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FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS

Honorary Secretary: Jeremy Montagu. 171 Ifley Road, Oxford OX4 1EL, U.K.
Another week, and that's it, the end of fourteen years at the Bate. They've been a good fourteen years, the job I'd wanted for years, ever since that one year at the Horniman back in 1960, which a few of you may remember. I'd like to thank many of you for your support, for drawing our plans, buying our plans, coming to our Weekends, and so on. My only disappointment is how few have become Friends of the Bate Collection. I thought that anyone who had benefitted from us would be happy to support us and to give something back, especially those who make instruments based on ours. There's still time to do so, if any of you wish to. I'm sure that my fellow curators in other museums will join me in this, in hoping that all who benefit from our collections will support us. Most museums have a Friends or a similar support group which help in all sorts of ways, fundamentally by providing things that the museum's own funds can't cover. Obviously by buying instruments, often by subsidising publications (Bate Friends have paid for several postcards), sometimes by covering a restoration that costs more than the budget can cope with (in our case the restoration of the Shudi & Broadwood harpsichord and the Astor & Horwood square piano were both paid for by the Bate Friends), sometimes by providing extra invigilation (the Ashmolean Friends do this, thus keeping galleries open that would otherwise close for lack of staff), and above all just by being there and giving that all-important feeling of support and appreciation. There are so many more ways that Friends can help, so do please bear them in mind at any museum that helps you.

The new Curator at the Bate is Dr Hélène La Rue, as I told you last time. Unfortunately, I still cannot tell you who her assistant will be; the Faculty has been almost unbelievably slow in advertising the post, though they should, I think, be interviewing later this week or next. Depending how soon whoever is appointed can leave his or her present job, there may be a hiatus and it may be necessary to ask people to be patient, especially when they arrive, as some do, without an appointment and ask to examine an instrument. It should be fairly obvious that this is a chance — there may be nobody there with authority to open a show case — the instrument may be away being repaired or played — there can be all sorts of reasons for being unable to grant access, and yet it happens all the time. Do please make an appointment when you want to do anything more than look at things through the glass, and even then there can be problems — perhaps today is the local Saturnalia and the museum is shut; ours is the Wednesday of 9th Week of Trinity Term and the whole University shuts down for the honorary degree ceremony and Vice-Chancellor's garden party, and unless you have written in advance you may find that you have come all the way from Hawaii and the museum is shut.

Not that the above is all that relevant to a FoMRHI Bulletin, but it's good to get some of it off my chest! Now to business.

**SUBSCRIPTION RENEWALS:** It's time to renew again. Rates are the same as last year; our new printer is enough cheaper than the old one that we can go on holding them. The basic subscription, which covers surface mail worldwide is: £10.50. For airmail to Europe add £1.50, making it £12.00; for airmail outside Europe add £3.00, making it £13.50. If you pay by
personal cheque in anything except pounds, please add the equivalent of at least £5.00 (for example $8.50) to cover conversion costs. If you sent a dollar cheque for $21.00, which would translate to about £13.50, we would only actually get about £8.50, and as a result, you would only get three quarterlies, instead of four, and those by surface instead of by air! But Euro-cheques made out in pounds are OK and so are bank drafts in sterling — you pay the conversion costs with these, so don’t add the £5. As always, if you can add a little to help those in countries where currency conversion is either impossible or ludicrously expensive, it’s much appreciated.

There is a renewal form in this Quarterly. Please send it back, with your cheque, to Barbara Stanley, 21 Broad Street, Clifton, Beds SG17 5RJ. But first look at the back of it. A number of people are already in credit for 1996. If your name is on that list, don’t pay again!

**FORMAT FOR COMMS:** I had a query recently about the size of the typed area. Apparently what I said on the back of the List of Members is misleading. What is important is to leave a clear space of 1 inch (25 mm) all the way round, i.e. top and bottom and left and right margins must be 1 inch (25 mm) on A4 paper. On other paper, the print area must be no more than 9½ inches high (240 mm) and 6¼ inches wide (160 mm). And if these inches and mm are not exactly the same as each other, they’re near enough!

Incidentally, two people sent their Comms for this issue stapled to their letter. This does avoid me losing contact between the one and the other, so that there’s no question of forgetting who sent what, but on the other hand, the two little holes may well still be apparent when they’re printed. Now you can look through this Q and see if you can find them — no prizes offered.

**OBITUARY:** Len Stanners, one of our members of long standing in New Zealand, has died. A note by one of his colleagues appears elsewhere here. I was in touch with him frequently at one stage because he did much of the work for the catalogue of the Auckland Institute & Museum in New Zealand, even though in the end it appeared under someone else’s name.

Another recent loss is Harry Shorto, whom also I knew mainly as an ethnomusicologist. It was he, many years ago, who proposed that slit drum was the origin of the bell, something that I have quoted him for ever since in my lectures, demonstrating it with a series beginning with a small slit drum, progressing to a wooden cattle bell, and thence to metal bells. They were both good FoMRHI types.

**FURTHER TO:** Comm 1367: Charles Stroom asks me to say that I was not meant to print his formal address at the end of the Comm (Sorry — I just printed what came), since he was not writing as a member of the Space Technology Centre, but just as himself. Apologies. But do remember, what comes goes in unless you warn me.

**THINGS AVAILABLE:** Paul Hailperin has sent me a flyer from the firm of Dick GMBH, Postfach 1127, D-94523 Metten, Germany who have dug out some of their old catalogues going back to the 1920s and 30s. Stocks are limited, so phone (0991/910923) or fax (0991/910950) before ordering.
Stewart MacDonald’s Guitar Shop Supply would like to send you all a catalogue of their tools and so forth. I didn’t particularly want to send them one of our Lists of Members, but you’re welcome to write to them and ask for a catalogue. P O Box 1087, Bozeman, Montana 59715, USA, fax (406) 586-1030. A lot of things in it look as though they might be handy.

A year ago, the back cover had an announcement from the ApprenticeMaster Alliance. They have now printed a Directory of Masters. There are no musical instrument makers among them. If you are interested, get in touch with them at 100 Southgate Road, London N1 3JB.

OTHER SOCIETIES: After the successful inauguration last year of the British Clavichord Society, which is now thriving, a Swiss Clavichord Society has been founded. One of our members, Bernard Brauchli, is the President. If you’re interested, the address is POBox 1418, CH-1001 Lausanne, Switzerland.

Also new is the British Violin Making Association. Whether they reckon to include real violins as well as the 19th century things used in the modern symphony orchestra (!) I don’t know, but their Secretary is John Topham, 114 Mid Street, South Nutfield, Redhill, Surrey RH1 4JH.

Neither gives any details of subscription rates.

NEMA (National Early Music Association) has reorganised itself somewhat. Their Information Officer, Annette Heilbron is now their Administrator. Congratulations (or commiserations) to her; she is in our List of Members because we exchange with them.

COURSES: West Dean College has a Renaissance Music Weekend with Nancy Hadden, 19-21 January on the music of Orazio Vecchi. Cost £91 non-residential, £142 and up residential. There is also a number of Instrument Making Courses in January and April. Their address is West Dean College, West Dean, Chichester PO18 0QZ.

There are no Bate Weekends in the pipeline at the moment — give Hélène time to settle in. But after writing the above paragraph, I wonder whether we have been daft charging only £20 for a non-residential weekend!

CODA: That’s it for the moment. I’ll hold it open, as usual, while I do the members update (a lot of new members acquired at the Early Instrument Exhibition, where it was good to see many of you) and the Renewal Form. Don’t forget that, please. It costs us your money to remind you, and if you haven’t paid by the time the January Q goes out, you won’t get it till the April one goes.

DEADLINE FOR NEXT Q: January 2nd please. And remember the address (two things for this Q went to the Bate) — it’s on the front cover — 171 Iffley Road, Oxford OX4 1EL.

Jeremy Montagu
Hon Sec FoMRHI
Dear Jeremy,

It is with regret that I write to advise you of the death on 24th July of Len Stanners. I believe Alec Loretto had told you that he was not well.

For some years he was working full-time (5½ days per week, voluntarily, unpaid) at the Auckland Museum. He had a bypass operation about 2 years ago which was not too successful, and his health deteriorated slowly. Towards the end of last year he could manage only 6 half-days a week at the museum, and this year none at all. He went into hospital in July expecting to have the bypass replaced, or a leg amputated. The bypass failed, his leg was amputated, and then his kidneys failed, so the end came relatively quickly.

I was proud to be asked to speak at his funeral, where I was able to describe his interest in instrument making, mainly viols and hurdy-gurdies. He helped many others in this area, always enthusiastically sharing his knowledge.

You may not know that when returning here in 1983 (after visiting you) he called a meeting of everybody he could find involved with making or repairing instruments. I think he hoped to found something rather like FoMRHI in this part of the world. Some of us, principally Alec Loretto and I spoke against making it a formal organisation with office holders, constitution, etc., because we did not know what interest there could be, nor in what direction it might go. So we have remained a completely informal group (even without a name or title) of about 30 people covering a wide range of interests and skills. It has been most successful, and will no doubt continue to be so.

We meet at somebody's home late on a Sunday afternoon every 3 months. Wives and friends are welcome (and very supportive). Everybody brings a contribution towards what is invariably a superb meal. In one way, the "host" for the occasion is done a favour by the visitors, because he feels obliged to tidy his workshop specially for the event! It is of great interest to everybody to see somebody else's workshop and discuss problems and pass on and learn various hints and tips.

In one way I am the odd man out, because having been trained as a watchmaker and instrument reparer I am mainly working with metal, whereas most of the others making recorders and stringed instruments are mainly working with wood. However, we all learn from each other, and I have never been to a meeting when I haven't learned a lot, and also been able to help others.

Even without Len to organise it, I am sure the group will continue to thrive, a fitting memorial to Len.

David Smith

FoMRHI Comm. 1384

Len Stanners
Here follows several very useful TABLES of different Measures.

A TABLE of the Foreign Measures, carefully compared with the English.

<table>
<thead>
<tr>
<th>Measure</th>
<th>Conversion Factor</th>
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<tr>
<td>London</td>
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<td>Paris the Royal</td>
<td>1.068</td>
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<tr>
<td>Brussels</td>
<td>1.103</td>
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<td>1.184</td>
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<tr>
<td>Rynland or Leyden</td>
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<td>Lorraine</td>
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<td>Middleburgh</td>
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<tr>
<td>Old Roman</td>
<td>0.977</td>
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</tbody>
</table>

From Stone's New Mathematical Dictionary 1726
Philip Stubbes:

Anatomy of Abuses: 1583

I say of Musick as Plato, Aristotle, Galen & many others have said of it; that it is very ill for young heads, for a certaine kinde of nice smoothe sweetnes in alluring the auditorie to nicenes, effeminacie, pusillanimitie, &- lothsomnes of life.

I think that all good minstrels, sober and chaste musicians (speaking of suche drunken sockets and bawdy parasites as range the Cuntryes, ryming and singing of vnicleane, corrupt, and filthie songs in Tavernes, Ale-Houses, Innes and other publique assemblies,) may daunce the wild Moris throw a needles eye. For how should thei bere chaste minds, seeing that their exercise is the pathway to all vn-icles. Their is no ship so balanced with massie matter, as their heads are fraught with all kind of bawdie songs, filthie ballads and scurvie rymes, serving for every purpose, and for every Campanie.

Who be more bawdie than they? who vnclidean than they? who more licentious and loose minded? who more incontinent than they? and, briefly, who are more inclined to all kind of insolencie and lewdnes than they? Wherefore if you would haue your sonne softe wommanish vnicleane, smooth mouthed, affected to bawdrie, scurrilitie, filthie rimes, and vnsemely talking; briefly, if you would haue him transnaturally into a woman, or worse, and inclined to all kind of whoredome and abomination, set him to a dauncing school, and to learn musicke, and then you shall not faile of your purpose. And if you would haue your daughter whoorish, bawdie, and vnicleane, and a filthie speaker, and such like, bring her up in musick and dauncing, and, my life for youres, you have wun the goale.
The music of Psalm 117

David Z. Crookes

In the AV Psalm 117 reads as follows:

1. O praise the LORD, all ye nations: praise him, all ye people. 2. For his merciful kindness is great toward us: and the truth of the LORD endureth for ever. Praise ye the LORD.

The Hebrew text reads thus:


Both the eighteenth and twenty-seventh characters, set in bold type above, stand for double letters. When vowels are added the psalm transliterates as follows. (A single underlined space separates monophthongs. The silent or merely glottal letters Aleph and Ayin are not represented in the transliteration. Ch = ch in Bach, gh = g in Leipzig, and bh = v in viol):


You count 62 characters and 37 syllables, and you notice that the total numerical value of the psalm in 400 alphabet gematria is 2294 ( = 62.37). How were the 37 syllables of text sung? Were all 62 characters involved in the melody? You sing the 62 characters consecutively and decide that they weren’t. What then? Did the melody consist of initial letters, in the manner of Psalm 7? If you ignore the hyphens, or “maqqephs”, there are seventeen words. Their initials transcribe musically as follows:

These initials provide you with a credible piece of melody: but how can 17 notes be made to cover 37 syllables? A double statement of the melody will cover 34 syllables. Does the Psalmist then intend us to follow a double statement of the melody with a final statement of its first three notes (He, Aleph, and Yodh)? Two facts about the text incline you to think so. First, verse 1’s 17 syllables can be set to a single statement of the melody. Secondly, the psalm’s final three syllables constitute three-quarters of the phrase Halelu—Yah (= Praise ye the LORD), and if you’re right these three syllables will be sung to the initial letters of the opening phrase Halelu eth—Yahweh (= O praise the LORD). So much for the melody: 62 beats of music altogether, set to 62 characters of text! How was it harmonized? The psalm is untitled, and you can find no verbal clues in its text, so you resolve to examine its gematria.
You've operated a good deal in terms of the 400 alphabet, which runs as follows:

1 2 3 4 5 6 7 8 9 10 20 30 40 50 60 70 80 90 100 200 300 400

And you've operated to a lesser extent in terms of the 22 alphabet, which runs as follows:

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22

But at times, by subtracting 22 alphabet totals from 400 alphabet totals (see comm. 1326), you've operated in terms of a third numerical alphabet, each of whose first ten terms is zero:

0 0 0 0 0 0 0 0 0 0

You name this third number-line the "378 alphabet". Its first ten terms make you ponder. Was anything like our notion of zero (the point of nullity between positive and negative quantities) known to the ancient Israelites? Deciding with some reluctance to leave that question for another day, you return to the gematria of Psalm 117.

The psalm's total numerical value in 400 alphabet gematria, as you've already noticed, is 2294. You take that total to denote the three notes of an organal chord—melodic b, Alamoth e, and Sheminith e—each of which is positionally represented by the 400 alphabet sixth-note notational number 246 (= Wau + Mem + Resh).

512 + 768 + 768 + 246 = 2294

The psalm's total numerical value in 378 alphabet gematria is 1728. You take that total to denote the three notes of another organal chord: melodic g, Alamoth d', and Sheminith g.

648 + 432 + 648 = 1728

You therefore conclude that Psalm 117 was given an Alamoth-plus-Sheminith performance—but not the whole psalm. The 400 alphabet total value of verse 1 is 1080. You take that total to denote melodic d played against Alamoth g (or vice versa).

432 + 648 or 648 + 432 = 1080

So you deduce that verse 1 was performed in only two parts (melody-plus-Alamoth), and that verse 2 was performed in three parts (melody-plus-Alamoth-plus-Sheminith). Is that the full story? You look at the text of verse 1. "O praise the LORD, all ye nations: praise him, all ye people." You collect that in Alamoth-plus-Sheminith organum six out of the eight possible chords are thirdless. Not quite omnisonal! Was a fourth part ever added to create the effect of climax or luxuriance? Such a part would have needed not an eight-note scale, but merely the recurring tetrachord f, g, a, b.

And any instrumental part written in terms of the f to b tetrachord could have been played on a three-holed horn or cornett or shawm. You remember the "instruments of musick" in 1 Samuel 18.6 (AV). The margin says "three-stringed instruments", but the Hebrew word is Shin-Lamedh-Shin-Yodh-Mem, which means simply "threes". Were they three-holed wind instruments, and were any such instruments included in the cryptic score of Psalm 117? Could they have been fingerhole horns? In Hebrew the
musical direction "on horns" can be written Beth-Qoph-Resh-Nun-Yodh-Mem. "On psaltery" (used for Alamoth) is written Beth-Nun-Beth-Lamedh-Yodh-Mem. "On harps" (used for Sheminith) is written Beth-Kaph-Nun-Resh-Wau-Tau. The numerical values of those three expressions in 400 alphabet gematria are respectively 402 (= on horns), 134 (= on psaltery), and 678 (= on harps). 402, 134, and 678 add up to 1214. And the 400 alphabet total value of Psalm 117's second verse is 1214. You deduce that for verse 2 of Psalm 117 the melody was accompanied by Alamoth, Sheminith, and horn parts. You've already taken the 400 alphabet total value of verse 2 (1080) to indicate melody-plus-Alamoth, but now you see that 1080 is the sum of 678 (= on harps) and 402 (= on horns). Does that mean that the whole psalm should be performed twice, and that verse 1 should be given a melody-free Sheminith-plus-horns treatment second time round? You doubt it.

And now it remains only to write out the words and music of the whole psalm. The f to b tetrachord is an outrageous supposition, which is going to need a lot of proving, but you furnish verse 2 with a fourth part nonetheless. (Adams and Le Verrier worked out that Neptune existed shortly before Galle and D'Arrest actually discovered the planet!) Your fourth part represents an avenue for further research. It may turn out to be all wrong. For the moment, you are content to judge it aurally. One of your Hebraist friends objects to the short e of shabbechuhu being set to a dotted minim, but you remind him that in the Septuagint Psalm 117 the very same vowel the second vowel in Halelu, is transliterated by Eta (η), the long Greek e: Ἐλασθαι.

Part 1 = melody, part 2 = psaltery on Alamoth, part 3 = harps on Sheminith, and part 4 = horns. Voices sing all four parts.

Correction to comm. 1378, page 45, preantepenultimate and final lines: read Sin instead of Shin.
The essence of Stroom's Comm. 1366 is that the standards in scholarship, and particularly in historical scholarship, have been overstated by me, and that the clarity with which truth (and consequently deception) can be approached by scholarship has been similarly overstated. This IS 'a debate on scholarly and scientific methods', and it IS very much a 'subject of FoMRHI' since a central issue is whether claims of historical accuracy in instruments and how they are used can be criticised as dishonest.

I have never claimed that all scholars follow the rules that I have stated. Far from it. All I claim is that very few scholars indeed would argue that objectivity is not fundamental in the pursuit of scholarship, or that there is another set of rules that leads to more objectivity. In practice most just rely on their intuitive commitment to objectivity.

Scholars (including scientists) have a culture as well as do scholarship. That culture includes widely-held opinions which are matters of fashion. The opinions appear in reports but rarely effect the scholarship. In arts scholarship this happens more often. That is because the public looks to the arts scholar for guidance on the 'quality' of arts works. So arts criticism is a lot of what arts scholars do. It is culture, an art in itself. At its core is judgment using modern fashions of thinking about arts works, reporting them as innate characteristics of the works themselves. It is convincing presentation of judgment that counts, not logical relationships with evidence. Fashion knowledge is more important to more people than the permanent knowledge that scholarship strives to generate. Arts scholarship can be good scholarship as long as there is no conflict between judgment and evidence. When there is such a conflict, arts scholars are more prone than others to say that the evidence is wrong without feeling the need to show how that could have happened to it. Aren't we all trying to raise standards?

Occam was against unnecessary complications, whether it was in an hypothesis that related to evidence or a speculation that did not. He did not distinguish between these, and he cannot be cited as considering speculations as unnecessary and so 'should be cut away'. I would expect criticism of my speculations when they are disagreed with, but am surprised that Stroom criticises them because they are speculations. The formulation of Occam's Razor that I presented is not pure Occam. It only applies to an hypothesis that includes explanations for all of the relevant evidence. Only by applying the Razor to such an hypothesis can it help promote fidelity to the evidence (and thus objectivity) in the scholarly (including scientific) method.

Of course the logical structures of different fields (including different sciences) vary widely, but the need for some logical structure to relate hypotheses with evidence remains universal. And there are many sciences (including cosmology, astronomy, geology, seismology, meteorology and archeology) that deal with unique (and not so unique) events that cannot be repeated or experimented with.

The quality of consistency that an hypothesis must have includes consistency with all of the relevant evidence. But an hypothesis necessarily covers situations beyond that provided by the evidence. Another hypothesis consistent with another complete set of relevant evidence could well have a generalisation that is not consistent in some way with that of the first one. The tension created by this inconsistency is stimulating, and sorting it out does advance knowledge.

Stroom defines 'truth' as answers to 'why' questions. As discussed in Comm. 1302, a 'why' hypothesis requires 'why' content in the generally-accepted evidence. This type of evidence is rare in most scholarship, so we are practically always dealing with 'how' hypotheses. 'F=ma' is an hypothesis of how force, mass and acceleration are related. That is the relevant truth that this hypothesis is close to. Requiring a 'why' answer for an explanation to be adequate is inappropriate, asking for magic from the evidence and from scholarship.

Stating or implying that the historical accuracy of something is supported by scholarship is to
me either true or untrue. Stroom has been arguing that such truth is uncertain enough that the apparent lack of it does not warrant criticism.

'Dishonesty' is a strong word, and Stroom strongly resents my using it. I apply it to leading someone to believe something that one knows is untrue. This often does occur in circumstances where it is not criticised. A common example is the response 'fine' to the greeting 'how are you', which is often untrue but this response is the way to avoid an unwanted discussion. Another example can be in the playful teasing of a child or someone else one is close to. The teaser considers that it is harmless and good fun, while it is much less fun for the person who is teased, but he or she often (but not always) prefers being teased rather than the alternative, which is not to have the attention of the teaser. Another example is making another person feel good by telling him/her what he/she would like to hear (such as a compliment) even though one knows that it is untrue. That other person may well also realise that it was said without belief, but appreciates the underlying desire to make him/her feel good.

If such saying of untruths is forgiven and uncriticised, Stroom may argue that this should also apply to saying that an instrument/performance is authentic when the musician/audience wants to believe that it is historically accurate because that adds to enjoyment, and is not interested in being told the details of which aspects are authentic and which are not. They are asking for deception, so why not give it to them? If only instrument makers, musicians and audiences were involved, there is apparent virtue for all to maintain an agreed fiction (as often occurs in other performing arts), where 'as authentic as is agreed to be appropriate' is shortened to 'authentic'. But when we take into consideration the scholars of instrument and performance history, and those members of the public that are really interested in the history, such an agreed fiction hopelessly confuses and undermines historical research. It would not if the field of scholarship has been thoroughly explored, safely written up in the journals and textbooks, with current activity only mopped up minor details. But the field of the history of instrument characteristics is still in its infancy, and that of performance history has hardly even started looking objectively at the evidence (art-criticism assumptions have inhibited this). Such studies are undermined by the public assuming that they already know the answers.

Stroom raises the question of distortion in the presentation of scientific results in the popular media. These usually omit the 'if' and 'but' details so as to present the main ideas in a form simple enough for easy assimilation. If members of the public happened to be aware of omitted exceptional situations where the main ideas did not apply, they would be confused by such writings and could consider that they have been deceived. Stroom argues that if such sloppiness is acceptable in these circumstances, it should be acceptable in the question of authenticity. I argue that there is an important difference in honesty between a statement that is fully valid in most situations and a statement that is not fully valid in any situation.

Opinion by experts on the authenticity of surviving instruments or their components will clearly vary because of a great many uncertainties. These opinions are only educated guesses. But uncertainty on the question of authenticity of a characteristic of a surviving instrument, because of not knowing when it was introduced, should not cloud the question of non-authenticity of such a characteristic in a 'copy' when there is no evidence that it was there then and we have good reason to expect such evidence to have survived if it was at all common.

I don't advocate that instrument makers and musicians should broadcast the unauthentic aspects of what they do to make their customers happy. These customers usually don't want to know. It is also bad for business (I know, since I do it). What I do advocate is that they generally avoid the issue of authenticity, but when direct questions are asked, that they be answered honestly. Historical scholarship is important in FoMRHI, so our Q is an appropriate forum for criticism of false claims in this area. I would be reluctant to air such criticisms in the general music media because most would not consider it constructive, and I don't want to upset friends who still fear that the 'early music bubble' will 'burst'. Nevertheless I believe that their fears are unfounded, since early music is robust and musically valid irrespective of (and probably helped by) unhistorical factors, and it has no apparent enemies wanting to undermine it.
In Comm 1370 I pointed out that the article *Happy Birthday, Whenever That Might Be* (FoMRHI Quarterly - Issue 80; Page 31) first appeared in *Continuo* of June 1993. Within the article I advised *Continuo* readers that, while they would not be privy to certain facts, *Continuo*’s Editor would be, and I supplied him with the sources of the various conflicting statements contained in my article. Under the title *Accuracy of Detail*, (Issue 80; Page 7) Eph refers to my *Continuo* article, and informs *FoMRHI Quarterly* readers, “Alec Loretto’s Comm in this Q Is mainly about the accuracy of detail. One detail in it states that the Editor has the sources of the various conflicting statements. This is not (yet?) true.”

I can assure readers that the Editor of *Continuo* for whom the *Happy Birthday* article was written and to whom I referred as having received certain information, did in fact receive it. Indeed, at this very moment, I have in front of me *Continuo*’s Editor’s letter acknowledging receipt.

When forwarding the original *Continuo* material to be reprinted in *FoMRHI Quarterly*, the article itself as well as the sources of the various conflicting statements were sent to *FoMRHI*’s Hon. Sec. If Eph wishes to receive information concerning the sources of the various conflicting statements, I will be happy to provide it.

**FoMRHI Comm. 1390**

In Comm. 1271 I was inquiring for a method of bending wood as to get a crumhorn as result. Since then I tried it successfully.

My method:
1.) making the bore and turning the wood
2.) filling bore with fine sand and closing the ends
3.) laying the wood into liquid ammonia (25%) for 5-8 days
   (attention! dangerous!)
4.) cooking the wood in a pressure cooker for one hour
5.) bending the wood in a bending device (see: article by Toon Moonen in Galpin Soc. Journal XXXVI/1983)

Remarks to 4.):
I mounted a metal tube (inner diam. 27 mm, length about 70cm) on the lid of an old pressure cooker. On top of tube I fitted a screw lid. In the screw lid I mounted a hook where the wood can be hung on. With this method I bended the wood for four crumhorns, 3 alto, 1 soprano.
One of the more nerve-racking processes in making a viol is half cutting through the carefully jointed and thicknessed back plate to allow it to be folded at an angle where it tapers to meet the neck root. I have found the following device very useful in making the cut idiot-proof. I have made up two rectangular blocks of wood, about 4" x 1.5" x 1" with a scalpel knife fastened so as to protrude from the bottom surface of each just far enough to make the necessary cut and no further (just over three-quarters of the thickness). The first scalpel blade is set to make a vertical cut parallel to the long side of the block at a fixed distance from one of the edges. The second is set at an angle equal to a degree or two more than the intended fold angle, and again parallel to the long side. But this time the long side is made adjustable so that the distance of the knife from this long side can be varied lightly to allow the angled cut to be made at the correct distance from the vertical cut. I have found that this distance is that at which the line of the two cuts would, if produced, meet at the bottom surface of the back. The waste piece can be conveniently chiselled out using a narrowed-down purfling chisel, leaving a flat-bottomed groove. This allows for compression on the upper surface of the remaining piece as well as tension on the lower surface, and this reduces the tendency to cracking when the wood is bent. In use the block carrying the vertical blade is run along a straight-edge cramped to the viol back until the knife has cut its full depth. Then the other block is adjusted until the knife is cutting at the correct point when run along the same straight-edge, and it too is run repeatedly until it has cut its full depth. In practice the angled blade has to be started at the correct point and then, when it has cut half its depth, a couple of pieces of the packing paper are removed to allow the angled blade to continue downwards (and inwards).

After the waste wood has been removed the back can easily be bent using a warm iron and a damp cloth, taking care not to wet the centre joint too much.
ANOTHER MOULDY IDEA

The readers of FoMRHI might be interested in an alternative system for making viols. For some years I have used, and taught others to use, outside moulds made in two separate halves linked along the centre line. They are not disposable and are no easier to make than the normal mould but I have found them easier to use for several of the stages of making English 16th and 17th types of viol. The ribs are bent and fitted in the usual way with the mitred corner joints strapped together on the outside with sellotape, glue run into the joints and then the ribs folded into the mould. The neck and bottom blocks are carved and cramped into place from inside, pushing against the mould. The neck can then be jointed direct onto the neck-block and fastened from the inside, checking the action angle against the top edge of the ribs. The taper on the top bouts of the ribs and the combined neck-block and neck-root can then be very simply planed off and the back bent and glued in place with the whole assembly held in the mould.

The beauty of this is that all the joints proceed in the simplest possible order, with no compound joints required, as is normally the case when the neck has to be fitted simultaneously to the projecting bit of back and the neck block. With this system the joint is identical but is done in two separate simple operations with a plane. Then, with the assembly still held firmly in the mould, the tapes and soundpost-plate can be glued in place and, finally, the top can be glued on. Only then, right at the end, does the mould come away in two halves; and so the chance of distortions between front and back is largely obviated.

Naturally I have no evidence that this was the English system of making but it does offer many of the advantages of building without a mould, which several people have suggested as a possible English system, without the obvious disadvantages of loss of symmetry and control of shape. It seems to me at least arguable that it is the obvious way to build a viol with linen liners and no corner posts, just as an inside mould is the obvious way to build a corner post viol or violin. Using this system several music students at the University of East Anglia with no previous woodworking experience have built very creditable viols.
Gansar Lute Strings

In Comm. 1351, Mimmo Peruffo brought to our attention the reference from the Diderot Encyclopédie concerning the meaning of the French word 'ganse.'

Out of curiosity, I spoke to some of my French speaking colleagues at my place of work to find out what they knew about the word, its pronunciation and meaning.

Apparently, the word has a number of meanings, in current usage, for a flexible cord used on clothing. For example, it can describe the small cord loop fixed to the collar of a coat so that the coat may be hung from a peg. It can also mean the tie cord of an apron. It also means a decorative braid or piping for clothing or a rope handle or loop.

A 'ganse de cheveux' is a plait of hair.

The construction of a 'ganse' is rope like or, more specifically, of braided or plaited construction for maximum pliability.

In Diderot's time, this type of braiding could be made from a variety of thread materials including silk, and silver and gold wire, or a combination of these materials depending upon the decorative effect required.

In English phonetic, 'ganse' would today be generally pronounced as 'gans' i.e. without any emphasis on the final 'e'. However, in parts of South-West France the pronunciation would be 'gansa' – with strong emphasis placed on the final 'e' – a pronunciation that might have been widespread in earlier times.

The Capirola Lute Book MS mentions 'le corde da ganser' and Robert Dowland talks about 'strings of a more fuller and larger sort than ordinary (which we call Gansars). These strings for the sizes of the great and small Meanes, are very good, but the trebles are not strong.'

A string of plaited construction would be very pliable and more elastic than a string of simple twisted form but would be less strong due to the extreme distortion of the fibres.

Strings of plaited construction were made by the catgut manufacturers in Britain during the 19th C. for use as sash cords and clockmaker's cords. These cords had to be very pliable in order to run over small diameter pulley wheels and were made from sheep's intestines.

It is possible, therefore, that "gansar" lute strings of the 16th C (and earlier) might have been of plaited construction to provide a better performance than simply twisted strings for lute second and third courses. In Capirola's time, at the beginning of the 16th C, these strings were made from sheep's gut as they may well have been in Dowland's time also. However, the possibility exists that they were later made from other, more uniform fibres than gut for the early 'ganser' strings, like the small diameter strings, suffered because of the natural taper in the gut which affected their tuning. On this, the Capirola MS says 'Ma sapi che patis piu le corde sotil che le grosse, et masime le corde da ganser et altre, che non fa quelle da monaco ut supra'. The strings from 'monaco' being more elastic than the ordinary type of lute string, did not suffer from this fault.

[Continued on p. 22]
"They delight much in musicke, but chiefly in harps and clairschoes of their own fashion. The strings of the clairschoes are made of brass wire, and the strings of the harps, of sinews; which strings they strike with their nayles, growing long, or else with an instrument appointed for that use. They take great pleasure to decke their harps and clairschoes with silver and precious stones; the poore ones that cannot attayne hereunto, decke them with chrestall. They sing verses prettily compound, contayning prayses of valiant men. There is not almost any other argument, whereof their rhymes intreat. They speak the ancient French language altered a little." ( Anon 1597 (1) ).

The word 'sinew' meaning tendon, originated from Old English. During the Middle English period (1150 - 1475), it was also used as a term for a musical instrument string (2). While this word may have been used to describe strings made from a variety of materials such as silk or gut, it is apparent that strings (for plucked instruments at least) made from tendon fibre, were in use at the end of the 16th C. and may have been commonplace for many centuries before that time.

According to Diderot (3), ropes made from tendon fibre were extensively used by the ancients to power their machines of war such as siege catapults etc. - machines that, according to Ramelli (4), were still of military importance at the end of the 16th C.

The preparation and processing of tendon fibre to manufacture cordage is similar to that of flax in that the fibre must be mechanically separated by beating and combing before being spun and twisted into cord or rope. No doubt the manufacture of tendon fibre ropes was, therefore, a specialised branch of the rope making trade which may well have been, because of its military importance, a closely regulated and controlled operation.

No doubt, it is this trade that also provided the smaller diameter twines, cords or lines that were used for musical instruments.

By the middle of the 18th C., it is would appear that the manufacture of cordage from tendon fibre was an obsolete trade for Diderot does not include it in his description of the trades dealing with cordage manufacture. Instead, he describes the preparation of tendon fibre rope according to the accounts of a M. le comte d'Herouville, an entrepreneur who, through his researches and experiments had discovered how these ropes were originally made with a view to using them, because of their great strength and elasticity, in the suspension systems of horse drawn carriages.

The following translation summarises Diderot's commentary dealing with the manufacture of cordage from tendon fibre:--

Tendon cordage was extensively used by the ancients for their machines of war. They preferred sinews taken from deer or cattle and selected those tendons subject to the greatest stress and use such as the leg tendons of a stag or neck tendons of oxen. The ropes made by M. le comte d'Herouville used the latter material which was more readily available than the former from the Parisian abattoirs at that time. The tendons were drawn in their entirety from the still warm carcass of the slaughtered animal.
The handling and storage of the tendons was critical. To prevent excessive hardening, their exposure to the sun was to be avoided. On the other hand, high humidity and freezing temperatures caused deterioration and weakening of the fibre.

The condition of the tendons at the time of working was also of importance for if too dry, the fibres would break and if too fresh, they would be too greasy. These two extremes were to be avoided.

In order to separate the fibres from the membrane, the tendon was beaten thoroughly with an iron hammer, weighing about half a pound, on a stone block with a polished surface measuring about eight to ten inches square. Prior to beating, the hardened ends of the tendon were cut off. The beating of the tendon was continued until the membrane was seen to be completely detached from the fibres.

The fibres were then separated from each other and the membrane by drawing a bundle of them through an iron comb furnished with eight to ten teeth set about a centimeter apart (six lignes). The fineness of the processed fibre was dependant upon the thoroughness of the beating operation. Insufficient beating would result in broken and unseparated fibres during the combing process.

The prepared fibres were then spun, twisted and layed together like hemp cordage.

Before use, it was essential to preserve the cordage by soaking it in a greasy oil.

Tendon ropes were very elastic and very strong (17% stronger than a hemp rope of the same size). Under load, these ropes would reduce in diameter in proportion to their extension and, after rupture, would return to their original dimensions in the unloaded state.

Diderot noted that this type of cordage was easily damaged by moisture which could largely be avoided by protecting ropes with a wrapping of animal skin. He also suggested that treatment of the fibres with a caustic solution (in the manner of gut strings), might serve to further improve their durability and increase their elasticity.

From Diderot's account, the following conclusions may be drawn about some of the characteristic features and properties of sinew instrument strings. Sinew strings were "very strong". The strongest cords would have been made up from single lengths of tendon fibre which, if taken from a large animal, might be four or five feet or so in length. They were "very elastic" - a property that would allow their use for larger diameter bass strings.

They were sold saturated in a preservative oil - presumably to prevent drying and hardening of the fibres and their subsequent loss of elasticity. They were made by spinning and laying together the fibres in the same manner as cordage made from hemp. (A process necessary primarily to produce a coherent, uniform and smooth cylinder of fibres rather than a more elastic assembly). These small diameter strings would have been classified as twines in rope making terminology. In Britain, they may also have been called "lines" (from Old English originally meaning a spun linen thread but later used as a general term for small diameter cordage).
The oil treatment combined with the twined construction would likely have resulted in a string that was quite pliable - one that could be bound up in tight knots or bundles without damaging or otherwise weakening the string. These strings were easily damaged by moisture or damp conditions. The fibres of the string could also be weakened by freezing temperatures during manufacture - a risk for strings manufactured during the Winter months in Europe.

Dowland (5) wrote "... we choose lute strings by the freshness or new making; which appears unto us by their cleere and oyliness, as they lye in the Boxe or bundle yet here we are often deceived, for Oyle at any time will make strings looke cleare and therefore this tricke is too commonly used to them when they are old ...... the string makers bring their best strings which were made in Summer to Frankford and Lyppiz Martes. Contrarily at Easter they bring their Winter strings which are not so good".

Burwell (6) wrote that lute strings were "preserved in white paper dipped in oyle of Almonds or in a hogges bladder they endure no moisture nor any excessive heat .... but of the two moisture is the worst."

Mace (7) says that lute strings "may be very good when you buy them but spoiled in a quarter of an hour if they take any wet or moist air ... for moisture is the worst enemy of your strings."

If the oiling of lute strings made from sheep's intestines was practiced by the 17th C. string makers, it was not by the 19th C manufacturers for Heron Allen, writing about gut violin strings (8), informs us that "Some people wrap their spare strings in bladder or flannel moistened with oil ... a process which can only be described as 'horrid' ... the mess involved in putting on a new string is enough to make you touchy for the rest of the performance" and, concerning a recommendation to coat gut strings with almond oil after each performance "I do not know whether this has ever been done; it would certainly be quite impossible to play on strings so treated."

19th C string makers used olive oil and pounce to polish their strings after which they were lightly moistened with the oil before being thoroughly dried. They were not oily!

Either the 17th C makers were using a different kind of sheep's gut or process for instrument string manufacture or were using a material other than gut that required oil treatment.

There are other apparent differences between modern strings made from sheep's gut and lute strings of the 16th and 17th C that might indicate some lute strings at least were not made from sheep's gut. For example, it is clear from the iconography (9) that lute strings were made up in tightly bound hanks or knots - treatment that would surely damage the carefully coiled strings of today. The early strings were, therefore, much more pliable than modern strings. Furthermore, the performance of 17th C. lute bass strings was, by all accounts, equivalent to modern overspun strings (10). Mersenne (11) wrote that"... & que le son des grosses chordes de Luth est apperceu de l'oreille durant la sixiesme partie, ou le tiers d'une minute, c'est a dire pendant qu'artere du poux d'un homme sain, & sans emotion bat dix, ou vingt fois: ..." which I take to mean that the sound of the largest strings of the lute can be heard to last for between a sixth or a third of a minute, i.e. for the time it takes for the normal pulse of a healthy man at rest to beat ten or twenty times. Burwell commented that "The lute makers have taken away that great string (i.e. the eleventh course) because the sound of it is too bigg and smotheres the sound of the others."

As far as I know, such a performance cannot be achieved by well twisted
modern lute bass strings made from sheep's gut (I have no recent experience in using all gut basses on my lutes - I use nylon overspun basses), unless they have been loaded by overspinning with wire or other means. The alternative might have been to manufacture lute bass strings from a gut like material that was more elastic and denser than strings made from sheep's gut. Could such a material have been tendon fibre and were sinew strings used not only on Scottish harps at the end of the 16th C. but also on lutes and other plucked string instruments of the day?

Unlike some other early writers (12) who specified that lute strings were made from sheep's gut, neither Dowland nor Mace have anything to say about the material of construction of their strings. Mary Burwell, on the other hand, states that "The stringes are made from Sheepes & Catts gutte ..." Burwell was, therefore, aware that lute strings were made from two distinct kinds of material - the historically conventional sheep's gut and another material known as "catgut".

Notes:

1. Vide "Certayne Matters concerning the Realme of Scotland &c as they were Anno Domini 1597 - Lond.1603." from 'Lady of the Lake' Appendix note K by Sir Walter Scott, Adam & Charles Black, London 1893. See also 'The Irish and Highland Harps' p.140 Robert Bruce Armstrong, Edinburgh 1904.


3. 'Encyclopedie', Diderot & d'Alambert, Paris 1751 (from a photocopy of the original text kindly sent to me by Mimmo Peruffo)

4. See Comm. 1318. In Comm. 1351, Peruffo confirms that Ramelli did not specify the material of his catapult ropes - only the form of construction. Perhaps the material of construction of catapult ropes in Italy was common knowledge by the end of the 16th C?

5. 'A Varietie of Lute Lessons' Robert Dowland 1610 under 'For Chusing Lute-strings.'


7. 'Musick's Monument' Thomas Mace 1676, Chapter 6, p.66.

8 'Violin Making as it was and is' Heron-Allen 1885, p.208.

9. For example, see engravings of lute strings being tested for trueness in 'Musica Teutch' Hans Gerle, Nurnberg 1532 and in 'Harmonie Universelle' Marin Mersenne 1636, p.51 book 2, proposition 2 - Mimmo Peruffo describes the knots as being like bundles of shoe laces - an appropriate analogy.

10. See discussion between Eyler and Segerman on catlines, Lute Society of America, Newsletter May 1987, Vol.XXII, No. 2.


[Continued on p. 23]
"(The Highlanders) are exceedingly fond of music, and employ harps of a peculiar kind, some of which are strung with brass, and some with catgut. In playing they strike the wires either with a quill, or with their nails, suffered to grow long for the purpose; but their great ambition is to adorn their harps with great quantities of silver and gems, those who are too poor to afford jewels substituting crystals in their stead. Their songs are not inelegant, and, in general, celebrate the praises of brave men; their bards seldom choosing any other subject".(13).(G.Buchanan 1582)

The origins of the word 'catgut' meaning a musical instrument string are obscure but it appears to have come into general use around the end of the 16th C in the English language.

In Britain by the 19th C. gut string manufacturers were known as makers of catgut (14). They made not only musical instrument strings, but cordage such as Hatter's cord, Clockmaker's cord, sash cord, and rope for machinery drives and other industrial use. The industry used the treated intestines of animals as their raw material – primarily, but not exclusively, the gut of sheep. (Horse, ass or mule intestines were also used for the strongest ropes) While there are anecdotal historical references, in early times, to the use of more exotic intestinal fibres for instrument strings such as those of the wolf, the serpent or a young lion, there is, otherwise, no tradition in the trade for use of the intestines of a cat.

Heron-Allen wrote scornfully of a M.F.J.Fetis who had expressed wonderment that the ancient Egyptians had used the intestines of cats for their instrument strings when the animal was sacred to them. He also wrote "It is a matter of everyday occurrence to hear people talk of fiddle strings as catgut - indeed a great writer alluded to a violinist as a man who 'stretches the bowels of a cat over a wooden box and rubs them with the tail of a horse'. However this may be, it is one of those carefully-persisted-in errors made on the 'lucus non lucendo' principal".

Clearly the word 'catgut' has no relationship to the feline animal and likely never did!

The word 'catline' seems also to come into use during the second half of the 16th C and again, its origins are obscure.

The earliest known reference to 'catline' is in the London Port Book where on 2nd of April 1568 there is an entry recording the import of '3 grs coarse catlins'.(15) - hardly a description that would fit finely made lute strings! Dowland (1610) and Mace (1676) also mention 'catlines' as a kind of bass lute string.

While I have not seen the document myself, I understand that there is yet another reference to 'catlines' in the Talbot manuscript, c.1694 which is quoted as 'Bass Violin all Venice Catlines'.(16). Could there be a connection with Venice catlines and the description of the bass violin strings by Ramelli a hundred years earlier? (17)

To take this guessing game on word origins a little further, is it possible that 'catline' was an abbreviation of 'catapult line' i.e. a small diameter cord made from sinew fibre originally made as a component part for constructing catapult ropes of all sizes and later, in a more refined form, used for lute strings? Is it also possible that 'catline' and 'catgut' were one and the
same thing, 'catgut' or 'catapult gut' being a general term for sinew strings - a misnomer by the end of the 16th C perhaps because of its similarities to cordage made from sheep's gut?

While catapult rope making probably was originally part of the rope making trade, it is possible that, as the material lost its importance in military applications the manufacture could have been adopted, for a period, by gut string makers - the source of both gut and sinew fibre being the abattoir. The process of preparing and manufacturing sinew strings would appear to have been more complex and critical than that of a string made from sheep's gut - so the former would not only have been more expensive than gut strings but may have been less durable. They might, therefore, like any other product, only have been made to fill a market niche, lasting from the end of the 16th C. until about the end of the 17th C when cheaper and more reliable strings of the overspun type may have come into general use and the lute itself was in its final stages of decline. With little market demand for sinew strings its manufacture would have been abandoned and likely forgotten within a generation by the trade - but the word 'catgut' could have been retained as a generic term for the products of the gut string makers of Britain - the word does, after all, have a jingoistic ring to it and easily rolls off the tongue! The word 'catline' did not fare so well and fell into disuse presumably coincident with the demise of this type of string again around the end of the 17th C.

By the middle of the 18th C. according to Diderot's account, the trade of sinew string manufacture was defunct and, apart from the efforts of M. le comte d'Herouville in Paris, was never again to be revived.

Notes:

13. Taken from a history of Scotland, George Buchanan, 1582, translation by James Aikman, vol i p.41 Edn 1827 ('The Irish and Highland Harps' R.B. Armstrong, Edinburgh 1904 p.140). The similarity between this passage and that quoted in the opening paragraph of PART 1 is obvious. I do not have access to Buchanan's history so cannot check if anything has been lost in the translation of the original.


17. See Comms. 1318, 1320, 1351, 1352.

[Continuation of Comm. 1393 from p.16]

Capirola's solution was to rotate a string that would not hold its tuning i.e. to remove the string from the lute and refit it the opposite way round. (Were the elastic strings from 'monaco' later to be known as 'catlines'?)

The other possibility is that 'gansars' may have been an early type of loaded string with thin wires plaited into the cord as suggested in Comm. 1320.
In Comm. 1362 I stated, correctly, that I could find no reference to overspun strings in copies of Playford's *Introduction to the Skill of Musick* dated 1655/1660/1662/1664/1666/1667 (at the Royal College of Music). Since then a reference in *The Purcell Companion* has led me to the following in an article by Michael Lowe in GSJ XXIX (1976) p. 24:

"At the back of *Introduction to the skill of Musick*, 1664 edn.: 'There is a late invention of Strings for the Basses of Viols or Violins, or Lutes, which sound much better and lower than the common Gut strings, either under the Bow or Finger. It is Small wire twisted or gimp'd upon a gut string or upon silk. I have made tryal of both, but those upon Silk do hold best and give as good a sound...'."

Here is a disconcerting example of a 'primary source' in the ordinary sense being misleading. I should no doubt have remembered that "1664" on a 17th-C title-page would not have the definitive significance which we attach nowadays to an 'edition'. The preference for a silk core is interesting.

Another scrap of information; Mace says that he wrote his section on *The Lute made Easie* in the one year 1671/2 (p. 45). That was 12/13 years later than the Hartlib record and 7/8 years later than Playford, with four more years or so in which he could have amended his text had he wished. So, three possibilities: he did not know about covered strings; he disapproved, and refused even to mention the new-fangled gimmick; or (and we should not reject this possibility too hastily) are they included obscurely in his account? We tend to think of covered strings as a very remarkable invention, but our Restoration forebears may have seen them as a useful improvement which was briefly noted, quickly came into fairly wide use, and then taken for granted.

**Notes:**

11. (cont). Mersenne was here referring to the strings of a ten course lute of 19 strings run from a single pegbox not the extended bass strings of the 21 string theorbo which he separately describes and illustrates.

12. For example in the Capirola Lute Book manuscript (c. 1530) is written "Sapi che le corde sono fate de bueli de castronj" or you know that the strings are made from wether gut. From the Persian manuscript " Kanz al-tuhaf" c1350 (The Structure of the Arabian and Persian Lute in the Middle Ages' H.G.Farmer, Glasgow, Civic Press, 1939, p.95) "(lute) Strings are made of either silk or gut.... As for gut strings, the gut from sheep is better than gut from goats. Some say that white sheep gut is better than black gut, but this is an exaggeration ...."

Note that these two references describe lutes that are strung with six courses or less, a less demanding application for the strings than would be later be required on the lutes of Dowland, Mersenne, Burwell and Mace.
The 'Clearness' of Early Gut

In Dowland’s opening discussion of choosing strings, he wrote: ‘Ordinarily … wee choose Lute-strings by the freshness, or new making; the which appears unto us by their cleer and oilinesse, 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 used to them when they are old.’ This was a general statement before he distinguished between different types of strings, and so should apply to Basses (4th course and lower) as well as Meanes (2nd and 3rd courses) and Trebles (1st course).

Following is: ‘Now because Trebles are the principall strings wee neede to get, choose them of a faire and cleere whitish gray, or ash-colour’. At the end of this section on Treble strings is: ‘then draw it hard betweene your hands, to try the strength, which done, hould it up againe against the light betweene your hands, and marke whether it be cleere as before; if it be not but looke muddie, as a browne thread, such strings are old, and have beene rubbed over with oyle to make them cleere.’ This is followed by: ‘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 bourne 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.’

In Comm. 1255, I read ‘greater strings’ in the above passage as meaning ‘greater than great Meanes’, i.e. the Basses. In Comm. 1288 Peruffo argued that the meaning was the ‘great Meanes’, citing for support Dowland colon and lower-case g in ‘greater’. In Comm. 1307 I showed why the colon and lower-case g did not support his position, and added a quote from Mace saying (without possible ambiguity) that for Minikins and Venice-Catlins ‘the signs of Goodness, both the same; which are, first the Clearness of the string to the Eye, the Smoothness, and Stiffness to the Finger, and if they have Those two [?] qualities, dispute their Goodness no further.’ Concerning the Lyon Strings, he wrote ‘Their Goodness may be perceiv’d, as were the others’: ‘Clearness’ was thus a property of gut strings over the full range.

I was therefore disappointed to read in Comm. 1351: ‘From Mace’s and Dowland’s treatises it is clear that the concept of “transparency” is only applied to treble and mid-range strings’, especially since Peruffo (in the same Comm.) wrote ‘we are trying to carry out a research based exclusively on historical elements’. The issue of ‘clearness’ is crucial in the question of whether loaded bass strings are historically possible.

Peruffo’s disbelief of what Dowland and Mace wrote is understandable. If one twists up raw cleaned gut, dries it and polishes it up, there is little evidence of clearness, especially with thicker strings. But there was more to gut string making then. What Mersenne wrote (First Book, Prop. II) about making strings was: ‘As soon as they have been stretched, they are twisted many times, and after they have been twisted enough, they are wiped off, rubbed, and polished, with both linen rags
and hemp strings, which are pressed all along upon them, as well as with an herb which is a species of mare’s tail, called Shave-Grass, and finally they are cured so that they will be proper for musical instruments, or for the other things to which it is wished to apply them. Then after some speculation about why the intestines of Italian sheep make better strings than French sheep, he wrote ‘I am omitting the method by which it is necessary to oil them to conserve them, and many other circumstances that can be learned from the rope makers or those who sell the strings.’

As Dowland mentioned, oil makes gut strings translucent. In our experience, it confers considerable translucency to very thick high-twist and roped strings as well as thin ones. Mersenne’s mentioning curing implies a process that takes time, and this could well involve the ‘drying’ (i.e. polymerisation) of a drying oil. I suspect that the opacity that develops in time involves the oil getting stiff enough so that when some surfaces between oil and gut shear (possibly from stresses induced by swelling under wet conditions), they can never fit together that closely again, leaving small voids that scatter light. Subsequent re-oiling of old strings fills these voids with fluid oil, some of which gets squeezed out on hard stretching.

We’ve measured the diffusion of water vapour through thin films of a variety of materials commonly available in the 16th and 17th centuries, and found that the material that was best as a moisture barrier was linseed oil. This is thus a likely candidate for the drying oil used. The oiling of gut strings for musical instruments was probably for better tuning stability and longevity (by resisting moisture rises in the gut that lower the tensile strength), and possibly reduced internal sound absorption.

The index of refraction of oils varies from 1.45 to 1.50. Since they clarify gut, the index of refraction of gut is not far from this range. To not destroy clarity, a loading material would have to have a similar index of refraction. Since index of refraction tends to vary the way density does, it is highly unlikely that a material that is dense enough to be useful for loading would allow clarity to remain in a gut string.

Dowland described new string colour as ‘cleere whitish gray’ for Trebles and ‘cleere against the light’ but otherwise ‘blackish’ for Basses. I can’t imagine how grey or black pigments could be involved, and suspect that these were what we would describe as transparency when there is a dark background. He only mentioned the colour brown with respect to old strings. Surviving 19th century strings are very brown. They weren’t originally since the guts were bleached with sulfur dioxide (from burning sulfur) before being twisted together (hydrogen peroxide is used for this nowadays). If we make a thick string from unbleached fresh cleaned gut, it is very brown. Treating it with linseed oil leaves it still more brown than blackish. Thus early string makers either bleached their gut or their oil treatment led to more transparency than linseed oil gives (or both). The latter could have happened if a resin was mixed with the linseed oil (making it a varnish), and it raised the index of refraction to more accurately match that of gut than linseed oil does. We need to explore this problem further. It is probably unfortunate that the more we can get the gut to look like nylon, the more authentic looking it seems it will be!
Both Martin Goetze and myself are keenly interested in a reconstruction of an early English 10ft organ. I would like to help him to write a formal proposal for financial support for such a reconstruction, and to canvass the grant-giving bodies to generate interest. But no organisation would consider giving a grant to recreate an important historic instrument if there was still controversy on the research that defines that instrument's character, of which pitch is usually considered a crucial part. This introduces an urgency into our efforts to resolve our differences.

A reconstruction of Dallam’s original Magdalen College chair organ is Martin’s objective. That is very much worth doing, but it won’t attract the finance if there is any question about how typical of the period it is. This has limited the objectivity with which Martin has been able to approach our dispute, since I contend that its original pitch was atypical, but it became typical after the first pitch change not long afterwards. If I am right, I would support reconstructing it to that more typical later state.

It is important that we both try hard to resolve our differences. That can be done only if we both give up the luxury of trusting in our judgment on the issue (which for both of us was based on weak evidence that we found convincing), and commit ourselves both to get the most out of the evidence that is available, and to accept the pitch that best fits all of that evidence. The evidence produced so far does not thoroughly favour one position or the other. Further evidence, that Martin has access to and I not, is in the pitch history of St Paul’s organs and in the work that can be seen was done on the ends of the surviving Magdalen College chair organ pipes. In this Comm. I hope to show clearly how this evidence can decide the issue.

The Disagreement

As I see it, the basic issues are: Martin claims that ‘choir pitch’ associated with the 10 ft organ pitch standard was 1 1/2 ± 1/2 semitones above a’=440 Hz, that the Magdalen College organ was built to the 10 ft standard, and that ‘Church pitch of f’ was another name for this standard. My claim is that ‘choir pitch’ associated with the 10 ft pitch standard was 2 1/2 ± less than 1/2 semitones above a’=440 Hz, that the Magdalen College organ was at the 10 ft standard by 1690, but was built at a semitone lower 10 1/2 ft pitch level, and that ‘Church pitch of f’ was a standard a whole tone lower than the 10 ft one.

‘Gamut in De Sol Re’

We agree in interpreting the standard name ‘Gamut in De Sol Re’ as the same pitch level as standard 10 ft ‘choir pitch’ but disagree about the keyboard. Martin assumes that it was a C-d³ keyboard transposed to FF-g², and the name meant that if one pressed the untransposed ‘d’ key, one sounds the transposed ‘G’ note. This is ambiguous as to whether that ‘G’ note was on that organ or on a standard 10 ft organ. If the latter was the case this would be a true pitch standard, and if the former was the case it would not, only describing how the transposition works. This is important because there were organs that were not at the 10 ft standard. Whenever an organ’s pitch was mentioned, it was usually supplemented by a statement equating that pitch with the pitch of specific other organs. If all organs were at the 10 ft standard, this would not be necessary, and there would be no need to mention the pitch at all. On a non-standard organ the transposed ‘G’ note would obviously be at a different pitch than on a standard organ.

My assumption is that it was an untransposing FF-g² keyboard, and the name meant that if one pressed a ‘g’ key, one sounds a note that would have been called ‘d’ on the traditional standard
old keyboard. In support of my contention I cite the Nathaniel Tomkins 1665 specification of the pipe called 'F' in 'quire pitch' and 'c' according to the keys for his father's music written before the Commonwealth. If that was identical to current common practice, as Martin's position implies, there would have been no motivation for mentioning that specification. Additional support is in the Thomas Tamer 1665 Winchester organ contract, which specifies a 'Gamut in De Sol Re' pitch level with the longest pipe being 13ft long. Since this pitch level implies that the 10 ft pipe would be called FF, the 13 ft pipe would be called CC. It is much more likely that the keyboard had the normal C chromatic compass than it was G chromatic and transposed down to C chromatic when played. So in this instance at least, this pitch standard name was associated with an untransposing keyboard. If Martin has clear evidence for a transposing keyboard then, he should mention it, and I will then happily accept his position.

'Church pitch of F'

Concerning Gerard Smith's term 'Church pitch of f', Martin assumes that it meant that the lowest note was called F in 'choir pitch'. This is ambiguous as to whether 'choir pitch' referred to the standard 10 ft pitch or to the local 'choir pitch' of an organ that might not conform to the standard. G. Smith wrote that it was the pitch of St. Paul's in 1724/5. That organ was built by Bernard Smith in 1695/6, replacing an earlier Dallam instrument which was a standard pre-Commonwealth 10 ft organ as mentioned in Tomkins's 1665 specifications. So, according to Martin's assumption, the B. Smith organ was identical in pitch and type of keyboard to the earlier Dallam instrument. Subsequently we know that the B. Smith organ was considered 'sharp' around 1800, was dropped a semitone in 1802, and that by 1835, its pitch was a'=425 Hz according to the notes with the Foundling Hospital fork. Thus for Martin to be right, the pitch of St. Paul's would have had to drop another semitone between 1724/5 and 1802 without a record of this being noticed. How reasonable this might be depends on how complete the records of this organ during this time are.

My assumption of what 'Church pitch of f' meant is that pressing a 'c' key sounded a note that would have been called an 'f' on a standard traditional old 10 ft organ. This implies that the B. Smith organ at St. Paul's was built a tone lower than the standard 'choir pitch' of the Dallam organ it replaced. This pitch level was called 'Gamut proper' by R. Harris, and if there is a difference it might be that 'Gamut proper' implied a G short octave and 'Church pitch of f' did not. On my assumption, no unreported pitch changes to the organ are required to fit the evidence.

Of course my assumption is based on a standard 10 ft pitch that is a semitone higher than Martin's, and which assumption fits the evidence better strongly supports the associated view of what 10 ft pitch was. I've mentioned all I know about the history of St. Paul's organs. I'm sure Martin knows more. If in the 18th century a 10ft pipe was controlled by a 'C' or 'FF' key, this would support his assumption, but if it was controlled by a 'GG' key, it would support mine. If records of work done on the organ were well kept and survive, it would make his assumption very unlikely. From what happened to other organs around 1700, I would think that it would be very unlikely for a new organ to be built at 'Gamut in De Sol Re', lower pitches being preferred. This favours my assumption. It would be helpful if Martin would think about all this and tell us what he knows. It is not good enough to give us his expert's opinion without fully carrying through its implications with respect to the evidence.

The Magdalen College Chair Organ

We know the original pitch of this organ from the undisturbed original pipes (Comm. 1261 with further calculations in Comm. 1290). The pitch for C at a'=440 Hz was FF# + 40 cents ±12 cents. This is just a couple of cents short of an equal temperament semitone lower than the theoretical pitch of a 10 ft pipe reported by Mendel. This might not just be coincidence. When a semitone change in an organ was mentioned, the term used was 'half a note'. This was probably meant literally, being half of a tone in meantone temperament. That would be 97 or 98 cents for quarter comma or sixth comma temperament respectively.
The Harris 1690 contract indicates that before his alterations the organ was at 'Gamut in De Sol Re' position. This is a pitch standard, and if this organ originally was at 10 ft standard (Martin's position), there would not need to be any pipe movement before then. If it was a semitone lower than 10 ft standard (my position), the organ would have had to be raised to 10 ft standard at some time before the Harris work. The final pitch of the organ after the work on it in the 1730's is half a semitone below modern. That is a fifth below its original pitch, a tone below Martin's 'Gamut in De Sol Re' and a minor third below mine. The problem is to find whose position is best supported when we try to postulate a series of organ's pitch changes that is consistent with the evidence of the pipes. The evidence each pipe offers is the set of markings, the style in which each marking is written, the measured final pitch and the pipe pitch alterations possible from observation of the work done on it.

There is a large amount of evidence here, and instead of being bewildered by it because the interpretation is not immediately apparent, we should be thankful because its quantity is likely to narrow down the possibilities to very few clear histories of the organ's pitch and what happened to each pipe in time. It can be appropriate to assume errors in the evidence, but only if no solution of histories of the organ and each pipe can be found otherwise. Martin and his colleagues have not analysed the evidence to the point of offering such solutions, so their speculations about errors in the evidence cannot be taken seriously. Nor can their speculations about the pitch movements of the organ.

Analysis is made much easier if one has a system for keeping track in time of the nominal pitch of the pipe, the pitch of the organ and the pitch of the pipe's sound. My suggestion for this is a table for each pipe showing semitone differences. Each column represents the situation at a particular time. There are seven columns: The first is the original instrument built by Dallam. The second column covers the time between then and just before Harris did his work. The third column is the 'Gamut in De Sol Re' state immediately before Harris's work. The fourth column is what Harris did. The fifth column covers the time between the work of Harris and the work done in the 1730's. The sixth column is the work done in the 1730's modification. The seventh column represents the time between the 1730's and the surviving current state.

The top row is the pipe marking (nominal pitch) that one postulates was relevant in each time. In the next row is the information in the top row shown as the number of semitones above (+) or below (-) the pitch in the first column. It is the total nominal pitch change (called TNC) since the beginning. The next row is the nominal pitch change (NC) that occurred in that time. The TNC of each column is equal to the NC there plus the TNC of the previous column. The next two rows are identical to the last two, but referring to the organ pitch. So the TOC is the total change in the organ pitch since the beginning, and the OC is the the change in organ pitch at that time. The next two rows are identical to the other pairs but refer to the sounding pitch of the pipe. So the TPC is how many semitones sharper (+) or flatter (-) than its original state that the pipe was at, and the PC is the change in the pipe's pitch that occurred then.

Very useful relationships in each column are: TNC plus TOC equals TPC, and NC plus OC equals PC. This procedure is much more difficult to describe than to use. It is simply a matter of pitch book-keeping. It is to postulate the sequence of TNCs (pipe pitch markings) and OCs (organ pitch changes), and fill up the table. The object is to calculate the PCs (changes in the pipe's pitch), and see if they are possible in view of the evidence of the work done on it. If not, one keeps tinkering with the assumptions until one gets a possible pitch history of the pipe. Finding one is not proof that the assumed OCs are correct since others might also work, but not finding one is lethal. A very gratifying aspect of this analysis is that it does not require any assumptions to be made about pipe movements.

There is a problem about what one calls the pitch of an organ. One assumption is that it is the relationship between the pitches of the keys and the pitches of the pipes they control. The other is the relationship between the organ holes and the pitches of the pipes that go into them. I consistently am using the former. I suspect that inconsistency in the use of these assumptions is the reason for the difficulty in Martin's descriptions of pitch changes (-1 + 1 = -2). To find the number of holes left (-) or right (+) shifted by the pipe in any time column x, it is NC(x)
plus the keyboard shift in that column. A keyboard shift of -5 occurs in column 3, going from a G chromatic to an FF chromatic one, the latter being either real or by transposition. The nominal pitch (NC(3)) goes up a fourth (+5), so the hole shift is zero, as expected. The other keyboard shift, also of -5, occurs in column 4 for pipes other than the lowest ones, going from an FF chromatic to a GG short octave one. Since in the sample pipe, the Harris marking 'd#' is a fifth above the Dallam marking 'g#', TNC(4) is +7. Since TNC(2) is -1 (the 'g' pipe marking), TNC(3) is then +4, so NC(4) is +3. Thus the hole shift is -2. After Harris, since there are no more keyboard shifts, the hole shift is the same as NC.

Let us now use this scheme to find possible histories for the sample pipe. First let us see how much of the table can be filled in without touching on controversial aspects of the organ's pitch history. Harris dropped the organ's pitch by a semitone. So OC(4) is -1. Since this is the case and NC(4) is +3, PC(4) must be +2. Since the organ's original pitch at a\textsuperscript{#} = 440 Hz was f\# + 40 cents for 'c', and 'g#' (Dallam's original marking for this pipe) is 4 chromatic plus 4 diatonic semitones higher, the original pitch of this pipe at a\textsuperscript{#} = 440 Hz was d + 10 or 20 or 30 cents (to the nearest 10 cents) for quarter, fifth or sixth comma meantone temperament respectively. The final pitch of the pipe, as reported by Martin, is d + 15 cents, the same as it originally was. Thus TPC(7) is zero. Then, with the scoop cut out, TPC would be +2, with it filled in it would be +1, and with the extension added it would be zero. We still do not know which columns most of these will appear in.

The final pitch of the organ was a fifth lower than its original pitch, so TOC(7) is -7. Since TPC(7) is zero, TNC(7) must be +7, implying that the final pipe marking was 'd#'. We thus know the time sequence of all of the pipe marks, but after Harris (column 4), there still is some ambiguity as to which column each one starts at. When going from a C keyboard to an FF (or transposed C) keyboard, the organ's pitch drops a fourth, so OC(3) is -5. Since this is the case and, as mentioned above, NC(3) is +5, PC(3) must be zero, as expected. We expect that the only change in the pitch of the organ after Harris was in column 6, so both OC(5) and OC(7) are zero. Since TOC(7) is -7 and OC(7) is zero, TOC(6) must be -7 also. Of course, all numbers under 'g#' in column 1 are zero.

The crucial difference between Martin's and my assumptions is whether 'Gamut in De Sol Re' was a fourth (Martin) or a major third (me) lower than the organ's original pitch. So his TOC(3) is -5 while mine is -4. He can have his way if OC(2) (and TOC(2)) are zero. The 'g' pipe marking (making NC(2) and TNC(2) to be -1) would then imply that TPC(2) and PC(2) are also -1. This can only be accomplished by adding a semitone extension to the pipe that there would be no evidence for since it was later removed. We can now fill the rest of columns 3 and 4 by using the relationships between table entries as before, and get TPC(4) to be +1. Thus Harris cut away the extension and cut away half of the scoop. If the only pitch change to the organ after Harris was in the 1730s (column 6), OC(6) can only be -1. This allows us to fill the TOC and OC rows. There are three incomplete columns left and three changes to the pipe to be accounted for: cutting out the rest of the scoop (PC being +1), filling in the scoop (PC being -1) and adding the final extension (PC being -1). Putting these in the remaining three columns allows the table to be satisfactorily completed. Martin's standard pitch can work for this pipe assuming that all the evidence about it is true and is correctly interpreted. Mine can too (this replaces my former faulty analysis). Following are the tables of how they work:

<table>
<thead>
<tr>
<th>Martin's 10 1/2 ft = '10 ft' Standard</th>
<th>My 10 ft = '10 ft' Standard</th>
</tr>
</thead>
<tbody>
<tr>
<td>column 1 2 3 4 5 6 7</td>
<td>column 1 2 3 4 5 6 7</td>
</tr>
<tr>
<td>marking g# g (c) d# e e d#</td>
<td>marking g# g (c) d# e e d#</td>
</tr>
<tr>
<td>TNC 0 -1 +4 +7 +8 +8 +7</td>
<td>TNC 0 -1 +4 +7 +7 +8 +7</td>
</tr>
<tr>
<td>NC 0 -1 +5 +3 +1 0 -1</td>
<td>NC 0 -1 +5 +3 0 +1 -1</td>
</tr>
<tr>
<td>TOC 0 0 -5 -6 -6 -7 -7</td>
<td>TOC 0 +1 -4 -5 -5 -7 -7</td>
</tr>
<tr>
<td>OC 0 0 -5 -1 0 -1 0</td>
<td>OC 0 +1 -5 -1 0 -2 0</td>
</tr>
<tr>
<td>TPC 0 -1 -1 +1 +2 +1 0</td>
<td>TPC 0 0 +2 +2 +2 +1 0</td>
</tr>
<tr>
<td>PC 0 -1 0 +2 +1 -1 -1</td>
<td>PC 0 0 0 +2 0 +1 -1</td>
</tr>
</tbody>
</table>
The histories implied by the tables are: On Martin's assumption of 'Gamut in De Sol Re', the organ dropped a semitone in the 1730s, while on mine it dropped a tone then. His 'e' marking happened before the 1730s, while mine happened during the work then. His 'g' marking (before the Harris work) involved a semitone extension to the pipe, while mine involved raising the organ by a semitone then. Harris's work removed the extension and cut half the scoop in the pipe on Martin's assumption, while Harris cut the full scoop on mine. Between Harris's and the 1730s work the full scoop was cut on Martin's assumption, and the pipe was unaltered on mine. On both of our assumptions, the scoop was filled in during the 1730s work and the extension added afterwards, leading to the final 'd#' marking.

These are the implications of the evidence on the pipe. That evidence does not choose between the assumptions of Martin and me. The evidence on other pipes might. In Dominic Gwynn's article 'Organ Pitch in Seventeenth Century England' (BIOS Journal 9 (1985)), there is a table (Table 4, Appendix B) giving the markings that can be read on all of the front pipes. They are in four columns titled 'Original pipe-mark', '17th cent. re-marking', '18th cent. re-marking' and '18th cent. position'. The lowest 19 notes have pipes in three towers. There are 26 higher notes. The tower pipes had very different 17th century histories than the others since on the tower pipes the re-marking pitch is lower than the original (of the 12 where both are marked, one is the same (the lowest note), ten are 1 semitone lower and one is 2 semitones lower), while on the other pipes it is higher (of the seven where both are marked, all are 1 semitone higher). The sample pipe discussed above is typical of the tower pipes in this respect, with it being one semitone lower. Martin identifies the next marking in that pipe as that of Harris, so we assume that the '18th cent. re-marking' column has been reevaluated to be Harris's 1690 marking. Thus the '17th cent. re-marking' column corresponds with my column 2, the '18th cent. re-marking' column with my column 4 and the '18th cent. position' column with my column 7. Thus TNCs for each pipe can be derived from the pitches on Gwynn's table.

We cannot carry through an analysis on any other pipes because information about the work done on them was not reported. Yet we might pick typical pipes and make some predictions about what might be seen. To determine what might be typical pipes, let us survey the TNCs. The TNC(2)s are given above. It is useful to look at the tower and other pipes separately. Amongst the tower pipes, of Harris's TNC(4)s, one is +5, six are +7 and five are +8, and of the final TNC(7)s, two are +6, seven are +7, two are +8 and one is +9. Thus a typical pipe would have the TNC set of: 0, 1, 4, 7, 7, 7, 7. Amongst the other pipes, of Harris's TNC(4)s, two are +8 and nine are +9, and of the final TNC(7)s, one is +8, six are +9 and one is +11. Thus a typical pipe would have the TNC set of: 0, 1, 6, 9, 9, 9, 9, 9. Following are my tables for these typical pipes, with Martin's assumption on the left and mine on the right, and with the tower pipes above and the other pipes below:

### TYPICAL TOWER PIPE

<table>
<thead>
<tr>
<th>Column</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>TNC</td>
<td>0</td>
<td>-1</td>
<td>+4</td>
<td>+7</td>
<td>+7</td>
<td>+7</td>
<td>+7</td>
</tr>
<tr>
<td>NC</td>
<td>0</td>
<td>-1</td>
<td>+5</td>
<td>+3</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>TOC</td>
<td>0</td>
<td>0</td>
<td>-5</td>
<td>-6</td>
<td>-6</td>
<td>-7</td>
<td>-7</td>
</tr>
<tr>
<td>OC</td>
<td>0</td>
<td>0</td>
<td>-5</td>
<td>-1</td>
<td>0</td>
<td>-1</td>
<td>0</td>
</tr>
<tr>
<td>TPC</td>
<td>0</td>
<td>-1</td>
<td>+1</td>
<td>+1</td>
<td>+1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>PC</td>
<td>0</td>
<td>-1</td>
<td>+2</td>
<td>0</td>
<td>-1</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
</table>

### TYPICAL OTHER PIPE

<table>
<thead>
<tr>
<th>Column</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>TNC</td>
<td>0</td>
<td>+1</td>
<td>+6</td>
<td>+9</td>
<td>+9</td>
<td>+9</td>
<td>+9</td>
</tr>
<tr>
<td>NC</td>
<td>0</td>
<td>+1</td>
<td>+5</td>
<td>+3</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>TOC</td>
<td>0</td>
<td>0</td>
<td>-5</td>
<td>-6</td>
<td>-6</td>
<td>-7</td>
<td>-7</td>
</tr>
<tr>
<td>OC</td>
<td>0</td>
<td>0</td>
<td>-5</td>
<td>-1</td>
<td>0</td>
<td>-1</td>
<td>0</td>
</tr>
<tr>
<td>TPC</td>
<td>0</td>
<td>+1</td>
<td>+3</td>
<td>+3</td>
<td>+2</td>
<td>+2</td>
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<tr>
<td>PC</td>
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<td>+1</td>
<td>0</td>
<td>+2</td>
<td>0</td>
<td>-1</td>
<td>0</td>
</tr>
</tbody>
</table>
In all four tables we see that Harris cut the deepest scoops and the 1730s work filled them in. The difference between the final pitch and that of the deepest scoop previously is one semitone on Martin's assumption and two semitones on mine. This prediction seems to be a good way of letting the evidence on the pipes choose between the assumptions. Martin please check. Pipes numbered 19 and 20 are typical of the tower pipes, and those numbered 10, 12, 33, 34 and 35 are typical of the other pipes.

Other Points Raised in Comm. 1383

I am grateful for the information that display pipes, the longest pipes of the organ, generally were longer in front and shorter in the back. So when I have said 'the 10 ft pipe is really 10 ft long', I should have said 'the nominal 10 ft pipe that would have actually been 10 ft long if it was constructed like the non-display pipes were'. Sorry. I suspect that France was the only European country beside England where a single length standard was recognised over a region so widespread that each member of a school of organ makers and fixers could expect a lifetime of employment within that region. So when Mersenne wrote that organ makers couldn't go wrong if they cut their pipes accurately to the appropriate theoretical lengths (i.e. an 8 ft pipe is 8 Parisian feet long), there is reason to expect that this applied to England as well.

I would be very surprised if research into the ranges of vocal music in Magdalen College choir books in the 1640s compared to a similar set elsewhere would detect a difference of a semitone in pitch standards. Transposition for the sake of a semitone would probably be quite rare. But if someone is more optimistic about this than I am, I would support their efforts.

Martin raises the issue of wind ensembles playing for church services as if the wind pitch levels are more certain than organ pitch levels. Our ideas about wind instrument pitches derive from modern blowing of surviving instruments. For a pitch so derived to be relevant to an organ pitch level, there needs to be evidence relating the pitch resulting from modern blowing with the pitch resulting from the original blowing style, as well as evidence relating the pitch standard that the instrument was made to with that of the organ. The only example I am aware of in which these criteria are met is with late 17th English recorder players and the contemporary organ pitch standard called 'Consort flute pitch'. This is because differences in blowing style can only vary recorder pitch by less than half a semitone and the name of the organ standard associates it with recorders.

On other wind instruments, varying blowing style can vary the pitch much more than on recorders. There is evidence that apparently indicates that there is a difference of a semitone between modern blowing of sackbuts and original blowing in Praetorius's time (see Comms 1371 and 1372). Sackbuts were traditionally used in mixed ensembles, often supporting or replacing voices, so there is more likelihood that a surviving sackbut played with organs than than there is for most other types of wind instruments to do so. But since transposition was so common amongst professional players of wind instruments then, if there was evidence that a particular sackbut played together with a particular organ, that would not be strong evidence that they were built to the same pitch standard.

Nuremberg supplied sackbuts to all of Europe, and it appears that both of the sackbuts of Praetorius and Mersenne were made there. Their pitch standards differed, and so did their sackbut lengths (and thus pitches). There is no evidence about whether modifications to local pitch standards were made in Nuremberg or locally. Contrary to what many early music wind specialists today believe, the tendency towards uniformity in the pitches of surviving Nuremberg sackbuts says nothing about any local pitch standards, which we know varied enormously throughout Europe.

I hope that this Comm is clearer than Comm. 1347. Much of the ground covered there is covered here, but not all of it. For example, I am open to negotiation about the details of Martin's eating his hat (as he promised in Comm. 1329 if anyone offered a consistent pitch specification interpreting the Wells contract, which I did in Comm. 1347).
Veneering with Hot Hide Glue - response to Comm. 1382.

I congratulate Margaret Hood on her Comm. on Veneering. She is obviously very good at it, and she is right in saying that it is easy. May I add a few more tips?

1. **The glue.** It is available in slabs or pearls. Always buy pearls (or other fine dust-sized particles). Slabs are almost indestructible, but have to be broken up to get them into the pot, and have to be soaked for 24 hours. Pearls can be melted soon after putting in the water, if you have to. The quality of the glue is terrific. Don't listen to people saying that modern glues are better. The only problem is the bother involved in melting it.

2. **The pot.** I have always used a conventional double cast iron pot with water in the outer container, on an electric ring. No problems. But don't wash your brush in the water in the outer container or it will make it boil over. A wire stretched across the pot to wipe the brush on saves a lot of mess. Wash the pot out after use. Congealed dry glue in it will give you grief.

3. **The veneer hammer.** I have used a conventional veneer hammer, cut down to about 50mm wide to apply more pressure. Mine always came with a brass tongue in them, not iron which is obviously wrong. But better I found was a narrow wallpaper seam roller from a DIY shop. Rollers don't tend to push the veneer along, as the hammers do.

4. **The iron.** I used a pensioned-off standard domestic iron. Not a steam one. The base would get covered in burnt glue, which I had to scrape off from time to time.

5. **The hot glue.** Yes, consistency is absolutely critical. You dilute the hot glue gradually until it runs off the brush, dropping into the hot glue in the pot with a rattling sound. Either too thick or too thin and it doesn't rattle. The big secret is in the sound. Try it.

6. **The base.** I always toothed it with a teething plane. It holds more glue better. It also acts as a check that the base surface is flat. Toothing is at 45° to the base grain both ways.

7. **Laying.** I agree with Margaret's helpful description of laying, with only a couple of variations. I never put glue on the top of the veneer - it just burns onto the iron. And I never did it in a hurry. If the glue chilled before laying, no problem, I just warmed it up again with the iron as I worked across it. Glue has to squeeze out everywhere. If it squeezes out you know it's worked. Bumps can be easily found by brushing your fingers across the panels, you hear a a rustling sound on the bumps, as she says. These can be re-warmed and pressed down. Really obstinate bumps in bumpy veneers can be cramped.

But to add some more: As she says, it is necessary to put water on the veneer to prevent burning etc, but it also tends to make the veneer expand. This will put a massive pull on the surface when it dries, and will easily warp the base wood. Unsupported panels should be veneered on both sides on the same day. Minimising the damp on the veneer is one of the secrets of success.

8. **The veneer.** I usually used normal 0.7mm veneer, which can be easily cut with a scalpel after laying. But I have also laid thicker from time to time. I have layed many woods including Sycamore, Ebony, Burr Walnut, inlay lines, marquetry panels etc.

9. **Large panels.** If you cut the veneer larger than the panel it is easy to break bits off during laying, where it hangs over the edge. If you cut it smaller there is no problem. Then you can trim the edge of what you have laid with a scalpel and put an inlay line round, held in place with pins. And a cross banding, of short wide pieces cut from a straight-grained veneer. All in the traditional way. Faults in veneers such as Burr Walnut can be punched out after laying with wad punches and patches cut with same punch put in.

10. **Joints.** All joints in laid veneer must be immediately covered with gummed paper tape to prevent the veneer pulling away from the joint when drying.

11. **Cleaning up.** The next day, the paper tape can be moistened and peeled off, and any glue on the face of the veneer can be scraped off with a cabinet scraper. The type of scraper which has a plane-like construction and two handles is far better than the simple piece of flat steel which burns your thumbs in use and can catch on and damage the veneer. Also, if you want to scrape inlay or marquetry it is essential. Scraping is far better than garnet paper for this as it doesn't clog and it doesn't push the dust of one wood into the grain of another - just try Sycamore with black inlay lines in it.

OK, so that's just a bit more of an outline - have fun.
I read with interest the detailed review of the book *De Clavicordio* Proceedings of the International Clavichord Symposium by Peter Savington (Comm. 1360). About the early use of the word *manacordo* and sordino, which appeared in various written versions, which are already well-known, I think it is of interest to divulge some other informations I recently come across.

As to *manacordo*, the earliest and most interesting are in documents of the 15th century in Ferrara, Milan and Rome:

- 22.10.1438 one *manacordo* was bought by the sacristy of Ferrara Cathedral for the clerk Antonio Massimo da Vicenza, pupil of the organist of the cathedral canonic Iohachino de Canzeleris, for his organ study: «Item die XXII mensis octobris expendi quos solvi presente ipso Antonio Maximo clerico domini loachini de Canscleriis pro uno manacordo causa adiscendi pro eum, unum ducatum: L [iras] II, s[olidos] III, d[inaros] 0.».

The same *manacordo* was repaired or its strings were replaced in the subsequent year:

- Item die IIII mensis februarii, habuit [Antonio da Vicenza] causafaciendi aptare suum menachordum pro cordis: L [iras] 0, s[olidos] 0, d[inaros] 0.»

- Also, about the instruments in possession of Costantino Tantini, organ builder of Modena, we are informed by a letter dating 6th October 1734 of Galeazzo Sforza (Milano) to him, that he owned some instruments of harpsichord, organs, and manacord [type] well [built] «alcuni instrumenti de clavicembalo / organi et manacordi in perfectione».

- 8th March 1435 «due [two] manacordi» were listed in the testament of Francesco degli Agli.

The famous humanist Lorenzo Valla (Rom, 1405-1457) was interested [c. 1440] in *norgana*, *monachordium* and «clavicymhalum» presumably for semantical reasons.

As to the *sordino*, I have two quotations found in Rovereto (Italy):

- the first, *sordino* unquestionably used to indicate the clavichord, from G.G. Ferrari (1763-1842), that in Rovereto, where G.G. Ferrari was born and lived before going to Paris and London, there were some «spinette» and «sordine», not too bad, with a range of 3½ octaves: «A quel tempo, anno 1775, non s’era ancor veduto un Pianoforte in Rovereto, né si poteva procurare un Clavicembalo in affitto. Eravano delle Spinette e delle Sordine, passabili, a tre ottave e mezzo, fatte da un certo Chiusole, tabaccaio, genio naturale per le meccaniche».

- the second, thus, *sordina* like a kind of wind-instrument better known as «sordellina», that existed in the same town a «piva sordina», listed in the testament of Gaspare Troilo by the notar Andrea Cobelli, Rovereto, 17th July 1612.

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Was the fortepiano built as a folk instrument? Comm. 1401

Marco Tiella

Historical background.

About 1970 an uncommon keyboard instrument was found in an antique shop in the mountains around Modena (Appennino modenese, Italy). Neither the site nor the age of the instrument are known. Nothing is known about the use and musical tradition of such a keyboard instrument.

The Italian ethnomusicologist Prof. Roberto Leydi, Milano, informed me that at Barigazzo (in the country near Modena) the forebears of the stringed instrument maker Oreste Tassoli worked as “cembali” makers. It is extremely difficult to state which kind of keyboard instrument they intended by the name “cembalo”. It is well known that keyboard instruments can only be vaguely defined in Italy between the 19th and 20th centuries. The more common name for a keyboard instrument of unusual shape, whether a square or grand, was, and still is, “spinetta”.

Description of the instrument.

The instrument can be defined as fortepiano for it was made according to a building technique forerunning the use of metal frames and of repetition- and double escapement-actions.

The instrument has the shape of a tiny grand fortepiano, whose structure consists of a strong wooden frame, of thin or very thin walls, and of a robust reinforcing brace of iron running longitudinally between f and f#.

The chromatic range is of 51 tones (from F to g3) with double choirs for all the tones. The action is like the English single action, without stops and dampers.

The instrument appears to be roughly made in comparison to the quality of the more common fine grads, but of the normal standard of usual furnitures. The frame is also made of rough-hewed tree trunks.

State of conservation.

When the instrument was found, it appeared to be nearly complete, apart the lack of the nameboard and the front strip of the lid, of three keys and the strings for F and G#. Most hammers were missing.

On the soundboard there are two round holes, whose origin is uncertain. On the bottom there is a larger opening, apparently original, through which the internal framing and the soundboard bar disposition can be inspected.

The lid has three hinges on the left along the spine. The case without mouldings or decorations stands on three simply turned legs. The external parts of the case are of cherry-wood or other fruit-wood; the soundboard and the bottom are of spruce. The visible parts of the internal frame have been made of different woods: beech, maple(?) or fruit-wood(?).
Organological data according to the schedules of Leipzig University Musical instrument museum.

Builder: Anonymous

1 - No meaningful inscriptions.

2 - Range from F to g3, 751 apparently original iron strings of the following sizes (mm):

<table>
<thead>
<tr>
<th></th>
<th></th>
<th>(a)</th>
<th>(b)</th>
</tr>
</thead>
<tbody>
<tr>
<td>F</td>
<td>(lacking)</td>
<td>1008</td>
<td>103</td>
</tr>
<tr>
<td>G#</td>
<td>1.25</td>
<td></td>
<td></td>
</tr>
<tr>
<td>c#</td>
<td>0.95</td>
<td>870</td>
<td>82</td>
</tr>
<tr>
<td>f</td>
<td>0.88</td>
<td>754</td>
<td>70</td>
</tr>
<tr>
<td>c#1</td>
<td>0.75</td>
<td>547</td>
<td>53</td>
</tr>
<tr>
<td>f1</td>
<td>0.75</td>
<td>421</td>
<td>42</td>
</tr>
<tr>
<td>c#2</td>
<td>0.75</td>
<td>283</td>
<td>22</td>
</tr>
<tr>
<td>f2</td>
<td>0.72</td>
<td>223</td>
<td>15</td>
</tr>
<tr>
<td>c#3</td>
<td>(lacking)</td>
<td>156</td>
<td>13</td>
</tr>
</tbody>
</table>
| g3  | 0.72  | (a) Length of the strings | (b) Distance of striking points from nut (mm)

Some measurements of the strings are listed in the diagrams.

Tuning pins: diam. 7 mm., vertical, disposed on groups of two. Bridge on the soundboard of constant dimensions and cross-section (see on the drawing) h = 16 mm; b = 18 mm; nut h = 8 mm; b = from 25 to 40 mm (from treble to bass).

e - English action (single action), with the jacks made of wood screws of modern shape, which apparently replace the original jacks, as their previous holes are still visible. Hammer heads covered with 6 layers of common brown leather. No dampers at all. “Transmission ratio” almost constant. Minimal weight for moving the action: 50 gr. Distance between hammers and strings 40 mm. Distance of the point of contact from nut (see on the diagram).

Keylevers of spruce; rack with slots; octave span 160 mm.; naturals covered with plane-wood(?) l = 118; no decorations on keylever fronts; chromatic keys of stained beech, l = 75; key dip 7 mm. (original felts were missing); no stops.

4 - Case (see the drawings) with a large opening in the bottom and two other holes of uncertain origin in the soundboard. Bottom h = 12 mm; soundboard h = 5 mm., "mouldings" in rectangular cross-section; three legs to be screwed in.

5 - Height of the resonant box = 115 mm., volume 21.8 dm3; hole in the bottom 600 cm2.

6 - Sides and lid (omitting of the front strip) of solid cherry-wood. Thickness of the short side and spine = 12 mm.; of the bentside = 5,5 mm.
Plan view
Bottom view
Soundboard view from below

a) Cross-section of the ribs
Front view

Case transversal cross section
Internal frame

(carpentry)

(wrestplank)
Reinforcing brace plan view and longitudinal cross-section
Case longitudinal cross-section

Detail of the action
Length of equivalent c:

![Diagram showing lengths of c equivalent in semitones]

**Striking point ratio**

![Diagram showing striking point ratio in cm]
**1995 FoMRHI List of Members — 2nd Supplement as at 21 September 1995**

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