches with any of the other frets, it would seem that the frets in the engraving are not meant to represent the exact
relative positions of the frets – perhaps because instructions on precise fret placement appear elsewhere in the manuscript. The engraving, therefore, may give a general idea of how the fingerboard would have looked – partially fretted as far as the fourth finger fret. No doubt, expert oudists would have played in higher (unfretted) positions – as was the case with lute players.

Note 1
The upper part of the script has been cut off in this image but it may read as ‘dafr al oud’, literally ‘the oud intertwined’ referring to either how the strings are tied on an oud or how the frets are tied (or both).

Note 2
One researcher contacted failed to find the engraving in his copy of the manuscript (acknowledging, however, that the copy may be incomplete). Also, Dr H.G. Farmer – who set out to prove that the early oud was fretted, and who had access to several of the surviving al-Adwar manuscripts as primary sources - makes no mention of the engraving. This is odd, because Farmer notes, in his paper “Was the Arabian and Persian Lute Fretted?” (Studies in Oriental Musical Instruments) that “hundreds of pictures of the oud during the second period (13th to 20th C) do not reveal the slightest trace of frets ...”

Note 3

Note 4
Modern ouds usually have a shallow end block – however, some older ouds have thin – lute like – end plates. This layout suggests that the oud has a thin endplate, the inner edge of the banding defining the inner edge of the end plate. Banding on a modern oud is about 5mm wide.

Note 5
Marin Mersenne, Harmonie Universelle, Paris, 1635.

Note 6
Fret locations calculated according to ‘the rule of 18’, a practical approximation for equal temperament tuning (Vincenzo Galilei, ‘Dialogo dell musica antica e moderna’, 1581)

Note 7
“Studies in Oriental Musical Instruments”, Dr. H.G. Farmer, Glasgow, 1939
Some Parallels in the Ancestry of the Viol and Violin

The viol descended from the 15th century Spanish vihuela. It was both plucked and bowed, and though it ended up as a mostly melodic bowed instrument, the viol never completely abandoned its capability of being plucked.

The vihuela had 9 strings in 5 courses, the greatest number of strings that a viol ever had. As the viol developed, it first had 6 strings in 3 courses, and then, as it became more melodic, it had 5 or 6 single strings. The final number, 6, is 3 less than the historical maximum.

The bridge of the vihuela was low and flat, usually glued to the belly like a lute bridge. It was located very low on the soundboard. As it developed as a bowed instrument, the first step was to increase the bridge height so that bowing at an angle away from across all the strings (to play without the lower strings) could be freed from the position of the waist cutouts (about a quarter of the bridge-nut distance from the bridge). The next step was to raise it further, moving it closer to the cutouts (for a more incisive sound when bowed closer to the bridge) and to curve the top for either of two playing styles – shallower for the lira one where one could choose any adjacent 3 or 4 strings to play together, or rounder for the vocal one for playing only 1 or 2 strings at a time.

The violin apparently descended from the fidicula (see another Comm in this Q), a Roman type of lyre. It was plucked, and when bowing was invented at the end of the first millennium, as a fiddle, it was usually bowed. Plucking was never completely abandoned.

The fidicula originally had 7 or more strings. As a fiddle, it never had more than 7 strings. In medieval times it was usually 3, 4 or 5. Renaissance fiddles had 3 or 4. The violin developed from a French 4-string one. That number, 4, is 3 less than the fiddle maximum.

The bridge of the fidicula and subsequent plucked fiddles was low and flat. When it became the bowed medieval fiddle, the bridge was usually still low and flat, but its placement could be anywhere along the soundboard. The bridge was often integral with the tailpiece. When 12th century fiddlers wanted to copy the repeating drones of a harp, they introduced a 5th string plucked by the thumb. To avoid bowing it, they raised the bridge and the fingering plane (by adding a fingerboard) to allow angled bowing. Narrow fiddles had this freedom without such raising. In the 15th century, some fiddles acquired high rounded bridges to take part in playing vocal polyphony. In the 16th century, fiddles acquired the waist cutouts from the vihuela and had high rounded bridges for independent playing of individual strings.
Notes on the Polyphont

The polyphont (also called polyphon or polyphone) was a rare 17th century English instrument, strung with brass wire, that was well respected at the time. The body was essentially a shallow rectangular box with rounded corners and scalloped sides. Extending upwards from the body were two necks, and there was an arm between the extremities of the two necks. The neck on the right had a fingerboard with brass frets and nut angled like on a bandora. The courses on the fingerboard were stopped by left-hand fingers without the aid of the left thumb (making barres unlikely). That thumb was occupied with plucking the bass strings tuned from the arm between the ends of the two necks (like in playing the baryton). There was another set of strings played by the right hand, tuned from pins on the top edge of the body on both sides of the fingerboard, that continued (in the bass) with tuning pins running up the other neck (on the left).

Some details about stringing and the design of two example of the polyphont survive. They were written by James Talbot¹ and Randle Holme². They both gave the number of courses tuned from each place. Over the fingerboard, the Talbot example had 5 courses, while the Holme example had 3 courses. Both examples had 9 courses tuned from the arm between the neck ends, plucked by the left thumb. Talbot stated that they were all single. For the strings to the left of the fingerboard plucked by the right thumb, the Talbot example had 3 single (tuned from pins on the left neck) plus 7 double courses (tuned from pins on the upper edge of the body), while the Holme example had 9 courses (the number tuned from the neck or the body was not given). Of the strings to the right of the fingerboard, the Talbot example had 6 courses, while the Holme example had 8 courses. Holme stated that there were a total of 41 strings. This sum is most easily achieved by all of the right-thumb and the fingerboard courses being doubled, and the other courses single. This suggests that on the Talbot example, the courses to the right of the fingerboard were also single. It is also likely that at least 3 of the fingerboard strings were also doubled (he probably would have mentioned it if they were all single or double). The total number of strings was then 37 plus the number of doubled fingerboard strings.

Talbot provided a number of measurements that make the general design clear, while Holme provided a drawing but no measurements. The designs are remarkably different. Talbot's body length (42 inches) was three times the body width (14 inches), while the Holme drawing shows equal body length and width. The Talbot necks were short and of equal length since the straight nuts between them on the arm between their upper ends and below their lower ends (on the top of the body) were stated to be 6½ inches apart. On the Holme drawing, the left neck is almost as long as the body, and the right neck is half its length. There were a number of bridges at the lower ends of the Talbot strings that were all over the soundboard's length (the ones played by thumbs were mostly near the central line). The Holme strings were all attached to a single crooked S-shaped bridge. On the Talbot example, the left thumb could only pluck near the nut ends of the strings, (where the sound produced can be more incisive), while in the Holme example, it could not do (that with the lower basses.

It is possible to estimate the pitches of the strings by assuming that the fingerboard courses were in lute relative tunings, the other strings were tuned in diatonic sequences, and that the string stresses (as measured by the $f_L$ products) did not exceed the practical limits of brass strings as used on Praetorius's Irish harp. Those limits were 57-121 m/sec.³ We should expect that the highest string on the fingerboard would be at the highest string stress. Talbot's fingerboard was 7 inches long and

it had 8 frets. The nut was tilted like on a banjo, so the nut at the end of the highest string was about one fret length below the top of the fingerboard. The bottom of the fingerboard was perhaps half a fret length below the 8th fret. From this, we calculate that the vibrating string length of the highest fingerboard string was about 43 cm.\(^4\) At the highest practical string stress, it was then tuned to d' at Praetorius's Cammerthon, which was like English 'light-music' pitch (a tone above Consort pitch).

We would expect that the Talbot body was so long because it had to provide the length needed for the lowest string (of those plucked by the left thumb), which was at the lowest practical string stress. The maximum string length, in inches, would be the body length (42), plus the distance between the two nuts between the necks (6.5), minus the minimal total of how much lower the nut for the right-thumb strings was from the top of the body and how much higher the bridge was from the bottom of the body (say, 1.5). The result is about 47 inches, which at minimum practical string stress leads to a pitch of GG. The 9 strings for the left thumb would then be tuned to a diatonic scale of GG to A. Then the lowest of the fingerboard strings would be Bb or B.

With a d' top string, the fingerboard strings would be expected to follow the higher strings of a bass lute. If the top 4 courses were tuned to c e a d', everything fits. Following the Holme information, we would expect them to be pairs of strings. The 5th fingerboard course was part of the diatonic sequence, and so could be single. It could have been placed on the fingerboard to give the player a choice between Bb and B by fingering.

Talbot mentioned that most of the notes of the 6 strings to the right of the fingerboard could be played on the frets of the highest fingerboard string. This implies that they were tuned diatonically above that open fingerboard string, e' to c''.

What remains to consider is what the tuning of the courses plucked by the right thumb could have been. The Holme drawing shows that the higher right-thumb strings are about half the length of the left-thumb strings. This suggests that they were an octave apart. Then the 10 Talbot right-thumb courses could well have been tuned diatonically from G to b. Then, if the player wanted to play like on a harp only with the right hand, a full diatonic scale from G to c'' could be played if the left hand fingered c' on the fingerboard 2nd or a course, avoiding the 3 lower fingerboard courses.

Turning now to the Holme example, if we accept higher bass-lute tuning for the fingerboard courses: e a d', then the 9 left-thumb strings would be diatonically tuned C to d, the 9 right-thumb courses c to d', and the 8 strings to the right of the fingerboard e' to c''. A full diatonic scale c to e'' could be played by the right hand only by just avoiding the 3 fingerboard courses.

The above information plus that given by Talbot and Holme should be enough to attempt to make a speculative reconstruction of either version of the polyphont.

Appendix 1 – Transcription of the Talbot information by Donald Gill

Polyphone

There are 6 Trebles whose Nutts & Pins stand oblique below the Finger board[,] they rise gradually & have their Bridge oblique to the Nutt[,] these are = to string passing on the Finger board placed lower to shorten the string. On the Nutt upon the Plate are 5 strings with 8 Fretts[,] their bridge lower & oblique. On the 3d or Broad Streight Nutt (which is equal to h of the Plate) 7 pr Basses with their Brass Bridges at different lengths. to be touched with Thumb of right hand. above this Nutt 3 lesser Nutts carrying each 1 longest Bass touched as before.

On the 4th or upper backward lying streight Nutt whose strings come under those of the 3d Nutt are 9 strings all single & open touched with Thumb of left hand. These Basses have 4 lower single Bridges. I double 1 triple.

\(^4\) length = \(7 \times 2.54/(2^{\left(\frac{1}{12}\right)}\times 1-1/2^{\left(\frac{8.5}{12}\right)})\)
Its knott below the 3d Nutt.
Strings of Brass Wire & Consequently Bridge & Nutt & Fretts[,] Shaped broader at top narrower downward. scalloped at sides all the way.
Length of Belly 3f. 6'. Breadth below Knot. 1f. 2'. at bottom 5'. 4".
Depth. 3'. L. of Plate 7' between two nutts 6' 4".
Width below 2d & 3d Nutt. 2'. 4".

Polyphone

Its Back and sides Air-wood Belly Cullan Cleft – Strings Wire Nutts Fretts Brass as also Bridges & Loops.

1 On the oblique Nutt on the Plate are 5 strings whose Bridge oblique (as in Bandore)[;] on the Plate 8 frets.
2 Somewhat below the bottom of the Plate is another Nutt (with pins above it) this carries 6 strings whose Bridge oblique & higher than that of the first great Nutt[;] these 6 strings seem to rise gradually & are so placed that they may bear a pitch tho' one string on the plate carries nearly same Compass.
3 Equal to the 7th or h Frett of the Plate is a 3d Brass Nutt which is broader than the rest & streight[,] this carries 7 Open Basses whose Brass Bridges or Loops are placed at different Lengths oblique to each other the lowest (towd the Bas-side.) Above the Nutt are 3 lesser Nutts each of which carries 1 longest Bass & has its proper Loop or Bridge.
4 On the 4th or highest Nutt which is 6'. 4". above the 3d & lies more backward on the head & streight are 9 strings all single & open Basses which pass under the 3d Nutt & its strings: these 9 strings have 6 Bridges whereof the 4 lowest carry each one, the 5th two, the third three strings.

NB The strings of the 2 first Nutts are touched with the 4 fingers of the right hand, the 3d with the Thumb ditto, the 4th behind by the Thumb of the left hand whilst the 4 fingers manage the Fretts.
The Knott is somewhat Below the 3d Nutt.

Appendix 2 - Transcription of the Holme information by Ian Harwood

A polyphont of some called polyphon. It is an hollow yet flat kind of instrument, containing three dozen & 5 wier strings to be played upon. On the right side the neck are 3 pins, on the left side above 9 pins, & at the bending or corner in the middle of the neck 9 pins, & below the neck on the top of the body are 8 pins fixed, as the figure it selfe will give y^a the best description of it.
There is on the body a crooked Bridge & 3 small round holes.
The 'English' in English Violette

The Viola d'amore, was essentially a treble viol with variable chordal tunings played like a violin. There were two types. One just had metal playing strings (usually 6), while the other had 6 or 7 gut melody strings (metal-wound in the bass) and the same number of metal sympathetic strings. The English violette was like the second type, but with twice as many sympathetic strings.

When discussing the English violette in his 'Viola d'amore' entry in *The Oxford Companion to Musical Instruments* (1992), Anthony Baines wrote "Why 'English' is not known". Knowledge about the origin of the name of a musical instrument is rarely any more than a hypothesis that seems to make sense in the light of the evidence, while there has been no viable alternative offered. Such a hypothesis was given in the 'English violet' and 'Violetta marina' entries in *The New Grove Dictionary of Musical Instruments* (1984).

The 'English violet' entry said it 'might be identical with the Violetta marina'. The 'Violetta marina' entry said it "was developed by Pietro Castrucci (b 1679; d 1752), leader of Handel's opera orchestra in London for over 22 years. A pair of obligato parts inscribed 'violette marine per gli Signori Castrucci' occur in ... Handel's *Orlando* (1733), and a part for one instrument is included in *Sosarme* (1732)." The attribution to Castrucci is apparently in a brief passage about the Violetta marina in Burney (*History*, ii, 1782, p.698).

It then appears that the 'English' comes from the place of its invention, and probably where it was originally made. The 'marine' in its original name probably derived from the Trumpet marine, which by that time had a multitude of sympathetic strings inside the body.

On p.367, the Baines book illustrated an English violette made in Salzburg in 1712. The popularity of the sympathetic-string type of Viola d'amore grew rapidly in the first few decades of the 18th century, during which the English violette was a common alternative.
The Identity of the Lirone

From 1530 on, documents from Venice refer to sonadori di lironi (lirone players) using instruments with size names sopran, falsetto, contraltto, tenor, basseto and bason. Sonadori di violoni were also mentioned. It is possible that lironi and violoni were two names used for the same family of instruments in Venice, as Ganassi seems to have implied. Another possibility is that 'lirone' mostly referred to the type of viol, with flatter bridge (or bowed with slacker bow hair) described by Ganassi, that specialised in simultaneously playing chords with the melody, as the name 'large lira' implies. Supporting the hypothesis that such a distinction was made is the use of a lirone (size unspecified) in addition to viols to play in the 1565 Florentine Intermedii.

What is most unlikely is that it was the same instrument as the lira da gamba, an instrument called for in the 1589 Intermedii, because there is no evidence that an instrument with this name (or any of its other names - Praetorius gave three others) ever came in different sizes that played together. As the seventeenth century progressed and the lira da braccio became quite redundant, a size qualification became unnecessary, and the lira da gamba was just called lira. The lira da gamba appears to have been of the most common solo viol size – somewhat larger than the tenor viol. It had a novel tuning with no resemblance to that of the viol. There is no evidence on the tunings of lironi, but they were most probably just like viols.

In the 1568 edition of Giorgio Vasari’s book on the history of art, he described a painting by Paulo Veronese which includes what appears as a typical Italian bass viol of that time (of modern double bass size), which he referred to as a 'large lira da gamba'. The lirone looked like a viol, not like a lira da gamba. Banchieri wrote that the lirone and viola bastarda were special types of viols that needed much judgement in playing part music. This implies that playing these types of viols involved rather more awareness (than when playing other viols) of what is happening in more than one part. It is likely that the lirone, with a flatter bridge, usually played several strings at a time, like the lira. The lirone played chords all at once, while the viola bastarda arpeggiated them. A set of six lironi of different sizes probably sounded like a small string orchestra.

Modern organologists have unfortunately usually considered that the lirone was the same instrument as the lira da gamba, apparently because both names imply a large lira. An exception is Anthony Baines, who wisely never mentioned the lirone, apparently because he didn't know of any evidence describing it.

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2 Silvestro di Ganassi del Fontego, Opera intitulata Fontegara (Venice, 1535), Regulo Rubertina (Venice, 1542) & Lettione seconda (Venice, 1543).
3 G. Vasari, Le Vite de più eccellenti Architetti, Pintori, et Scultori Italiani... (1568), ed. Milanese (Florence, 1906), VI, p. 373.
4 P. Veronese, Allegory of Music, Palazzo Ducale, Venice, reproduced as Plate 6b in E. Winternitz, Musical Instruments and their Symbolism in Western Art (Faber & Faber, London, 1967)
5 E. Winternitz, op. cit. p. 55.
6 A. Banchieri, Conclusione nel suono dell'organo (Bologna, 1609), p. 69.
On the origins of the tuning peg and some early instrument names

The tuning peg was the most influential invention in the history of stringed instruments. Before its appearance, apparently in Roman times, all stringed instruments were tuned by gripping and twisting each string around a stationary tuning arm. The end of the string was tied to a stick or bead or to a collar already twisted around the arm. To give enough room for the twisting hand, that arm needed a considerable amount of space around it. The tuning peg made accurate tuning easier and allowed much more compact instrument designs. New instruments that exploited the tuning peg apparently developed in the Near East, and spread east and west. One was the psaltery, in which the tuning pegs were inserted directly into the resonator body. Another was the 'short lute', in which the tuning pegs were inserted into a pegholder at the end of the handle part (that was used for stopping the strings) of a club-shaped resonator body.

There are three statuettes of club-shaped instruments in the Louvre apparently from Greek times, a few centuries BC, illustrated by Panum. The end of the fingerboard and the tuning mechanism were missing in the first (from Tanagra), which from later photos, had been 'restored' in the 20th century to look like a medieval fiddle. The second (from Myrina) and third (from Phoenicia) appear to have had bent-back lute-like pegholders, but the carvings do not show any peg heads. They may not be pegholders, but old-style tuning arms that were bent back to take less space around the player (see Comm. 40, Q5). If they were pegholders, the tuning would have been by tuning hammer on headless pegs. Considering the obvious advantages, one wonders why it would have taken another few centuries for the use of tuning pegs to become widespread.

A club-shaped finger-plucked instrument (with the handle almost as wide as the body) can be seen in sculptures on Roman sarcophagi from the first few centuries A.D. These are shown in Panum (the second image is better shown in Kinsky) and Harrison & Rimmer. There is no tuning mechanism visible, but neither is there any tuning arm, so it was most likely tuned by pegs. The carvings show that it was over half a metre long and about 12 cm wide with 4 to 10 strings of equal length. In two of the four depictions, the strings are only plucked, like commonly on a lyre, while in the other two they are also either stopped, like on a fiddle, or dampened, like on a lyre when strumming. No specific Latin name for it has been identified. The plucked fiddle depicted in the 10th century Stuttgart Psalter had the same general shape and size as this Roman instrument.

Around 630, Isidore of Seville wrote: 'The ancients called the cithara fidicula and fidicen, because the strings are in good accord with each other, as befits men among whom there is trust (fides). Isidore mentioned that there were 'different species of cithara', of which the above instrument must have been one. Here is a more mundane suggestion for the origin: The entry under 'fiddle' in the Oxford English Dictionary says: "The Teutonic word [Middle High German 'videle'] bears a singular resemblance in sound to its medieval Latin synonym 'vitula, vidula'". My pocket Latin dictionary lists

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1. Hortense Panum, _The Stringed Instruments of the Middle Ages_ (London, 1940), Fig. 173, 174 and 175.
2. H. Panum, ibid, Fig 185, 186 and 187.
4. Frank Harrison & Joan Rimmer, _European musical instruments_ (London, 1964), Fig. 28.
'vidulus' as 'trunk, box'. Small boxes were then usually made by hollowing the body out of a thick piece of wood, with the thin lid on top made separately. This is just like how early fiddles, the above instrument and lyres were made. This makes this possibility of the origin particularly attractive. Isidore mentioned the wooden sound box aspect of a cithara when distinguishing it from the psaltery (cithara strings are actuated above the box where the sound came out, while psaltery strings are actuated below where the sound came out). According to his definition, a plucked fiddle as well as a lyre can be called a 'cithara'. Several sources from around 1000 identify what is obviously a fiddle as a cithara. In the light of these considerations, I should modify the narrative of the origins of the fiddle in Europe given in my book. Instead of the fiddle just appearing in the second half of the first millenium, it was there from Roman times (in the first half), in the form of an instrument considered to be a type of cithara but whose design did not have the wings or arms that we generally use to identify lyres. To complicate the situation further, the c. 1200 ms where Gerald of Wales described the rapid and lively playing of a stringed instrument in Ireland, and first mentioned the use of metal strings, also had an illustration – a trapezoidal psaltery held sideways so the strings lay vertically. The question is what was that instrument called? He reported that the cithara and the tympanum were played in Ireland, the cithara, tympanum and chorus in Scotland, and the cithara, tibia and chorus in Wales. The tibia was most probably a pipe. Isidore identified the tympanum as a drum, but Galpin presented an excellent case for that name applying also to a psaltery in Britain. A ninth century source stated that the name 'chorus' was used both for a bagpipe or a stringed instrument of unspecified type. Later appearances of the term referred to it as a 4-stringed instrument, a crowd or a stringed instrument played percussively. The chorus then was probably a type of lyre or cruit. The way it was held prevents Gerald's psaltery from meeting Isidore's criterion for being a cithara, so it most likely was the tympanum. So what was Gerald's cithara? At about the same time, Gerbert illustrated a 'cithara teutonica', which was a rounded lyre, and a 'cithara anglica', which was a pillar harp. Both conform to Isidore's criterion for being a cithara. It is possibly not a matter of chance that the Celtic harp and the psaltery both acquired wire strings at about the same time, because it could have happened in the same musical culture. When back at around 600, Fortunatinis's poem stated that the Britons played the 'chrotta', the name could well have meant any stringed instrument, which would have included a psaltery as well as a lyre. The terms 'chrotta' and 'rote' are closely related, and a rote was a psaltery held the way Gerald's one was. It is possible that the invention of the pillar harp owes at least as much to the rote as to the lyre. For a player, reorganising the soundboard can be easier than reorganising the stringing and playing technique. It also then gained the status of becoming a cithara.

7 W. Bachmann, ibid, textural context of plates 1, 3 and 18.
8 Ephraim Segerman, The Development of Western European Stringed Instruments (NR1 Manchester or Lulu.com, 2006), p.44.
11 Abbot Gerbert, De cantu et musica sacra, ms in the monastery of St Blasius, destroyed in a fire in 1768.
The bass strings of clavichords are foreshortened, meaning that they are shorter in proportion to their pitch than the treble strings. This is unavoidable, even in very large clavichords, but small ones are particularly affected; the result is that solid wire strings, whatever gauge you fit, sound muddy, weak and unfocused. One solution is to fit overwound strings, with a winding of soft copper or silver wrapped round a core of brass or iron, usually in an open spiral. Overwound strings sound better than solid wire because the core wire can be tensioned much as it would be if used in the treble, whilst the added mass of the winding lowers the mass per unit length of the string (the 'linear density') so that it produces the desired note. The design and manufacture of overwound strings was described by John Barnes in FoMRHI Comm 325 (1981), which is still the best summary of the subject that I know of.

The earliest mention of overwound strings seems to be in the 1664 edition of Playford's *Introduction to the Skill of Musick*:

> There is a late invention of Strings for the Basses of Viols and Violins, or Lutes, which sound much better and lower that the common Gut strings ... It is a Small Wire twisted or gimp'd upon a gut string or upon Silk ...

I do not know when the idea was adopted on wire-strung instruments such as clavichords, but it can hardly have been earlier than the 1660s and might have been a good deal later, since it is not immediately obvious that a wire winding can be made to adhere to a wire core as effectively as to a core of softer material like gut.

In any case, clavichords with foreshortened basses existed long before this. John Koster has shown that the principle of foreshortening was known in the fifteenth century, and there are surviving clavichords with foreshortened basses from the sixteenth. The C/E short-octave keyboard, a feature of nearly all sixteenth- and early seventeenth-century clavichords, inevitably involves foreshortening, because (for example) the sounding lengths of the strings for note C can be only slightly longer than those for note F, so that even if F is fully scaled (rarely the case), C will unavoidably be quite severely foreshortened. Similarly, notes D and E are even more problematic, since their sounding lengths are actually shorter than those of note F.

Did clavichord players at this period accept the poor sound and unstable tuning of solid wire strings on these bass notes, or was there an alternative to overwinding that solved the problem?

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1 Quoted in Ian Harwood: 'An introduction to Renaissance viols' in *Early Music*, vol. 2 No. 4, October 1974. In a very early FoMRHI Comm (No. 174), Segerman and Abbott drew attention to another reference to overspun strings for viols etc. probably dated c. 1680.


3 For example, nos. 2 and 3 in the Leipzig University collection.
A remark in Marin Mersenne’s *Harmonie Universelle* (1636) gives a clue. Near the end of his account of the wire-strung cittern he writes:

... la plus grosse chorde du 3, & du 4. rang est tortillé, & faites d’vne chorde redoublee & pliée en deux, afin de faire des sons plus remplis, & plus nourris.⁴

(the thickest strings, nos. 3 and 4, are twisted, and made of a single string doubled and folded in half, in order to produce fuller and richer sounds.)

There is no hard evidence that such twined strings were used on clavichords, but it seems a reasonable guess. Some of my fellow clavichord makers began using these strings on the smaller, earlier types of clavichord several years ago, and the results were so convincing that I began using them myself.

These strings are normally made from yellow or red brass wire. There is a technique to making them effectively. Whatever method you use starts, of course, with a double-length wire folded in half: the half-way point conveniently serves as the hitch-pin loop. You could, of course, simply anchor the two loose ends and twist the wires together with a revolving hook at the folded end. However, this has the disadvantage that the two wires would themselves be excessively twisted: they would be work-hardened, and might even break before you achieved the required closeness of spiral. They would certainly be weakened, and probably somewhat less flexible, and there would be a tendency for them to unwind before you could get the string safely on to the instrument.

So I looked for an alternative which would wrap the two wires closely around each other without twisting them (that’s why I prefer to say ‘twined’ rather than ‘twisted’ strings). After I had made some failed attempts, Darryl Martin explained to me the following method, which he had been using: I am most grateful to him for his help. It is very effective and does not need much in the way of special equipment (see diagram).

You need to fix a rotating hook, somewhere near the ceiling of the workshop: I use an electric drill on the slowest possible setting, fixed to a beam. To start and stop it, I use a push-button switch on a longish lead. You hang the fold in the middle of the wire on the hook, then attach equal, and fairly modest, weights to the free ends (mine were 500g each). To produce the twined string, you need a ‘spreader’ in the form of a piece of wood about 10 inches (250 mm) long with grooves in each end, which holds the two wires a fixed distance apart as they hang. I also fixed a ‘guide’ in the form of a rod pointing up vertically from the middle of it, and a handle opposite the guide to hold the thing by. Before you start the hook rotating, move the guide up close to the hook: if the guide is half the length of the spreader, the two wires will make an angle of 90° at the hook. Now start the hook rotating, and as the two wires wrap round each other move the spreader down, keeping the top of the guide close to the point where the two wires meet so that the angle between them is constant all the way down.

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⁴ *Livre Second des Instrumens*, Proposition XV, p. 98 v.
There is some stress to the two wires, but essentially they remain untwisted and wrap very closely round each other with a very small pitch. The weights revolve, eventually at the same speed as the hook, proving that the two constituent wires are not themselves being unduly twisted. The finished string shows absolutely no tendency to unwind; it displays a close and even pitch along its length.

It seems to be important that the two wires are twined extremely closely around each other: the pitch must be barely more than the diameter of the wire. I found that wires twined at 90° worked very well, and in the end added a block incorporating a 90° angle to the top of the guide rod, which also helps ensure a smooth and even twining. It would be possible to
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The acoustic results of strings made this way are quite extraordinarily good by which I mean that the sound they produce has a clear and definite pitch, with a good blend of partials and without marked inharmonicity, and with satisfactory volume and sustain. Moreover the twined strings are easier to tune and more stable in pitch.

It is not immediately obvious why they perform so much better than solid wires. Naively, I reasoned that since each constituent wire bears half the tension of the combined string, the relationship between cross-sectional area and tension - the stress on the wire - would be no different from that of a solid wire of equivalent cross-section, and one would not expect any improvement.

In order to understand what was going on, I ran a number of experiments on twined strings. As it seemed that twined strings were more 'stretchy' than the constituent wires, in the first experiment I attempted to measure their effective elastic modulus, so that it could be compared with that of solid wires. Sample twined strings were stretched between a hitch-point and an ordinary wrestpin mounted in a block. A spring balance was fitted at the hitched end so that tension could be measured. Two markers made of tiny gummed paper slips were attached roughly a metre apart on each sample, and the precise distance between them was measured as carefully as possible with a plastic rule. The tension on each sample was increased, and tension readings taken for each 1 mm increase in length. From the readings of force and length it was possible to calculate the elastic modulus of each sample string, and by dividing this by the cross-sectional area (assumed to be twice the cross-sectional area of the constituent wire) a value for the effective Young's modulus could be calculated, which could be compared with published data for the material from which the sample was made.

The surprising result is that the effective Young's modulus of a twined string is considerably lower than that of the individual wires from which it is made; typically about 0.65 times the Young's modulus of the solid wire (though this varies with the composition of individual strings). This is why they work so well in practice: it is as if we were able to fit a super-dense, super-stretchy kind of wire for these low notes. One result of this is that whereas with solid wires it is next to impossible to tune the notes because a tiny movement of the tuning pin causes a large change in pitch, with the twined strings the notes can be tuned very much as for fully-scaled wires or overwound strings.

In the second experiment, I attempted to measure the effective linear density of twined strings. Two bridges were placed 1000 mm apart on an improvised monochord, and sample twined strings were stretched across them from a hitch-point at one end to a tuning pin at the other. Tension was applied by turning the tuning pin, and measured by means of a spring balance as before. Each sample was tuned to various pitches, measured using an ordinary tuning meter. From the resulting values for pitch (i.e. frequency), tension and sounding length it was possible to calculate a series of values for the linear density: these produced reasonably consistent results for each sample (somewhat to my relief). To express the results in a
convenient practical form, I calculated the equivalent solid wire diameter for each sample, i.e. the solid wire that would produce the same tension when tuned to the same pitch. Eventually I derived the following working rule-of-thumb for wires twined at 90°:

<table>
<thead>
<tr>
<th>Material</th>
<th>Equivalent solid wire = constituent wire diameter multiplied by:</th>
</tr>
</thead>
<tbody>
<tr>
<td>70/30 yellow brass</td>
<td>1.40</td>
</tr>
<tr>
<td>85/15 red brass</td>
<td>1.48</td>
</tr>
<tr>
<td>90/10 red brass</td>
<td>1.50</td>
</tr>
</tbody>
</table>

If the linear density of the twined string were simply twice that of the constituent wire, the factor in the second column of the table would be $1.414 \cdot (\sqrt{2})$. The larger values for red brass reflect the increased mass resulting from the way the wires are coiled round each other rather than stretched out straight; it seems that for yellow brass, the added mass is negligible.

These conclusions are based on a limited amount of data: weeks of experimenting would probably produce more reliable figures (would anyone with suitable laboratory facilities care to undertake this?). In the meantime, I would encourage makers interested in the smaller, earlier fretted types of clavichord to consider this type of string. At present, several European makers are using them, but they do not seem to have crossed the Atlantic.
Wood Fit for a King? – An Investigation.

We'll lounge beneath the pomegranates,
  palm trees, apple trees,
  under every lovely, leafy thing,
  and walk among the vines,
  enjoy the splendid faces we will see,
  in a lofty palace built of noble stones.

Ibn Gabirol, 11th C.
Al-Andalus

According to the 15th C. Kitab kashf al-humum, four kinds of wood were preferred for making the bowl of an oud – Beech, Elm, Walnut and Vine – which have qualities not found in other woods. Beech gives a ringing tone, Elm gives a fineness (of tone?) and Walnut lasts forever and resists insect attack. As for Vine, this has a quality “only to be found in the treasury of kings” (1).

We know from extant instruments of the 16th and 17th C. and writers like Thomas Mace (2) about some of the materials used in the construction of European lutes of this period. Prior to the 16th C. however, we have no information. It is generally accepted that the lute is directly descended from the oud, so it is probable that the earliest lutes were not only identical in design to the oud but were also made from the same materials. However, no examples of ouds or lutes made from vine wood are known to exist. The species of vine that may have been used by ancient luthiers in the Middle East is not known but is assumed to be grape vine (Vitis Vinifera). Vine, on the face of it, would seem to be an unlikely candidate for instrument construction. Nevertheless, as wild grape vine is readily available locally in Eastern Ontario, Canada, it was decided to test this material in a preliminary investigation attempting to confirm the potential value of vine as an instrument wood.

Wild grape vine may grow to a reasonable size if left undisturbed for a century or so but tends to form a tangled mass of twisted and bent stems – useless for any commercial woodworking application - although it does today find some use in hand crafts and is popular as a fuel wood for flavouring barbecued meats.

One ancient reference to vine wood is found in Ezekiel, chapter 15 of the Old Testament. The interpretation of this parable is not relevant here, but taken literally we learn that vine wood is not suitable for making anything larger than a peg from which to hang pots - it is good only for burning.

A more recent reference to vine wood is found in the mid 19th C publication “Turning and Mechanical Manipulation, Volume 1” by Charles Holzapffel. The first part of this book deals with materials suitable for turning. Under the heading “Apricot-Tree”, 18th C. French turner Bergeron is quoted as writing “often those woods that bear agreeable fruits are not handsome in appearance or agreeable in scent as might be expected”. In this category – in addition to Apricot
In the Middle East and Mediterranean have been important wine producing areas since at least Roman times so vine wood would have been a common enough material during the Medieval period. Nevertheless, purpose-grown vine wood – perhaps only to be found in vineyards controlled by royal decree – might have been another story, making it a rare and, hence, costly material reserved only for ouds presented to kings and princes?

Commercial vineyards – in order to avoid losses due to viral diseases of the vine – now prefer to use grafted, disease resistant rootstock (with a maximum useful life of about 40 years) rather than risk retaining old original rootstock (which might be productive for over 100 years). Therefore, modern vineries – unlike those in the past – may no longer be a good potential source of vine wood suitable for instrument making.

Material Tests

Vine is generally classified as a ring porous hardwood (similar to Elm, Ash and Oak). Some vine species are comparable in cell structure to Grape Vine, readily identified by growth rings comprised of large early wood cells and very small latewood cells, as well as wide, closely spaced radial rays. (3). (see Fig. 1) It is not known, however, if all species of vine, worldwide, are closely related in structure and physical properties to that of grape vine.

Two short lengths of wild grape vine, measuring about 2 inches in diameter were recently cut for testing, both taken from stems at ground level. The stems are about 20 years old, determined by counting growth rings. The first length, from a stem severed about a year ago, was partially dried and comprised mostly of heartwood. The second length was "green" – part heartwood and part sapwood. A piece of the second sample, when cut in half, revealed a dark brown heartwood and white sapwood with a subtle but distinct ray fleck figuring running across both sapwood and heartwood. After exposure to the air for several weeks, the white sapwood of this sample appears to be oxidizing to a pale brown colouration.

Specific Gravity

The specific gravity (density compared to that of water), a dimensionless number, is useful for comparing the heaviness of woods and is an indicator of relative hardness.

One industry standard measurement of specific gravity for wood is determined by taking the weight of a sample when oven dry i.e. after all the free water has been removed and dividing this by the volume when ‘green’ - comparing this calculated density to that of water. Green wood has a moisture content greater than about 30% - a saturation point above which the specific gravity does not change with moisture removal. The ‘oven dry’ standard allows the specific gravity at any other moisture content below 30% to be determined by extrapolation using standard graphs. (4)

Two samples taken from each of the freshly cut lengths of vine were prepared by machining each sample smooth, square and parallel sided. The volumes were calculated from the overall linear...
dimensions measured with dial calipers. The samples were also weighed using digital scales, so that total moisture loss might later be determined.

After air drying for two weeks, the samples were oven dried (using a microwave oven to accelerate the drying time). Drying was undertaken in stages at low microwave power until the samples attained a stable weight - indicating that all free moisture had been removed.

Comparing the weights before and after oven drying, sample 1 was found to have had an initial moisture content of about 30% and sample 2, about 55%. Both samples were, therefore, considered, technically, to be 'green' (when freshly cut). Calculating specific gravity using the 'oven dried' standard gave a value of 0.50 for each sample. This is equivalent to a specific gravity of about 0.54 at 12% moisture content (the lower limit for air-dried wood). Wood is a variable material so the estimated specific gravity for vine, based upon these measurements, would fall somewhere within the range 0.49 to 0.59.

For comparison, the specific gravity (oven dry standard at 12% moisture content) of American Beech is 0.56 to 0.64, European Elm 0.47 to 0.51 and European Walnut 0.49 to 0.54.

Cold Bending Tests
Test strips measuring 20 mm wide by 140 mm long by 2mm thick were prepared from air dried Vine, White Ash, Pacific Yew, European Beech, White Elm, Hard Maple and Black Walnut. Each test strip was 'squeezed' into a bow shape, by applying a force at each end and the deflection at the center measured when the sample was judged to be close to breaking point. The measured deflections were Ash 38 mm; Yew 32 mm; Beech 32 mm; Elm 29 mm; Maple 26 mm and Walnut 19 mm.

The Vine test strip, on the other hand, could be readily deformed into a complete loop (a degree of pliability that was evident even after oven drying). This would make Vine a good material for weaving into baskets or chair seats but surely not for making into oud or lute bowls?

Hot Bending Tests
Each test strip was then hot bent into a 'U' shape (radius of about 40mm) on a propane heated 'bending iron'. The temperature of the iron was sufficient to scorch the test strips (surface temperature of the bending iron of about 225 C). The vine wood was much easier to hot bend than the other woods and retained its shape well with little or no 'spring back' on cooling. The hot bent strips were then squeezed between finger and thumb as a subjective test to compare the stiffness/flexibility of the samples by 'feel'. The Vine was judged to be comparable in stiffness/flexibility or 'springiness' to the Ash, Yew, Beech and Elm strips after hot bending.

Both Maple and Walnut were much stiffer than the other woods. Each sample was then tested to destruction. The Vine, like all of the other woods, failed suddenly, by brittle fracture, across the width of the strip - well before the ends of the strip could be fully closed together. The heat of bending had significantly altered the structure and properties of the vine wood. Simply 'seasoning' or kiln drying vine wood would likely not be enough to cause this change. (5)

Conclusions
From these, albeit limited and rough and ready trials, it is concluded that vine wood might be a viable option for making lute or oud bowls provided that the wood is sufficiently 'heat hardened' to modify its natural pliability.
As a work in progress, the next step in this investigation will be to build a reconstruction of an early (fretted) oud from vine wood. This will depend upon being able to collect a sufficient quantity of vine of adequate dimensions and straightness. Some larger wild grape vine stems (up to 6 inches in diameter) have been located and will be cut this winter for further processing. Ideally, the vine will be quarter sawn into rib stock (requiring a stem diameter of at least 4 inches as ouds of the 14th C. were usually made with either 11 or 13 ribs). However, smaller diameter straight stems may be “slab” sawn as an alternative in order to achieve the required width of rib stock. Furthermore, advantage may be taken of the pliability of the ‘green’ vine wood to straighten any curved stems during air drying by securing them to flat boards.

Notes

(1) Translation by Dr. H.G. Farmer “Structure of the Arabian and Persian Lute” 1939. It is assumed, for the purposes of this study, that Farmer’s identification of the woods is correct. However, none of the Persian/Arabic words mentioned by Farmer in his translation (zan, dardar, saz or shiz) would appear to be currently used in the Arabic or Persian languages. The exception is ‘sasam’ which Farmer translated as Walnut but which today, in Arabic vernacular, refers to a type of rosewood (Dalbergia Sissoo), a species globally distributed from Iraq to India – not to be confused with the true (East) India Rosewood (Dalbergia Latifolia) native of the Bombay region in India. Note that Persian Walnut (Juglans Regia) was traditionally held in high esteem – regarded as a ‘royal’ wood by the Greeks (a wood also fit for a king?)

(2) Thomas Mace “Musick’s Monument”, 1676, Chapter 3, p.49 – “Next, what wood is best for the Ribbs. The Air-wood is absolutely the best. And next to that, our English Maple. But there are very good lutes of several woods: as Plum-Tre, Pear Tree, Yew, Rosemary-Air, Ash, Ebony and Ivory etc. The two last (though most costly and taking to the eye) are the worst”. Note that Air-wood may be Sycamore (Hare-wood = Sycamore) according to Holzapfel.

(3) Bruce R. Hoadley “Identifying Wood” Appendix 1. The Taunton Press.

(4) “The Encyclopedia of Wood” a handbook originally published by the Forest Products Laboratory, U.S. Dept. of Agriculture. Chapter titled “The Physical Properties of Wood” Fig. 3-4 Graph relating specific gravity and moisture content.

(5) Flame hardening or ‘tempering’ of wood is an ancient technique used by primitive societies for hardening the wooden points of arrows and spears – a method still recommended in the survival manuals of the USA army. One prominent Egyptian luthier hot bends oud ribs over an open flame. The ribs of an early Egyptian oud in my possession are charred black in places.
Fig 1

Cross section of wild grape vine (sample 2) showing the typical ring porous structure of vine wood. Growth rings are comprised of large early wood and very small late wood cells. The radial rays are wide and closely spaced. Reference scale is in mm.
Temperaments for Gut-Strung and Gut-Fretted Instruments

Chris Coakley’s interesting and painstaking Comm. 1808 stops short of addressing the question, of primary interest I would suppose to most readers, of what was historical practice.

Temperament is an interesting study of some historical importance, but its devotees are apt to get it out of proportion. Practical musicians as a whole fall into two groups. The first, a huge majority, don’t bother their heads about it; significantly, this group includes nearly all the big composers and performers from Haydn and Mozart to the present day. The second is a small minority, almost all in the early music fraternity, who are greatly, indeed passionately, interested, too often with more passion than understanding.

In the world of viols the myth of ‘fretting in mean-tone’ has surfaced at intervals of a decade or so. It was raised in the US Viola da Gamba Society in the 1970s, in the VdGS of Great Britain in 1985 and for some following years, and more recently in 2007. The Appendix to this Comm. summarises my criticism of this last revival.

Correspondence with a supporter of ‘mean-tone fretting’ made it clear that that term has been used loosely and widely to cover various unequal tunings supposed to provide a mean-tone fretting which, as the Appendix shows, cannot do so. In addition to this laxity of terminology writers on temperament commonly show other failings, as:

- Imperfect grasp of the technicalities;
- Unwarrantable ‘common sense’ assumptions, as that instruments playing together must have been in the same temperament.
- Failure to recognise the significance of ‘negative’ evidence, as on fretting patterns.
- Failure to recognise that hearing, like other sensory impressions, is not simply acoustic but essentially cerebral, and subject like other sensory impressions to illusions, pre-conceived beliefs, suggestion and auto-suggestion. We cannot ‘hear with the same ears’ as musicians in the past. Physiologically, indeed, we have not changed, but our brains, stored from childhood with very different musical experience, cannot be trusted to have the same aesthetic criteria.
- For any temperament other than equal the pattern will change substantially with varied tunings, such as those in Ganassi, Playford’s ‘Lyra Viol’, and Mace, but neither these writers nor any other that I know of give the least hint that such changes are needed.
- Failure to recognise that, as musicians in the past. Physiologically, indeed, we have not changed, but our brains, stored from childhood with very different musical experience, cannot be trusted to have the same aesthetic criteria.

The references provided in the Appendix will help the interested reader to follow the subject in more detail, but there is a crying need for a scholarly text on temperament which is truly temperate - balanced. Shallow, ill-informed, biased, but opinionated books such as that of Ross W. Duffin (‘How Equal Temperament Ruined Harmony’; W.W.Norton, 2007) obfuscate a subject which is in all conscience difficult enough in itself.
The Myth of Meantone Fretting

Introduction

This pamphlet has its origin in the report in 'The Viol'\(^1\) of a lecture by Elizabeth Liddle on fretting of viols, my critical comments on the report\(^2\) and subsequent responses\(^2,3,4\). My concern is primarily and essentially with the spacing of frets as a matter of factual historical truth. Anyone debating the matter should of course have read attentively earlier publications on the subject which I have referred to\(^5,6,7,8\). Whether all my opponents have done so appears to me doubtful.

The early music revival – historically informed performance – is necessarily founded and reliant on trustworthy historical information. The spacing of frets in the past is just one example. Historical information – what happened in the past – is matter of fact, prosaic objective fact, and is to be determined by evidence, not by fancy or present-day taste. Evidence is not proof. It may be good or bad, and the best way of judging this is by criteria such as we expect and rely upon in more important matters than the fretting of viols, in law courts and reputable scientific literature; not that these criteria are infallible (though they are often self-correcting in time) but there are none better. This question of evidence calls for particular attention, as some of my opponents have curious notions about it.

Evidence

Primary evidence is indeed of primary importance. This will be illustrated below in more detailed examination of publications by Praetorius and Simpson, but first some more general comments. Such publications are in themselves facts. So also is the (re-drawn) picture of Simpson’s player in Heron-Allen’s book\(^9\), which shows frets becoming wider as the fingerboard is ascended. A less obvious example is Blumenfeld’s version of the ‘Tabella Universalis’ from ‘Syntagma Musicum’, which has been re-set, again with errors in significant details. A primary source does not necessarily represent the truth, but secondary sources are always less trustworthy, as is exemplified by Dr. Munck’s handling of Praetorius.

The reader who has taken an interest in this controversy must have noticed that I rely upon primary ‘hard’ evidence, as appropriate to a question of objective factual history, whereas my opponents rely largely on artistic judgment – on fancy rather than on reason.

In lawsuits about violin fraud, it was usual about a century ago to call expert witnesses who professed to be able to identify an alleged ‘old master’ violin by its tone. Experience of contradictory opinions, and of controlled experimental investigations, has shown that such evidence is worthless. Another example; the extraordinary collection of early Spanish harpsichords described in ‘Early Music’ a few years ago, each of them unique in one way or another, would have been accepted as genuine in my young days. How could that be determined by their tone? It was careful examination of such things as tool marks and pigments used in decoration, coupled with the failure to produce plausible evidence of provenance, which showed them to be exceptionally clever and well-executed modern fabrications.

Nearer home we have the example of ‘catlines’. So long as these were supposed to be supported by historical evidence they were held to be artistically ‘right’ and became \textit{de rigueur} for original instrument ensembles. Since the evidence was shown to be worthless – nothing but ‘popular etymology’ – this is no longer confidently asserted; ‘loaded strings’ have had a similar but briefer history. Present-day value judgments are not necessarily representative of those in the past.
Our present problem of fret spacings illustrates this question. Praetorius, as we shall see later, gives us a verbal account of fret spacings on viols which unmistakably describes that for equal temperament (ET) and cannot possibly apply to any mean-tone (MT) system. The text is not in itself proof, but its truth is strongly supported by evidence from pictures and by the technical constraints of fretting viols and lutes. These technical constraints alone are sufficient proof that fretting was never in a MT. They have been explained already but are obviously so little understood or indeed noticed by my opponents that I now explain them again, being a special kind of primary evidence which cannot be disputed without denying the facts of physics and arithmetic. My opponents are notably wary of addressing them.

Technicalities

A temperament of the kind 1/x MT is a precisely specified series of twelve notes of exactly defined pitches centred, virtually always, on C - this last point being unrecognised and ignored by our MT-freternity but, as we shall see, important. A keyboard tuner can, and should, tune to those pitches as accurately as he is able; if he modifies them he no longer has 1/x MT but an arbitrary tuning. The tuner of viol or lute, for whom each string must provide eight notes defined by the 'stops' of frets and nut, has not the same freedom unless he either a) abandons his seven gut frets and replaces them with fixed mini-frets allowing him to tune each individual note like the keyboard tuner, or b) adds more frets, which will require him to remember which are the right places, for that temperament, for his fingers.

The correct spacings can be calculated, most simply from vibration frequencies and the 'Mersenne' law that, other things being equal, the frequency is in inverse proportion to the length. The use of a logarithmic unit such as the cent is less easy, and the 'historical' approach through commas and major and minor tones will be even more puzzling. (The statement which I have seen somewhere that ET requires an understanding of logarithms is wrong. Logarithms were unknown to Praetorius, but he got his ET right, and a musician seeking complete freedom of modulation would reach it empirically). Here are the frequencies for 1/4-comma MT and, for comparison, for ET:

<table>
<thead>
<tr>
<th>1/4-C-MT</th>
<th>ET</th>
<th>1/4-C-MT</th>
<th>ET</th>
</tr>
</thead>
<tbody>
<tr>
<td>c' = 263.2</td>
<td>261.63</td>
<td>f'# = 367.8</td>
<td>369.99</td>
</tr>
<tr>
<td>c'# = 275.0</td>
<td>277.18</td>
<td>g' = 393.6</td>
<td>392.00</td>
</tr>
<tr>
<td>d' = 294.2</td>
<td>293.67</td>
<td>g'# = 411.3</td>
<td>415.31</td>
</tr>
<tr>
<td>d'# = 314.9</td>
<td>311.13</td>
<td>a' = 440.0</td>
<td>440.00</td>
</tr>
<tr>
<td>e' = 329.0</td>
<td>329.63</td>
<td>b' flat = 470.8</td>
<td>466.16</td>
</tr>
<tr>
<td>f' = 352.0</td>
<td>349.23</td>
<td>b' = 492.0</td>
<td>493.88</td>
</tr>
<tr>
<td></td>
<td></td>
<td>c' = 526.4</td>
<td>523.25</td>
</tr>
</tbody>
</table>

They are for the international a' = 440, but the ratios and fret spacings will be the same for any reference pitch. They define, to begin with, the tunings of the open strings.

The resulting patterns for four different tunings are appended from my earlier paper 7:

For 1/6th comma MT the discontinuities and irregularities will remain but will be less pronounced, and the pattern will get nearer to ET as the x of 1/x MT becomes a larger number.

The difference between patterns for I and II is strikingly obvious. It is consistent with the shapes of frets 5 (= nut of II) and 6 of I. Look also at III and IV. To affirm, as an earlier disputant has done and Thomas Munck does, that such patterns may be achieved with seven gut frets is nonsense; and bear in mind all the varied tunings of Ganassi and the lyra books, all with their own patterns. MT-fretters simply ignore these facts, which
have never been challenged, and are not challenged by any of my opponents in 'The Viol'. They ignore them, maybe from pure ignorance - i.e. they are unqualified to debate the subject; maybe from such confidence that their existing beliefs must be right that they dismiss them without proper examination - belief rather than reason.

At this point the non-specialist reader may wonder whether I am trying to 'blind him with science'. I assure him that I am not. I am not asking him to believe me as an 'authority'. 'Authorities' must never be accepted as infallible. I am trying to enlighten him. With only simple arithmetic, a pocket calculator, a metric-scaled ruler, and (to be sure) some patient application, the above account will allow him to check these patterns for himself. There is no 'give-or-take' in this matter, any more than in the statement that 2+2 = 4.

We may note that 1/4-comma MT is exceptional in that it gives common chords in eight keys with perfect thirds and very tolerable fifths. It is, within its limits, a reasonable temperament. No other 1/x MT temperament can give any perfect thirds, and the fifths will not be greatly improved. A late Victorian (1870) organ builder's use of the term a 'sweet tuning' prompts a suspicion that he meant 1/4-comma MT, however surprising at that date. No one I believe doubts that it was formerly much used for keyboards. How far the various later temperaments were actually used - as distinct from being advocated - is more open to doubt.

Specific Criticisms of my Conclusions

I begin with Thomas Munck on Praetorius. Here is what Praetorius wrote, in full, in the translation by Blumenfeld, which I have however checked against the original German from facsimile:

"Cambas and especially lutes, of course, afford all chromatic tones; yet their tuning is not as pure and true as that of a harpsichord of this kind". [He refers to one of about 1590 with '77 keys in all its four octaves']. "This is because the frets on gambas and lutes are all equally spaced (though the nearer the bridge, the closer the spacing - and this goes without saying). Therefore the chromatic tones produced from them are neither semitonia majora nor minora - neither large nor small half-tones - but should be termed intermedia, or intermediate; for each of the frets is spaced at the interval of 4 1/2 commata, while the semitonium majus is properly 5, and the semitonium minore, 4 commata in size. But then the frets are false only by a half-comma on either side, and this does not disturb the ear very much, since the discrepancy can not be discerned very clearly. Thus the large semitone sounds just as well as the small one produced on the same fret - just as though it were correctly tuned. The main reason for this is that the player can influence the pitch of the strings by the position of his fingers on the frets. This cannot be done on the harpsichord and organ, for the strings and pipes of these instruments cannot be adjusted in pitch during the course of playing, but on the contrary must remain set as they are tuned. The chromatic genus thus cannot entirely be adhered to on keyboard instruments unless they have differentiated keys for chromatic tones. If it is desired to play chromatically on the lute, all its frets would have to be taken off and it would have to be played without them entirely".

That is all; an admirably clear and exact account of ET fretting, which cannot be reconciled either with any genuine MT fretting or with approximations to it as described by Liddle. Here is Munck's version; remember that most readers, unlikely to have the original to hand, will suppose it to be a fair, unbiased statement:

"Praetorius does indeed seem to prefer viols and lutes with ET fretting, but makes it clear it is just his opinion (he recognises disagreement) and notes that the resulting discrepancies are not very noticeable because lutes and viols are so quiet..." (my italics).

Comment is hardly necessary; such corruption and gratuitous fabrication of evidence is inexcusable. The original reads as a plain statement of current practice without a hint of 'opinion'. And
'disagreement'? not at all; a clear statement of a rational solution without a hint that it was actually practised - as it never was until the days of the cellambists (who, by the way, justified their abandoning the frets by a value judgment - artistically it was an 'improvement').

Next, on probabilities in pictorial evidence. "72% of...images is manifestly wrong...Can we trust the remainder?". This is an example of the fallacy called (I believe) 'false dilemma'. Of 69 pictures 19 show substantially ET spacings, some of them very accurate. None of the 69 shows a genuine MT pattern (impossible, indeed). One only shows (rather obscurely) a rough approximation such as Liddle proposes. That is good evidence for concluding that the 19 are historically representative because it is supported by a) the technical evidence; b) the verbal text of Praetorius; c) the proposal of the harpsichord maker Jean Denis, about 1640, of a fingerboard with fragmented fixed frets to accord with a MT keyboard; d) what Thomas Salmon wrote in 1705. 'To make all our whole Notes, and all our half Notes of an equal size...as the common practice is...' (my italics).

Now for Marais. I am arguing, remember, about fretting patterns - matter of prosaic material physical measurable fact - not about conjectural aesthetic values. The Marais picture shows, beyond reasonable doubt, neither a genuine MT pattern nor a Liddle-style approximation, but substantially ET. That agrees with the evidence of Praetorius, Jean Denis, Thomas Salmon, and Simpson's pictures. If Marais did in fact 're-tune every note as he played it' it would not be 'a substantial extra challenge'. Every competent violinist, violist or cellist learns to do so all the time, without thought, as Mr. Carter recognises. Whether Marais did so is an open question. What is certain is that players in my young days such as Arnold, Rudolph, and Nathalie Dolmetsch, August Wenzinger and his colleagues, Robert Donington, and Richard Nicholson, musicians of some standing, all had frets in substantially ET and never to my knowledge advocated or even considered any other pattern.

Elizabeth Liddle is disarmingly frank. She does not pretend to be competent to evaluate the historical evidence, does not attempt to confute my conclusions, but -cannot believe them. Belief overrides evidence and reason.

The contribution of Stewart McCoy is more interesting. He disagrees, be it noted, with Munck, holds back from disputing my conclusions, and thinks that 'some players accepted ET but others used some kind of MT'. Amending that to 'Virtually all accepted ET fretting but there is a little evidence that a few experimented with other fret patterns', I would agree. Stewart quotes Simpson helpfully, verbatim.

Simpson, like Praetorius, is indeed a writer to be taken with respect. He 'does not deny' that doubling the first fret 'may be useful in some cases' (this quotation was in fact raised with me in debate about twenty years ago) but his subsequent text shows that this was not done with a clear understanding of the result - 'I do not conceive that the enharmonic scale is therein concerned (etc.)...'. Even if the doubling gave correct enharmonics it would not constitute MT fretting; it would be comparable to one split key in a keyboard. No doubt Simpson knew of some players who experimentally doubled their first frets, but I know of no evidence, verbal or pictorial, for wider use of the practice, nor it seems does Stewart.

As for 'The Division Viol': 'Some more curious observers....these and other like observations being less requisite to our present purpose, it sufficeth to have mentioned them; leaving a further disquisition thereof to such as find leisure and pleasure to search into these nicer subtilties (my italics). Exactly so; Simpson in his time had to deal with temperament enthusiasts - not 'practical' but 'speculative' musicians. And all the time Stewart ignores the obvious facts, staring him in the face, that Simpson himself neither recommends such doubled frets nor shows them in his pictures - and he suggests rather lamely that Simpson may not have bothered to look at the pictures. But such selection of evidence is common among believers in a myth. I am reminded of one of Walter Bergmann's 'Golden Rules for Recorder Players' - 'Use historical evidence as some people use the Bible; they pick out the bits that support their own opinions and ignore the rest'.

I can offer as good/bad arguments for Thomas Salmon's ridiculous interchangeable fingerboards to

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achieve 'Musick in Perfect and Mathematicall Proportions'. These were demonstrated to the Royal Society in 1705 by Mr. Frederick and Mr. Christian Stefkins...whereby it appear'd, that the Theory was certain, since all the Stops were owned by them, to be perfect...Signior Gasperini plaid another Sonata upon the Violin in Confort with them, wherein the most complete Harmony was heard.' I should not I suppose have any doubts about the reliability of a report written by a Rector of the Church of England, the Rev. Thomas himself - perhaps I am just a cynical old man.

Salmon's proposals were not new at the time. Locke (d. 1677) had already ridiculed them; the elder Richard Meares advertised in 1688 his readiness to supply 'lutes, and viols fretted according to Mr. Salmon's proposal'. May we look forward to a lecture to the VdGS advocating Salmon's interchangeable fingerboards as having been 'in common use for at least fifteen years'?

Mr. Carter raises the question of the wire-strung instruments. It was raised with me eight years ago. I have not made a special study of these, but can usefully offer some comments. Information on their fretting is indeed frustratingly meagre. We have no verbal account such as Praetorius has given us for viols, reproduced above. But we do have his pictures (Plates VII, XVI and XVII). For all that I can see these, citterns and all, show a quite close approximation to ET. Unlike the gut-fretted instruments, genuine MT would be quite simple with fixed metal frets, which may easily be chopped up and staggered, but, again, the pattern for a genuine MT would vary with the tuning, and require interchangeable fingerboards. I append diagrams for two cittern tunings. There are a fair number of pictures for citterns, but I have never seen one with such patterns as these, or that of the Liddle compromise; they vary a great deal and are odd in having very often incomplete part-length frets, and I cannot see any suggestion of MT in them.

Mr. Carter thinks that 'Citterns, orpharions and bandoras' are 'relevant to viols and lutes because they played together in the so-called broken consort, and I would prefer to believe that the musicians aimed to play in tune together...'. Here again we have 'belief in an aim' taken as evidence. I go back to the primary evidence of fret spacings. I have dealt with this subject in an earlier publication, and have repeated above some of the evidence that viols cannot be fretted in genuine MT, but that with ET fretting or very near it they were played together with keyboards in genuine MT; evidence, not just belief.

Mr. Carter makes the common assumption that instruments must have been fretted to match keyboard tunings - though he goes on to explain why they need not have been. That assumption implies that all those pictures, the texts of Praetorius, Denis and Salmon, and my account of the technicalities, are all wrong. The reader should also refer to my discussion of performance by instruments of different temperaments.

I have already dealt with all the issues raised in Richard Campbell's lengthy and discursive contribution except one; he refers to my use of the term 'random sample' as 'fetishist'. Either he has not bothered to read my earlier papers with attention or he is remarkably naive about probabilities. Mine was a sample of 76 pictures taken at random; he counters with a single sample taken at random; just the 'breathtakingly unscientific' kind of 'evidence' to which I have referred. It is an example of what might be termed the 'Wilberforce gambit'.

Discussion

It is demonstrably impossible to set seven gut frets in any MT temperament, properly so called. It requires frets which are stepped in a way which is simply impossible; very obviously so in 1/4-comma MT, less so as the x of 1/x becomes greater, but still impossible with anything short of ET. All attempts are necessarily compromises; we really should not tolerate the term 'MT fretting' but speak of some compromise or another.

One such was offered by Elizabeth Liddle in 1989. It relied upon sloping and/or split frets - for which not a scrap of historical evidence was or has been offered. It was also flawed by failure to recognise that patterns are substantially different for different tunings, even the conventional
tunings of bass and tenor viols being quite different given. Used for the tenor that pattern would give 1/4-comma MT not in C but in F; the tenor viol would have been seriously mis-tuned. Yet the practice was acclaimed at the time by some enthusiasts (as I well remember) as an improvement on ET. Gordon Dodd told me that he did not find it so, adding that the consort he heard had great difficulty in tuning – which is hardly to be wondered at, apart from the likely contributory cause of insecurity of a fret which is significantly sloped.

That compromise has been abandoned and replaced by a simpler one, but still without recognising that patterns are different for different tunings, although less markedly so than for the 1989 proposal; this may be seen from inspection of the given diagrams. This compromise in its turn is now offered to us as better than ET. I am tempted to the suggestion that all this is re-enacting, belatedly, the historical progression in a wider context from 1/4-comma MT to ET.

Summary

I showed in a published paper 7 years ago⁷ (and have repeated above the simple mathematical proof given there) that it is impossible with seven gut frets to produce any MT pattern properly so-called, and that any MT pattern varies substantially with varied tunings. I gave also an extensive review of the available textual and pictorial evidence, showing (surprise, surprise) that it fully supports the above statement, and does not support the supposition that the ‘compromise-MT’ frettings which have been advocated by Elizabeth Liddle and accepted by others ever had any significant measure of use.

How have our ‘MT-fretters’ responded? By ignoring completely the mathematical proof and the problem of varied tunings, without attempting to controvert my conclusions. By recourse to belief in the teeth of the above evidence. By attempting in general to evade the evidence of texts and pictures, by simply ignoring it. By lame attempts to ‘interpret’ the evidence to suit their preconceived opinions. Worse, by falsifying and inventing evidence. And in the last resort, by asserting that their aesthetic judgments take priority over the mathematical proof and historical evidence (as did the cellambists of a century ago; frets ‘must have been merely aids to beginners, because they prevent good intonation’). In short, they believe, and are not to be confused with the facts.

If players choose to shift their frets about in the belief that the resulting sound is better it is no business of mine. If they go further and believe that they are achieving the impossible, I merely shrug my shoulders, being well aware of how tenaciously odd beliefs can be held (the Queen of Hearts called for ‘Sentence first – verdict afterwards’; the White Queen could believe six impossible things before breakfast). But when they step forward and tell us that their ill-thought-out schemes were in fact historical practice I overcome my habitual indolence and speak up for historical truth. Historical truth is not to be trifled with in the proceedings of a Society professing serious study of the viola da gamba.

I do not suppose that this myth will die. Myths have wonderful vitality. The Indian rope trick, which originated from a ‘spoof’ newspaper article of the 1830’s, does seem to be dead at last, but ‘Mowgli’ stories of feral children are still going strong, as is the centuries-old myth of fish (and many other much more extraordinary things) coming down in rain; I note a report from the Welsh border in 2004¹⁶. Expect MT-fretting to come up again, though it will probably be after my time.

12th March 2008

John R. Catch
Notes:

1. 'The Viol', no.6, Spring 2007, pp.7-14.


   This picture, dated about 1570, is in Kinisky's 'History of Music in Pictures' (Dent/Dutton 1929) p. 96 no. 4. It is an irregular pattern.


9. 'Violin Making as it Was and Is', E. Heron-Allen, London 1885, p. 54.


11. Lindley, 'Lute, Viols, and Temperament', Cambridge Univ. Press 1984 (I regret that I cannot find my note of the page number).

12. Thomas Salmon, Phil. Trans. Roy. Soc. XXIV, 1705, p. 2077 (the page numbering is irregular).

13. 'Grove' 5th ed., art. 'Meares'.

14. At the meeting of the British Association in Oxford in 1860 Bishop Wilberforce, setting out professedly to 'smash Darwin', wound up with an unwise schoolboy witticism at the expense of T.H. Huxley. Huxley's measured, crushing response is historic.


For more examples of such stuff see e.g. 'The Natural History of Nonsense', Bergen Evans; Michael Joseph, London 1953; 'Why People believe Weird Things'; Michael Shermer, Souvenir Press, London 2002.
Bass viol

Tenor viol

'The Bandora Set'
Fig. 5. Patterns for 4-course curious tunings in $1\frac{1}{4}$ c. mean-tone.

$e'd'g't$  $Nut$  $e'd'g'a$
Reply to Hebbert's Comm. 1803 on early bending methods

On reflection, I was hasty in suggesting that changing the shape of wood strips by pressing them in a mould was unlikely to be common in the Renaissance. What I had not adequately considered was Gug's Comm. 881, in which he showed that it was traditional to treat wood by immersion in salted water for long periods (or shorter periods at elevated temperature) to inhibit rot and worm, to stabilise by reducing dimensional change with changing moisture content, and (according to one report from 1580) to improve the sound. Dendrochronological analysis of the soundboards of some Guarnieri violin soundboards indicate that they were made very soon after the tree was felled, indicating that some accelerated curing treatment such as stewing the green wood was most probably used. If the stewing was near to boiling point, the wood becomes quite plastic and can relatively easily be pressed into a desired shape. There are some modern violin makers (led by Bill Fulton, of propolis and pine-resin varnishes fame) who stew their plates (usually two-piece, but sometimes one-piece) and bend them before carving to final dimensions to maximise the parallelism between the grain and the final plate arching. They don't create the desired shape by clamping between a form and counterform, but they just clamp at the edges to a flat surface and get the curvature by using wedges in between. This could be the way that later English viol makers bent their soundboard staves into complex curves without scorching.

Hebbert's observations on burn marks on the undersides of 17th century viol soundboards are very welcome. He has looked at more of these than I have. His speculation that the purpose of the scorching was to harden the wood for acoustical advantage is argued against by saying that if this effect is true, it would have been used extensively on violins as well, and that has not been the case. In my understanding of what happens in the scorching, the charred surface, where the wood has burned, has no remaining integrity. Just below the charred surface, the wood has been subjected to intense heating which, besides driving off the water, breaks down enough of the hemicellulose component of the wood material (the part that adsorbs the water) that its ability to later re-adsorb water is seriously curtailed. Thus it is permanently contracted there, and if done on a concave surface, that makes it more concave. Having made bent-stave viol soundboards ourselves, I can testify that the distribution of arching curvature after the top is assembled is often not quite as it was intended. Scorching regions of the soundboard after assembly can be a way of making small corrections.

I can't see why scorching with a small iron would ever be considered for bending viol ribs. Ribs don't have to be bent in more than one direction. Also, I don't see how re-curving with a small scorching iron is inconsistent with the stave being previously being given a general curvature with a normal larger bending iron. Another of Hebbert's points that escapes me is how if scorch marks are associated with bending, this is incompatible with puncture marks. I expect that puncture marks would have been associated with thicknessing, which would happen after a soundboard is assembled and the outside shape finished.
I will discuss the issues in dispute under the headings Peruffo used in Comm.1804:

1. The four ages...
The third era

Our dispute about John Dowland’s discussion of visual clearness in gut strings involves two passages in ‘Other Necessary Observations ...’ in Robert Dowland’s ‘Varietie ...’. In the first paragraph introducing ‘for Chusing of Lute-strings’, he wrote: ‘Ordinarily therefore wee choose Lute-strings by the freshness, or new making: the which appeares unto vs by their cleere and oylinesse, as they lye in the Boxe or bundle; yet herein we are often deceived, for Oyle at any time will make strings looke cleere, and therefore this tricke is too too commonly vsed to them when they are old.’ Here, lute strings are discussed in general, and to maintain his position, Peruffo has to assume that Dowland inadvertently omitted to indicate that his comment referred only to thin strings.

Dowland’s terminology was that strings for the 1st course were called ‘Trebles’, 2nd course ‘Smale meanes’, 3rd course ‘Great meanes’, 4th course ‘Contra-tenors’, 5th course ‘Tenors’ and 6th course ‘Bases’. Strings for the 7th, 8th and 9th courses were called ‘Accessories’. In a more general sense, he divided the strings into the ‘Great and Small’ sorts. At one point he mentioned ‘For the greater sorts or Base strings’, implying that ‘Base’ could also refer to all of the strings thicker than the Means.

The second chapter starts with details about choosing Trebles. It ends with ‘This choosing of strings is not alone for Trebles, but also for small and great Meanes: greater strings though they be ould are better to be borne withall, so the colour be good, but if they be fresh and new they will be cleere against the light, though their colour be blackish.’ The obvious interpretation of ‘greater strings’ is that they referred to strings thicker than the ones just mentioned (that ‘greatness’ with a lower-case ‘g’ referred to comparative diameter is illustrated in the later passage ‘Thus as the sounds increase in height [pitch], so the strings must decrease in greatnesse’). To maintain his position, Peruffo has to assume that ‘greater strings’ referred only to the just mentioned small and great Means, claiming that the colon supports this. Dowland used the colon to indicate continuation of the same topic, which in this case is strings greater than the Trebles and Means. I don’t understand why Peruffo seems to insist that Dowland would have used ‘Bases’ for lower courses rather than ‘greater strings’, which he indicated was equivalent. Also, he should know from the iconography that the ‘blackish colour’ of these greater strings applies much more to lower strings than to the 2nd and 3rd Means courses.

Dowland’s statement ‘greater strings though they be ould are better to be borne withall’ implies that thicker strings were rarely replaced with new ones, so a picture of a lute is unlikely to be showing new ‘cleere’ greater strings. The phrase ‘cleere against the light’ indicates that ‘cleere’ means that some light is transmitted though it when viewed against a light source. This is not the lighting situation in a picture. And ‘though their colour be blackish’ when ‘cleere against the light’ implies that even when new they will look dark in non-transmitted light. This dark look is what I see in the 17th century pictures of the thicker strings (though I haven’t seen dark red). I don’t see how Peruffo can consider the dark look of thicker strings in pictures is inconsistent with the statements of clearness of good gut by Burwell and Mace. Then he states that ‘neither the Burwell Lute Tutor nor Mace have anything explicit to say about the transparency of the basses’. Let us look again at what was stated in these sources:

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Burwell wrote 'The good Stringes are made at Rome... except the great Stringes & Octaves that are made in Lyons... The Stringes are made of Sheepe & Catts gatte and are twisted with a great deale of Art [;] to be good they must be cleare and transparent smooth and well twisted hard and strong'.

Mace wrote 'First know, Minikens are made up always in long-thin-small Knots, and 60 are to be in a Bundle. Venice-Catlins are made up in short double Knots, and 30 doubles in a Bundle. Both which, are (generally) at the same Price, and the signs of Goodness, both the same; which are, first the Clearness of the String to the Eye, [and second] the Smoothness, and Stiffness to the Finger, and if they have Those two qualities, dispute their Goodness no further. The Lyon String, is made up in a double Knot, but as Long as the Minikin. They are sold (commonly) by the Dozens, and not made up into Bundles. Their Goodness may be perceiv'd, as were the other. But they are much more Inferior Strings than the other.' We should remember that Minikens were thin strings, Venice-Catlins were mid-range strings and Lyons were mid-range and thick strings (the only kind of thick strings usually available). Pistoy Basses were better than Lyons for the thick strings, 'but are hard to come by; However out of a Good parcel of Lyon Strings, you may (with care) pick those that will serve very well.' No criteria for Goodness were mentioned for Pistoy Basses.

In both of these sources, generally available types of thin and thick gut strings were mentioned, and then the criteria for acceptable quality (including clearness) given. No distinction was made concerning the clearness between the thin and thick strings. Peruffo insists that such a distinction must have been the case, so to maintain his position, he must postulate that both authors inadvertently neglected to mention it. If one is an advocate for a cause, such a low probability for the interpretation of the supporting evidence is acceptable. If one is more objective, it would not be.

When new modern catlines are treated with an oil, as Mersenne indicated was the case in his time, and the oil had a refractive index close enough to that of gut, they would transmit light and thus conform to Dowland's criterion of being 'cleere'. We have found this to be the case with linseed oil, and Stoppani has observed this with olive oil. As for whether a red dye will destroy the clearness, it depends on the dye. If it was something like madder mordanted on a mineral with an index of refraction close to that of the oil, there would not be a serious effect on the amount of red light transmitted. As I've said before, Peruffo's implication that Pistoy Basses were standard strings for 17th century instruments is strongly argued against by both Mace's statement that they were 'hard to come by', and the scarcity of pictures that show the strings red where we would expect Pistoy Basses to have been and not red on the other strings.

The fourth era

There are no arguments here. When the 17th century bass viol was used as a small violone in 18th century Germany, it needed to have a wound 6th to get down to its GG pitch, and I don't know of any evidence indicating whether the 5th (or even the 4th) were wound as well. That would have been a matter of whether they preferred a groggy gutsy sound or a refined richer sound. We have reason (including the evidence Peruffo mentions) to expect more wound strings in France than in Italy or England, with Germany divided between those with Francophile and Italianate leanings.

2. Gut string manufacturing...
Mace

I have no complaints about Peruffo's theorising about properties he suspects applied to Mace's three types of strings. What I did complain about was 1) the implication that Mace's three sorts reflected Dowland's earlier types (and I pointed out the differences), 2) that he omitted the properties for these types that Mace gave, and 3) that one of these, 'Stiffness to the Finger', is in contradiction
with the bending-flexibility interpretation of the term ‘elasticity’ that Peruffo uses.

Catlines

Here, concerning the 16th century history of this type of string, Peruffo complains that I ‘offer conjectures that have become fact as if by magic’. In his scientific training, he should have learned about how theories contribute to scholarship. The only facts are the evidence. Theories have to be able to explain all of the relevant evidence (otherwise they become falsified), and they go beyond it. The theory that most comprehensively explains the evidence is the best knowledge that scholarship can offer at the time. Which theory is the best can change with the emergence of new evidence or a better theory. No theory (i.e. the issue remains a mystery) is an option only if no theory has been offered that can reasonably explain all of the evidence.

My initial nautical theory on the origin of the ‘catline’ name was the only one offered at the time. The evidence was that this string and the cat rope were both unusually elastic (in its stretchy or low Young’s modulus sense). The transformation of ‘rope’ to ‘line’ seemed reasonably likely because the string is thinner than a cat rope, and lines are defined as thinner than ropes. But then I noticed that other string names were derived from where they came from, and a nautical name would be exceptional. This cast doubt on the theory. Thinking about relevant place names, I thought of Catalonia, remembering that the vihuela in Spain had a unison-pair 6th course, which required more elastic strings than those usually used on lutes at the time. After that, I was pleased to find out that there was a thriving string-making industry in Barcelona. As any competent scholar would do, I changed my theory to better fit all the evidence. I am proud of changing my theory, and if this damages my reputation in any way, as Peruffo implies it should, it should only be amongst people who don’t understand how scholarship works.

Peruffo then contrasts my statement that there is no direct evidence that catlines had rope construction with his ‘great deal of evidence’ pointing towards his loaded gut. This is grossly distorted. There is direct evidence and indirect evidence that ‘points’. Direct evidence that obvious roped construction was used for thick musical strings, he admits exists, and lyon strings, not being smooth like catlines, could well have had obvious rope construction. That catlines did not have obvious rope construction is not direct evidence against them having such construction because strings can easily be made with that construction without it being obvious. As for direct evidence for loaded gut, there is none for it ever being used for anything. Direct evidence against catlines having been loaded is that it is highly unlikely that an appropriate loading material exists that will allow the string to be clear, as reported by Dowland, Burwell and Mace. One of the two pieces of indirect evidence that Peruffo bases his case for loaded gut on is the appearance of strings that are dark red-brown in paintings. Dark brown would be expected from thick unbleached gut without loading, and the appearance of red colouration is not consistently associated with strings that we expect would be catlines, and not for strings that we expect would not be. The other piece of indirect evidence he relies on is the lute bridge-hole diameters:

According to the physics, a loaded gut string would be thinner than an unloaded one for the same tension and pitch. The bridge hole diameters on surviving lutes that Peruffo measured are good historical evidence on early string diameters, and deserves a proper analysis, which I did in Comm. 1807. When doing that analysis, making reasonable assumptions about the diameter of the first string and the relative size of a string and the hole it went through, I did a power-law fit to estimate the diameters of all of the non-octave strings on a typical Renaissance and on a d-minor tuned baroque lute. I was surprised to find that all of the courses except for those Dowland called ‘Accessories’ were quite close to being in equal tension. The tensions calculated were for unloaded gut. If the gut on the bass strings was loaded, their tensions would be greater, and one could have equal tension with thicker higher strings.
After submitting that Comm, I thought of comparing lute string diameters from those given by Mersenne, and found them closer than I had any right to expect. Mersenne’s is the only other direct evidence of Renaissance lute string diameters we have, and that agreement increases our confidence that both types of evidence are not far from historically typical. The bridge hole diameters say nothing about whether the strings were loaded or not, but Mersenne’s diameters include strings too thin to have possibly been loaded, and he certainly did not take any differences of density into account. This is indirect evidence against gut strings being loaded. The opinions of modern lute players imagining whether gut strings at somewhat lower tensions than they are used to would work musically is not historical evidence, and so these opinions are not relevant to the historical validity of the rope-construction theory. They are not even relevant to modern acceptability since modern musicians have not yet tried it.

The question of interpreting the ‘equal feel’ evidence of early writers is complicated. My lute is strung with equal tension, but my thin strings feel less yielding than my thick strings. That is because the dent into my finger of a thin string goes deeper (with the same denting force) than the dent of a thick string (where there is a larger area of skin resisting that force). To make the yielding feel the same, the thicker strings would have to be much thicker (and be at higher tension) than they are. I know of no-one that advocates increasing tension for strings at lower pitches. ‘Equal feel’ must be some other criterion, and the simplest is equal displacement for equal force (with the force sensed by muscle tension rather than the indentation depth in the skin of the finger). That implies equal tension.

I remember reading about a study in which physics graduates were tested for their expectations of the results of some mechanical problems. It was found that some students still expected some results that violated proper principles of physics. In spite of their training, they continued to believe common misconceptions that are popular in the general population - a kind of folk physics. Peruffo’s belief, that the amount that a string with a given string tension is displaced by a given displacing force depends on how stretchy the string material is, is another example of folk physics. It is not true because the geometry of the forces follows the geometry of the displacement. To learn about the true situation, he should do a real experiment, putting two strings with different stretchiness and the same weight per unit length (say an ordinary gut string and a steel string with a diameter 0.42 times the gut one) on a handy instrument, tune them up to the same pitch (that makes the tensions the same) and compare the displacements resulting from the same realistic plucking displacing force. The elastic (Young’s) modulus for steel is over 30 times that for gut (see Comm.1766).

The geometric and physical relationships for string displacement are derived in Comm. 1806. The rubber-band effect Peruffo is worried about is quantified in the equation on the last line of that Comm. If the pitch initially drops rapidly as the amplitude decays, we hear only a twang. Using the tension and diameter of the lowest string in the bridge hole analysis, the Young’s modulus of a catline given in Comm. 1766 and reasonable assumptions about the string length and plucking position on a mean lute, that equation predicts that with any reasonable displacement in plucking, it will give a clear tone (i.e. the displacement stretches the string less than 8%). That tone may not be heard clearly throughout a concert hall, but lutes then did not play in concert halls. In a difficult acoustic environment where more projection is needed, some lutenists used finger picks (see my review of Smith’s book). We should not confuse what modern musicians might consider practical with historical information.

Methods involved in making gut strings have varied considerably with time and place, and I agree that modern methods are far from identical with any of the old ones. And each of these methods can produce better and worse strings (according to some criterion) depending on the skill in using them. What I have disputed is Peruffo’s general claim that the older methods produced better strings. It is likely that some early methods, skilfully used, produced strings that were better than
modern ones according to some criteria. Too many details about early methods are missing for us to try to faithfully reproduce any of them. What we do have is what the criteria for good quality were written about then.

The characteristic Peruffo concentrates on as being ‘better’ is bending flexibility, which he calls ‘elasticity’. There are two problems with this. One is that he does not specify how it is ‘better’ (this opinion is a fashion in some early-music circles, especially amongst the nylon-using lute players who need excuses for not using modern gut, and people who have seen pieces of really old gut that have endured the ravages of time). The other is that all of the pre-19th century statements of criteria for quality that appear to refer to bending say the opposite. This includes Dowland (for the first 3 courses): ‘if they finde stiffe, they hould them then as good’, Burwell: ‘hard’, Mace: ‘Stiffness to the Finger’, Galeazzi: ‘elastic and strong, not limp and yielding’. One can always increase bending flexibility on a gut string by destroying the bonding between fibres, but that drastically reduces the clearness. This is done by forcing it to bend sharply, and that is usually necessary when tying a fret to make it fit snugly and tie tightly.

When faced with the above evidence for bending stiffness and the apparently conflicting evidence for bending flexibility in the pictures showing curled string ends sticking out of pegboxes, Peruffo apparently just simply chose the latter. A more objective method exists. There is only one truth, and the choice should seriously take into consideration how readily one can make a reasonable explanation of the evidence that seems to favour the other choice, thus eliminating the conflict. Assuming that new strings were stiff, it is easy to explain the picture evidence by postulating that early players stored their old strings in their pegboxes, either to use for frets or to splice onto a string broken near its end to get more life out of it. On the other hand, it is virtually impossible to reasonably postulate how the statements about stiffness by the early authors were somehow mistaken or misunderstood by us. I don’t understand how Peruffo seems to be free of worry about how the evidence he decides is wrong could possibly have gone wrong.

I am happy that Peruffo has done the experiment to compare the sound produced by whole gut compared to split gut, finding no difference, and that he agrees that the breaking load (tensile strength) of modern gut is not less than that of old gut (another excuse of the nylon-using lute players). We agree that how long a gut string lasts depends on the degree of breaking of gut fibres, either by splitting of the gut early in the making process or by polishing at the end. The amount of fibre breaking during polishing depends on the unevenness in thickness before polishing, which is reduced by splitting and by careful choice of pieces of gut to be twisted together. With such a careful choice, modern polishing by centreless grinder is not necessarily worse than hand polishing.

Modern strings and high twist

I can’t imagine how Peruffo got the impression that I wrote that ‘all musical strings made today always have a genuine high twist’. What I wrote was that I agreed with his statement ‘As a rule, modern strings are less twisted than the strings of the past’. It is well known that a high-twist string is more stretchable than a low-twist string of the same diameter, which leads to more higher harmonics in the sound, making the sound richer. This is not necessarily the most desirable in all situations.

String types

In scholarship it is important to keep an open mind, and to always explore alternative possibilities. So, contrary to Peruffo’s claim, I hadn’t changed my mind when I suggested the possibility of a string looking lighter because of sulphurisation. I agree that a white low string is more likely to have been a wound string. The Gabbiani painting that I have a reproduction of depicts an ensemble of 2 violins, mandolin, 2 violas, cello and harpsichord. We can clearly see all of the strings on the
cello and on one of the violas, and the only light 3rd. We would expect violas to use violin 4ths for their 3rds, but the viola 3rd is as brown as any of the unwound strings. The violin in the foreground on the right is tilted so one sees the bass side and back, but one sees the top of the bridge, so if the violin 4th was white, one should see a bit of it. I don't a white string there. One can also see the pegbox and part of the fingerboard being fingered on the second violin. The 4th string seems a bit lighter than the others, but no more so than the 4th string on the other viola. I would expect the Italians to have experimented with a wound 4th soon after they became possible, but there is no evidence before Ricatti, and the scaled-tension stringing he described, for their preferring it. I suggest that the reason for adopting both changes was the desire for faster playing on the G string, and for a faster response, lower mass and more elasticity helps, with audibility preserved by the richer sound of a wound string.

Mozart

Peruffo questions my report of Leopold Mozart writing that the four violin strings were progressively thicker with lower pitch. In the 1st chapter, 1st section §13 of Versuch, Mozart wrote: ‘The smallest, or thinnest, string is named E; next to it a somewhat larger or thicker A; the next D, and the thickest G’. Progressive string thicknesses does not necessarily imply an all-gut G, since this could also be true if the D was open-wound and the G close-wound, in the French fashion. My assumption has always been that the all-gut old Italian fashion and the French wound fashion were both popular in Germany in his time, and he deliberately avoided discussing them. He mentioned covered lower strings on the viola d’amore probably because they were standard on that instrument. A possible reason for avoiding wound strings is embodied in his complaint about rarely finding evenly-made gut strings. Any unevenness in a string's diameter along its length is amplified into a much greater unevenness in weight per unit length when it is wound. Strings can be hand-polished to be very uniform, but that requires much skill and time. Perhaps there was a luxury market for really true strings at the time, but evidence for it has not survived.

Peruffo cannot explain Mozart's specification of equal tension amongst the four violin strings according to his theory, which extends Riccati's stringing more generally. When irrefutable evidence conflicts with one's theory, a good scholar modifies the theory to remove the contradiction. The necessary modification here would be to reduce the range in time and place for which his theory applies. A major problem with his methodology is his great reluctance to do so.

Stradivari

Strings then were not sold with any specific indication of their diameters, and musicians didn't have any devices for measuring string diameters. The choice of a string for use was picked out of a supply, with the diameter being the closest available (meeting quality requirements) to that preferred, either according to visual estimation from memory or by comparison with an old string being replaced. My guess is that this usually involved a precision with error of perhaps 10% or more. Stradivari kept snippets of strings, mostly wound, probably to be copied for replacement of those on special instruments he made. The theorboed guitar that Stradivari made was such a special instrument, where guidance for replacement would be required. The lines he drew for the long strings varied in thickness with pitch in the way we would expect, so to deny that the thickness is related to string diameter is rather perverse. The precision of the thickness of the charcoal lines is about right for the guidance purposes apparently intended, if the strings were unwound.

If the lowest string, indicated to be a violin 4th, was close wound (as Peruffo would expect), the
diameter would be very much less than the width of the charcoal line. I can’t imagine any reason why Stradivari would ever want to make the line that much wider than it really was (it is wider than any other line on the drawing). A wound violin 4th would be far from long enough, so such a string would have to be specially made. Because of the graduated thicknesses of the lines for strings, all of the other bass strings would have to be wound as well.

There are several virtues in interpreting the line as representing an all-gut string. First, the bass strings of all other theorboed gut-strung instruments at the time were not wound. That was the main reason for the extra string length. Second, the thickness of the line makes sense as a violin 4th since it is close enough to being in an equal-tension relationship with a violin 1st made up of three gut strands. An equal-tension relationship is supported by Stradivari’s countryman and contemporary Di Colco, who reported that applying this criterion should be more consistently successful than just choosing by ear.

Peruffo writes ‘in Italy there is at present no written proof that all-gut stringings on the violin in the 18th century’. Demands for ‘proof’ are common in arts scholarship when one is trying to discredit a theory one argues against, justifying its rejection. It is meaningless in most cases, since we are usually dealing with a balance of probabilities. I have presented an excellent case that the violin 4th that Stradivari used for the lowest long string of his theorboed guitar was much more probably all gut than wound.

Brossard

In Barbieri’s translation, Brossard wrote of the G string ‘if it is merely of gut it must be at least twice as thick as the D string, but if it is overspun with silver it is only a little thicker than the D string’, and of the D string ‘if it is merely of gut it must be at least twice as thick as the A string, but if it is half-overspun with silver, as is almost always the case nowadays, it is no thicker than the A string’.

There is a problem with Barbieri’s translation. If the all-gut G string were over twice the thickness of the D, the tension on the G string would be at least 80% greater than the tension on the D string. No other violin stringing I know of has the tension of the G string much greater than that of the D string. This problem is resolved if we translate Brossard’s ‘grosse’ as ‘bulk’ rather than ‘thickness’, as is necessary to make sense out of Mersenne’s discussion of string physics (see Comm. 344). Bulk implies volume, and in the case of a string, it is the cross-sectional area. The cross-sectional area varies as the square of the diameter. In equal-tension stringing, the G would be 1.5 times as thick as the D string, the square of which is 2.25, which is the ‘over twice’ that Brossard stated. The same applies to the relative dimensions of the D and A strings. This is very strong quantitative evidence that Brossard’s stringing was in equal tension.

Guntershausen (1855)

What Sachs wrote was ‘In 1855 an instrument maker, Heinrich Welcker Gontershausen, 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’. From what Sachs wrote previously, he clearly interpreted Gontershausen as comparing a wound G with an all-gut G, not (as Peruffo would have it) a wound-on-silk G with a wound-on-gut G. I trust Sachs’s understanding of German much more than my own, so I have never chased down the original. If Peruffo wants to, Sacks gives the reference as Heinrich Welcker von Gontershausen, Neu-eröffnetes Magazin musikalischer Tonwerkzeuge, Frankfurt, 1855, p. 241. I suspect that Peruffo’s reluctance to accept historical diversity is that he expects that the judgements he shares with other modern musicians were also shared by all of the people he encounters in his historical studies.
The string gauges

My complaint was that Peruffo apparently neglected that hand-polishing can improve trueness. If he felt belittled, that was not my intention, and I am sorry. From what he writes now, I am glad that we can agree that polishing does not necessarily reduce trueness. Whether it can improve it is still in dispute. With thicker strings, there should be no doubt, but we have found that if more than a touch-up improvement is needed on a violin E, the resulting loss of durability makes it undesirable. I have polished strings to trueness. Many of our string makers through the years have been rather better than I. Why he asks whether I’ve done it is puzzling, unless he doubts whether it can be done.

I must congratulate him on the quality of his gut if he can make violin Es with medium/high twist that have a long life at 440 Hz pitch. Is he actually using lamb’s gut? According to the physics of the situation, if he made Es out of the same gut but with low twist, they would last even longer.

I have never maintained that gut has an average breaking load of 32 Kg/mm². Peruffo must be confusing me with someone else. In my 1974 GSJ article, I gave the equivalent of 42-45 Kg/mm² reported by Kaye and Laby (1959). In my papers, I have concentrated on the highest working stress for a gut string derived from Praetorius’s evidence, which is 24.3 Kg/mm², equivalent to an fL product of 210 m/sec. That is a measure of strength that takes into consideration both the tensile strength and the judgements of a minimum acceptable expected string life time by musicians at the time (i.e. ‘as high as it will go without breaking’).

A violin E string with more twist produces slightly more higher harmonics, which gives it slightly more brilliance. On some violins, that will make it sound more harsh, so it would not be preferred universally.

None of the sources that specify three lamb guts for a violin E were written in the second quarter of the 19th century, so that evidence is not in conflict with the evidence for heavier stringing at that time. Peruffo has no right to ignore the unambiguous evidence for heavier stringing then, which is the comments by Hart, Fetis and Savart. His report of the dimensions of a set of strings originally used by Paganini that is similar to those used in a later period is very interesting, but it does not conflict with the heavier-strings general conclusion because of the 1839 evidence of a Berlin critic who wrote that Ole Bull and Paganini used relatively thin strings, so they apparently did not join the others in following the heavy-stringing fashion. The fundamental job of a scholar is to find conclusions that are consistent with all the evidence, thus eliminating apparent conflicts between them, and not to use apparent conflicts to justify rejection of evidence that contradicts one’s chosen conclusions.

Spohr and Gauges

I quoted Spohr writing ‘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’. Peruffo offers another quote ‘the somewhat thin stringing, however, is always to be preferred so that the violin should not lose too much’. We have no reason to expect Spohr to be inconsistent, so we need a single interpretation which makes both quotations true. A likely possibility is ‘for a really big sound, you need the thickest possible strings, but moderation is preferable’. I interpret this as recommending strings that are somewhat thinner than the thickest possible, but admit that this evidence for higher tensions is much weaker than that of Hart, Fetis and Savart. Spohr’s drawing of a string gauge could be more informative. My assumption has been that the drawing described a gadget that was available in music shops (and could be bought if one wanted one for oneself), and that string diameters were specified during purchase according to the scale markings on the gauge. It was also presumed that the markings for individual violin strings were intended to apply when the centre of the string was
up to the mark, but it is possible that the intention was that the mark should be over the leading or trailing edge of the string as it entered the tapered slot. Visual comparison of a string’s diameter with the width of the slot at the string’s mark in the book’s drawing is imprecise. It is quite possible that for later editions of the book, the size of the drawing was reduced to 1:1 in the later thinner fashion for this to work (mistakenly neglecting to change the scale markings, which then became meaningless). Since Spohr’s is the only German evidence from this period, if another set of assumptions can make at least as much sense of Spohr’s string gauge, including its markings, then it is possible that Germany may not have partaken in the heavy-string fashion that England and France did.

Working tension and ‘feel’

Here Peruffo offers a variety of quotes from me about ‘equal feel’, claiming that I keep changing my mind and thus ‘In practice what Segerman does is demolish all by himself all his laboriously confectioned theory of the lower working tensions and thus indirectly confirms the theory of loading’. What florid rhetoric! And what nonsense. There was no mind changing here. The differences here are ‘equal feel’ defined as equal displacement for the same displacement force, which implies equal tension and appears commonly historically, and ‘equal feel’ defined by equal indentation into the finger for the same force, which implies increased tension in the bass and does not appear historically. After this rhetoric he quotes my ‘final gyration’, where I said that the bridge-hole data indicated that lute stringing was not in equal tension. I gladly admit to this as a ‘gyration’. I wish that Peruffo had the same scholarly conscience and also insisted that his theory has to fit all the evidence. If (as claimed by Peruffo) that has reduced my credibility, it must be completely destroyed by my next ‘gyration’ in Comm. 1807, where a fuller analysis of his bridge-hole data indicates that all of the strings except for the lowest basses appear to be essentially in equal tension.

What remains of Peruffo’s arguments supporting his loaded strings theory is that the deduced lute string tension of a bit over 2 Kg would appear to be too low to be practical to lute players. These are modern lute players, not original lute players, whose opinions would be the only historically relevant evidence.

Tests

I understood Peruffo’s graphs and described what they represented. I do not question his experimental measurements, but his graphs were of the wrong experiments to demonstrate his claim that lateral displacement of a string mounted on an instrument in response to a lateral force depends on its elasticity (stretchability). His Graph 1 shows the variation of T as a function of L in the first equation of Comm.1806: $\Delta L T = X(L - X)F$, where $X$ is 10 cm, $F$ is 0.5 Kg and I calculate that $D$ is in the region of 1.2 to 1.4 cm (he did not provide the value of this constant displacement in the paper). His Graph 3 has standard elasticity measurements with stretching different diameters, but if it was plotted properly, with stress (change of length divided by original length) vs. strain (tension divided by cross-sectional area), the lines should be parallel (if the twists were the same). His Graphs 4 and 5 look more like they should do because the diameters (and cross-sectional areas) are kept constant, and the different slopes represent different elasticities. As for lateral displacement, according to the physics of the situation, it depends on the string length, where on the string it occurs, the tension, and the displacing force, but NOT on the string diameter or elasticity. He should do the proper experiment that duplicates the results of Huggins with enough accuracy to distinguish between the equal-tension and scaled-tension theories. Just showing that scaled tension seems to lead to equal displacements with a sloppy experiment proves nothing.

I never thought that Peruffo was dishonest, only that he is a post-modernist who so strongly believes in his intuition about what the truth is, that he can’t be objective about the evidence and be fair to it.
I was discussing why he had not reported the results of a repeat of Ruggini's experiment with a Ruffini set, and speculated that he possibly had done it, but didn't believe the results, thinking that he had made some mistake. Now he states that he did the experiment, and invites the reader to repeat it. Let us see the results of his experiment, repeated with equal-tension stringing (as a control), showing the difference.

Equal tension

Tartini
Rejecting evidence because it is uncertain is not the way to do historical scholarship. One should look at the evidence and try to evaluate the possibilities allowed by the uncertainties. Fetis's report of a total tension of 63 pounds for Tartini's strings is 30 or 31 Kg irrespective of the uncertainties in how that figure was arrived at. We should agree that the 1st was probably a standard Italian 3-guts string with a diameter of about 0.70 mm. The tension of this string would be about 11 Kg at the Venetian organ pitch standard and about 7.5 Kg at the corista (and Roman) pitch standard. The total string tension of 30 or 31 Kg can be assembled by a Riccati tension variation at Venetian organ pitch or by equal tension at corista. I would pick the latter because that was the usual pitch that Italian bowed strings played at. In 18th century Italy, I associate equal-tension stringing with an all-gut 4th and Riccati's scaled-tension stringing with a wound 4th because they represent distinctive aesthetic concepts of violin sound, different also from the French sound with equal tension and wound 3rd and 4th.

Riccati
Riccati did not claim that the scaled-tension stringing he discussed was new, but he is the earliest source that indicated that this type of stringing was usual.

Mozart
I can't imagine how equal-tension stringing described by Mozart is in conflict with the scaled-tension evidence of Angelucci (de La Lande) and Riccati. There were different countries involved, and the Italians published somewhat later (it doesn't take long for some fashions to change). Peruffo's opinion that a 1.6 mm D is grotesque is not relevant to string history. The fifths problem has only ever been a problem with musicians using a stringing that is different from what they are used to.

Di Colco
Di Colco had late 17th century Italian stringing practices all around him, and Peruffo's rejection of his evidence by accusing him of preferring a theory to the reality around him can only be taken seriously if there is other evidence indicating that he was a theoretician out of touch with reality.

Stradivari
If scholarship dealt only with certainties, most histories, including Peruffo's writings would be very much curtailed and uninformative.

Fetis and Savart
The arithmetic of the statement of 20-22 pounds for the chanterelle and the rest of the strings up to 80 pounds is obvious. The three other strings add up to 58-60 pounds. With a monotonic (i.e. non-increasing tension with increasing string number) distribution, as observed in almost all known historical stringings, the 4th string had 0-2 pounds (up to 10%) lower tension than the 1st string. This difference is much less than in any scaled-tension set that has been reported. I don't mind it if such a stringing, that I would call essentially equal tension because it is slightly scaled, is called a scaled-tension one by Peruffo.
Detezenne
Many theories present an oversimplified picture of the current reality, and are found to be inadequate for the situation in hand.

Maugin & Maigne/Savaresse
Though there is evidence for a printing mistake in their list of tensions (7.025 for 7.25 Kg on the 4th), it is quite possible that their report of 7.5 Kg on the 1st and 8.0 Kg on the 2nd were both true. They might well have had an Italian 1st, known to be superior, with the other strings being French.

As for the breaking tensions going down with thicker strings, that would be expected with their having more twist. We must remember that a high-twist string is stretchier than a low twist string because the stretching stress on an individual fibre is greater than the axial stress on the string. The fibre stretches the amount that the string stretches divided by the cosine of the angle between the fibre and the axis of the string. This is also the way that the modulus of elasticity varies with the average angle between the fibres and the string axis. It is the stretching force on the fibres that causes breaking, so the breaking stress of the string should vary the same way with twist angle as the modulus of elasticity. We have found that the modulus of elasticity of a high-twist string is about half of that of a low-twist string, so we would expect the breaking stress also to be about half. Thus the breaking stress data seems about right for increasing twist with lower strings. If we take into account the effect of string fattening with increased twist (for the same number of guts in the string), it is easy to add appropriate twist to the data on the number of guts to get the slightly-scaled set of string diameters that is consistent with the data on working tensions.

The job of the historical scholar is to try to make sense of the historical evidence, not to reject it (assuming it is incompetent) when one does not understand it with the knowledge one thinks one has.

Conclusions

When faced with no string diameter or tension evidence on the stringing of violins played in later 17th century France and England, I would have to make the best guesstimate I could. Peruffo's evidence on the thinnest strings made (of two guts) offers a minimum diameter for the violin 1st, which in Comm.1675, I wrote was 0.48 mm (I don't remember how I got that figure - in Comm. 1351, Peruffo accepted a previous estimate of mine of 0.42 mm, adding that the minimum should be between 0.40 and 0.50 mm). Talbot's 'bass viol treble string = 2nd of violin' broadens the search for clues to the bass viol, for which there also is no direct stringing evidence. The heaviest stringing that an authentically made modern bass viol can bear without introducing instabilities could provide an estimate of the maximum diameter for the violin 2nd. My estimate of what that heaviest set is has a 1st of about 0.8 mm, leading to a maximum for the violin 1st of about 0.55 mm. We can expect the violin 1st to be between these limits.

Peruffo questions how, in some unspecified Comm, I got 0.57 mm for a string made of two guts and 0.70 mm for one made of three guts with reference to Angelucci/ de La Lande's information. I don't remember where I got 0.70 mm for three guts (it might have been from what Peruffo wrote), but the figure of 0.57 mm derives from it by being it multiplied by the square root of 2/3. That assumes that they were made of the same thickness of guts with the same twist. In Comm. 325, I analysed the information from Maugin & Maigne, and concluded that for thick whole guts, the diameter = 0.50 times the square root of the number of guts, and for very thin whole guts used in violin 1sts, the multiplying factor is 0.32. Then the diameters of strings made of two and three thick guts would be 0.71 and 0.87 mm respectively, and those made of very thin guts are 0.45 and 0.55 mm. The figures of 0.57 and 0.70 mm are in between these.

What Peruffo calls my 'poorly concealed animosity' is completely and absolutely untrue. I consider
him a valued colleague, and a real asset to the fi

like us to be friends in the future. The objections I have voiced are not about his personality or integrity, but about his inadequate training in the principles of good scholarship analysis, and in the physics of strings. Trusting one's intuition and mistrusting the evidence can be appropriate approaches in everyday experience and in the law, but they are not adequate for good scholarship, where the evidence is very rarely deliberately misleading. My hope in writing all of the above is that he will learn from it, so that his future contributions to the field, which I hope will be many, will be better thought out. Our debate is certainly not a 'struggle for life', but neither is it simply an 'exchange of ideas'. 
Reply to Downing’s Comm. 1805 on silk/catgut

Of course, I cannot agree that I 'create mountains of probability out of molehills of slight probability'. I would never knowingly postulate a theory with slight probability. Such a theory would have to be contradicted by evidence. I have often postulated theories of unknown probability because there is hardly any supporting evidence, but there would have to be no contradictory evidence. There is a big difference between unknown probability and slight probability, as there is between a probability judgement based on evidence and one based on expectations. Many researchers in the arts equate the amount of supporting evidence for a theory with its probability of being true. This is not good scholarship. Contradictory evidence is much more important than supporting evidence, since any unambiguous evidence that contradicts the theory should make it invalid, as in scientific scholarship.

My familiarity with linguistic theory is minimal, so I am not in a position to invent any new ones. I remember long ago reading a book exploring how objects were named in early times. People often used the same word in naming different things if they had common characteristics, such as if they moved in the same way or if they performed the same function. One doesn’t need any theory to realise that modifiers to the names have always been used to distinguish between particular objects and others with the same name to avoid ambiguity when that has been important and not obvious in the context. And in a context where the ambiguity has disappeared, the modifiers tend to be dropped. Thus when in the 16th century the name *lira* was used both for a small instrument tuned in 5ths and a viol played in the same chordal way, either the small one was called *lira da braccio* or the viol was called *lirone* when the ambiguity was to be avoided. Later in the century, when a new bass *lira* was invented, it was called *lira da gamba*. By the 17th century, in places where both the *lira da braccio* and the *lirone* were not played, the ambiguity was gone, and the *lira da gamba* was just called *lira*.

In the 18th to 20th centuries, the name *catgut* was used for thin gut strings of high uniformity used for musical instruments, while gut that was not catgut was used for a multitude of other purposes. It has usually been assumed that the ‘gut’ used in contexts other than fishing came from the intestines of mammals, most usually sheep. If John has any evidence otherwise, it would give his case some credibility (we have a right to expect such evidence to exist because technology has been extensively written about in recent centuries). If not, John has to explain how the meaning of gut for musical instruments could have changed from coming from sheep recently, to silkworms, and back to sheep recently without any supporting evidence. This is a classical situation for the application of Occam’s Razor.

As for whether silk strings were more or less expensive than sheep’s gut strings, I do not have at hand any relevant direct evidence on the matter. What is clear is that silk cloth has always been a luxury item in European civilisation, reserved for people of ‘quality’ showing off their status. It is true that most players of lutes and viols had that status, but if the use of a luxury material wasn’t obvious, as in the strings of an instrument, it seems that the more mundane material was preferred. A good example is the lower strings of keyboard instruments, where red and yellow copper were used. Silver would be ‘better’ (i.e. it would give a brighter sound), but it was rarely used.

John’s charge that I try to “blind ‘em with science” implies that I don’t expect readers to understand the science I write about here. I do try to confine the physics and mathematics that I write about to what I learned in high school before university, and that includes what the elastic modulus is. I am surprised that John, who is thoroughly immersed in historical technology, appears not to be familiar with the concept. It is true that the strong effect of elastic modulus on a string’s tone is not taught in schools, but it becomes evident when one experiments with a variety of strings. As strings get
heavier for lower pitches, changes are required to
way of doing that is to lower the elastic modulus by increasing twist, leading to high-twist and roped
strings. Another is to add weight without affecting the load-bearing part of the string by winding or
otherwise loading the string. The silk lute 6th by Alexander Raykov that we have examined falls in
this latter category. It is made by winding the silk thread over the first part of it which acts as the
core. The fraction of the string’s weight that bears the load and gives it its brightness is about a
ninth of the total. The brightness is due to his construction method and does not demonstrate the
properties of silk as a string material. If John claims that this kind of construction is historically
valid, I want to know how far back evidence for it goes. The same applies to the ‘early’ smooth
thicker Chinese silk strings John describes.

I am eager to learn whatever John can teach me about making viable instrument strings that have
historical validity. The definition of historical validity in historical scholarship is that there is
evidence of considerable use at the time. Those in the historical re-creation business often use the
much broader definition of it being historical possible. The moral difficulty I see with having such
different definitions is that the audience is usually led to believe that what they are enjoying is valid
according to historical scholarship. There is an element of deception when a different definition is
actually followed, but which one had not been made clear.

John claims that when Mace referred to gut strings, it is uncertain as to whether they were of
sheep’s gut, two versions of silk, or sinew. There have been considerable attractions to mentioning
sinews in literature since they can symbolise internal strength (they come from inside and pull on
muscles), but there is no evidence of them ever being used in professional string making. As for the
two versions of silk, I doubt whether John can provide any evidence from before the 19th century
(and as a separate issue, evidence from a field other than fishing) for a product made of silk to be
called gut. Those for whom there is a big difference between certainty and anything less than
certainty, John’s statement is true and quite meaningful. To those who are willing to deal with the
real-life issues of probabilities will conclude that Mace most probably was referring to sheep’s gut.

John’s concept of the inadequacy of modern gut strings to successfully fill the needs of modern
musicians is rather out of date, at least on this side of the Atlantic. I am told by an officer of the
Lute Society that the use of gut for stringing lutes by professionals is increasing steadily. The leader
of one of the leading early baroque orchestras has recently told his violinists that they all have to use
all-gut stringing, including a catline 4th. The availability of smooth catlines and strings with an
antique unbleached look has enhanced acceptability. But the increased use of gut is mainly because
some musicians have been learning how to get the best out of gut, and others can hear the
attractions.

A ‘proper historian’ collects all of the available evidence, explores possible theories that can explain
it all, and picks, as best, the theory with the explanations that are most probable. There is an
important contribution made by skeptics like John, when they uncover new evidence that, by
needing to be explained with high probability, can change the choice of the best theory. Or they can
propose a new theory that is better than previous ones. But often such skeptics are also playing to
another audience: those people who insist on certainty for knowledge, and are very happy with most
interesting historical questions being unanswered, remaining mysteries. Some of these use
uncertainty is an excuse to reject any theories they don’t like, so they can follow and promote
whatever speculations they do like as possible historical theories. This activity tends to undermine
the the public’s trust in the disciplined contributions that proper historians make to the advancement
of knowledge.
On stringing of lutes (Comm 1807) and guitars (Comms 1797,8)

In Comm. 1807, the diameters of strings of several types of lutes were deduced from the sizes of holes in old bridges reported by Peruffo. Though the diameters of holes for strings with the same position on the bridge vary considerably, their averages make considerable historical sense as the best indications we have of original stringing. Concerning lutes in Renaissance tuning, on the assumption that the string diameter was 90% of the hole diameter, the average diameter of a 6th course string was 1.35 mm, and of the lowest string 2 octaves and a fifth below the first was 1.71 mm. It was assumed that the first string diameter was 0.43 mm, in the range of the thinnest type of string made according to the statutes of the Roman string makers. Making allowance for stretching after mounting, and its needing to be at higher tension than the others because it had to balance with paired strings on other courses, a virtual string that would have the same tension as a 2nd-course string would have a diameter of 0.34 mm. With these three diameters, their tensions were calculated using Praetorius’s Chorthon pitch standard and string stop. Then a power law in string tensions vs frequency was fitted. This interpolation procedure produced the other string diameters except for the octave strings.

In that Comm, the resulting table was modestly called ‘Conjectural stringing of Praetorius’s lute’. The information from Praetorius was not necessary for the interpolation, and it applied to the averages of all the information on late Renaissance lutes. Since the bridge hole size for the 6th course of the sole earlier Renaissance lute measured is the same as for the later lutes, we can expect that the same string diameters should apply. In this stringing, the tension on the 6th course is only 2% less than that of the high courses, implying that Mersenne’s rule of equal tension applied well enough to all courses except for the low basses. What was not remembered when that Comm. was written is that on p.79 of the Chapman translation, Mersenne wrote that the string diameter on the lute 7th course was 2/3 line (1.52 mm), the 4th course was 1/3 line (0.76 mm) and the 2nd course was 1/5 line (0.46 mm). The corresponding diameters deduced in the Comm. were 1.56, 0.76 and 0.45 mm. The agreement between this analysis of the bridge-hole evidence and Mersenne’s reported diameters is much closer than we could ever have hoped for (or can be justified by the precision of the evidence).

On p. 65, Mace indicated that larger lutes should have somewhat thicker strings than smaller lutes, and this has been quantified with some success as the tension-length principle, in which the string tension is proportional to the string stop. The bridge-hole evidence does not show this effect. The sample size is perhaps too small.

The Montesardo evidence on guitar stringing, as given in Comms. 1797/8 compares the string diameters of the second and third courses with those of a large lute. This implies that string diameters of large lutes are different from those of small lutes. According to the tension-length principle, the corresponding string diameters of a larger lute would be one semitone-step (6%) thicker for each two frets longer the string stop is than that of a smaller lute. Assuming that the string stop of Montesardo’s large lute was 5 frets longer than the usual lute, the string diameters would then be about 15% thicker than normal Renaissance lutes, which presumably were as shown in the Praetorius lute table discussed above. The guitar 2nd and 3rd string diameters then are 0.52 and 0.70 mm.

The approach in Comm. 1676, giving interpretations of guitar stringing instructions leading to probable diameters in each set, is continued here for Montesardo’s instructions. The above gives us the diameters of the 2nds (the same as the 5th octave) and the 3rds. The 3rds are one step thinner than if they were in equal tension with the 2nd, and we assume the 4th bourdon has the same relation with the 3rds, resulting in 0.88 mm. As in Comm 1676, we assume that the relationship between the octave and bourdon was the same in the 4th and 5th courses. We can expect that the number of steps between the 1st and 2nd is about 3, which is equivalent to an equal-tension relation. Then the 5th bourdon is 3 steps thinner than the 4th bourdon. The resultant stringing, from the first string to the 9th is: 0.44, 0.52, 0.52, 0.70, 0.70, 0.88, 0.44, 1.05, 0.52 mm.
TAPERED LUTE STRINGS AND ADDED MASS

Comm 1810 on tapered strings, paper 3, did not mention the small holes reported in some surviving lute bridges, which have suggested that gut bass strings required extra density and loading by metallic powders or salt solutions (M Peruffo, Lute News 56). Since the emphasis was on whole natural guts the only allusion was in two paragraphs ending section 6, and paper 2 had already discussed the merits of added mass.

The two areas have different technical and historical characters that are worth explanation. For taper, we know it existed, from Capirola and modern sheep, but it was not clear how it was used acoustically, and this has involved the research in papers 2, 3, and 6. In contrast, the suggested loading is inferred from bridge holes and the colour of strings in paintings, without written evidence according to P Barbieri (references as Comm 1810), but the acoustics is broadly clear (eg paper 2). The problem for taper was physical explanation, whereas the difficulty for loading may be finding some written evidence. For these reasons the two topics were best kept separate.

A shared problem is large-scale production, since the ancient use of whole guts may be a lost craft, and there have been modern difficulties with loading. Also, other sources showed substantial 2½mm holes for several lutes made around 1600 (eg R Lundberg). This crucial detail needs extraordinary certainty, from many aspects, of the maximum allowed string thickness. Red bass strings in paintings might be dyed and hence a contrast with paler thin strings, or even early metal windings. Red and blue dyes were used as a coding of quality, thickness, intended instrument and use, or for covering up discolorations (eg P Barbieri). The positions of the red strings are strikingly similar to modern metal-wound strings, but this might just reflect the thickness where better elasticity etc is needed. However, the loading was suggested for strings as early as 1550 and, looking at paper 3 just before printing, the sections on lutes with seven or more courses might have seemed incomplete. A brief constructive extension can be provided.

Paper 3 noted that the eleven course baroque lute had no need of frets or taper on say the lowest three basses. The next two or three basses might require the use of only a few lower frets. These strings could therefore be of similar or lower sensitivity to taper as the lowest bass on a six course lute. Higher strings using more frets, for all lutes, would depend on taper as analysed in paper 3 for the earlier strings recorded by Capirola. Next, if loaded bass strings had indeed been used then their mass distribution could, at best, mirror a natural long-range taper of early whole gut strings. It is more likely that this extra process would give a degraded or different tapering of mass. The intention of any such loading would have been a greater effective elasticity for a good tone etc, and to reduce bulkiness, which were mentioned in paper 3 as possible problems for thick unloaded gut. In these ways the analyses of taper and various angles of necks and bridges could be consistent with a use of loaded bass strings. There might have been a technical pressure, as well as the musical use, for the rapid addition of the intermediate strings from seven to eleven courses – to reduce the use of frets with less controlled tapers. One could even speculate on a gradual decline in using a tapering of mass, and it is worth seeing if string lengths are more equal in later lutes (eg R Lundberg). Metal-wound strings after perhaps 1650, might have been tapered or uniform with a gut core, but more uniform on any silk cores.

In summary, a resulting scheme might be a combination of (i) upper courses and octaves of tapered gut, which has historical basis but no modern use, with (ii) lower basses of tapered gut with loading, for which there is no known written record but some modern interest.

Some theory from paper 2 can be adapted, but it is worth noting that the acoustic relations and many other aspects apply equally to any form of added mass, such as over-wound wire. As a start, for a string with an unchanged frequency f, length l, and tension T, the diameter d will vary with average density ρ as 1√ρ. For example, a doubling of density reduces the required
diameter by a factor of 0.7. The volume of metal $v_m$ in the total volume of string $v$ needed for an average density $\rho$ is

$$(v_m/v) = ((\rho/\rho_g) - 1) / ((\rho_m/\rho_g) - 1)$$

where $\rho_g$ and $\rho_m$ are the densities of gut and metal. For example, $(\rho_m/\rho_g)$ is about 6.6 for copper, so that for a $(\rho/\rho_g)$ of 2, $(v_m/v)$ is a reasonable 18%, but a $(\rho/\rho_g)$ of 4 needs a large $(v_m/v)$ of 54% and probably a core with metal winding. The mass of metal as a fraction of the total mass is $(v_m/v)(\rho_m/\rho)$, and the two examples give values of 50% and 75%.

The possible variations of $f$, $l$ and $T$ that would be needed without extra mass on gut basses of a reduced diameter $d$ can be seen clearly by rearranging the equations for frequency as

$$f^2 f' = (T/d^2)/((\pi p) = c/(4p)$$

This holds for all the courses, with the terms containing $\rho$ for gut kept constant. The effects are similar to paper 7, sections 2 and 8. If $f$ and $l$ are fixed then strong reductions of $T$ as $d^2$ would be needed for basses. This is very sensitive to the suggested diameters, but tension decreases of order a half would be unacceptably large. At least the stress is unchanged which is useful for gut. However, $f / l$ could be increased, unusually implying a longer string and/or a higher pitch. This allows a smaller reduction in $T$ for the basses, but the stresses increase in all the strings, which could become a problem for a treble made of gut. If evidence for ancient strings eventually proved the use of loading, then this distinction would open up many possible variations of $\rho$. This has given useful freedom for designing modern strings, generally with metal winding, but it would be a further problem for deducing historical designs.

The criterion for elastic modulus was shown to depend simply on added mass such that a required modulus $E$ could usefully increase with the effective density. The practical effect is just an addition of mass on a given string or core to give a lower pitch string.

There are some further relevant points. Several minor problems could be reconciled, but the main issue is not settled easily by pure reason. By simple geometric effects, metal particles are difficult to spread evenly in long fibres, and might even tend to reduce elasticity and strength. Thick wire was already used for citterns etc, and also a fine wire for making decorative threads like miniature over-wound strings (see paper 2). It therefore seems uncomfortable at present to need reasons for starting with powder or salt solutions, and then for changing to over-wound strings with a first reported use around 1660. An alternative to not initially inventing wound strings might have been a conservative desire to keep a recognizable gut string. Red basses in paintings might result from strings with any added mass being coded by a dye, to avoid confusion with unlabelled normal gut of equal thickness. Metal powder might produce dullness, like a discoloration, so that bleaching unloaded strings could also help. However, 'red' also suggests cinnabar (mercury with the burning sulphur?), lead oxide, and then perhaps copper winding before any loading. Dowland (1610) and later Mace (1676) mentioned blue and green strings with light colours best, but red sometimes rotten. This suggests dyes or copper salts, and a survey of chemicals may be useful. More natural yellow and brown strings were often rotten, and this may have resulted from a low amount of sulphur treatment and bleaching effect.

The hard to come by 'Pistoy' basses favoured by Mace were commonly dyed, with a deep dark red and have been seen as possible loaded strings. He considered these to be thick versions of the 'Venice catlins' for 4th and 5th courses, and this may put upper limits on a degree of loading. For example, a deep bass C would need a $(\rho/\rho_g)$ of 4 in order to have the same thickness as the higher c, so a $(\rho/\rho_g)$ closer to 2 would be more likely. However, Mace and also Burwell said 'Lyons' were the normally available basses, and these appear to be thick, naturally coloured, twisted gut. But then the thick Pistos might also be pure gut, and the red dye effectively a code for superior and stylish basses. The qualification 'commonly' may imply that some Pistos were plain. These two types of string were not specified as 'clear', but they were likened to a thinner clear type. Some of the strict logical deductions from these writings may be tenuous and not firm evidence. However, it seems that as late as 1676 some lutre used thick unloaded basses.
Small holes might have been supplied as standard, so a player could choose where to use octaves, or wound, or loaded strings, and then enlarge other bass holes as required. This would be sensible practice, and it effectively happens today, but changing from thick to thinner strings can be a problem. Other issues are the safety of closely spaced holes in later slimmer bridges; and the way of tying thick strings to a bridge, which in paintings is obscured by a right hand. Records of hanks for packaging may be worth examining. The example in paper 3 of say 96 trebles giving the same size of hank as 6 thick basses might require 12 or more thin loaded basses. This depends on the diameter squared of many strings rather than the high accuracy of clearance for a single hole, with allowance for shrinkage, distortion etc. Other glimpses into the records (P Barbieri) are Roman 'settima' for harp, or maybe a seventh course on a lute but 1638-54 may seem late for a single extra bass string; and around 1640 almost 400,000 lute strings of many types were sent from Rome to Lyons, sufficient for 20,000 eleven-course lutes.

Even for strings with no added mass, there are substantial differences between ancient gut strings and modern versions. The original makers used whole tapered tubular guts, of many types from sheep etc, twisted and combined together in various ways to give a range of diameters. Attempts to revive this craft have problems with trapped air bubbles, and 'whole' often refers to guts that are split and no longer tubular. A more frequent method uses several narrow cut strips of gut twisted together. As well as losing taper, the internal geometry and properties will differ, with many free edges that can interfere, reduce lateral structure, strength, and maybe elasticity. This can be visualized from the twisting process, but analysis would be difficult. The ways of building thicker strings, including ropes, from basic twisted units may also be different, and chemical treatments have changed (eg paper 3). Next, the modern 'rectification', a parallel grinding between wheels, deliberately produces long-range uniformity and a generally smoother string. However, it is recognized that the surface gut is damaged, in a complex way down each strip. A limited grinding retains some irregularities, like a rippled pond after the analogies in paper 3. The process removes any long-range taper and would damage ancient tapered whole gut designs, which were probably wiped by hand with horsehair etc. Modern processed forms of wood, stone, leather, textiles, food and drink may come to mind.

There is a problem with any short, simple, material descriptions or assumptions about lost ancient strings. In the usual absence of a complete recipe, it may just be impossible to recover original methods and designs. Additional mechanisms from old lutes and writings, as found with taper, may help quantitatively and also in understanding ancient approaches and mentality.

**An addition to the elastic modulus criterion**

Paper 2 part 3 should have included a more detailed treatment of the elasticity required for a string. This was used for part 2, and also in papers 3 and 7, and the derivation can be seen from the relations in part 3. The increase in stress $\sigma$ on plucking or pressing down to a fret was illustrated as 2% of $\sigma_0$. This can be generalized to say $s\sigma_0$, and with the approximation of $E_0\varepsilon_0$ for $\sigma_0$, the modulus needed at the initial stress $\sigma_0$ is given by

$$E_0 = 4\rho f^2 l^2 \left(s/\varepsilon_0\right)$$

This concentrates on the modulus at the precise point of interest, but requires further acceptable values for $s$ of less than say 0.02; and values of $\varepsilon_0$, which can be calculated individually for each string from an $\chi_0$ of order 0.1mm. The general guide of equal initial strains $\varepsilon_0$ for all strings gave a condition involving the whole curve up to $\varepsilon_0$, but no need for $s$ or $\varepsilon_0$. Both conditions have the same main dependence on $\rho f^2$, and they are similar for the simpler cases such as linear stress-strain curves. The extension can deal better with general forms of $\sigma(\varepsilon)$ where the slope or $E$ increases with strain. In some practical cases $E$ might change rapidly over the small interval of strain $\varepsilon_0$. This would need more accurate values of $\sigma_0$ and $\varepsilon_0$, with a loss of simplified formulae.
Review: *A History of the Lute from Antiquity to the Renaissance*  
by Douglas Alton Smith (The Lute Society of America, 2002)

This is the most comprehensive history of the lute before the baroque that has yet appeared. To tell his story, Smith has very effectively distilled the vast literature produced by previous researchers on the lute's music, players and makers and their political, social and cultural environment, and added some of his own research to fill in gaps.

The story starts with the ancient Greek lyre and kithara. There is probably a connection in historical development between these instruments and the lute (they have in common plucked gut strings that drive a box resonator), but that connection has not been researched. Smith includes them because of myths about their involvement in the origins of the lute. These Greek instruments were supposed to have had the power to control the emotional response of listeners (which musicians throughout the ages have always craved). This has led many early writers on the lute to claim that ancient Greek inheritance. Smith's is a book about the lute, so he does not mention that this inheritance was also claimed by many other instruments at various times, including the fiddle, the cittern, the viol and the lira da braccio.

Next discussed is the Arabic 'ud, which Smith calls a lute. I agree with his objection to the terminology of the Sachs & Hornbostel instrument classification system, which uses 'lute' as a generic term for any stringed instrument with the strings parallel to the soundboard. Whether a term is needed here is questionable. But he also objects to the term 'long lute' which covers the ancient pandore, the colascione, the bouzouki and the saz. Here a term is needed but no alternative has been offered. He doesn't offer a general definition of a lute, but suggests that early lutes had a shorter neck than the body and had a bent-back pegbox.

The Arabs adopted the lute early in the 7th century from the Persians. As for earlier ancestors, Smith quotes a study of ancient Indian music iconography which suggests that the earliest oval instruments with hollowed bodies that narrow to become the neck were in the area of Gandhara around the beginning of the first millennium. These instruments appear to have had lateral pegs (inserted from the sides), which distinguishes them from the early fiddles (with pegs inserted from the front or back), which emerged at about the same time in the same part of the world. I suggest that the direction of peg insertion is a better criterion for classification as a lute than a bent-back pegbox. Smith included illustrations of two surviving 8th century Japanese lutes (biwas), and only one has a bent-back pegbox.

The lute adopted by the Arabs had 4 courses tuned in fourths (with nominal pitches of $A \ d \ g \ c'$), and had frets. There were two types, one with a wide rounded body and wooden soundboard, and the other (a proto-gittern) with a narrow club-shaped body having either a wooden or leather soundboard. Within a century of its adoption by the Arabs, the lute became the most prominent instrument in their culture. It was played by nobles, professionals and slave girls, mainly to accompany songs. A 9th century treatise specified double frets of graduated thickness, scordatura tunings varying the lowest string pitch, and thumb-under finger-plucking technique. Smith does not mention quill or plectrum plucking but an illustration he reproduces (the earliest, from the 8th century) shows it played with what appears to be a quill.

An important early 9th century professional player (formerly a black slave) was Ziryab. He introduced a lute that was the same size as a normal lute and made of the same wood, but had a third less weight. When we look for evidence for when the lute back changed from being carved to being constructed from glued staves, this is an excellent candidate. Unfortunately, Smith doesn't make that speculation, yet he recklessly speculates that another of Ziryab's innovations, which was to add an
additional red course in the middle of the original sulphide) or lead oxide, as suggested by Peruffo.

A fuller quote from the same source used by Smith was given by J. D Roberts in 'The Lute: Historical Notes', *The Lute Society Journal* (1960), pp.17-20, where it is clear that 'in the middle' meant just that, between the 2nd and 3rd courses. The four original string courses had different colours representing aspects of the body and the emotions associated with them: the 1st (made of silk) was dyed bright yellow for bile, the 2nd (made of gut, as were the others) was red for blood, the 3rd was undyed and thus white for phlegm, and the 4th black for the black humours of the body. The new course was red for the soul, which co-exists with the blood, so it sits next to the 2nd. There was no indication of thickness for this new course given, but Smith gives evidence that it was tuned to the 1st. It is much more likely that it was tuned an octave below the 1st (a tone below the 3rd), acting as a drone for the first two courses, than an octave below that (a sixth below the 4th), as Smith suggests. There would then not be any practical reason for using a loaded string (if indeed, loaded strings were ever used). The colour of the added course was the same as the 2nd, which was obviously dyed and couldn't be loaded.

Incidentally this source also said that Ziryab introduced the use of eagles' talons instead of the wooden plectrum which was formerly in use. Another interesting point mentioned in that source by Roberts was that the string sizes of adjacent courses amongst the original four were related by a factor of two. It is very unlikely that 'size' meant diameter since that would imply that a 4th would have almost three times the tension of a 1st. It most probably meant 'bulk' (i.e. volume), which would imply equal-tension stringing.

The Arabs conquered most of the Iberian Peninsula early in the 8th century, and in the next century (with Ziryab playing a significant role), that area became a thriving centre of Arab culture. When a few Spanish cities were retaken by Christians in the next few centuries, the new Christian rulers retained many Arab musicians, but they apparently discouraged integration of the two musical cultures. Thus the Spanish only played a minor role in the spread of the lute in Christian Europe. The major role was played in Italy via Sicily.

In the 9th century, the Arabs conquered Sicily and brought in settlers. By the end of the 11th century, when Norman knights conquered the island, most of the population was Arab. The new Latin rulers kept the peace and prosperity with religious and cultural tolerance and a considerable amount of cultural integration. Within a century, lute players were accompanying singing in Italian, and this practice (including the lute) gradually spread to Tuscany and the rest of Italy. German family connections of Sicilian rulers in the 13th century made the lute known there as well. During the over a century and a half of this set of rulers, Arabs did well at the court and in the army, but not otherwise. Many of the disaffected emigrated while others rebelled, leading to their expulsion.

Late in the 13th century, the lute and the gittern became known in England and France, but only the gittern spread to widespread use. Lutenists were in the employ of the English kings from then onwards, but it became popular in that country only centuries later. The lute was known to French poets, but it was not considered a serious alternative to the harp by the nobility until late in the 14th century.

During the 14th century, the lute spread to be a major instrument throughout Italy (played mainly by minstrels called *giullari*, who were poet singers) and southern German-speaking areas, (often played by students for courting). Its image in Italy changed from being that of an exotic oriental instrument, to become that of the reborn lyre of humanist poetry, which presumably was recreating the ancient Greek and Roman culture. The lute was still played extensively in Spain, and this continued till near the end of the 15th century.
The 15th century saw the lute grow to be the most popular stringed instrument in Italy and southern Germany, and to be an instrument well known and played some in the rest of Europe. Though it still was largely used for vocal accompaniment, it commonly played with another instrument, which in the pictures was usually a gittern or a harp in the first half of the century, and another lute in the second half. In these duos, one was the leader and the other was the accompanying ‘tenorista’.

Smith doesn’t try to explain the apparent inconsistency between the above interpretation of the pictures and the usual description of the most famous virtuoso of the two-lute period as a gittern player (Pietrobono del Chitarino). Smith reproduces a 1457 medallion showing Pietrobono playing what appears to be a small lute. During the 15th century, some gitterns acquired a 5th course as the lute did, and I suggest that the term ‘gittern’ broadened then to include any instrument that played like a lute and was descant size (see below) or smaller.

The lute was played with a plectrum throughout the 15th century. Up to about 1430, that was the only way it was played and frets were rare. From then on it was often played polyphonically with plucking fingers, and the use of frets grew. That was also when lutes with 5 courses first appeared in the European tradition, and its popularity grew rapidly. A common performance was for the leader of a duo to play a very ornate version of the descant of a Burgundian chanson with a plectrum, while the tenorista accompanied with the other parts plucked by fingers. A sixth course first appeared around 1475. Smith suggests that the Germans were the first to play all the parts of polyphonic compositions on the lute. He includes many biographical details of the famous Italian, German and Burgundian lutenists of the time.

Before going on to the 16th century, Smith inserts a chapter called ‘Lutes and Lute Making’. It starts with a cursory discussion of design variations, followed by excellent discussions of the major Renaissance lute makers and their workshops. A couple of pages introduce the cittern, bandora and orpharion, and then another couple of pages discusses the invention of the chitarrone and archlute. In his 1979 JAMIS paper ‘On the Origin of the Chitarrone’, Smith reported that Piccinini (1623) wrote that he invented the extended neck in the 1590s by adding it to an ordinary lute to create the archlute, after which others added a similar long neck to a previously-existing large lute in reentrant tuning called a chitarrone. Smith, without offering any evidence inconsistent with that claim, assumed that Piccinini was mistaken, and that the long neck was there on the chitarrone when it was invented by Naldi in the 1580s. Naldi was asked to explain the chitarrone, which he did with a diagram, and Smith felt that it was much more likely that the diagram was of the extended neck rather than a tuning diagram. I and some others were of the opposite opinion, that a long neck can be seen without a diagram, while tuning cannot be seen and needs a diagram. Smith felt justified in rejecting Piccinini’s evidence since all of the other evidence was consistent with his assumption.

When Smith’s article appeared, I recognised it as a blatant violation of the principles of scholarship, and I wondered how the paper got past the referees. In my training as a scientist, I learned that proving a theory to be true is impossible, but that just one clear piece of evidence that cannot be explained by it (with reasonable probability) proves a theory to be false. The theory that Piccinini’s account is correct is not falsified by any contrary evidence, but Smith’s theory that Naldi’s original chitarrone already had the long neck is falsified by Piccinini’s evidence.

I did not realise until comparatively recently that Smith’s theory about the origin of the chitarrone was quite acceptable to most arts historians. The principle of theory falsification is not recognised. The model for proof is that of the courts in legal proceedings, where it is expected that some pieces of testimonial evidence will be deliberately misleading, and so they can be rejected in the light of the other evidence. What the scholars do not consider is that deliberately misleading historical evidence is very rare (and is quite obvious when it occurs), so individual pieces of evidence deserve more respect than they give it. Judgements about the truth of a theory are usually based on how well it fits
in with expectations, and how impressive is a quantity of evidence that it is consistent with. Without falsification to eliminate inferior theories, knowledge becomes rather more a matter of theories in fashion than of care in ensuring fidelity to the evidence available. This fits in with the post-modernist approach, where truth is no more than what each individual believes.

There are non-post-modernist arts historians who believe that theories should be proven to be true. Realising the limitations in objectivity of most theories presented to the field, they avoid offering them, concentrating only on the collecting, sorting and useful presentation of evidence. To maintain fidelity to the evidence, they insist that theories are only valid if they are overwhelmingly supported by evidence. Unfortunately, their kind of scholarship wastes much evidence which could offer understanding of very many important historical issues, which to them remain as mysteries.

Fortunately, there are not many other examples of Smith rejecting early evidence because he believes he knows better.

There were standard sizes of Renaissance lutes, and Praetorius gave seven size names and the nominal pitches of their first string of each. Smith presents his own association of Praetorius's size names with vibrating string lengths, as a guide for identifying surviving instruments. His name 'chamber lute' for the most popular size (with a g' nominal pitch of the first string) must be questioned. Praetorius's name for that size was ChorLaute in the drawing, Gemeine Alte Laute in the tuning tables (Alte meaning 'old', as compared to the new ones with long necks) and Recht Choristoder AltLaute in the text (Alt meaning 'alto'). There is very good reason to interpret the Chor in the name to have implied that it was at the Chorthon pitch standard that was a tone lower than his more usual Cammerthon (chamber pitch) standard. Using the name 'chamber lute' for that size is thus misleading.

Smith's name apparently comes from his Appendix II, which is a list of the lutes in the 1566 inventory of Raymund Fugger's collection. The entries include 133 lutes, of which 73 were in sets (1 set of five, 15 of four and 6 of three). We cannot be sure about which sizes were included in each set. Of the remaining 60 lutes, there were 2 of octave size, 1 small descant, 5 descant, 3 chamber, 25 with no size name, 5 tenor, 3 large, 14 bass and 2 contrabass lutes. All of Praetorius's names are represented, but the 'chamber' and 'large' names are not included in Praetorius's list. The number (3) of chamber lutes is too low for it to be the usual most popular size with a g' first string. The 25 lutes without a name are much more likely candidates. We often drop size-defining adjectives when the usual size is implied. My guess is that 'chamber' was used when the size was uncertain between the usual and descant sizes, and 'large' was used when the uncertainty was between tenor and bass sizes.

While most of Smith's description of the design and construction of Renaissance lutes is derived from Robert Lundberg's Erlangen Lectures, his sizes of the smaller lutes are an improvement over Lundberg's, as compared with my calculations of the middle of the string length range from the string physics, made for each of the two mentioned pitch standards. These string lengths are shown on the following table:

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1 The assumptions are: the string tension of the lowest string was proportional to length, the top of the string length range had the same string stress as in his depicted lute, the bottom of the range had the same string inharmonicity as in the depicted lute (the length shown above is the average of the top and bottom of the range), the open-string pitch range was 2 octaves and a 4th, Cammerthon was a tone higher than Chorthon and the notes were related by equal temperament.
The entry in the Fugger inventory for one of the chamber lutes includes that it came with 'ivory plectra'. This is a welcome correction to Smith's previously published version of the list where he surprisingly translated *Negeln* as 'pegs'. It seems that there was considerable use of finger picks to play the lute in the 16th century. Smith quotes a 1524 report that one member of a lute duo, who was Francesco da Milano, played 'using two silver thimbles, inside which were two small quills'. He gives two other reports of this practice.

Around 1500, there was a great increase in the writing of lute music in tablature. This was enhanced by the development of music printing. At this point in the book Smith writes 'Because the music created for an instrument is the primary reason for our interest in it, the focus of this study now turns to the music and its composers.' This is just what he does in the final two-thirds of the book, in chapters devoted to Italy, Central and Eastern Europe, France and the Lowlands, Spain (for the vihuela) and England, covering from 1500 to as long as the Renaissance style persisted.

Smith provides good summaries of what is known about the biographies of the major composers and performers (including those whose compositions have not survived), poets who contributed to lute songs and influential humanist philosophers. The major printed and manuscript collections of music are well surveyed in the usual ways that musicologists discuss it, concentrating on the genres, forms and styles used by individual composers, how they developed and who copied what from whom. Compositions without a known composer are mentioned but not discussed. Also the quality of the music is usually judged, which is 'criticism' which can only be according to criteria of modern 'musicality', and so has nothing to do with music history. This is nevertheless very useful to lute players wanting to explore repertoire and to musicologists interested in the relationship between the music for lute and for other media. The readers of this Quarterly are more interested in what the musicologists are less sensitive to, such as the ways that the instrument developed, and how the music was originally transformed to sound in performance. Consequently, only a few bits from the remaining chapters will be discussed in this review.

Barbetta's book *Novae tabulae* of 1582 was the first publication that specified a 7th course a fourth lower than the 6th, making the open-string range two octaves and a fourth. The 6th course had previously been tuned down a tone at times (and occasionally a 7th course a tone lower than the 6th was fitted), but this signified a much greater expansion of the open-string range than was previously used. Every new development has its detractors, and Vincenzo Galilei, in a book published two years later, disapproved of the use of more than 6 courses. Reymann's book *Notices Musiciae* of 1598 was for 8 courses with the 8th tuned down to C, making the open-string range two octaves and a fifth. This range is the maximum known on a lute with all strings known to be on the same nut. Smith
didn’t notice the anomaly of a range of one tone greater than this in the accordes avallée tunings used by Francisque in his Le trésor d’Orphée of 1600. I suggest that these tunings should be associated with the few lutes with two bent-back pegboxes that survive from that time.

Smith mentions a problem of tempo in performance of some lute duets by Terzi, where one part is ‘so profusely ornamented with diminutions as to be virtually impossible to play at what for the other part is a logical tempo’. This is not a historical problem. To musicians and musicologists, the word ‘logical’ often means ‘makes sense’, where ‘sense’ has everything to do with acceptability to modern musical taste, and nothing to do with logical deduction (which in historical studies should relate theories to historical evidence). The historical evidence on tempo history has been analysed (with proper logic) in my two papers in Early Music in 1996, where it was shown that Renaissance musicians were happy with much slower tempi than is considered ‘logical’ today. This problem is relevant to the common opinions, repeated by Smith, that advanced pieces of dance music are not danceable and that galliards on melancholic themes are ironic. Modern attempts at reproducing early dances are as much influenced by modern tempi as musicians and musicologists are.

Smith shows convincingly how the Christianisation of Spain and its culture by the Inquisition rejected the lute as a Moorish instrument, leaving the vihuela to perform the role that the lute played elsewhere. The vihuela was a 15th century Spanish invention that the Moors apparently contributed to (when bowed, it was held like the Moorish rebab), but it was not tainted as part of Moorish culture. Smith notices that vihuela sizes mimicked those of lutes elsewhere in Europe. I would like to add that there were differences, namely that there were no small octave or small descant vihuelas, but there were guitar equivalents. The usual 4-course guitar apparently was the same size as the small descant lute. The Spanish descante was apparently the same size as the descant lute. It would be called a vihuela when it had 6 courses, or it could be called a guitar when it had 5 courses (the third in the tuning was between the 2nd and 3rd courses). The Diaz guitar in the Royal College of Music is one of the latter.

At the end of the book is an epilogue which very briefly summarises the history of the lute after the Renaissance. He suggests that the popularity of the lute was intimately associated with the popularity of humanism, this being an explanation for both its rise and fall. There are two Appendixes other than the Fugger inventory previously mentioned, one giving a 1581 inventory of a Tieffenbrucker workshop and the other the 1562 regulations of the Lute-Makers Guild of Füssen.

This book is very well produced, with 26.5 x 20 cm page size, many illustrations (a few in colour) and musical examples (almost all in staff notation), good notes at the end of each chapter and ending with a good bibliography and index. Smith has surveyed an enormous amount of evidence and of the writings of others relevant the history of the lute, and with usually good judgement, put together a very competent, comprehensive and readable story.
Review: Die Renaissanceblockflöten der Sammlung Alter Musikinstrumenten des Kunsthistorischen Museums (The renaissance recorders of the collection of old musical instruments of the Kunsthistorisches Museum in Vienna) (No. 3 of the 'Sammlungskataloge des Kunsthistorisches Museums', published by Wilfried Seipel).

This catalogue, published by the Kunsthistorisches Museum in Vienna, was anticipated by many people, especially by woodwind makers who want to know more about the 43 renaissance recorders in the Sammlung alter Musikinstrumente (collection of old musical instruments). That is about 20% of all renaissance recorders found in collections world-wide. The instruments in Vienna vary in sounding length between 219 mm for a Diskantblockflöte (in f') (a ‘sopranino recorder’ in modern terminology) to 1692 mm for a great bass recorder, 3 octaves lower in pitch. One of the Viennese recorders (inv. no. SAM 135) became famous because it was the first of only a very few original instruments for which the discovery was made that they could be played with fingerings published in 1535 by Sylvestro di Ganassi in his Opera intitulata Fontegara, La quale insegn a sonare di fiauto. But surprisingly, we find in the new catalogue two other (and longer) ‘Ganassi recorders’: SAM 138 and SAM 147, of which instrument the upper section was completely renewed.

Ganassi was not a recorder maker himself (as far as we know), the instruments in Vienna are stamped with letter combinations as ‘AA’ (probably by Sigmund and/or Arsaziusschneider from München or Nürnberg), ‘HIERS’, ‘HIES’ (both attributed to members of the Bassano family), ‘CS’ or a single ‘M’. Other recorders are anonymous or stamped with a crown, an apple, a clover leaf, or several variations of the ‘silk moth’. These last marks have often be described as ‘rabbit feet’ or ‘hare feet’ and depicted in literature as ‘!!’ (exclamation marks), but since the research of David Lasocki we now know that there is a relation between these marks and the Bassano family, instrument makers who worked in Venice and London. The silk moth occurs in their coat of arms and points to the silk trade in which the family was involved. Adrian Brown, who is an instrument maker himself and who did the measurements on the recorders in Vienna, is surprisingly reluctant in acknowledging the silk moth theory (see end note 22 at p. 121 of his introduction) and says that he accepts the assignment of the recorders to members of the Bassano family, but that he still is preferring to refer to the stamps as ‘!!’

The main part of the book is the proper catalogue section with measurements, descriptions and high quality photos to the instruments. This section begins on p. 125 with an introduction to the long list of abbreviations, symbols and technical terms, but is such a pity that this list is only explained in German language. Only a part of the symbols and technical terms are elucidated by schematic drawings. Almost all measurements of the instruments are given in tables, also those of the exterior (such as the thickness of the turned profiles and the dimensions of window and labium), and you really have to master the symbols and abbreviations in the tables to understand everything. Woodwind makers who have no or only a little knowledge of the German language will have real problems, also because you need the descriptions rather much to understand - for instance - the information about the condition of the instruments. There are no summaries in English or other languages as well.

Another disappointment for me was the fact that for the investigations the blocks have not been removed; the motivation was that too many blocks were apparently not original and windways damaged. That means that there is only very limited information of the shape of the windways (such as the curvature in N-S and E-W directions) and the ‘step’ (the effective height of the windway). Some photos give us a clue of the shape of the opening of the windways, but such photos are missing for the bassett and bass recorders, which have a cap over the top end. A peculiar thing is that of most recorders detailed information is given of the size and shape of the chamfers (at the South end of the windway), and that of some other instruments there is also a measurement of the size of the step and even about the general shape of the windway (such as flat in N-S direction). I can’t understand how this information is
obtained without removing the block. It is like investigations, but there is only limited information in the book about that procedure.
The bore measurements are published as graphs and in tables. The bore diameters are given from a point close (2 mm) to the blockline; in a separate Appendix, the same tables are published again, and more convenient for recorder makers, in relation to the top end (North end) of the instruments. To understand the dimensions of the caps (such as the inner space, the 'air chamber' when the caps are put on the top of the recorders), you have to go back to the tables at p. 116.
The descriptions of the instruments in the catalogue section give interesting and important information about the history, the relation to other instruments in this or other collections, the preservation, the shape and condition of the tone holes and sometimes about the intonation of the tones. The descriptions end up with a general conclusion about the state of preservation and the importance of the recorders.

Is it possible to make a copy of one of the recorders in the book, using the measurements, descriptions and photos? Apart from the fact that quite a lot of the described instruments are damaged or not in original condition (shortened, new parts), I have to say: yes, you can make a copy with the presented information. But then you have to be an experienced instrument maker who has a clear idea how a renaissance recorder must sound and must be played. Such experience can only be obtained by experimenting, by designing recorders in various sizes ('scaling'), varying bore profiles, tone hole positions and undercuts and you must also know about all the variables of windway, block and labium. This new catalogue will help the experienced maker to find ways to execute such experiments, especially in relation to bore profiles and tone hole positions and sizes, but not, or hardly so, in regard of the blocks and windways.

About the pitch and other 'musical' aspects: the recorders have been blown with a wind machine and for the major tones (or fingerings) not only the pitch is given (as deviation in Cents) but also the air pressure (in Pascal and in mm H2O). This is an objective way to do such measurements, and it also shows us that many of these renaissance recorders have intonation problems. It is however difficult to use or reproduce this kind of pitch measurements if you are making a copy. And some more explanation (for instance by depicting one or two photos) should have been a welcome addition to understand the procedure of pitch measuring.

About the other aspects: the sound, the stability, flexibility (and so on) of the tones you will not find in the catalogue no information at all. Not in the descriptions, not in the three Beiträge ('contributions') by Beatrix Darmstadter (who works as a curator in the museum), nor in the contribution by Adrian Brown. I was hoping that one of them was making at least some general remarks about those musical aspects, just because it is so hard to get an idea. The only remark I found is that some instruments could be used for solo playing (i.e. the Ganassi recorders), as well as others were more likely made as consort instruments. The Ganassi recorder SAM 135 was blown (with the wind machine) in the lower register with pressures about 2 times stronger than SAM 133 (which has the same pitch, the lowest note being g# at a= 440 Hz), but there is no explanation for this (nor in the introduction, nor in the descriptions) in relation to the different sound or playing characteristics of these instruments.

About the three Beiträge by Beatrix Darmstätter: the first of her contributions deals with the provenance of the recorders. Most of them were collected by Erzherzog Ferdinand von Österreich-Este (therefore the term Estensische Kollektion is used) at the Catajo castle (near Padua, Northern Italy). The second contribution by Darmstätter is titled (translated): 'The recorder in not-musical sources of the Renaissance'. This is a long article (40 p, 219 notes) about the mythical origins of the duct flutes, the iconography, the recorder in literature and philosophy, but the main part of this article is about the recorder (or flute) in the works of the Musikgelehrten (scholars in the field of music) in Renaissance and early Baroque. Systematically we are told what Johannes Ciconia, Franchinus Gafurius and Sebaldus Heyden have written about music and wind instruments, and what Johannes Tinctoris,
Sebastian Virdung, Johannes Cochlaeus, Martin Agricola, Hieronymus Cardanus (he very extensively), Michael Praetorius, Marin Mersenne, Sylvestro di Ganassi, Phillibert Jambe de Fer and the Dutchman Gerbrand van Blanckenburgh had to tell us about recorders (the instruments and how they are played) in particular. It is an interesting article and it is good for both players and makers of the recorder to read it thoroughly, to increase their knowledge of the various historical aspects of their instruments. But why did Beatrix Darmstädter write in the title that these sources are 'not-musical'? In her third contribution, Beatrix Darmstädter deals with the esthetical and technical parameters of the recorders in the collection of the museum and with the question which of the instruments were made together as a consort. This in relation to the four fitted cases in the collection, in each of which 4 to 8 instruments could be kept; it is obvious that the recorders with the 'HIERS' stamps all fitted in one of these cases (SAM 170) and were made as one Stimmwerk.

While Darmstädter is explaining the technical parameters, her contribution is rather much overlapping the fourth contribution, written by Adrian Brown, where he is telling the reader about his approach as woodwind maker to the instruments. As a result, some aspects as bore profiles and tone hole undercuts are now explained twice. To understand the construction of the caps on the basset and bass recorders, you have to read both articles by Brown and Darmstädter and see the photos in the catalogue section. That is not so easy for the readers, also because Darmstädter's contribution is not very conveniently arranged: most of the information is given without headlines or other indications that she begins with a new subject. The register at the end of the catalogue isn't a great help; for 'undercutting' (Unterschneidung) there are over 30 references, in fact for each time that the word occurs somewhere in the text. What I am also missing, is a general table with the most important information of the instruments: inventory number, type and pitch, size (length and sounding length), most important aspects of the state of preservation (playable, damaged, changed etc.). Such a table should be a great help to get a better survey of the instruments.

The alto recorder SAM 132 (not a Ganassi type) with the graphs of the bore of this instrument
Reading the contributions of Beatrix Darmstädter and Adrian Brown, it became clear to me that both authors were very eager to share with the reader their enthusiasm and knowledge about the instruments. Some of their observations are not so important, such as what Darmstädter writes about the proportions of the fontanelles of the longer recorders, or about the regularities (and irregularities) in the distances of the tone holes on each instrument. Brown is coping with the simplified ideas of many people who describe the bores of renaissance recorders just as 'wide, with constrictions'. In several graphs he makes clear what the variation is in bore profiles of the instruments in Vienna, and that there are several types of constrictions. Unfortunately, he gives no definition of the term 'choke bore', which is so often used - without explanation - by instrument makers. Both authors make a statement that the metal parts of the recorders (keys, crooks) were their most expensive elements. But they do not give examples of this theory (are there any bills?) and I was wondering about that, because making a key must have been not a very difficult job in the 16th century. No specialist tools had to be used, and I can not imagine that brass was an expensive metal to buy. About a century later, in 1685, Richard Haka sold a 'S' (crook) for one and a half guilder to a bassoon of 42 guilders (and a bass recorder of 26 guilders). You can't say that such a crook was a relatively expensive part!

So, at the end of this review, it so looks that I am rather critical about several contents and editorial aspects of this catalogue. Indeed, I have some critical remarks. With some better arrangement of the information and coordination between the authors, this catalogue could have been better accessible and some duplication of information avoided. And I am missing just those bits of information (about windways, sound and other playing characteristics) which are the most rarest in the literature about renaissance recorders. But it is and remains an important catalogue, which should be studied by all people who are involved in making or playing/teaching renaissance recorders. They will have quite a job to combine all bits of information so that they can use them some way, some time. But that is also a good thing: this catalogue invites us to do some study, to start with experiments, to look with other eyes to such important instruments, which were made in such an interesting period of our history.

It is becoming more and more difficult to play historic woodwind instruments (and especially the very old ones, as renaissance recorders are). There is a paradoxical effect of publishing a catalogue such as this one: a good and understandable reason that the museum did this good job, is surely to avoid that the instruments will be measured (and played) again and again; but the publishing will also arise the curiosity of the reader: he or she wants to know eagerly more about the described objects.

Key construction and some measurements of the bassett recorder SAM 157 (the original photos in the catalogue are in full colour).
SAM 157 Fig. 5: Klappe.

SAM 157 Fig. 6: Maße der Fingerauflage.