Quarterly No. 79, April 1995

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FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS

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

FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS

Bulletin 79 **April, 1995**

Sorry for some delay with this. I did go off on holiday (for once both my wife and I had meetings in the same country at much the same time, so we were in Amsterdam and Leiden, combining conferences with holidays, work with pancakes and Indonesian food, and very pleasant it was, too). So I'm a bit late starting, as well as having to invigilate the Bate myself because of staff holidays over Easter, plus dealing with all the post that was waiting for me to get back, so it's all added a bit more delay, but probably all in all not much more than you're used to.

There will, if Eph and Barbara decide we can afford it, be a new **List of Members** herewith. I did mention this last time, but I repeat it now: **Note the new UK phone numbers** - they have all been changed (I hate to think what this is costing everybody in printing new stationery etc).

FURTHER TO: **Advertising:** In addition to the Comm from John Rawson elsewhere herewith, I've had comments from Bernhard Folkestad: 'Stay independent - avoid ads', and Phil Lourie: 'Don't want adverts but if only way of continuing, will put up with them'. Nobody has written in favour of them, and I find John Rawson's arguments very convincing, so let's forget the idea.

Comm.1311: David Lasocki has promised a counter to this, though it's not yet arrived (it has now). He did correct me on one point - 'cane' in American does not only mean bamboo or similar material, which I took it for, but a walking stick (which I should have remembered; even in this country when we were caned at school, it was a walking stick that was used!). I should perhaps also say that one paragraph was perhaps too short; I said 'These all, however, are fairly minor considerations, and the book is laid out to be an invaluable research tool for anybody working on almost any aspect of the recorder. As I said at the beginning, 'when it is good it is very very good' and that is certainly true.' When there are points one wants to correct, inevitably they take up more space than the general praise, and this, at least in David's eyes, has led to imbalance. This is no comment on what he has to say, for I've not yet seen that, only a public repetition of what I've said to him by email.

Comm.1321: Anyone who is interested in more detail about the English theorbo is very welcome to come to the Friends of the Bate Collection AGM on Thursday, May 11 at 5.30 pm, after which Lynda Sayce will be talking about, showing us, and playing the English theorbo that David Van Edwards made for her, the first one to have been made since Talbot's time.

REQUEST: Lynda Sayce is compiling a catalogue of surviving extended-neck lutes of all types for her PhD thesis (I'm her supervisor, hence her request here), including archlutes, theorbos, liuti attiorbati, and also fragments such as bodies which have been reused on something else. Also fakes because it is important to get them off the lists of genuine ones! She would be very grateful to hear of anything on these lines, especially of course of examples in private collections and small museums which haven't got published catalogues. She says 'if your local museum is not certain which type of lute it has, please err on the side of caution and include any dubious specimens! All contributions will be acknowledged and all postage costs reimbursed'. Her address is 18 Park Town, Oxford OX2 6SH; tel/fax 01865-56137.

BOOKS: Jacques Leguy has taken over his father's bookshop 'Ars Musicae', which deals with all books on music, especially organ, and sheet music for the organ. He says that they have also many rare books and out-of-print books, and sell by post all over the world. His address is still the same - see the List of Members.

FoMRHI STRUCTURE: Paul White suggested that I ought to describe how we work for the sake of more recent members. The original idea was that there would be a small inner circle, called the Fellows, as a committee which would make any necessary decisions on subscription rates and any matters of policy. The Fellows would be those who had published information, ie those who were willing to provide knowledge for their colleagues, rather than those who just took information without giving. Their names are marked in the List of Members with a capital F in the left margin.

In fact, we have turned into an oligarchy, instead of the intended limited democracy, because there have been hardly any policy decisions to make, and we have only increased the subscription rates (we began at £2.00 a year in 1975!) when it was either do so or shut down, so while the Fellows did vote on these they didn't have a lot of choice! So the Fellows have had so little to do that it's been several years since anybody applied to join their ranks. It is also quite a while (other than just before the last Bulletin) since I asked the Fellows to decide on anything - partly my laziness, partly to save costs of mailing 40 or so people all round the world. So, as I said, we have wound up with, in practice, Eph, Barbara, and me taking any decisions. That seems to be the way you want it; when I asked in the last Bulletin whether you wanted a pretty radical policy change, the inclusion of advertisements, only four people responded (the fourth to say that he didn't mind one way or the other).

If any of you don't like the present situation, write and say so. If enough of you think that we ought to consult the Fellows more often (what about?), say so; I'll try to sleep less so as to have time to do so. If more of you want to join the Fellows, say so, but remember the basis under which we work, that it is the givers of knowledge who are entitled to control the direction in which we go. That sounds elitist, and so it is. If you want to change it, you're more than welcome to do so - join the elite by giving! That's all that the so-called elite is: those willing to spare the time to help others.

SHAVING SCHOLARS: Two or three more members have written to say that if they read any more about Occam's razor, scholarship, scholarly approach, and so forth they're going to cancel their subscriptions (and several wrote to say that they had done so for that reason). I'm inclined to agree with them. It's not what we are about. My own opinion is that we are concerned with instruments; there are plenty of other journals for articles on musicology, speculative biblical numerology, and so forth. Our subject is organology, and let's stick to it. The science, history, use, construction, etc of instruments is a quite wide enough field of study to keep anybody happy, whether Anglo-Saxon logicians, or practitioners of the scholarly approach, or just ex-drummers like me.

INVITATION TO DISCUSSION: Padraig ó Dubhlaoidh would like to see even more debates and suggests that people should air their pet theories. He is interested at present in the violin makers of Brescia, particularly in relation to the possibility that many instruments were double purfled and decorated to disguise drastic reductions, sometimes by the makers themselves. He would be happy to share what he describes as his little knowledge with any one similarly interested.

EARLY INSTRUMENT EXHIBITION: I imagine that you all know by now that this is at the Royal College of Music this year, and not at the Horticultural Halls. I hope they've changed their rules - I remember that we stopped having the Exhibition there years ago because we weren't allowed to buy and sell there, and we all got fed up rushing in and out to do all cash deals on the steps outside. Anyway, we'll be there, Friday to Sunday, September 8-10, and I

hope to see many of you. It'll be a bit early, but no reason why you shouldn't use the opportunity to renew your subscriptions - saves postage (even if we do have to adjourn to the steps)!

INTERNATIONAL CRAFTS FAIR: The Commission d'Encouragement aux Métiers d'Art, Région Nord/Pas de Calais, BP 21, F-59530 Englefontaine, is putting on a Crafts Fair July 8-17 in Le Touquet. There will be a forum on Musical Instrument Making during the Exhibition. For some inexplicable reason they are awarding the prizes for exhibits the day before the exhibition starts. If you're interested, write to them.

WOOD SUBSTITUTE: While I was in Holland I called on Jan Bouterse and he showed me a piece of what I can only call artifactual wood. It is something devised in Holland called, if I remember rightly, lignastone. It is a way of making fairly hard wood into really hard wood by taking beech, slicing it into very thin strips (they look like half a millimetre to me) and then glueing them back together. He is experimenting with it to see if it's any use for making woodwind or other musical purposes, and I hope he will report further in due course. He gave me a small block of it, which I'll keep at the Bate in case anyone wants to see it.

COURSES: The London Guildhall University has an interesting series of Music & Technology Research Seminars: May 9, Anthony Elmsly of Basle on *The internal examination of stringed instruments of the violin family*; May 16, Matthew Spring on *Towards a database of historical European Lutes*; May 23, Jamie Linwood on *The manufacture of the bronze instruments of the Javanese gamelan*; June 6, Terry Pamplin on *The reception history of musical instruments*; and June 13, Lewis Jones on *Musical instruments represented in intarsie in the studiolo of Federigo da Montefeltro, Urbino*. All at 41 Commercial Road (what used to be called the London College of Furniture), 5 pm, in Room 100A. All FoMRHI members welcome. More information from Lewis Jones (0171-320 1854) or Hilary Clay (0171-320 1842). And Lewis would welcome offers to contribute to the series.

The Bate Collection Bow-Rehairing Weekend in May booked up so quickly that we've arranged another one: July 1/2. That's filling up quite rapidly, too, so if you're interested, send your booking (cheque for £20, £15 student or Friend of the Bate) as soon as possible.

The Teknisk vitenskapelig dokumentasjonssenter for musikkinstrumenter provides prefessional training in Norway for instrument makers and has a series of courses 18-25 June for restoring and repairing wind instruments of most varieties, also for acoustics etc, and also for making technical drawings. It seems to be part of the Oslofjord Festival but while the courses are described in English, there is no information about where it's taking place (in the fjord would be a little damp) nor where to book. If you want a working holiday in Norway, which could be fun at that, write to Helgerødgata 33, Folke Bernadottesgate 8, P.b.1103 Jeløy, N-1510 Moss, Norway, which is where the information has come from.

CODETTA: That's it for the moment, while I do the List of Members.

CODA: That's now done; it's come out more closely spaced than last year and I don't know why. Doubtless I should have said 'Oh bother' and gone back and corrected it, which would have meant reparagraphing all the index entries. However, I'm afraid that I didn't and you'll have to put up with it as it is; at least it means that it will be shorter and therefore cheaper.

A couple of things have come up while doing it:

1 Barbara points out that a number of you are paying twice – nice for us but less so for

you. What happens is that the note saying 'time to pay for next year' is in every Bulletin, and the invoice goes into every envelope. If you're one of those who has sent us two or three years' sub, or a lump sum to nibble at, you'll still get the same Q as everyone else, so unless you remember that you've paid, there's the risk of paying again. We do know who you are and Barbara or I will try to write during the year to tell you that you're in credit, but try to remember yourself, too.

2 The Bate has a new plan, for the Potter 1 key flute with 3 corps de rechange, no.18; measured by Alun Williams, Ardal Powell, and Mathew Dart, drawn by Alun, 3 sheets, cost £15.

3 Whether chance, coincidence, non-representative numbers or what, but membership seems to be dropping outside Europe (USA, South Africa, Japan, for instance) but increasing in Europe, especially in the south. Several more members in Italy and Spain, for instance (and all very welcome). If this indicates a spreading awareness of and interest in early instruments it's good news. More surprising, a drop in the Netherlands which was one of the strongest countries outside the UK and still has some of the best training for performers.

DEADLINE FOR NEXT BULL: June 30, please; then I can get cracking while I'm invigilating the Bate for the Bow-Rehairing Weekend! And while I think of it, advance warning for at least a fortnight earlier than usual for the October Bulletin. Since I finish at the Bate on September 30, it is my present intention to go off on holiday on October 1st, so unless I can get the Bull off to Eph before I go, it'll be at least 3 weeks and more likely a month late. So put a note in your diary now for a deadline then for September 15.

FINAL POINT: Nobody noticed my request in the last Bulletin to note a **change of address** from the Bate Collection to my home, 171 Iffley Road, Oxford OX4 1EL. I asked Eph to change the address on the front of the Q, and he didn't, and I asked you to note it, and everything for this Q has arrived at the Bate! Get into practice please – I leave the Bate on September 30 and while inevitably some stuff will arrive there, we mustn't be too much nuisance to my successor. Difficult to get out of the habit after 13 years, but please do try. Email still works as usual, but please change your habits with snail mail.

Have a good spring and early summer.

Jeremy Montagu
Hon Sec FoMRHI

Bulletin Supplement

Ephraim Segerman

E-mail

I forgot to tell Jeremy that I am now on the Internet. I couldn't afford the equipment but a prominent maker gave it to me so that I could contribute to the violin makers group. A result is that Comms finished a week or two after Jeremy's deadline can be sent directly to me at: e_segerman@cityscape.co.uk

This Q

Contributions to this Q have been so numerous that to avoid getting into the next bracket of postal cost, I've held back a 4 page Comm. of mine on early 17th century gracing and sign interpretation.

Corrections for my Comm. 1316

There were a few errors in my Comm 1316 that need correcting. In the instructions of Greeting it is clear that shaking involves more than one oscillation, so the wind evidence is not all ambiguous. Marianne Mezger has written to me and pointed out a few more: Carr did not define both the shake and the beat in the strings way, only the shake. His beat also was shaked with the upper auxiliary, but started on the lower one. The dominance of lower shakes over upper ones in the repertoire changed in the last decade of the 17th century. The Anon tutor *The Compleat Flute Master* (1695) and following tutors defined the shake with an upper auxiliary and the beat with a lower one, as the string tutors had done.

Mezger also wrote: 'Your conclusion that *what falls comfortably under the fingers will often do*, is a much too simplified statement. In the flageolet and recorder sources there are quite a number of concordances, some notated in tablature, some on dot notation and some equipped with ornament signs. They most often vary only a tiny amount, even though the fingering and pitch between the two instruments is different. Why would tutors designed for beginners (amateur gentlemen) and illustrating the pieces of the hit parade of the day, go to such lengths to explain and indicate ornaments if they were not an integral part of the music and understood as such'. I don't disagree.

On Comm. 1318 Reporting 16th Century Evidence for Roped Gut Viol Bass Strings

I love the sound of roped gut bass strings, both plucked and bowed. The way I play them, they have a noisy sound, thunky when plucked and groggy when bowed. It is unashamedly masculine, and is dominated by the true fundamental frequency of the note it is supposed to be. I prefer that sound to the smoothness, sharper focus and slick blend with the treble strings that one gets with metal-wound basses. Most others prefer the sound of wound basses because that is the sound we all heard when we first fell in love with music. In an environment such as early music, where there is respectability value in being historically accurate, I am luckier than those others. That is because, for the music I particularly like (from before the late baroque), the evidence clearly indicates that wound strings are not historically correct, and roped gut strings have been the only historically viable alternative. Those others have either avoided the issue or postulated that there must be some historically valid alternative to roped gut that sounds (to them) better. One point they have often made is that there is no direct historical evidence for roped gut strings ever being used on a musical instruments. In Comm. 1318, John Downing reports finding a piece of such evidence.

In the Post Script of Downing's Comm he asks a few questions. As for the ropes of war machines being made of gut, a specialist on historical war machines once told me that this was the case with French medieval cross bows. He said that a very significant factor in the French defeat at Crécy was a rain shower earlier in the day which effected the gut of the cross bows, seriously impairing their operation, and the English long bows (with draw strings of waxed hemp) won the day. Gut is an excellent and durable rope material. A gut factory in the Manchester area that closed down a dozen years ago used to make a gut rope for premium quality window sash chords. There is at least one steel factory in Sheffield that still uses half-inch diameter twisted gut (when they can get it) to lower and raise a bucket of molten metal in a furnace because it is better than any other material they know of in surviving the effects of heat and corrosive vapours.

As for comparison with a big bowed-instrument string rather than a bass string from another instrument, the most prominent other instrument then would have been the lute. Venice Catlines were available from well before 1588, and most lutes were probably equipped with them by then. Mace tells us that Venice Catlins were smooth. That does not stop them from having rope construction (indeed they need something like rope construction to work properly), but it does stop them from looking like ropes. We are now making smooth catlines that our customers tell us are better than ever (we call them 'polished supertwist catlines').

BOOK REVIEW MICHAEL COLE

FORTEPIANOS AND THEIR MUSIC, Germany, Austria and England 1760-1800
Katalin Komlós, Clarendon Press, Oxford. 158 pages, UK price £27.50

Those who have read anything of Katalin Komlós's previous writing will know what to expect: a wide ranging selection of eighteenth century quotations from all manner of sources -- some of them delightfully unfamiliar -- pasted together in a sort of album style. Musical examples are marshalled in a similar way, selected snippets serving to illustrate and enforce quite general arguments. For anyone accustomed to more rigorous methods of proof it can be rather disconcerting. It is rather as if one were allowed only to peep through a tiny keyhole at a wonderful sunlit landscape before being whisked off to some other topic. If we are not quite convinced of the validity of an argument that we have just heard, our guide promises plenty of other treats to keep us moving along.

Dr. Komlós has divided this study into three sections -- dealing successively with the instruments, the music, and the players. The central premise, signalled as early as the second paragraph of the preface, is that the divergent characteristics of touch and sonority found in the English and German/Viennese pianos had a formative effect on composing styles. If this is correct specific traits in the works of Clementi, Dussek, and Cramer might be identified and contrasted with the Viennese style of Mozart, Koželuch and Vanhal. Thus the proposal that emerges is that "the singing melodies and tinkling accompaniment of Italian rococo keyboard music remained an essential element of mature Viennese classical fortepiano writing" which (we are told) is more linear and sparer in texture than the denser chordal sonorities beloved by the London school; the latter resulting from the richer sonority of the English pianos whereas the former was influenced by the cleaner articulation but more meagre tone of the Viennese instrument. Examples by Mozart and Dussek are contrasted to enforce the point. Such thoughts are further elaborated in a chapter devoted to Joseph Haydn, a special enthusiasm of Dr. Komlós as well as this reviewer, naturally paying particular attention to the change that came over his keyboard music after the first visit to London. The sonatas written for Therese Jansen and Rebecca Schroeter come readily to mind, but Dr. Komlós is equally anxious to draw our attention to the keyboard parts for the English Canzonettas; especially the "accompaniment" for "She never told her love" that threatens to become almost a piece for pianoforte with the accompaniment of the voice.

But to précis the author's arguments so briefly does her no justice. There is much more breadth to her argument, and diversity in the material she has assembled, than can possibly be condensed into a brief review. Everywhere one finds novel and interesting materials: how intriguing for example, to discover that Dussek played on an English piano in Hamburg as early as 1782. It is also clear that Dr. Komlós has painstakingly assembled a vast library of quotations from the early piano tutors, as well as studying a great variety of keyboard sonatas by playing them through on all sorts of pianos. Hers is not one of those tiresome studies that concentrates exclusively on the familiar "masterworks" of a select band of "great" composers. How welcome it is to find that Schroeter, Eckard and John Burton provide musical examples for discussion alongside J.C.Bach, (perhaps rather too much of the blundering Burton) and that Dussek and Clementi are properly elevated to positions of some significance. If rather less space is given to Mozart and the young Beethoven the author excuses it on the grounds that they already command an enormous literature. I would say further that Mozart is only Mozart. For a true

understanding of the musical culture of late eighteenth century Vienna one must study and relish the whole gamut of music which was then circulating. As Komlós pertinently reminds us, the *Jahrbuch der Tonkunst von Wien und Prag* extols not Mozart but Leopold Koželuch as the prime author of the Viennese enthusiasm for the pianoforte. "*Ihm verdankt das Fortepiano sein Aufkommen*". To be sure Koželuch's music is not in the Mozart class; the best of it is very beautiful, but some can also be rather shallow. Nevertheless, it is what seriously musical people were playing, and enjoying, not only in Vienna but all over Europe.

Interesting as this study is, a certain weakness pervades some of the key arguments. Nowhere is this more critical than in the much-vaunted distinction between the London and Viennese touch, which is crucial to this study. It is a curiously unnoticed fact that none of the authorities who are cited on this matter are, strictly speaking, contemporary with the music under discussion. Hummel's oft-quoted comments on the deeper and heavier touch of English pianos it should be noted date from 1828! Kalkbrenner, produced as a witness by Komlos (page 28), opines that "English pianos possess rounder sounds and a somewhat heavier touch" -- but this was written in 1830! By that time the design of the piano (particularly the English type) had passed through an exponential period of development. What had happened between 1800 and 1830 was nothing less than a systemic transformation. String diameters had increased by about 40%; this implies that the tensions had doubled. Soundboards became much stiffer. To activate this string and soundboard system the weight of the hammers had to be enormously augmented. This in turn implies that the gearing ratio between the finger and the hammer head had to be changed, otherwise the weight of the touch would have soon become quite insupportable. Consequently the makers had already begun that process of gear reduction which culminated in the 1860s in a key dip of 10mm, and 60 gms touch weight which has since remained more or less the norm. Is it therefore safe, in such an extraordinarily rapid period of evolution, to parade the comments of Hummel or Kalkbrenner as if they were relevant to eighteenth century pianos? The only cited source that comes near to the right period is Andreas Streicher's letter of 1805. But by itself this is not enough to justify any general conclusions about the music and instruments of c.1780 -- the mid point of this study.

Of course we do not need to rely upon historic witnesses; there are plenty of surviving pianos from which data might be collected. From her experience of playing on many such instruments Dr.Komlós has tabulated (p.18) some comparative data on keyboard measurements -- as to the key dip and the length and width of keys. Unfortunately, only six instruments from the requisite period are listed and there is no explanation as to how the figures have been obtained, nor how representative they purport to be. In fact many of the measurements are somewhat misleading, exaggerating differences which in themselves are very subtle. No information at all is provided regarding the touch weight. If the touch of English pianos truly was significantly heavier than the Viennese, the actual touch weight of (shall we say) the Stodart of 1784 and a comparable Walterflügel might be very informative. Without establishing these facts it is difficult to see how one can proceed very safely. [N.B. Backers' touch -- and Zumpe's -- was lighter still!]

The first of the three sections (which will be of special interest to FOMRHI members) presents an outline history of the eighteenth century piano. In this there are some unfortunate mistakes -- as for example "No pianoforte was produced in England before the 1760s", or "In the 1780s Zumpe developed the English double action, which then became applied in English squares". But it is when addressing the German grand that Komlós repeats a frequent error which ought not to be passed over in silence. On page 7 a quotation is printed taken

from J.A.Hiller's *Wöchentliche Nachrichten* of 1769. "*Herr Johann Andreas Stein has been working on the improvement of the defects of the pianoforte for ten years*". These major defects, continues Komlós, "were the inefficient damping and the lack of a proper escapement". But, as many readers of the FOMRHI Quarterly will know very well, the Silbermann instruments (which Hiller had expressly said that Stein was aiming to improve on) had an excellent escapement mechanism and more than adequate dampers. Importantly, Hiller says that the two defects which Stein had identified were the rather dull tone [*etwas stumpf Ton*] and the inability of the instrument to bring out all of the *Mannieren* as cleanly as on the clavichord [in fact C.P.E.Bach's criticism]. Stein's claim to have remedied these faults should be cautiously received. For, as Hiller triumphantly announces, his solution was the newly invented *Poly-Toni-Clavichordium*; not a pianoforte *per se* but a three manual combination instrument comprising a four register harpsichord with some form of pianoforte -- very evidently not the type for which he later became famous -- enclosed in the cabinet work underneath. Evidence that Stein had been actually making pianofortes prior to 1769 is still wanted. Komlós persuades herself, as had Eva Hertz in her seminal study of Stein, that the Augsburg organ-builder had constructed pianos as early as 1758 when he travelled to Paris, and maybe even earlier. This is not what Hiller says.

This error turns up more insidiously on page 39 when discussing the "piano" works of J.G.Eckard, published in Paris in 1763/4. The teasing enigma of these works seems to have become an insoluble riddle. Eckard's preface assures us that he has inserted all kinds of dynamic markings [*pp* to *ff* as well as *cresc.* etc] so as to make his works equally useful to performers at the harpsichord, clavichord and pianoforte. Yet, as Komlós remarks, the most puzzling question concerns the compass. For in sonata Op.1 No.2 Eckard calls for FF and g'''. Noting that Eckard came from Augsburg, and accepting Eduard Reeser's theory that the sonatas were composed there before 1760, Dr. Komlós proposes that "the Forté et Piano mentioned by Eckard must have been Stein's instrument". Nevertheless, she very rightly goes on to say that no Stein fortepiano known to us has a greater range than FF-f'''. I would add that there is no reason at all why the "Forté et Piano" should have been made by Stein. Nor, if we read his preface with care, that Eckard conceived these pieces for a hammer instrument. A Parisian newspaper source reveals that in 1761 there were four Piano et Forté instruments in the French capital. All were made by Silbermann -- Henri Silbernam (sic) of Strasbourg according to the report -- and they were astonishingly expensive. 1500 Livres is mentioned -- vastly more than the finest harpsichord. No wonder that two of them were owned respectively by a banker and Prince Louis! It takes no great effort of imagination to see that Eckard was probably in no better position to own such an instrument than C.P.E.Bach in Berlin, but, like Bach, he might have had an opportunity of performing on such an instrument belonging to a wealthy patron. However, crediting these Silbermann pianos with a top g''' would be a little chancy.

Happily, as readers will know all too well, Eckard and his rather awkward compositions played a less than decisive role in the history of music. Beyond his rather dubious pianoforte citation his meagre output is of no great importance. The advent of Stein's piano, however, is exceedingly pertinent to the history of the classical pianoforte. Nevertheless, there is so much of value in Katalin Komlós's book. Any student of the early piano should find it both interesting and informative. Perhaps the best commendation from me would be that, after slipping pieces of paper between the pages to mark some of the more thought provoking passages, I soon found the book so stuffed with tickets that I immediately resolved to read the whole thing through a second time!

Review of: The New Langwill Index: A Dictionary of Musical Wind-Instrument Makers and Inventors: William Waterhouse, Tony Bindham, London, 1993, 518 pp., xxxvii intro pp., geographical distribution map, ISBN 0-946113-04-1

I know Jeremy has already reviewed this book in No. 74; nevertheless, I've found it such a rich source that I feel compelled to discuss some of the wonderful things I've found in it so far. It is a great book. Tell all your friends, libraries, and universities to go out and buy it so Tony Bingham will be able to publish more of the same calibre. The following is extracted from a review recently published in *Consort*:

The *NLI* is a fitting memorial to Lyndesay Langwill. Langwill gave much of his life to developing and establishing the cause of historic wind instruments. Clearly, without him the state of period instrument performance would not be as advanced as it is today.

Since Langwill's 8th and final edition (1980), many of those interested in wind instruments have eagerly awaited the *NLI*. After Langwill's death in 1983 William Waterhouse began the daunting task of revising a highly useful resource into a form that would carry scholarship into the next century. Bearing in mind that Langwill's last book predated both the universal establishment of period performance and computers, what has been accomplished here is no less than monumental. In keeping with earlier volumes, the *NLI* also remains open-ended: contributors have been invited to send the editor supplementary information for inclusion in future editions.

Incorporating the work of dozens of scholars from over twenty-two countries, the NLI now contains 6400 entries. Information in each is effectively and efficiently organised into a minimum of space. Not one word appears to have been wasted. Major headings are limited to three categories of mouth-blown, brass or wood wind makers: Makers, Trade Names, Inventor/Patentee. Of these, the earliest entry for a woodwind maker dates from 1293 (L'ESCOT) and that for a brass maker, *fl* 1441 (HANS TRUMETHENMACHER). The cut-off point drifts well into the twentieth century. Where possible, entries include working dates, extensive biographical information, a transcription or description of trademark, a street address, at least one located example of instrument produced, and specific bibliographic references. Family trees are provided to help clarify generational confusion and associated makers are highlighted for easy cross-referencing. Apart from the 447 pages of A-Z listings, one also finds a geographical index of maker's workplaces and an illuminating map showing the industrial concentration of wind makers in central Europe.

The dictionary is prefaced with an important essay by Herbert Heyde concerning the historical development of trade marks after the late middle ages. This clarifies the trading regulations that governed the guilds associated with instrument building. Although much of this is concerned with German practice, we can assume that similar strictures applied elsewhere in Europe during the 16th, 17th, and 18th centuries. We now know, for instance, that the registration of trademarks was hereditary in nature and that a maker's stamp, along with tooling and other workshop equipment, was often passed on and used well after an individual's death. This explains certain progressive design characteristics on instruments attributed to makers who obviously predeceased the earliest logical establishment of these features. We also know under what conditions makers were allowed to 'farm out' work to other makers and, from this, the probability of 'graded' work. This suggests, for example, a reason for the irregularity in J. C. Denner's instruments, which were probably 'roughed out' to his design by non-journeymen, and thereafter finished and stamped by him. French law after 1564 treated trademark imitation as currency forgery. But increased international flow of instruments through dealers and wholesalers during the eighteenth century made theses laws difficult to enforce. Subsequently, we see a Rippert-like *dolphin* used by Stanesby of London and the *fleur-de-lis* of important Parisian makers adopted by Germans such as Grenser and Scherer. Presumably, this was done either because of the higher commercial status of French makers elsewhere in Europe, or as a necessary means to enter the Parisian market. All things considered, organologists will find that for each problem solved by Heyde's diligent research an equally perplexing one crops up to take its place.

One has to marvel at the breadth and depth of information Waterhouse has supplied under the individual maker headings. No stone has been left unturned; no lead unchased. The

writer clearly has complete bibliographic control of his subject. Apart from normal archival techniques drawn from legal documents, commonly used for the dating of professional and familial association, he has included references from tutors, advertisements, literature, catalogues, exhibitions, correspondence, inventories and, even in one case, illegible names scribbled on the backside of a French sketch of early-Baroque winds. The listings are tightly seeded with facts, many of which beg for further connection. In these, we find cases of industrial spying, 'early' consumer comparisons, contemporary cases of forgery and unauthorised copying, patent disputes, family feuds, damaged egos, drunkenness, bankruptcy, imprisonment and all sorts of issues that continue to dog instrument builders today.

What follows is a jumbled sample of broadly ranging subject matter that caught my attention or taught me something new. Someone named EHRHARD (*fl* 1815), for instance, made a flute for one-handed player: early niche marketing. A.G. (*fl* ?19c.) built a 6 key recorder out of brass. Impressive, until one notices that ISAAC EHE (fl 1607-32) constructed a brass dulcian two hundred years earlier. The American, CATLIN (*fl* 1816-50) sourced his boxwood from Turkey, while QUANTZ's best trunk of ebony came from Portugal (ALMENRADER's entry misses his stated preference for American maple). Double reeds for Haydn's Esterhaza orchestra were supplied by ROCKOBAUR (*fl* 1764-77). S. TANAKA studied acoustics under Helmholtz and returned to Tokyo in 1899 to found a research institute for Japanese music. The important early English brass instrument maker, BEALE (*fl* 1643-82), not only repaired, but also 'altered' recorders in 1675. This upgrading of indigenous recorders supports Lasocki's dating for the introduction of 'new' French Baroque winds into Britain. BOIE (*fl* 1789-1809) made (unauthorised?) contemporary copies of GRENSER and POTTER flutes, which suggests the probability of many forgeries predating those of FRANCIOLINI and SCHETELIG.

After reading through NLI it is now clear that women have played a greater role in instrument building throughout history than was previously believed. Bearing in mind that most early workshops were little more than family-ran cottage industries, it is not unreasonable to assume that both women and children were involved in supportive production work. This implication seeps through many of the entries. While there are dozens of examples from the 17th, 18th, and 19th centuries of widows marrying apprentices and foremen, there are also many examples of widows continuing to run workshops long after the death of a master builder. Women were certainly involved in building even if they were unable to stamp their own names on work. In the case of BESSON it is clear that a dynasty of women (widow, daughters and grand-daughter) guided this internationally based firm over a long period of time. We know that Marthe Besson was trained specifically as a maker by her father long before she took on the directorship of the company. Reading between the lines, it is clear that without a thorough understanding of industrial process and the solid respect of her workforce the company would not have survived.

I have few quibbles with this book. It would have been useful to have included reed and key makers, considering that brass mouthpiece makers were included. We can hope these will appear in future editions when the massive task of getting *NLI* into print has lessened considerably. Realising an effort was made to keep price within reasonable limits, I still have reservations about how well the binding will hold up under the rough treatment I am bound to give it over the next few years. There is a simple solution to this problem, which would greatly enhance the power of the book.

The *NLI* was originally conceived as a companion book to the newly published *4900 Historical Woodwind Instruments* by Phillip Young (London, Bingham, 1994). As an integrated unit these volumes cry out for an interactive software format, not unlike that developed by Microsoft for *Musical Instruments*. The choice now is really between lugging heavy books around the museums of the world or popping open a laptop. Scholarship is quickly moving beyond the page-thumbing stage. I would much rather spend my time electronically descending through topics, side stepping my way through cross-references and resurfacing in information that had never before crossed my mind. This is not to imply that the book is not already brilliantly cross-referenced, which it is. But to its credit it contains such a wealth of information that only a computer can draw together connections still hidden. Until such time as all major references are available on CD-ROM, I will remain content with what I have in hand. Within the limits of its form, the economy and precision of the *NLI* renders it a matserpiece of ergonomic design.

FoMRHI Comm. 1332

THOUGHTS AFTER THE NEW LANGWILL INDEX.

Now that the dust has settled from the excitement that the long-awaited arrival of the NLI caused, perhaps we all should be thinking ahead to the NNLI (Next New Langwill Index).

Bill Waterhouse and Tony Bingham have done a fantastic job. Bill may be permitted to enjoy a well-earned rest after so many years of hard slog.

Suggestions have been made that the book is expensive. Nonsense! Considering its size, the amount of research and information it contains, and the very specialised (and relatively small) market it appeals to, it appears to be fairly priced and good value. I am glad to see that its price was not pushed up by a glossy dust jacket, and that the colour of the cover is a sensible dark shade to conceal the inevitable marks it will get from frequent handling.

No doubt Bill will receive new information and corrections relating to the present edition and incorporate them in his records. But what concerns me for the future is the amount of information that must remain uncollated about makers after 1950. Naturally, Bill had to have a cut-off date, but that rather tidy figure half way through the century does not take into consideration the big changes in ownership in the industry since then (e.g. Boosey & Hawkes, Rudall Carte, Schreiber, Buffet, Conn and others, not forgetting the Japanese and Taiwanese developments).

Especially important are the smaller makers, some of them individual craftsmen and probably members of FoMRHI. As long as somebody is making instruments with a name or brand, it is important for the sake of students, museums and collectors of the future that information is collated and recorded. The longer it is left, the more difficult this will be.

Is there someone out there who will start gathering post-1950 information together?

The best format to date has already been worked out by Bill Waterhouse and Tony Bingham in the NLI. Perhaps a supplementary companion volume to the present NLI could be an idea? Could sponsorship be sought? It seems as if the present NLI is near the maximum size for handling comfort.

Sales are vital to a publisher, so could not the present NLI be a clear guide to potential sales of a 1950+ supplement? I would guess that all who have the present NLI could want a supplement, and vice versa.

It seems that all that is required for such a project is some enthusiastic angel with a computer, an unflappable temperament, an independent income, a secretary, an understanding spouse and family, and some years which they can devote to the task!

FoMRHI Comm. 1333 David Lasocki

A Reply to Jeremy Montagu's Review of Richard Griscom & David Lasocki, *The Recorder: A Guide to Writings About the Instrument for Players and Researchers* (Comm. 1311)

I am pleased that Jeremy Montagu found our guide 'an invaluable research tool for anybody working on almost any aspect of the recorder', and our publisher's publicity department has doubtless already excerpted that phrase for inclusion in its catalogues and advertisements. But I felt that some of his criticisms were unfounded and I would like to take the opportunity to address them here.

Jeremy writes 'There are quite a lot of entries from our Q, but there are some that would expect to be in that are not, and it's difficult to see why they are missing.' If he had told us *which* entries were missing, it would have been more helpful to readers, not to mention the authors, who did their best to choose items according to certain stated principles.

I mention those principles because Jeremy apparently neglected to consider one of them in his criticism that 'in the Collections of Historical Instruments section, at least three major omissions jump to one's [sic] mind, one of them the Dayton Miller Collection...' In fact the preamble to the chapter in question clearly states that it is 'concerned only with those articles and books about collections of historical instruments that discuss their recorders.... To save space, catalogs and checklists of individual collections have not been included', and readers are referred to the latest edition of Duckles for information on those. If Jeremy knows any books or articles (other than catalogues and checklists) that actually *discuss* the recorders in the Dayton Miller Collection, we would like to hear about them.

Another instance is Jeremy's criticism of the chapter on Miscellaneous Fipple Flutes, which he dubs 'an entirely random selection of articles on duct flutes of other types.... so random, and so small, that it is really rather misleading'. The preamble of the chapter explains that the principle of selection was not random and that we recognized the chapter's limitations: 'During the course of compiling this book, without making a concerted effort to do so, we came across a number of sources on fipple flutes other than the recorder.... We present these sources here in the hope that they will be useful to readers, although clearly more could be said about fipple flutes, particularly in folk music'.

Jeremy also contends that that chapter is inaccurate. His first example is our description of the czakan as a 'cane recorder', which is says is 'neither made of cane nor ... a recorder'. He apparently did not take into account that our book is American and therefore written in American English, in which usage the word 'cane' refers to what in Britain would be called a walking stick. We were simply translating the opening statement in Marianne Betz's new book on the instrument, *Der Csakan und seine Musik* (Tutzing: Hans Schneider, 1992), 1: 'Der Csakan ist eine Spazierstockblockflöte'. I believe that the instrument has a family resemblance to both the recorder and the English flageolet. Jeremy's second example is our statement that the instrument Bob Marvin discussed in his Comm 'A Double Recorder' was based on 'a 14th-century work of art'. That is indeed what Marvin said. Although bibliographers sometimes have superior knowledge to authors about the subject matter of the articles they are annotating, I do not believe that they can be reasonably expected to have it all the time. I am pleased to have Jeremy's correction in this case.

Finally, I come to what Jeremy says about our treatment of the controversy about the so-called Ganassi recorder. First, he assumes that I must have written this section and that Dick Griscom should have dissuaded me from doing so. In fact Dick wrote most of the annotations, to which I made slight additions; I supplied the long preamble. Second, my purpose in writing the preamble was not, as Jeremy states, 'to prove [myself] correct', but to sum up the controversy

from a scholar's point of view and to suggest ways in which such controversies could be avoided in future. I had no wish to 'revive' the controversy, as Jeremy states about me without having asked, and in fact it may fairly be said that it is he who is reviving the controversy by accusing me of using the book 'to take advantage to represent [my] own side'.

Since it is impossible for me to rebut Jeremy's accusation without saying a little about the controversy, I hope readers will allow me to do that now. He avers that 'the documents of the period' (i.e. presumably, the 1970s) 'prove the contrary' (i.e. that I was incorrect). The question is: incorrect about *what*? The controversy may be said to have begun with my comments on an article by Alec Loretto about four approaches to making copies of a 'Ganassi' recorder and his reply to those comments. In that reply, Alec made a curious statement about Fred Morgan's work on the Ganassi recorder which *suggested* to me (as I said in my published reply) that he was either (still) surprisingly ignorant of Fred's work or else trying to disguise the fact that Fred had both made a Ganassi recorder based on Vienna C 8522 and published an article about it before he did. Alec then proceeded to produce evidence that he had made such a copy before Fred and that at about the same time he gave a lecture about the instrument in New Zealand and handed out a booklet at that lecture relating to it. All this is clearly stated in our chapter. Therefore, on part of the facts, it is at least implied that my *suggestion* may have been unfounded.

I believe I was 'correct' to maintain that a lecture booklet is not an article, and this is not just quibbling: an article generally has a far wider and broader circulation. Alec has had a long history of publishing articles in magazines of wide circulation, so why didn't he write one about his Ganassi recorder? Because he didn't, his work remained relatively unknown, whereas Fred's became widely known through his article in *Early Music* in 1982 and Frans Brüggen's recordings in the mid-1970s. In the final round of letters to the editor before the public controversy was cut short by the editors of two recorder magazines (*American Recorder* and *The Recorder Magazine*), I asked Alec to explain this and several puzzling aspects of his involvement with the Ganassi recorder and his published treatment of Fred. He has not done so, in public or in private, and until he does so I cannot, as a conscientious scholar, call his evidence conclusive, only 'suggestive', as I said in our book. I believe that any fair scholar, examining the same evidence scrupulously, would have similar reservations. In the meantime, I stand by everything I have written on this subject, including in our book, and I believe that Jeremy has misrepresented what Dick and I did there.

FoMRHI Comm. 1334

About the difficulties of delivering a message and some replies to Comm. 1314
Peter Spohr

One thing I was trying to say with my Comm. 1301 was that I believe that mixing contradicting arguments with personal attacks or debasement of other scholars will delay progress in scholarship. In my opinion there is no other magazine in this field like Early Music, GSJ, Instrumentenbauzeitschrift, JAMIS, Das Musikinstrument, TIBIA etc. where this has happened so often. This part of my message does not seem to have arrived. Let me add that I suspect (and I know of an example) that authors have not sent in the preliminary results of their research for publication (a declared goal of FoMRHIQ) because they did not want their persons to be "torn viciously to pieces" instead of only their arguments and argumentation.

Also for reasons of space I do not want to introduce my replies to Comm. 1314 by a psychological digression (for example on how people may react depending to the way a criticism is formulated) instead of Ephraim's sociological one (I feel that I am expected to write "Segerman's" now though FoMRHI members were once told that they are using their first names).

I think that Segerman's story about the leading authority with the enormous modesty is a good example for a message "between the lines". The example insinuates that people with a distinct modesty should be watched most carefully because they may not only have reached their position by means of enormous modesty but they may also be "the liars". I believe that both, position and honesty, are independent of modesty (famous people with all grades of modesty can be found easily) and that the often quoted "dog eat dog society" in our countries in the past is even pointing into another direction. By the way I do not think that being modest or not has anything to do with the things I was trying to criticize, and I would not condemn anybody for being proud of his ideas, abilty, achievements etc. unless it is not only emphasized to demonstrate a superiority to others.

I agree with the following paragraphs of Comm. 1314 and if there is a general consent that controversies in FoMRHIQ have been handled in the described way, my time and paper have been wasted. The more I think about it the more I believe that the style (and I do not know a better word) of debates that has made me so furious is for Segerman a matter of honesty. Segerman seems to believe (and I do not doubt that he does) that sometimes suggesting to scholars that they should eventually look for another job because they are unsuited (besides the rightful criticism of their arguments and methods) is unavoidable, honest and useful for good scholarship while I think it is impolite, delaying progress in scholarship and leading to a useless absorption of energy by provoking mainly self-opinionatedness.

I thought that it would go without saying that my Comm. was not meant to be an example for politeness. Nevertheless going through it again I feel that I have to apologize for the exaggeration of some attacks and the obvious hostility. I hope that my annoyance through many years and Segerman's remark in Bulletin Supplement to Quaterly No. 76 that he is "so thick-skinned" will make at least part of this "outburst" understandable. Mentioning FoMRHI official's merits was something which I thought was missing in FoMRHIQ for years - I recognize that it was done in the wrong context.

The reasons for my Comm. were given there and had <u>nothing</u> to do with Bruce Haynes's <u>theory</u> (maybe applying "Occam's Razor" could have helped to avoid this misunderstanding ?). A sad example that occurs to me along with others is the "Cary Karp controversy" (and again to avoid further misunderstandings: as a player and private collector I did <u>not</u> support Cary's <u>opinion</u>). Jeremy's review of "The Conservation and Technology of Musical Instruments" (Comm. 1107) and Cary's answer (Comm. 1117) show that this has not been forgotten - bringing considerable disadvantages for all FoMRHIQ readers.

Let me end my (last) attempt to make clear what I was also meaning by an example: I could start to contradict Segerman's Comm. 1309 "On Melodic and Continuo Bowing ..." in the following two ways (to avoid a last misunderstanding: this is only meant to be an example and not to make scholarly statements): "It was a surprise to find under the above mentioned heading the main thesis, based on iconographic sources, that chordal playing on viols as a different mode was mainly common during the 15th century. In that case would not have it been better to leave out the term "Continuo" which is closely connected to Baroque music and even more to the figured bass flourishing between abt. 1650 and 1750 and replace it by chordal accompaniment or a similar term instead of expanding the definition of continuo to a large extend etc.etc." or "Good scholars try to use established terms in the way they are commonly accepted. Against this important principle the author of the above mentioned Comm. has offended heavily by giving a useless and wooly definition of the term "Continuo Playing" which for a long time is accepted to be closely connected to etc.etc.". I think the first version is offering a better basis for a promising discussion - at least among less thick-skinned scholars.

FoMRHI Comm. 1335

Ephraim Segerman

On Stroom's Comm. 1315

In Comm. 1315 Stroom expresses opinions about 1), my absolutism and certainty (unusual amongst scholars), 2), my rigid and unrealistic interpretations of authenticity in music and instrument making, 3), my claiming of general validity for my private interpretations, 4), my arrogance in judging quality of scholars and scholarship (including my non-recognition that other ideas can be as good as my own), and 5), my misinterpretation of Occam's Razor. In addition, Stroom 6), questions the existence of objective truth, and 7), writes that these subjects 'have no direct bearing for makers and researchers of musical instruments'. This quotes part of the name of our organisation with 'historical' replaced by 'musical', perhaps implying that 'musical' is much more important.

I don't recognise absolutism, rigidity, unrealism or arrogance in myself or my writings, though it is quite possible for those who don't like what I write to read those qualities into it to justify their anger and rejection. Similar complaints were expressed in Cronin's Comm. 1278 and Spohr's Comm. 1301. My responses to these (Comm. 1304 and 1317 respectively) have relevance to this one.

Some friends of mine advise me not to reply to these Comms. They say that the authors are not interested in debating the basic issue of what is valid scholarship, but are only wanting to undermine my credibility to undecided readers. My approach is to take what is written by others at face value (as I hope others take what I have written). Since my writing can be difficult to read, I want to keep trying to clarify that which seems to be misinterpreted. In the following, the initial numbers indicate the points above that they are most addressed to.

5) I agree with Stroom's statement of what Occam wrote, and that ultimately, every conclusion must be based on judgement. The purpose of Occam's Razor and of objectivity in scholarship is to change the issues where judgement is applied from areas where arguments between differing judgements are fruitless to areas where the judgements of most rational participants in the field can agree. Occam's Razor as it originally was expressed eliminated unobservable entities from consideration, reducing the number of hypotheses to argue about. The rules of evidence ensuring objectivity given in Comm. 1276 were deduced from Occam's Razor, which thus implies them. I never claimed that he stated them himself. I was taught them in my youth (in my scientific training) as a formal way of ensuring a maximum of objectivity in a scholarly investigation.

3) In Comm. 1314 I discussed why the use of Occam's Razor to resolve conflicts is rare amongst scholars. A demonstration of how one's theory is better than another's can be hurtful to the other, and so is impolite. It is usually not necessary since the audience is other scholars, the vast majority of which can agree about which is better because they are committed to the same basic principles. These are that the validity or quality of an hypothesis is based on its fidelity to all of the relevant evidence, and that objectivity and fairness needs to be applied in judging evidence and how it is interpreted. I don't know of any recognised scholars who would argue against this. I may well be alone in the community of music history scholars to interpret Occam's Razor in the formal way that I have. But Occam's Razor is not necessary, since the commitment to objectivity and fairness to evidence is near universal.

4) When people do research and report their conclusions as scholarship, and they do not accept these basic principles (eg. by dismissing relevant evidence as 'questionable' or 'unreliable' without presenting a reasonable supporting case), are not acting as scholars. I don't condemn anything about them other than their interpretations in particular works they have published as scholarship. I hope that they will learn from my criticisms and become good scholars. The quality of historical ideas depends on the degree that the historical evidence supports them, not opinions about them.

6) That vast majority of scholars is also convinced that there is such a thing as objective truth, and that their efforts are directed towards finding what it is. Since it is impossible to guarantee that one has found objective truth, what it actually is can be a matter of opinion, but it is usually quite clear (and not a matter of opinion) concerning which of competing hypotheses is most faithful to the evidence available at the time. Some scholars may well have the opinion that objective truth is better represented by a different hypothesis, but any claim that such a different hypothesis is supported by that field of scholarship at that time can only be false. It is such claims that I object

to, and the dispute is largely about my insisting that people maintain the standards of scholarship when research claims to be scholarship.

1) The reason why most scholarly reports are expressed very tentatively is a matter of culture, as discussed in Comm. 1314. The quality of the scholarship is usually quite obvious to fellow scholars in the field, so one can afford to be very modest. If it is good and its merits understated, others will acclaim it and one's reputation grows. That is the goal for professional advancement. If one makes unexpected claims, the reader's first response is to look for faults, and if they are not found, the response can be grudging acceptance rather than praise. If one is to be liked, one needs to be modest. In FoMRHI, scholars are not writing to a readership of mainly other scholars, so one often cannot expect such widespread appreciation of the quality of one's scholarship. One may then consider that one cannot afford the luxury of expressing oneself more tentatively than the situation warrants.

2) I observe two types of musicians that use historical instruments. One is interested in music history and they want to be as historically accurate with their equipment and performance style as is practical. One cannot expect any more. A completely accurate reconstruction of a performance is of course impossible, even of the concert that happened yesterday with a full battery of sound-recording equipment and video cameras. That is not an issue. Nor is a performance that is as historically accurate as is possible, given the current state of knowledge about music history. The reason for this is that many aspects of historical performance style are difficult to assimilate into a modern performance and still be musically meaningful to the musician or the audience, and we may never be able to do so. To these musicians, historical accuracy is like objective truth to the scholar, something to work towards but not expect to fully achieve.

The other type of musician is not interested in music history, but is interested in the historical repertoire and in the opportunities for performance of it on historical instruments. As for historical accuracy, they are only concerned with the acceptability in the early-music market place of their equipment and performance style. I only make value judgements about members of either of these groups when they apparently make claims of historical accuracy in what they do that cannot be justified by the current state of knowledge in music history.

7) Music history is objectively best represented by the results of historical scholarship. The degree to which an instrument is an 'historical' instrument depends on how well it conforms objectively to what that instrument is according to historical instrument scholarship. Yet there is another approach followed by Bavington, Stroom and many others, and that is a largely subjective view of music history. To them, if it feels historical enough in spirit, it can be called historical. I am acquainted with quite a few people who have jobs in which they are very objective, but when they turn to their music hobby, they only want to be subjective. Music is for enjoyment and not for objective analysis. To them, and many others, music history scholarship is irrelevant unless it offers things that add to their enjoyment of music. It may be irrelevant to their interests, but it is part of scholarship in general, and they have no right to try to suppress it or dilute its standards.

There is nothing wrong with being subjective. That is how I listen to or play music. I can't imagine how reading a score while listening to music can help its appreciation. But that is because I don't have the facility in score reading that they have. The score is an objective representation of the music, and that doesn't stop their enjoyment in listening; most claim that it enhances their enjoyment. My objective knowledge about music history doesn't stop me from enjoying it, no matter how unhistorical the performance is (my favourite recording of early music was played on modern instruments: Boulanger's Monteverdi). I doubt whether it would stop anyone else who enjoys music for the sound. I enjoy Wagner's music though I reject his philosophy. Thus I don't think that preserving enjoyment of the sound is a real reason for rejecting objectivity about music history.

My guess is that a reason for an early-music professional keeping an exclusively subjective approach relates to knowing that the audience's view of music history is a subjective one, and keeping one's own approach the same makes it easy to reassuringly give the audience the background information it wants to hear, and not having to suppress what it doesn't want to hear. My opinion is that those who are in the position to know what the objective position is, and choose to ignore it, are trying to justify dishonesty. Those using the term 'historically informed' are being honest.

FoMRHI Comm. 1336

—[illegible] Segerman

A Revised Hypothesis for the Early Musical Meaning of the Word 'Consort'

Sydney Beck was not one of my favourite people. He denied everyone access to the early sources of c.1600 English music held by the New York Public Library during the many years he took to prepare and publish his edition (in 1959) of *The First Book of Consort Lessons* by Thomas Morley. It was his life work, intended to be the definitive work on these ensembles, a classic on publication. As with all such efforts to produce the definitive version of anything, it loses some of its intended authority as a result of further work done by others. Amongst the contributions he made that are now ignored is his term 'Broken Consort'. It was not fully satisfactory because its historical origin (with Locke) referred to a different ensemble. Warrick Edwards replaced it with the term 'Mixed Consort', which is no better. If an adjective for 'consort' would have been used originally, it would more likely to have been 'broken' than 'mixed'.

Everyone realises that in the publications by Morley and Rosseter the word 'consort' apparently was sufficient to specify the intended ensemble. There appears now to be a need for an adjective because the word has acquired the general meaning of 'ensemble', with the expectation that it would consist solely of members of one family of instruments. This expectation dates from the definition in the first edition of the Grove Dictionary in the 1880's (and has been stated to be false in the first and every edition of the Oxford English Dictionary since).

Nevertheless, the meaning of the word 'consort' at that time did not only mean the particular set of diverse instruments that the publications were for. The earliest musical use of the term appears indeed to be for this set. It was mentioned in the published description of an entertainment for the Queen given at Kenilworth Castle in 1575 by the Earl of Leicester, and was described as a 'melodious noiz, compoonded of six severall instruments'. But Spencer (written 1580-90) in the *Countess of Penbroke's Arcadia* referred to a performance by 5 viols with 5 voices as by a consort, while Campian in *Lord Hayes Maske* (1607) considered 6 cornets with 6 voices not to be a consort. In that Maske, the two other groups that were called consorts were 9 violins and 3 lutes (otherwise referred to as 'the consort of 12' or just 'the violins'), and 2 violins, double sackbutt, bandora, harpsichord and 3 lutes (referred to as 'the consort of 10'), obviously an expanded version of the original set to fill a larger acoustic environment.

A generation or so later, Butler wrote (1636) 'The several kinds of instruments are commonly used severally by themselves: as a Set of Viols, a Set of Waits [shawms], or the like: but sometimes, upon some special occasion, many of both Sorts are most sweetly joined in Consort'. In 1627, Francis Bacon wrote 'in that Musick which we call broken Music or Consort Musick: some Consorts are sweeter than others ... As the Irish harp and Base Viole agree well: the Recorder and Stringed Musick agree well: Organs and the Voice agree well. &c. But the Virginalls and the Lute: or the Welsh Harp and the Irish Harp: or the Voice and Pipes alone, agree not so well'. Bacon's lists are curious if we consider just mixtures of instruments, since it is hard to imagine how the voice with organ agrees well, but not with pipes alone (pipes accompanied by a contrasting continuo instrument are here carefully excluded). How could they differ?

This makes rather more sense if we take note of Bacon saying that Consort Musick was equivalent to 'broken Music' ('broken' meant 'divided' at that time), and assume that the first instrument (or voice) in each pair performed in a divided way and the second not (rather like in the treble and ground lute duets). A difference between the level of division could explain how Spencer's 5 viols and 5 voices could be a consort and Campian's 6 cornets and 6 voices was not a consort. The hypothesis here is that to be a consort at this time, it was not only required that more than one type of musical force (instrument or voice) was involved, but also that a high level of division was a feature of performance. A consort could be characterised as having simultaneously both contrasting movement and contrasting timbres amongst the parts. The modern terms 'broken' and 'mixed' consort are both inadequate, a combination being more appropriate. I propose that we recognise this by calling it a 'Broken Mixed Consort'.

The hypothesis is supported by the description of the entertainment for the Queen at Norwich in 1578, where 'broken Musick', 'a consorte of Musicke, viz sixe Musitians' and 'a consorte of broken Musicke' were mentioned. Further support is given by the Morley and Rosseter prints themselves.

[illegible] ormance of the music, the printers wouldn't have gone to the expense and trouble of printing the lute part with the divisions, which made it much longer and required a different size of paper (folio rather than the usual quarto) to minimise page turns. The Henry Unton painting shows this difference in music page size. Not all skilled musicians, amateur or professional, could improvise a highly divided part that works well, while all could embellish adequately by gracing. This is demonstrated by the various continental division manuals which just mention gracing in passing if they mention it at all.

The most popular consort had not only contrasting movement and contrasting instrumental timbres. It also had the contrasting sonorities of polyphony and monophony with chordal accompanyment by plucked strings. This latter characteristic apparently was not obligatory in England for an ensemble to be called a 'consort', but it was prominent in Praetorius's expanded imitation of what he called *ein Englisch Consort* and in Campian's 'consort of 10'.

It appears that later in the 17th century, the requirement of divided performance for consort identity was relaxed, and the term applied to a set of instruments, mainly of one type, with an obligato accompaniment by what Charleton (1654) called 'consortative instruments such as the virginalls and lute'. The consortative instrument played either continuo or all of the parts (the latter offering the convenience and flexibility of not needing players for all of the parts). Ensembles of this nature existed earlier in the century (eg. Dowland's *Lachrimae or Seven Teares* publication), but they were then not called consorts, apparently because division was not prominent in the performance.

The organ is the consortative instrument we hear most about later in the century. It had a tuning pitch called 'consort pitch' which most probably pertained to viols and instruments that played with them. It was over a tone below modern. Later 'flute consort pitch' was specified for the new French woodwind instruments. It was a semitone higher than (viol) consort pitch, and is probably the original authentic English name for a' = c.415 Hz. In the 18th century, no more organs were built to play with viols, so there would be no ambiguity if the 'flute' part was dropped from the name 'flute consort pitch'. That happened, so we have 17th century (viol) and 18th century (flute, ie. recorder) 'consort' pitches a semitone apart to confuse the unwary.

The first mention of the bandora was in the play *Jocasta* (1566), which had a dumb show accompanied by 'a doleful and straunge noyse of violles, Cythren, Bandurion and the like'. The entertainment for the Queen at Kenilworth Castle nine years later that initiated musical use of the word 'consort', using most of the same instruments, was called a 'melodious noiz'. It is likely that the difference was in the 'broken' aspect of the consort.

Around the time of the two editions of the Morley *Consort Lessons*, consorts were so fashionable that many of the sets of instruments in the royal musical establishment took on the name 'consort'. It is likely that they then used lutes for continuo like the violins in Campion's *Lord Hayes Maske* did, and that a high level of division was used in playing. When court records referred to the plural 'consorts', these groups were probably included. But when used in the singular, it probably referred to a specific group that was more diverse in instrumentation, including the best singers, plucked instrument players and viol players. Such a group, called 'The Consorte' performed for the funeral of James I. The social status of the musical forces involved in this kind of consort was the highest, and it seems that its function was mainly to provide music for listening to, while the functions of the other groups seems primarily to have been to provide music for other activities such as dancing, eating, etc.

FoMRHI Comm. 1337

James Beament

[Wood Science]

I see the Quarterly regularly because one of your members Richard Wilson shares a workshop with my wife, Juliet Barker, and I am called upon to answer questions from, and lecture to, the dozens of makers who attend the classes in violin making she runs. Amongst other things this has included the chemistry of glue and of varnish, and my eye was therefore attracted to Ephraim Segerman's comments on Wood Absorbtion (page 7 of Bulletin 78).

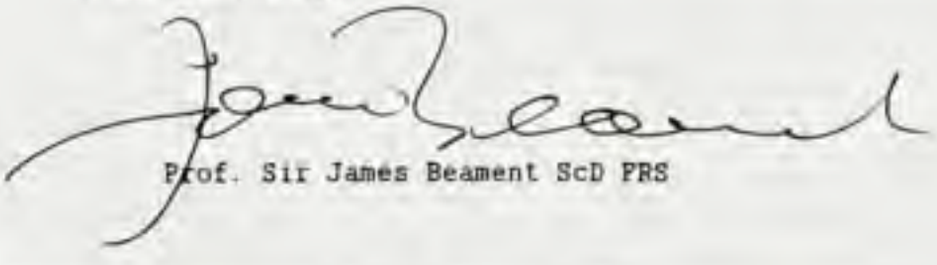
Simmons' underlying science may have been elementary, but Segerman's is wrong. Methane is almost insoluble in water. Marsh gas is methane, and if it was water soluble it would dissolve instead of bubbling to the surface. This was observed by John Dalton and noted in his publications of 1805-8 which outline the very foundation of the theory that matter consists of atoms. Methane is a symmetrical molecule, therefore completely non-polar and without attraction for highly polar water. Symmetrical saturated hydrocarbons are all non-polar and water insoluble. It makes very little difference to solubility in water if there is just one double bond producing one small dipole. The bulk solubility of oily materials in water is a good guide to how polar they are under any circumstances.

Wood as used for instrument making contains about 15% by weight of water and many of the polar groups of the celluloses will have water molecules strongly attracted to their surfaces. Animal glue depends almost entirely for its adhesion by sharing attraction for the 10-15% of polar water between the wood and glue molecules. Agreed an unsaturated oil molecule with a minute amount of polarity from one double bond will be very slightly more attracted to a dry cellulose molecule than a completely non-polar molecule would be, but the difference is extremely small.

The absorbtion of oil by timber is by capillary forces. As long as the contact angle between oil and wood surface is less than 90° - and it invariably is, the oil will be sucked in, and the smaller the pore and the rougher its surface, the more powerfully it will go in. Any oil will block up pores and act as a physical barrier to water. But the swelling of timber by absorbing oil is minute compared with the effect of absorbing water, where the small molecules of water can get *in between* the bulk cellulose molecules and force them apart. Typical drying and non-drying oils, such as linseed or primrose seed, are gigantic molecules compared with water and have no hope of getting between wood molecules and swelling them in normal wood. If a teak bowl cracks, it must have been so dried (by central heating?) that there will be many unseen places where the wood molecules have also been ripped apart. Fill these with oil and you create oil-lined holes which will suck in more oil and cause the wood to swell by capillary forces. That is how a sponge swells in water.

Finally, a drying oil goes solid by polymerisation: otherwise it would not be called a drying oil. If it swelled or changed volume when it dries it would not have been the basis for paint and varnish for many centuries. A non-drying oil stays liquid - and if wood is soaked in it it will leave oily traces on anything put in contact with it for years. The best way to prevent water absorbtion is to use a filler and seal with a good varnish.

Yours sincerely,



Prof. Sir James Beament ScD FRS

Queens' College
Cambridge

FoMRHI Comm. 1338 Paul White

Preserving passion and conviction: fend off the flaccid

For a long, long time FoMRHI has provided an open-ended forum for the free flow of information. Given this is one of the few platforms where musicians, makers and scholars actively interact, it is no wonder that we find disagreement within this large family. It may get rough and tumble from time to time, but that is a natural consequence of sharing relevant views infused with passion and conviction. These are a damned sight more than you get in many 'scholarly' or 'refereed' journals these days and simply the price we should expect to pay for knowledge and progress.

I have been interacting with listgroups on the Internet of late and realise now that FoMRHI provided a model for this sort of interaction long before the Internet was an undivided electronic zygote deep inside an as yet uninvented silicon chip. Like the internet, FoMRHI should remain self-regulating, uncensorable, and relatively immediate. Other journals can take years to publish articles, which by then often have lost relevance or usefulness. Just as with the Internet, when anyone crosses over the lines of civility a bit too far there is a certainty that he or she will be told to back up and have a rethink. Flaming is not a new concept in FoMRHI, nor is the concept of apology. Given the choice between the Internet and FoMRHI I much prefer the latter. It gives you this wonderful bundle of paper to tack up on the shop wall or fling across the room when an article inside it pisses you off. It also gives you something to pick up and dust off and carefully reconsider.

Recently, I ran across a quote by Howard Mayer Brown in his essay contained within Authenticity and Early Music (OUP, 1988, p 54). It sums up one vital role that FoMRHI plays within the greater musical community.

> In the past, instrument makers have often worked at the leading edge of research and have pushed both performers and scholars into thinking in new ways about a number of things. Although some of them would surely disagree, a consensus does in fact exist that we need to go on copying faithfully particular old instruments for a long time to come. They still have much to teach us, and the practical research involved in copying the essential features of an instrument (and more important, of deciding which the essential features are) has hardly yet begun, in spite of the spate of activity during the past fifty years.

FoMRHI deserves a pat on the back for grappling with some very difficult problems that sit at the heart of music making throughout the world today. Looking back at many of the issues covered in these pages over the years I marvel at the presentation of ideas other 'more scholarly' journals avoid for fear of opening a Pandora's Box they cannot control. It is rare these days to find individuals willing to look squarely in the face of issues that might undermine their own authority or wear holes in their pocketbooks. FoMRHI has never shied away from dangerous self-reflection like this. Although organology remains the ever unwanted bastard child of musicology and performance, word is slowly seeping out that this is a journal that cannot safely be ignored.

For my two pennies and two cents worth, I think FoMRHI should stay exactly as it is. It does what it needs to do very well.

Where do we want to go?

Jeremy raises two interesting points in the last Bull. on advertising and non-renewing subscribers.

I happen to have another hat which I put on from time to time - I edit a magazine. It is in fact in the steel industry, but some of the lessons I have learnt there might be useful to us here.

Magazines that carry advertising are an example of the triangular contract. Triangular contracts don't normally work very well. For instance in the health service we contract with the government to pay doctors to provide us with health care. If we don't get the care there is no point in complaining to the doctors, because we don't pay them. And complaining to the government doesn't help because the government doesn't itself provide the care.

In magazines, the advertisers contract with the publishers to deliver readers to the advertisers. The advertisers are the paymasters and what they say goes. They pay the whole cost of the editorial team. The subscribers pay only for printing and postage. To the advertisers, editorial matter is something that gets in the way of the ads. To the editor ads are something that gets in the way of the editorial. They do not speak to each other. Editors *never* include anything that could *possibly* upset an advertiser, but they try to put in things that interest readers as their jobs depend on it - the advertisers won't advertise if the circulation drops.

If you take ads you lose all freedom. That's why Which? takes no ads.

In my steel mag I have much the same problem with authors as here. I too have academics and makers. The academics write all the time, the makers never. The academics want to get lots of impressive long articles published to make their CVs look good. The makers don't want to give away commercially confidential information, and anyway they haven't time. You can guess where the articles come from.

I can offer no solution. We just have to encourage maker-authors in every way we can think of.

FoMHRI is a bit like Which? in being a paid-for mag with no ads. We, the readers, pay the Editor, Eph, through our subscriptions to put in what we want to read. If he doesn't put it in we cancel our subscriptions. It is a paid-for magazine, not a true newsletter. A true newsletter is issued free by an organisation which just puts in what it thinks the readers *ought* to be given.

Eph does not in fact have an infinite number of pages. We keep on hearing about printing costs and postage problems etc. I too have a fixed number of pages (39). If I tell a feature writer that he is allocated two pages, he gets two pages. If he sends in three, no matter how powerful his argument for more space, I cut one third of his copy and he gets two. Pages are as valuable as gold dust. You don't just give them away.

I think FOMHRI is in danger of losing its way. It was formed to carry stuff primarily of interest to hands-on craftsmen. We should not compete with *Early Music* and carry stuff on performance practice and the names of things.

And - this will put the cat amongst the pigeons - how about saying that Comms should not normally exceed two pages? That up to four pages can be carried only at the Editor's discretion. For anything longer a half-page summary would be printed, and the author would make copies of the article available to interested readers who send an sae?

I would like to encourage the maximum number of separate Comms, to give readers a choice. People are interested in all sorts of different things. We need younger makers to come forward with Nuts and Bolts Comms. People leave because they feel intimidated, and unwelcome. They stay involved because they feel interested and valued.

What do you think?

FoMRHI Comm. 1340

EDINBURGH UNIVERSITY COLLECTION OF HISTORIC MUSICAL INSTRUMENTS

Progress Report 1994

In the year, the Collection has been given instruments by Frank Dodman, B.D. Mackenzie, Timothy Tozer, Fettes College, the University of St Andrews and the Janissary Band Museum, Istanbul. Further items have been lent to the Collection, including a Joachim Tielke viola da gamba and other important instruments from the Glen Collection, lent by the National Museums of Scotland.

An endowment fund has been established, the income from which will be used solely for additions to the Collection.

The cataloguing programme has continued to advance: two further fascicles of descriptive text have been published. These cover oboes, and cornets & tubas respectively. In addition to the printed editions, they have also been published electronically.

Three further technical drawings have been published, of the Schnitzer tenor trombone and the Huschauer tenor trombone and trumpet, prepared for the Collection by Raymond Parks. These bring the total number of workshop drawings on sale to 33.

The University, jointly with the National Museums of Scotland, hosted the 1994 meeting of CIMCIM (The International Council of Museums Committee on Musical Instrument Museums), the Honorary Curator being responsible for the local arrangements. The Collection, with the Russell Collection, also contributed the Centenary celebrations of the Faculty of Music by hosting a 4-day Symposium on Musical Instrument History under the auspices of the Galpin Society and the Historic Brass Society.

Concurrently with the Symposium, the Collection organised a masterclass in historical performance practice given by Crispian Steele-Perkins (trumpet) and Susan Addison (trombone), and one of the Faculty's series of Historical Concerts celebrated the multi-centenaries of three of the instruments in the Collection dated by their makers: the tenor trombone (Anton Schnitzer, Nuremberg, 1594), the tenor trombone in B flat and the natural trumpet in D (both Joseph Huschauer, Vienna, 1794).

The Reid Concert Hall was open to the public as part of Doors Open Day on 10th September, when many visited the Collection. The Collection has been used for teaching purposes by University Staff, in particular for courses in the Faculty of Music on the History of Instruments, Ethnomusicology and Musical Acoustics. Several parties have made organised visits, and various scholars and instrument makers have visited to study particular instruments.

Arnold Myers, Honorary Curator.

FoMRHI Comm. 1341 Susan Alexander-Max

BRITISH CLAVICHORD SOCIETY AT THE OPEN UNIVERSITY
11TH March 1995

It was nearly Spring. The 11th March to be exact. The daffodils were in full bloom, and the sun was truly trying to shine. Tucked away in what seemed to be a totally new complex of buildings at the Open University, Milton Keynes, was a lovely old church, small and intimate, the perfect environment for a clavichord lecture-recital given by John Barnes and Derek Adlam for the British Clavichord Society.

John Barnes began the afternoon with an illustrated talk comparing the Hass and the Friederici clavichords of the 18th century. He explained that the most significant instruments of this period, the mid and late 18th century, reached perfection of construction, and that it is vital to study the design of these instruments and to learn from them. He explained the geography of these two makers in Germany which resulted in Hass being influenced by the Flemish and Friederici by Italian makers. He discussed the differences in stringing, both lengths and gauges, in barring, in pinning and in the shape of the bridge.

Derek Adlam followed this talk with musical illustrations, each demonstrated a point that John Barnes had made when comparing the instruments structurally. He began with W.F. Bach on the Friederici clavichord which gave an example of the expressive use of bebung and portamento which would not have been possible on earlier instruments. The he moved to the Hass and to C.P.E. Bach which illustrated wandering long phrases, a sustained and harmonically rich bass part. John Barnes had said that the Hass instrument had long tenor strings. Derek Adlam's next musical example was Haydn, on the Hass clavichord, because the very rich tenor was needed to produce the quality of tone for this piece. This was followed by W. F. Bach on the Friederici which needed instantaneous tone for clarity for the flickering about of musical ideas. The final illustration by Derek was again Haydn on the Friederici. Although most probably written for the clavichord, Haydn was looking forward, this instrument leading logically in touch characteristics to that of the forthcoming fortepiano.

The recital was followed by a lively question and answer discussion.

Sincere thanks to John Barnes and Derek Adlam were expressed by Peter Bavington for an informative and extremely enjoyable afternoon.

The British Clavichord Society would like to extend their thanks to Donald Burrows who helped to organize the meeting at the Open University, who provided us with a delicious tea after the lecture-recital, and who presented David Rowland and Lucy Carolan in concert that evening. It was the culmination of a wonderful day.

FoMRHI Comm. no. 1342 (sequel to no. 1326)

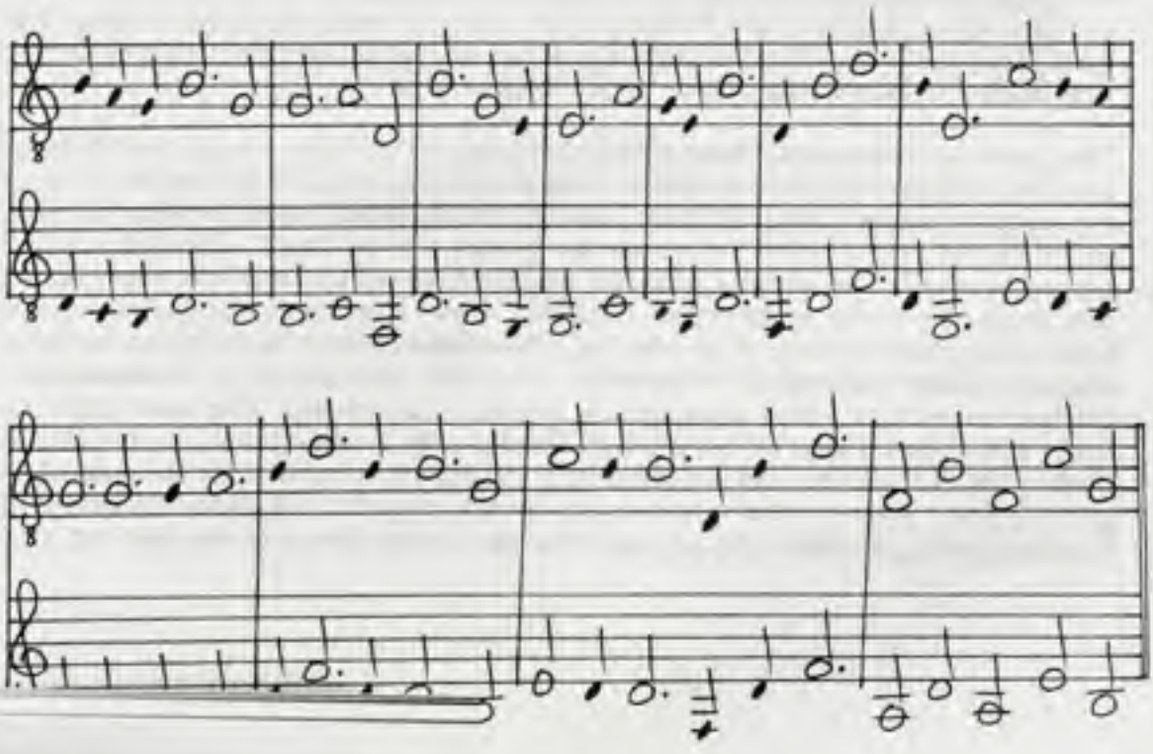
The grounded staff

David Z. Crookes

You happen to be leafing through Isaiah when your eye is caught by a difficult musical verse—chapter 30, verse 32—which in the *AV* reads as follows: "And in every place where the grounded staff shall pass, which the Lord shall lay upon him, it shall be with tabrets and harps: and in battles of shaking will he fight with it." What is "the grounded staff"? While you've no positive idea, you're pretty sure that it relates to neither *ground* nor *staff* in the musical sense of those two English words. But aside from Isaiah, you've been wondering of late whether the composers of ancient Israel ever employed something like a ground bass. You've found an obvious melody in verse 5 of Psalm 45 (see comm. 1299), and its tunefulness has confronted you with a problem. Psalm 45's title begins, "To the chief Musician upon Shoshannim", so if verse 5 is the tune of the psalm, then "Shoshannim" isn't. What does **upon** mean here? Is verse 5's melody played and sung **upon** "Shoshannim", in the manner of Purcell's *Three parts **upon** a ground*? And apart from the preposition, what is meant by **Shoshannim**? (In the Hebrew text of Psalm 45 that word, normally rendered *lilies*, is spelt Shin-Shin-Nun-Nun-Yodh-Mem—its third character functions as a double letter.) The margin of *Bagster's Comprehensive Bible* (London, n.d., but preface dated 1826) suggests that we take the word in a musical sense:

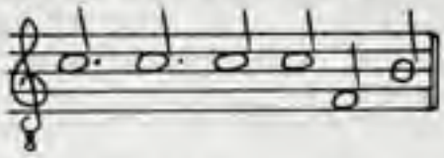
> Shoshannim, שׁשׁנים, *most probably denotes* hexachords, *or* six-stringed instruments, *from* שׁשׁ, *'six'*....

After working with the eight-note melodic, Alamoth and Sheminith scales, you find it hard to believe in instruments having the range of a sixth. But you wonder about שׁשׁ. Did the word Shin-Shin ever denote the *interval* of a sixth? And if so, does the word **Shoshannim** in Psalm 45 indicate parallel organum of the sixth? The sixth is the most luxuriant of harmonic intervals, and seems absolutely right for "A Song of loves" (*AV*, title of Psalm 45). You recall six uses of the word **Shoshannim** in Solomon's Song (2.16, 4.5, 5.13, 6.2, 6.3, and 7.3). Well! What happens if you write out Psalm 45's tune, which is contained in its fifth verse, and add a lower part down the sixth?



It sounds euphonious, in fact almost excessively so. The combination of luxuriance and harmonic predictability, with no tension, no contrary motion, no real counterpoint, makes you want to add an independent part: and the thought of that leads you back to the word **upon**. Is there anything more to ***upon*** **Shoshannim** than parallel organum of the sixth? May the words mean *in sixths upon a ground*, with the actual *ground* being **Shoshannim** (Shin-Shin-Nun-Nun-Yodh-Mem)? Letting **Shoshannim** bear such a dual significance strikes you as reasonable enough, given the double numerical meanings of some of the musical cryptograms that you've already found in the Psalter. You notice incidentally that while the word **Shoshannim** is spelt Shin-Wau-Shin-Nun-Nun-Yodh-Mem in Psalm 69.1, and in Solomon's Song 2.16, 4.5, 5.13, 6.2, 6.3, and 7.3, here in Psalm 45 it is spelt Shin-Shin-Nun-Nun-Yodh-Mem.

Anyway, suppose it's a ground. As it stands it has six notes, like the bass-line of Purcell's *Three parts upon a ground*. In modern terms it runs tonic, tonic, tonic, tonic, subdominant, and dominant (or leading note). The sheer amount of 'tonic' reminds you of some of the bass-lines that you used to work with when you played piano in a jazz band. Now when you transcribe the word **Shoshannim** it comes out as a 14-beat phrase, thus:



And now three questions pose themselves.

1. *How many times is the ground meant to be played?* If the word **Shoshannim** really is related to the word ששׁ (= six), then you might expect six statements of the ground. That would make **Shoshannim** a triply significant cryptogram!

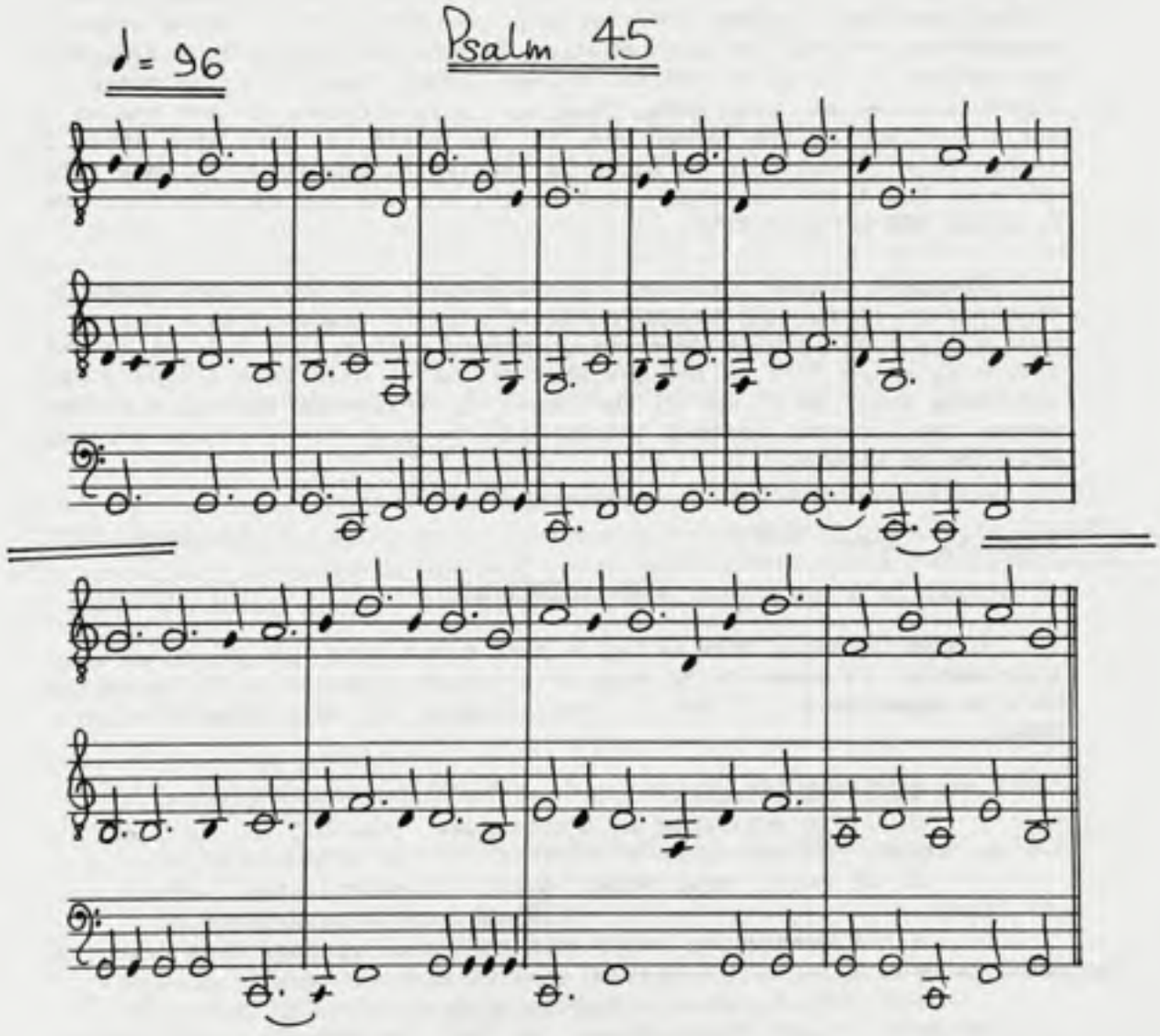
2. *At what pitch is the ground to be played?* You can't believe that the *c'-c'-c'-c'-f-b* pitches are meant to be taken literally.

3. *Are the ground's time-values (two dotted minims and four minims) meant to be taken literally?* You remember what happens to the 'tune' in some of the *L'homme armé* masses that you know, and you wonder.

[These questions answer themselves against the background of *a presumed euphony*. You expect a two-parts-on-a-ground Psalm 45 to sound about as euphonious as one of David's three-part organa. You're inclined to regard the pitches and time-values of the two parallel-sixth parts as given immutables. So if you do a three-part reconstruction of Psalm 45 and it sounds like Webern, you'll know not merely that you're wrong, but specifically that your *use of the ground* is wrong. By corollary, there can only be one right answer to the question, "How should the ground be used?".]

The ground as it stands is fourteen beats long, and the melody of verse 5 is 86 beats long, which allows for six statements of the ground and a final 'tonic' minim. Ten of the ground's fourteen beats are 'tonic', and the presumed final minim is 'tonic' as well, so for 62 out of the psalm's 86 beats the ground will be 'tonic'. How should the ground be used? You spend a good few hours in your contrapuntal laboratory before you get an answer that satisfies you. The only 'tonic' note that strikes you as possible is the note *G* (bottom note of the bass clef): **Shoshannim** has to be transposed down a compound fourth through the white notes. Any other transcription of **Shoshannim**'s pitches, including a literal transcription, produces cacophony. The same goes to a lesser extent for a literal transcription of **Shoshannim**'s time-values. Eventually, by treating the ground's six notes as temporally variable, you arrive at the reconstruction given below. One of your musicologist friends calls it "a chainsaw massacre of Gordian knots", but when you ask him what the Gordian knot was and who cut it, he

doesn't know. So you set your reconstruction down for musical people to look at, and decide to leave the question of supporting numerical cryptography for another day. Notwithstanding the titular "sons of Korah" (whom up to now you've regarded as indicating *adult male vocal pitch*), you think it sounds much better if all three parts are transposed up an octave. You wonder if the final words of the tune-verse—"...thy right hand shall teach thee terrible things" (*AV*, verse 4)—refer obliquely to dissonances like *G-A-f* and *F-e-c'* in the last 'bar'. And finally, recalling that in 400 alphabet gematria the word Lamedh-Daleth-Wau-Daleth (= *by David*) adds up to 44, you ask yourself if the tune's 44 notes indicate Davidic authorship.



הללו-יה

FoMRHI Comm. 1343

Donald S. Gill

CUTTING THICK VENEERS

I spent a long time and wasted quite a lot of wood learning how to produce thick veneers in my workshop. The following method works for me and I offer it in the hope it may save someone the trouble of re-inventing the wheel. In my case thick veneers means anything from 2 mm. for key tops to 6 mm. for organ pipes. The tools I use are a planer/thicknesser and a bandsaw.

Firstly take a suitable board and plane a face and edge at right angles. Then prepare the bandsaw for cutting a veneer slightly thicker than the final thickness. Have a new or nearly new blade in the saw and increase the tension on the blade slightly above normal. Adjust the blade guides if necessary. A wide blade is a help but I use the saw for all sorts of cutting so tend to stick with a 3/8 inch as standard to avoid having to change blades. Make sure the fence is firmly clamped at both ends. Mine has only a clamp at one end so I use a G clamp at the other end. If the board is more than about 3 inches high it can help to build up the fence. Now feed the wood into the saw. Don't try to force it through, let the saw dictate the feed rate and USE A PUSH STICK.

The sawn veneer can now be thicknessed to the right size. My thicknesser table will not move nearer to the blades than 6 mm. so I have a piece of Formica veneered chipboard with a stop screwed to the underside which fits on the thicknesser table. With this I find I can thickness down to 2 mm. If you want to thickness the edge of the veneer feed several veneers through at the same time; one on its own will not be stable.

FoMRHI Comm. 1344

Donald S. Gill

CORK SUBSTITUTE

I have recently been using a cork substitute for joints on wind instruments. It seems to be made of a rubbery plastic which resembles cork in appearance. I find it has a number of advantages over cork sheet.

1. It is cheaper.
2. It is available in thicknesses from 0.5 mm. to 3 mm. in 0.5 mm. steps.
3. It will bend round small diameter tubes without any splitting.
4. It compresses more than cork and springs back when the joint is undone.

The only slight disadvantage is that it doesn't sand down as easily or as smoothly as genuine cork. However the roughness disappears as soon as the mortice has been in the tenon of the joint for a few minutes.

The material is called Arco synthetic cork which I obtain from Windcraft, 652 Bath Road, Taplow, Nr. Maidenhead, Berks SL6 0NZ. Tel: 01628 604335.

FoMRHI Comm. 1342

The Inventor of the Double-Action Pedal Harp with Fourchettes. Gröll Versus Erard

By Mike Baldwin

In 1808 Sebastien Erard (1752 - 1831) registered his first patent for the double action pedal harp with fourchettes.[1] The subsequent patents of 1810,[2] and 1821[3] document further the developments in his mechanism.

It is now generally accepted that Sebastien Erard was the inventor of the double action instrument.[4] Evidence has recently come to light during my research into the mechanisation of the harp that calls into question the validity of this attribution.

Before discussing this new information, which has been available in the form of a patent since 1807, it is necessary to look at the late years of the single-action.

Many makers were involved in the development of the single-action harp. These makers may be divided into three distinct schools, according to the method by which the pitch of the open strings is raised by one semitone. The crochette mechanism, pioneered by Hochbrucker in the first two decades of the eighteenth century[5], was adapted and developed by Nadermann, Cousineau, Renault & Chatelain, Wolter, Louvet, and Holtzmann among others, and remained in use until the early 19th century.

During the last twenty years of the eighteenth century Georges Cousineau (1733 - C.1799) and his son, Jaques Georges Cousineau introduced their bequille mechanism. This mechanism, consisting of two small crutch-ended levers placed both to the right and left of the string, stopped the string without moving it out of alignment.

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[1] Certain improvements upon Pianofortes, large and small, and upon harps. No. 3170.

[2] Certain further improvements on Pianofortes and harps. No. 3332.

[3] Certain improvement on harps. No. 4670.

[4] According to Roslyn Rensch in her books The Harp: It's History, Technique and Repertoire. (Duckworth. 1969. London) Harps and Harpists (Duckworth. 1989. London).

[5] According to The New Grove Dictionary of Musical Instruments. (MacMillan Press. London. 1984).

During the late years of the eighteenth century Sebastien Erard (1752-1831), an inventor who registered many improvements for the harp and pianoforte, was known to be working upon a more perfect method of stopping harp strings. In 1794[6] he patented his single-action (patent No. 2016).

This was achieved using what Erard described as a fourchette. The fourchette could be turned to stop the string by the depression of the pedals enabling two prongs to move the string equally from opposing directions

This advancement was considered to be the pinnacle of single-action technology. Indeed in his book, The Harp in its Present and Improved state. (3rd Edition. Paris. 1926) Pierre Erard (1796 -1855), nephew of Sebastien writes;

> "...the french harp and Sebastien Erard's compared might be said to have that striking difference between each other, in point of tone, which exists between a grand piano forte and a harpsichord."

In 1801 Erard registered his second patent[7] (No. 2502), the first in the UK for a fully chromatic harp. Erard devised a clever though complicated mechanism which;

> "...was capable of rendering the harp capable of modulating into every usual or practicable scale of music, by immediately rendering the tone or pitch of all the chords or strings of the same name or denomination either flat, sharp, or natural at pleasure, by the instantaneous operation of a lever, treadle, or other suitable instrument of communication from the hand or foot, so as to alter the tension of the said string, and also of turning each several chord or string with greater certainty and precision than has hitherto been done."

This mechanism employed a wheel attached to the tuning pin which by the depression of one pedal increased the tension of all strings of a particular note of the scale. By this method each note could be rendered flat, natural, or sharp.[8] However these harps were never produced for sale.

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[6] This date is noted incorrectly as 1792 in The New Grove Dictionary of Musical Instruments. (MacMillan Press. London. 1984), Also in Rensch's book The Harp: It's History, Technique & Repertoire. (Duckworth. London. 1969).

[7] Certain Improvements in the construction of harps and pianofortes, both large and small. Up-dated 1802 Certain new improvements in the construction of the musical instrument called the harp. No. 2595.

[8] In 1802 Erard patented an update (No. 2595) of his 1801 patent.

The Cousineau firm are also know to have achieved a fully chromatic pedal harp (C. 1782)[9] with fourteen pedals arranged in two rows of seven.

In 1807 a patent[10] was registered by one Charles Gröll of Leicester Field, in the Parish of Saint Martin, and City of Westminster. I propose that this patent (No. 3059) is in fact the first registration of a double-action pedal harp with fourchettes. The drawings that accompany this patent show an ingenious yet simple mechanism. They also illustrate two fourchettes acting upon one string. The Gröll patent reads as follows;

> "First, instead of raising each of the strings of the harp, through the interval of a semitone (whenever required), by means of one single order, rank, or arrangement of divisions, by means of forks, hooks, rings, or similar well-known contrivances, (namely one of the said forks, hooks, rings, or similar well-known contrivances being applied and adapted to each string,) I do apply, adapt, and use two such orders, ranks, or arrangements of divisions, (as herein-after is more particularly set forth), for the purpose of raising each of the said strings (whenever required) through the intervals of two semitones. And I do also cause the intended effect of the said division to be produced at pleasure by fit and appropriate machinery adapted to the pieces which constitute the said orders, ranks, or arrangements of divisions, and communicating with certain pedals, (which is like wise herein-after more particularly set forth).

The next Erard patent was the 1808[11] (No. 3170) patent, which describes an early version of the double-action mechanism on which modern harps are based. This harp, which we know was produced in huge numbers (over 4000), was achieved by doubling Erard's single-action mechanism - hence the name double-action pedal harp.

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[9]According to Rensch's book The Harp: It's History, Technique and Repertoire. (Duckworth. 1969. London).

[10]Certain improvements upon harps.

[11]Certain improvements upon pianofortes, large and small, and upon harps.

In the book, "Patents for Inventions Abridgements of Specifications Relating to Music and Musical Instruments A.D. 1694 - 1866. (Tony Bingham. London. 1984 (Facsimile of Second Edition of 1871), the following is written about Erard's 1808 patent;

> "1. Apparatus for stopping the string, either to give the natural note of the series to which the string is tuned, or the next semitone above on the sharp note designated by the same musical letter or character which shall or may have been appropriated on the said string."
> 2. Apparatus fixed within the neck of the harp, for the purpose of giving requisite motion at one and the same time to the axis of the stopping apparatus applied to all the strings of any one and the same note or denomination, in order that the whole of any such notes may be rendered flat, sharp, or natural at once, as may be required."

These statements refer to two similar inventions. The most notable differences occur between how each mechanism is activated. Both used a pedal interface between the player and the instrument. The differences occur within the capitol (or the scroll in Gröll's case). The following drawings illustrate this

Figure 1. Gröll's Mechanism. Patent No.3059

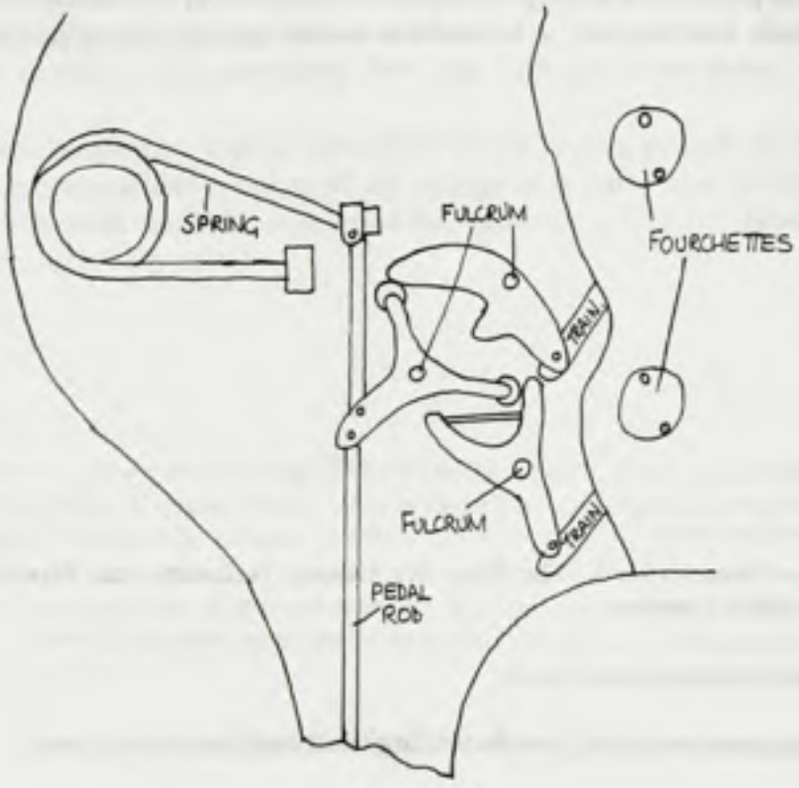
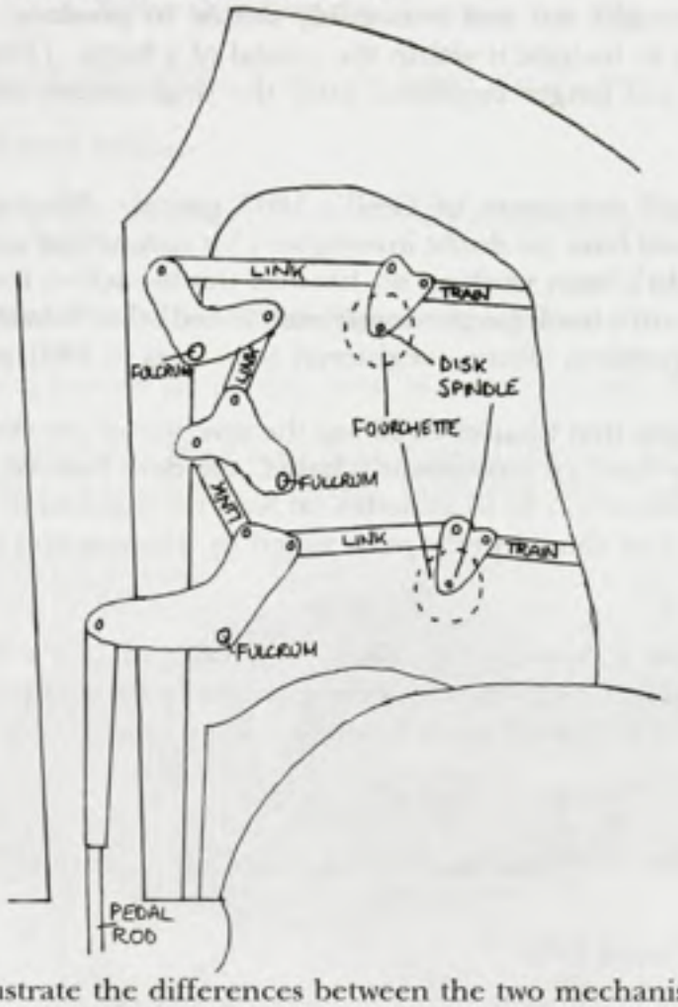


Figure 2. Erard's Mechanism. Patent No. 3170



These drawings illustrate the differences between the two mechanisms. It is the later which is applicable to the modern harp. The linkages used in Erard's mechanism remain practically unchanged. Erard's invention is of extreme importance to the harp world and indeed music in general.

It is interesting though perhaps not surprising that more than one inventor achieved a similar solution to the problem of doubling the action of a harp at approximately the same time. To the best of my knowledge no harps were made by Gröll. Other references to Gröll are in the Patents of 1812 (No 3531) and 1813 with Frederick Dizi (No 3642); the first of these is not related to the harp; the second, a perpendicular double-action system. There is evidence to suggest that Gröll was a clock maker by trade[12]. It is not unreasonable to imagine that a mechanic of clockwork might have been invited to seek a solution for doubling the action of the harp.

[12]This is unsubstantiated.

I can find no reason why the Gröll system could not have worked as well as that of Erard. It is well thought out and reasonably simple to produce, though it might require modification to include it within the capital of a harp. (The scroll for which it was designed was not longer employed once the single-action harps ceased to be produced).

It is likely that Erard was aware of Gröll's 1807 patent. Working in such close proximity, Erard would have no doubt investigated his competitor's invention. This is not to say Erard hadn't been working on his own double-action for some time. We know from Pierre Erard's book (as previously mentioned) that Sebastien Erard was not fully pleased by his previous efforts, registered as patents in 1801 and 1802.

I do not wish to suggest that Charles Gröll was the inventor of the double-action pedal harp upon which the modern instrument is based. He does however appear to be the inventor of the double-action by fourchettes (at least on paper as his patent pre-dates Erard's by a year). This should perhaps be noted by harpists and musicologists as a matter of interest.

It is conceivable that a harp as described in Gröll and Dizi's 1813 patent[13] was constructed. I would be very interested to hear from anyone who has any information about Charles Gröll (or perhaps owns a harp made by him).

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[13] An improvement in the construction of the pedal harp.

An addition to the following Comm 1346

Addendum to Report on Keene Spinet, sent by Robert Greenberg

After looking at this report, John Barnes writes that the 1715 Keene spinet he owns has its soundboard grain parallel to the register, or, at an angle to the spine. He notes, "Interesting that Keene changed" the direction of the soundboard grain, from the widespread habit of soundboard grain running along the back wall or spine, during and at the end of the seventeenth century, to the consistent direction after the start of the eighteenth century, where the grain of the soundboard runs parallel to the register. To know when and why is best for others; I know little. But, this change might be to stiffen the soundboard in general, to help lessen the "tubby" sound of the understressed strings. One thing we are certain of is that Keene left off five bars from the soundboard between the 1704 spinet and the one of 1715. That is, the number of bars went from seven to two, as the length of each of the soundboard pieces was much shortened.

Restoration report of English spinet by Stephen Keene, London, 1704.

Robert Greenberg, San Francisco, CA, USA

1 x 8' spinet; compass GG/BB—d^3; $C^\sharp$, $E^\flat$ split (54 notes). c^2 = 257 mm. See Figure #1 below.

I. Inscriptions and Recent History.

Nameboard inscribed in ink: "Stephanus Keene [floral intarsiated medallion with butterfly] Londoni Fecit" (Figure #4, page 6).

Top key (number 54) inscribed in ink "1704" (Figure #2, page 2).

Name at bottom rear of keywell (under distal ends of keys): "S. Rudd" (Figure #5, page 7).

The instrument was acquired by Mr. Ronald Haas, Aptos, California, through auction at Butterfield & Butterfield, San Francisco, California, November, 1993. Prior owner(s) unknown.

Figure 1. Stephen Keene Spinet, London 1704.

II. Dimensions of spinet (all measurements in millimeters, without moldings).

Spine length	1645
Thickness of rear case wall, including inner veneer	13
Height	165
Greatest width, to tip of projecting keyboard bracket	607
without projecting keyboard	525
Keyboard projection width (including brackets):	786
Keyboard at 27.5° from spine.	
Left tail length	178
thickness with two veneers (interior and exterior)	11.7
Left front board:	12.5 thick; 164 high; 60 long

Left keyboard bracket: 13.6 thick; 162 high; 100 long
Angle between left tail/spine: 62.5°
Right Tail length 189.
thickness 11.3; 162–164 high.
Angle between right tail/spine: 64.5°
Right front board: 10.7 thick; 165 high; 62.5 long.
Nameboard opening: 771.
Nameboard itself at top: 770; at bottom: 766.
Front strip (ahead of keyboard) ‡10.5 thick; ‡43 high.
Bottom thickness (spruce)★[1]: ~13.0

Figure 2. Date,"1704," inscribed on top key, Keene spinet.

III. Dimensions of Keys and Keyframe.

Keybed overall width: 758 at front; 751 at rear.
Balance rail: walnut,★ 25 high x 22.5 wide. Set back from front of siderails 84 both sides.
Side rails (2): spruce,★ 8.5 x 35 x 268 (bass); 8.5 x 35 x 244 (treble).
Back rail: spruce, 14 x 50, lap-jointed over side rails.
Rack of lime,★ 4.5 x 47, glued to upper side of backrail, set in from rear edge 11 bass; 3 treble.
Balance pins of iron, 2.4ϕ, tops flared.
Backrail covering: 2 ply, green. Upper = modern woven wool 3.6 thick; lower = lighter green 2.1 thick, crumbling on edges and possibly original.
Endblocks 23 wide x 56 high. Walnut sideplates surrounding tripartite decorative strip 5 thick; maple★ 1.5 thick on either side of walnut core 2 thick.
Space between endblocks: 713; total width of naturals = 710. Three-octave measurement = 481.
Keylevers of spruce.★ Bassmost (GG) = 255 long; treblemost (d^3) = 240 long (without endslips).
Scribelines on levers (from front edge of keyplank) 94(GG) to 97(d^3) for layout of natural balance pins; 102 to 105 for layout of accidental keys. Levers ‡ 12.5 thick, quatersawn. Guided by wooden slips fixed into sawcuts at ends of keylevers; these slips ride in the rack slots.
Length of natural keyheads = 30 (i.e. further of two scored lines across the ebony keytopping). There is a second scribeline forward of this one at 26.8 from the front of the keytopping; the natural key heads are rounded to this point.
Ebony keyplates overhang their levers in front by 1.7→2.0. Embossed paper keyfronts painted black=0.5 thick. Ebony natural coverings between 91–94 long; 2.8 thick at head of GG key; 1.5 rear. However, the majority of the ebony key-coverings do not taper downward from front to rear, averaging 1.8 thick throughout their lengths. Natural keyheads (the spruce portion) are not bevelled on their sides behind the embossed paper keyfronts.

Tail widths: all but "D" keys around 12.6; "D" tails around 15.5.

[1] All wood species have been determined by microscopic review. The guide for the identifications was Fritz Schweingruber, **Anatomie europäischer Hölzer/ Anatomy of European woods** (Bern, 1990). The identity of the one North American specie found on the instrument was confirmed through R. Bruce Hoadley, **Identifying Wood** (Newtown, Connecticut, 2nd Ed 1993). In the following report, a star (★) indicates that specie could be determined beyond normal doubt through reference to the above works.

Length of ivory risers: average 58; widths average 11.5. All risers about 9 high at front, tapering back to 5.5 high. Fronts cut off at –60°. All sharps now have thin spruce shims glued to their touchpoints against a thick touchrail under the keyheads. However, the rear portions of the split sharps do not land on the touchrail, even though these two sharps have been fitted with long shims on their bottoms. Although the touchrail is old, its failure to stop the keydip of the two rear split sharps is evidence that it was not original. Originally, the keydip was limited by the jacks' rise to butt against the padded jackrail, in what is often considered "French" fashion.

IV. Register: The box register is made from walnut blocks grooved prior to assembly, so as to create two isolated jackslots on each mating-face of the register blocks.[2] The angle between the faces on which the slots are located and the sides of the register is 27.5° (as is the angle of the register from the spine). Overall length = 723; 23.2 wide at treble; 23.8 wide at bass. The register is roughly 26 deep. The trimmed blocks are quite close to 1/2" in thickness; that is, the spacing parallel to the spine of the strings in the string band involves a repeated spacing between close pairs of strings of 1/2" at the register.

V. Frontboard, walnut, slides into place in slots behind keyboard brackets. Veneered on front with wide-grained red cedar★ from North America (*Juniperus Virginialis* sp) and on rear side down to level of wrest plank molding. Inscription on either side of intarsiated central lozenge containing design of flowers and a butterfly. Top of frontboard capped with cross-grain walnut.

VI. Speaking Lengths of Strings

These measurements were made prior to repairs of the soundboard, both by measuring between the nutpins and the bridgepins, and by recording plot points from along and perpendicular to the spine. There was agreement between the two methods of measuring the string lengths. The soundboard had suffered distortions from severe structural damage. Its ribs had separated from it. The bridge was broken in many places. Lastly, the treble part of the bridge had broken off and had been reglued at random. These mischances make these figures less than fully trustworthy.

Speaking Lengths of Strings, Keene Spinet, 1704

Key Name	String Length /Plucking Point
GG/BB	1409 / 140
C	1372 / 143
F	1202 / 150
c	944 / 124
f	781 / 120
c^1	508 / 97
f^1	391 / 94
c^2	257 / 71
f^2	207 / 68
c^3	136 / 48
d^3	124 / 43

[2] See Frank Hubbard, *Three Centuries of Harpsichord Making* (Cambridge, 1965), Plate XXIII, figures 4–5.

VII. Soundboard Layout and Barring.

The grain of the spruce soundboard runs parallel to the spine, as it does in earlier English bentside spinets.[3] All of Keene's spinets I've seen or have seen in photos maintain this grain direction.[4] The majority of eighteenth-century English spinets ran the grain of their soundboards parallel to the front register, and therefore at an angle to the spine.[5] The choice of grain direction is probably behind the placement of the many soundbars on the underside of the soundboard of this one of Keene's spinets, even though in time Keene may have judged these bars to be acoustically undesirable. This spinet dated 1704 has seven soundboard bars. Six of them would be termed "structural" in John Barnes' division of bars into the two main groups of those primarily intended to stabilize and strengthen soundboards, and those added primarily for "acoustical" reasons.[6] Here is a plot of the 1704 spinet soundboard with its bars dashed in and with soundboard thicknesses. The bars are numbered "1" through "7."

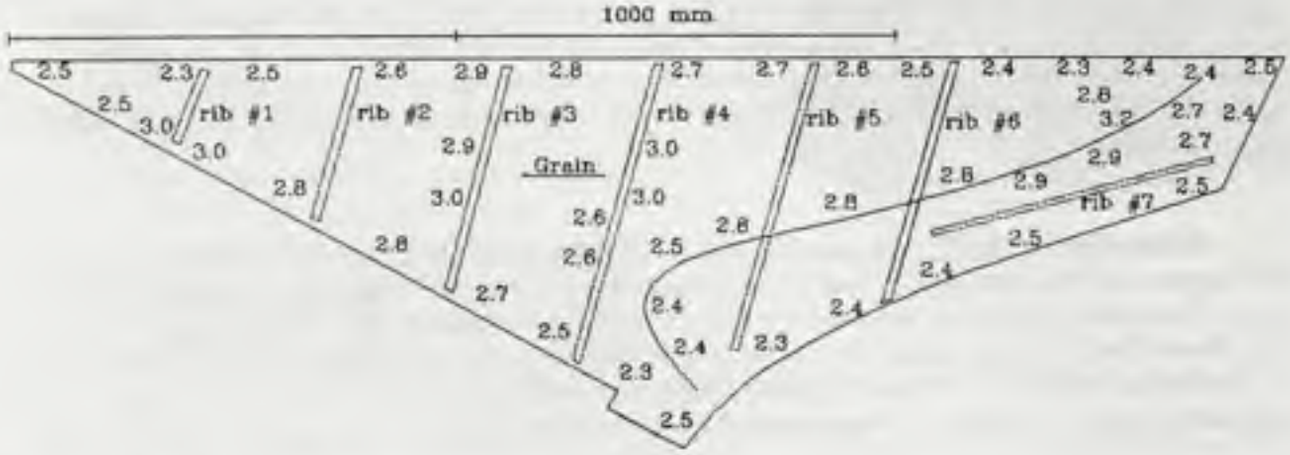

Figure 3. Plot of Soundbar Positions and Soundboard Thicknesses, 1704 Stephen Keene spinet, Collection Ronald Haas, Aptos, California, USA.

Bars numbered "1" to "6" would clearly be "structural." Bar "7" is of a different type. It is an "acoustical" bar, using Mr. Barnes' notation mentioned above. Other Keene spinets have similar bars, perhaps even other spinets of the year 1704, which have been made known to me by Darryl

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[3] E.g. anonymous "late seventeenth-century spinet" shown in Raymond Russell, **The Harpsichord and Clavichord** [London, second edition 1973], Plates 59-60

[4] Cf. photos and technical drawing by John Barnes of the 1715 Keene-Brackley spinet, made near the end of Keene's career. These materials have been privately published and are available from Mr. Barnes at 3 East Castle Road, Edinburgh EH10 5AP, Great Britain.

[5] E.g. the bentside spinet of Thomas Hitchcock, "c. 1705" [**The Russell Collection of Early Keyboard Instruments** [Edinburgh, 1968] pp. 22-23, catalogue #10.

[6] See "A Theory of Soundboard Barring," **De Clavicordio** [Proceedings of the International Clavichord Symposium . . . Magnano, 9-11 settembre 1993], 1994, pp. 161-169.

Martin.[7] I hope information about their barring becomes better known. The soundboards of Keene's spinets were oriented as were the soundboards in his English virginals. The system of barring in this 1704 spinet would stiffen the soundboard, stabilizing it against dimensional change as it passed through seasonal variation. Bar "7" here stiffens the soundboard in the midrange of the compass, possibly to diminish soundboard response to upper partials and thereby to increase the response to its fundamentals and lower partials. The bar also might have been included to regularize the soundboard response to the very flexible brass wire with which the instrument was originally strung. The overall scaling of this spinet and others similar to it is short—even less strong brass stringing, compared to iron stringing, still is stressed at normal pitch only to a low percentage of its total tensile strength. Unfortunately no original strings survive. The stringing has probably been redone many times. There were no indications of string gauges to be used.

Backpinning on the soundboard bridge runs from GG/BB to a. The brass bridge pins average 1.35ϕ. The brass nut pins range in diameter from 1.2 to 1.4. The (sawn) bridge and nut themselves are low but broad. The bridge is 10.7 high in the bass, and 16.2 wide. In the treble it is 10.5 high and 16 wide at c^3. The nut is 9.2 high in the bass, and 14.8 wide. In the treble, it is 8.2 high and 13.8 wide. There are scribe lines for the placement of the pins on both the bridge and the nut.

VIII. Social Setting of the Instrument and Playing of It.

Keene's spinet, like many others similar to it, were domestic delights. They were musically very simple. The disposition of the single eight-foot choir of strings lacks any means to vary its tone. The instruments are meant to be functional and to occupy minimal space, while not calling attention to themselves through elaborate applied decoration. They have been and apparently continue to be considered as good examples of solid and reliable "Queen Anne" or "Georgian" period furniture. One interesting feature of Keene's spinets, at least for North Americans, is his regular use of red cedar veneer on the nameboard and as the interior veneer surrounding the soundboard. This fragrant wood was used inside chests then as it is now.[8]

This spinet is fitted with a short and broken lowest octave. Manipulating the intertwined note-and-key layout takes a while to get used to. In general, spinets are not as easy to play as

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[7] In a letter of 12 January 1995, Mr. Martin kindly informed me of three other Keene spinets dated 1704. Of the three, two are signed by Keene and Blunt. The third, signed by Keene alone, is also dated 1704 on the top key. It is now held privately in Ayr, Scotland. On the matter of soundboard barring, Mr. Martin mentions that the non-structural bar, number seven in the above plot, is similar to a bar on the much earlier Keene virginal of 1668 in the Russell Collection, Edinburgh, and also appears on the soundboard of an Adam Leversidge virginal of 1670 at the Ashmolean Museum, Oxford University. In another letter of 25 February 1995, Mr. Martin amplifies on the Keene spinet at Ayr, noting that draft copies of the upcoming third edition of Donald Boalch's **Makers of Harpsichords and Clavichords** (in process) do not assign any Keene spinets to 1704, but rather assign one mentioned above to 1702. This latter position, that there were no Keene spinets dated 1704, was one told me by another researcher who had access to the present draft of the upcoming edition of Boalch.

[8] See John Koster, **Keyboard Musical Instruments in the Museum of Fine Arts, Boston** (Boston, 1994), p. 77, Catalogue #10, on the MFA's Keene spinet. The brilliance of the red cedar is now muted. A parallel I have imagined to the use of such North American veneer inside English furniture relates to another ocean empire and its colonies. Throughout the later Eighteenth Century, Portuguese keyboard makers such as the Antunes brothers fitted the interiors of their instruments with veneering of Brazilian rainforest woods such as Kingwood. In my opinion, this work was done to celebrate the authority of the central power and the lesser status of the contributory colony to that central power.

Spinet by Stephen Keene, London, 1704
Collection Ron Haas, Aptos, California

are single-manual harpsichords, even though they are meant to be simple. The keyboard feel of a spinet to the player may well be that of the crowded upper-manual keyboard of a two-manual instrument, but one with even shorter keys. The sudden stopping of the keyheads by the (not original) padded rail under the keyheads must have made the instrument feel even more abrupt to the fingers. The jackrail, on which the keys originally were stopped, is of very slim dimensions. It is flexible enough to lessen one's sense of needing to exercise continual caution in playing. The restored instrument was fitted with two kinds of bird quill as plectra. The lower one-third of the compass is fitted with raven quills, and that part beyond with crow quill plectra.[9] One feels the plucking of the strings very directly.

IX. Condition Upon Receipt and Changes to the Instrument.

When the instrument was recently offered at auction, it was unplayable. Its overall condition was very worn and often broken. The most serious problems lay with the soundboard. Large cracks had been dealt with by pouring modern glue into them in heaps. Screws were driven through the soundboard in unsuccessful attempts to capture the loosened soundbars. The jacks had been replaced with very crudely made modern ones. The original stand had been lost. Replacing it was a version of a cabriole-legged stand probably dating from the early years of this century.[10]

Figure 4. Nameboard, Stephen Keene Spinet, 1704. Collection Ronald Haas, Aptos, California, USA

[9] The combination of raven and crow follows the contemporary suggestions of James Talbot, in his notes on how to quill harpsichords at the end of the Seventeenth Century (See Frank Hubbard, **Three Centuries of Harpsichord Making**, pp. 260-1). Although Talbot rescinds his initial suggestion of raven quills for the bass of all keyboard instruments, emending himself to suggest raven quills only for "large Inst[ruments] for [the] 8 lowest Keys," raven works well in this spinet closer to the center of the compass. A word is needed here. To get raven quills legally is no longer simple, as the bird is now a protected specie. The quills used here were brought down in Canada by a farmer (legally).

[10] This opinion was kindly offered by Christopher Nobbs, in response to my sending him a photograph of the instrument and its stand. He goes on to mention the stand's "spindly columns and [its] soapy lathe-made cabrioles" (private letter, 6 November 1994).

Figure 5. Inked name on bottom boards of keywell, "S. Rudd."

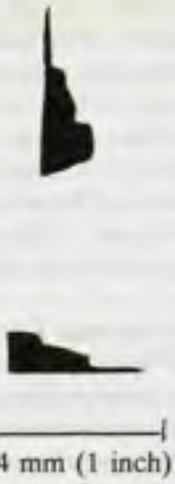


Figure 6. Impressions of Moldings, Keene Spinet, 1704. Above: #4, Bottom molding. Below: #20, molding surrounding soundboard.

When the instrument arrived for repair, the stand was found to have been screwed to the baseboards and then assembled, covering the screws. That is, the stand needed to be pried apart with the help of an automobile jack before the upper stretchers could be unscrewed from the baseboards. This effort was necessary as the baseboards required some twenty shims to solidify the bottom. The upper stretchers of the stand blocked such repairs as long as they were fixed to the bottom of the spinet.

The most serious repair effort was to remove and reglue the damaged soundboard (without significant further damage). Some of the walnut capping veneer running around the top of the instrument was missing from the keyboard brackets. It was replaced. One section of the nut and one section of the bridge had crumbled from serious woodworm boring. These sections were replaced with date-stamped fillings. The spinet possessed its original brass hinges connecting the lid flap to the lid, but had lost its original strap hinges connecting the lid to the spine. The modern butt hinges were replaced by replica brass castings from the Keene spinet of 1715, made by David Law, whose brasses were also used in refitting a front lock and a bentside/lid hold-down. The ebony keyplate covering key a^1 was missing and was replaced. The large walnut battens that reinforce the lid needed regluing but were retained, as the lid is now fragile through worm attack.

The instrument preserves its essential action parts, but for its jacks. Its sound is certainly pleasing. Following the restoration, a small group was treated to a fine performance on the spinet, played solo, and as accompaniment for a soprano. The skilled performers knew Purcell's works well, and all present were indeed lucky to hear them.

(see p. 34 for Addendum)

FoMRHI Comm 1347

Ephraim Segerman

REPLY TO COMM 1329 ON ENGLISH ORGAN PITCHES

In Comm 1329 Martin Goetze has generously offered us much evidence on 17th century organ pitches that has not appeared on these pages before. It makes untenable my hypothesis concerning the meaning of 'Gam ut in D sol re' in the 1665 contract at Winchester, which thus needs to be replaced. He has also outlined his views as to what pitches to associate with the various names of pitch standards given later in the 17th century and in the 18th, sometimes giving his reasons for these views. The names include Choir pitch, Gam ut in D sol re, Gamut proper, Church pitch of F, Consort flute pitch, Consort pitch, Old Consort pitch, New Consort pitch, etc. We are arguing about what these terms meant, as well as what the pitch situation was earlier in the 17th century, before these names were used.

Martin and I have very different approaches to these problems, so much so that it is very difficult for each of us even to understand what the other is writing. It is clear from several of my quotes discussed by Martin that he has not understood some of what I've written. We are both motivated to understand, and this makes me wonder about how many readers can follow what we are saying. It probably does not matter very much if few do, as long as we both keep an open mind about the issues, trying to learn as much as we can from the other, and eventually narrow the possibilities of what the historical truth might be to a minimum in the light of the evidence available. FoMRHI is here to help work out instrument history problems, not just to report worked out solutions.

Before all the names were used, all surviving organ specifications that mention pitch include a lowest organ pipe 10ft long associated with the pitch of CC. The Magdalen College organ (built in 1631, now at Stanford-on-Avon) is from this period, and from the sounding pitches of surviving original pipes, a CC would correspond with a pipe 10 1/2ft long, a semitone lower than the theoretical pitch of a 10 ft pipe. Martin argues that the pitch of this organ was typical of those with a 10ft specification, and I argue that it was not typical. This organ is the only evidence from surviving pipes of this period, and if it were the only evidence of the recognised pitch standard, we should generalise and accept that this is what the 10ft standard was. But there is other evidence that there was a recognised standard that was the same as the pitch calculated from a 10ft pipe. That evidence is from later than this period, but later organs were usually still with a 10ft longest pipe called by different pitch names, and there is no evidence for an upward creep of pitch in the interim.

I am referring to the history of the organ at St. Paul's, which during the 18th century was at a pitch standard of half a semitone above modern. It was called 'Church pitch of f' by Gerard Smith in 1724/5. I don't see how Martin can postulate a semitone higher than this for this pitch standard name. Does he dispute that history?

'Church pitch of f' must have meant that f was somehow involved in specifying the relationship between this pitch standard and a reference pitch standard that had authority respected by all at the time (though its actual use may have been limited). The obvious candidates are the pitch standards represented by a 10ft pipe called either CC or FF. An f at one pitch standard most probably corresponds to a special note such as c or g in the other. The four possibilities this leads to are listed below:

Church pitch of f	REFERENCE STANDARD		
c d e f g	relative to	Number of semitones	Standard
Reference standard	Church pitch of f	from modern	Name
g a b **c** d	4th higher or 5th lower	+5 1/2	none
d e f **g** a	tone lower	-1 1/2	Consort flute pitch
f g a b c	4th lower or 5th higher	-4 1/2	10ft CC
b c d e **f**	tone higher	+2 1/2	10ft FF

The first two of these possibilities, where the f for comparison is at the 'Church pitch of f' pitch standard, do not lead to pitches for the reference standard that seem reasonable, and the last two do, and are essentially equivalent. The surviving original pipes of the Magdalen College organ indicate that its pitch level was $5^1/_2$ semitones lower than modern if it was the 10ft CC reference standard, or $1^1/_2$ semitones above modern if it was the 10ft FF reference standard.

We thus have conflicting evidence about the pitch of what was called a 10ft pipe: the Magdalen College organ surviving pipes and the semitone-higher evidence from the meaning of 'Church pitch of f'. The scholar's job is to do the best one can to explore alternative explanations of the evidence that do not conflict. These include other possibilities for the meaning of the name, a different pitch history of St. Paul's, a semitone rise in the reference pitch standard later in the 17th century and the Magdalene College organ being pitched a semitone lower than the recognised standard. The last of these is supported by the Christ Church organist requesting just this pitch for the new organ at New College. Robert Dallam resisted this, suggesting that half a semitone change would do. This illustrates both his commitment to the reference standard as well as his willingness to deviate from it if pushed hard enough. The other possibilities do not have any supporting evidence, and until they do, the surviving-pipe evidence (though very solid in itself) is most vulnerable in its strength to imply what the reference standard pitch was. Martin would need original pipes from rather more organs that agreed in pitch with the Magdalen College organ pipes to upset this choice.

So, for the time being, I stick with 10ft being 10ft, not $10^1/_2$ft. All of the English pipe length specifications I remember seeing are no more precise than to the nearest half foot, or that figure ±3 inches. Therefore we can be more precise than usual about the 1662 contract at Wells Cathedral, with $12^1/_2$ft open Diapasons and 6ft Principals. The open Diapasons were between $12^1/_4$ft and $12^1/_2$ft, or 12ft $4^1/_2$in ± $1^1/_2$ inches to satisfy both figures at the usual precision. The 1665 agreement at Winchester Cathedral mentioned that the *'bigness [of the 13ft biggest pipe was] according to the Monycord'*, suggesting that pipe length specifications were approximate lengths associated with fairly precise pitches, rather than actual lengths measured by ruler. Thus a 13ft pipe would be a fourth below the standard 10ft pipe and a $12^1/_2$ft pipe would be a major third below such a 10ft pipe. The limits for its pitch on the Wells organ pipe length, when multiplied by $^4/_5$ to raise its pitch a major third give $9^4/_5$ft to 10ft, much closer limits on the 10ft standard than the limits of $9^3/_4$ft to $10^1/_4$ft that this figure implies.

The meaning of the name 'Gamut in D sol re' can similarly be investigated by running through the possibilities. Gam ut is G, the lowest note of the musical scale in the hexachord system of notation, and D sol re is the name of a note a fifth above it. It is presumed that G in the pitch standard of this name is the same pitch as D in a reference standard, or vice-versa. Working through these possibilities, in the former case, the 10ft pipe would have been called FF if the reference standard was 10ft CC, or BB^b if it was 10ft FF. In the the vice-versa case, a 10ft pipe would have been called by note names a tone higher for each reference standard.

The 1665 Winchester agreement was for an unusual organ at this pitch standard with the standard 51 keys, but with the longest pipe 13ft long. I presume that most organs at this standard had 10ft longest pipes and 51 keys. So both pipe lengths should be 'good' lowest notes for the standard keyboard. These notes for the four possibilities of what 'Gamut in D sol re' might be, in the order mentioned, are :

Pitch Name for 13ft Pipe	Pitch Name for 10ft Pipe
CC	FF
FF	BB^b
DD	GG
GG	CC

The AA assigned to the 13ft pipe by Dominic and Stephen Bicknell is not on this list, though Martin reports that Bicknell used the same criteria as above. There must be a mistake somewhere, and I hope it isn't mine. I'll continue, assuming that I am right:

When the Winchester agreement was made, organists were thoroughly familiar with using their keyboards assuming that the lowest note was both CC and FF. Since musicians tend to be very conservative about the playing qualities of the equipment they use, we can say that the first possibility above is the most probable. With a 13ft lowest note, one can play with the normal CC assumption and the sound comes out in Choir pitch. With a 10ft lowest note, one normally plays in Choir pitch, but with the natural keys including the b's instead of the b^b's.

The next most probable of the four possibilities is the last one. An organist with a 13ft longest pipe has a somewhat unfamiliar keyboard to cope with, but most organists with a 10ft lowest pipe have the same organ that they've always had. But why should it have the new fancy name which requires the pitch matching in 10ft FF reference pitch when it is already at the 10ft CC reference pitch? Martin has accepted this one, as a transposing pitch. To handle the Winchester agreement, he makes the dubious assumption that the 13ft specification referred to total pipe length, and the intention was a standard 10ft longest pipe. He probably likes this choice because 'Gamut in D sol re' was stated to be the pitch of the Magdalen College organ just before Renatus Harris lowered it a semitone in 1690.

Organs were often modified from 'Gamut in D sol re' to 'Gamut proper' by lowering the pitch a tone. The similarity of names argues that if 'Gamut in D sol re' was a transposing pitch, so would 'Gamut proper'. Then the keyboard in 'Gamut proper' would start on DD, but in its transposed Choir pitch version,it would be GG. In the first possibility discussed above (where 'Gamut in D sol re' = 10ft FF standard Choir pitch with a proper keyboard), 'Gamut proper' would have also started on GG. This is a proper pitch to start from for a pitch standard of that name. Being a tone lower than 10ft standard Choir pitch, it appears that 'Gamut proper' = 'Church pitch of f'. The change to a GG lowest note was probably associated with a short octave extension of a basically CC keyboard. Only CC and FF keyboards are needed here.

The only difference between the two possibilities considered here is whether the keyboard was a transposing one or not. Martin writes that short-octave instruments were not transposing. This argues against the last possibility. I consider the remaining two possibilities of what 'Gamut in D sol re' meant so improbable that they are not worth discussing.

If the $12^1/_2$ foot longest pipe in the Wells 1662 contract was called a familiar FF. Then the 10ft pipe would have been called AA, and the pitch level, at $1^1/_2$ semitones below modern, would be at the standard later called Consort flute pitch. If it was called a familiar CC, the 10ft pipe would have been called EE, and the pitch would have been a semitone higher than normal (10ft FF) Choir pitch. The former could be appropriate if the Cathedral had a choir only of men, while the latter could be appropriate if the choir boys had very good high ranges. Following is a summary table of English organ pitch standards as I currently see them:

Discussed English Organ Pitch Standards	Relative Pitches	Semitones from modern
ubiquitous early 10ft CC specification	**c** d e/f g	$-4^1/_2$
10ft FF Choir pitch, Gamut in D sol re	f g a/l b **c**	$+2^1/_2$
(old) Consort pitch	b **c** d/ e f	$-2^1/_2$
Gamut proper, Church pitch of f	g a b/**c** d	$+^1/_2$
(new) Consort (or flute) pitch	a b **c** /d e	$-1^1/_2$
ubiquitous late 18th century organ pitch	a b **c**/ d	$-^1/_2$
	↑ modern **c** @ a'=440 Hz	

Martin argues with my (old) Consort pitch. It is a fourth lower than 10ft FF Choir pitch, a major third lower than 10 1/2ft FF Choir pitch (Magdalen College organ) and between a minor and major third lower than the pitches recorded by Dominic that he compares it with. Where is the problem? The first strings of consort viols of the sizes used then (see 1995 GSJ, ~~in press~~) would break very quickly if the pitch was any higher. Some organs, like the one described by Mace, were made to play with viols. That they do not survive is no problem since they had no musical use after late in the 18th century (they were at Tan'sur's Old Consort,Concert or Opera pitch). Many lutes, theorboes and citterns were played in England in the 17th century and none of them survive; they are more attractive than organs to keep when not musically useful any more. Non-survival of historical artifacts is not evidence against their historical existence unless there is evidence indicating that they should have survived.

Then Martin mixes up my explanation of 'Church pitch of f' with my Consort pitch, and of course finds inconsistencies. I hope that he will read this Comm more carefully.

The history of the Winchester organ after 1665 (which apparently concerned the Great organ only) that Martin gives has no problems. Harris's recommendation in 1691-2 to lower the Chair organ a semitone to be in "Gamut proper' indicates that its pitch at that time was at the semitone between 'Gamut in D sol re' and 'Gamut proper'. It is a pity that this pitch level (the Magdalene College organ Choir pitch) does not seem to have had a name. We do not know whether the the keyboard was a GG short octave at a higher pitch than most or a FF chromatic one at a lower pitch than most. Following Martin, this work apparently was not done, and the chair organ was only repaired when in 1693 Harris rebuilt the Great organ with a 50 note compass, implying a GG short octave keyboard. Since Harris mentioned no pitch name, and he usually did if the pitch had one, the pitch was probably the same as that of the Chair organ. At this pitch level the GG lowest note would be played on a pipe that was called FF# on the original organ. The six longest original pipes would have been discarded. There is no problem with fitting into a case made to accommodate a 13ft (or 10ft) longest pipe.

It seems to me that there most probably were two pitch changes in the Magdalene College (Stanford-on-Avon) organ between its original construction by Dallam in 1631 and its 1690 pitch of 'Gamut in D sol re' before Harris lowered it. It first would have been raised a semitone from 10 1/2ft CC to 10ft CC, with pitch names dropping a semitone for unmodified pipes shifted one hole towards the bass. Then it would have been raised a fifth from 10ft CC to 'Gamut in D sol re', with names of unmodified and unmoved pipes dropping a fifth. If the latter change just involved reorganising the keys on the keyboard, remarking might not have been considered necessary.

Martin and I differ in our readings of the Harris agreement. It says he will '*alter the pitch of the said organs half a note lower than they now are; and the said organs, being now Gamut in De, Sol, Re, ...* I take this literally, and he only dropped the pitch a semitone from 'Gamut in D sol re'. Martin assumes that the scribe left out a change to 'Gamut proper' first (which would drop the pitch a tone), and then the pitch was dropped the semitone mentioned (marking the pipes for each change). I can't imagine any scribe's omission to be inserted in this quote that would lead to this reading. I also can't imagine why Harris would make the pitch change that Martin assumes in two steps, marking the pipes after each. The model that Martin thinks was Harris's intention is Harris's 1713 agreement to '*Alter the pitch of New College Organ from Gamut in DeSolre to Gamut proper And to make ye Organ one Note lower...*' Martin apparently reads this to mean lowering the pitch a tone twice, while I read it as saying the same thing twice, to be sure that it is understood. Evidence favouring my reading of the word *And* is in Harris's 1691-2 recommendation for the Winchester Chair organ *'to make the pitch of the Organ halfe a noat lower [a]nd to make it Gamutt proper'*.

The pipe Martin discusses in detail was originally a g# (between d and e^{b} at modern pitch). As I see it, that pipe's g marking comes from the conversion to 10ft CC. Then it would have been

marked c for 'Gamut in D sol re', but that change happened not to be marked. When Harris dropped the organ a semitone, if that pipe was unaltered when moved, it would have been marked c#. Since this note is not marked, this pipe was altered at that time, probably by the scoop, to e, which is marked. In the final alteration to d# the scoop was filled in and the intended pitch marked. It was found to be somewhat sharp and the extension was added, making it somewhat flat. Then it was tuned by scooping the extension and marked again. The two markings could be in different handwritings because different workmen were involved.

Martin's explanation of the g marked on the pipe involves a workman mistakenly marking for a semitone rise rather than a intended semitone drop. What semitone drop? The only semitone drop in his sequence of pitch changes is after the change to 'Gamut proper', and the pipe marking for that (ee) is already on the pipe. My scenario fits the evidence Martin provides better than his since it does not assume any errors in the evidence. Nevertheless there are sure to be other factors that I don't know about which will complicate the situation. And there are the other pipes of course.

The evidence from the Rites of Durham may well imply continuity in the 10ft CC standard from early in the 16th century to 1650, a standard that Dallam was very involved with. But Martin and I still differ by a semitone about what that standard was. Martin believes that it is very unlikely that Dallam would have built the Magdalene College organ a semitone lower than this standard. I say that the evidence that I know about is best fit by that being the case.

FoMRHI Comm. 1348 Ephraim Segerman

Response to Chiverton's Comm.1322 on Lute Belly Shapes

With two pins, the shape drawn is that of an ellipse with the two pins as foci. Three pins make a triangle. If one extends the lines of the three sides of the triangle outwards into the regions that the pencil can traverse, it divides the surface into six areas, three adjacent to sides of the triangle and three touching triangle corners only. If the area is of the former type, the ellipse in it has as foci the pins on each end of the side if the adjacent triangle. If the area is of the latter type, the ellipse has as foci the two of the three pins that it does not touch.

The drawn shape depends on the shape of the triangle and the length of the string. If the string length is little more than the sum of lengths of the triangle sides, the ellipses remain very close to the triangle. If the string length is much longer than this sum, the whole shape approximates a circle. It is intermediate string lengths that are of interest.

It is quite possible to set up the equations of the ellipses given the lengths of the sides of the triangle and the length of string, and plot the shape. It is a lot of work, even using a computer to do the plotting. It is much easier to use three pins,a string and a pencil.

We assume that lute belly shapes are intended to be symmetrical, so the triangle would be an isosceles one. There are three areas below the extended line of the triangle's base, and three above. The maximum width of the shape would not be far from that extended base line. In most analyses with circles, the curve below the maximum width involves three circular arcs in similar areas. Above the maximum width we need from one to three circular arcs on each side, not trying to fit any arc across the neck.

I played a little with this method and got some shapes that looked like lutes, but did not seriously try to fit particular lute shapes to compare closeness of fit with circular arcs. I would be very surprised if it could accurately fit many, since it essentially has only three variables (string length and the base and altitude of the triangle). If we were just interested in shape and not in it being full scale, the number of variables is only two (the ratios of two of these variables to the third). Lute belly shapes vary enormously.

Comm. 1349 Simple Design for Lute Rib Shapes Roy Chiverton

This comm suggests how to design any rib shape for a semi-circular lute body, given that the belly is symmetrical.

Using the thinnest practicable material, cut out a template in the shape of the lute belly, making sure the edges are particularly smooth. Divide this exactly down the centre line. Rejoin the two parts thus created by gluing a strip of very thin cloth along the centre line. This constitutes a hinge.

Make a blunt wedge containing an angle of 20 degrees such that it can be used between the hinged parts of the template to hold them apart at an angle of twenty degrees. Ink the edges of the template and print them from end to end on a suitable surface. The resultant outline will be the shape of a rib for a nine-rib semicircular lute.

The wedge can, of course, be made of any angle less than 20 degrees to provide rib shapes for a lute of more than nine ribs, although an angle of 3.6 degrees for a 50 rib lute might be a little difficult to manage.

If material thicker than ideal has to be used for the template, then chamfering the edge towards the surface which is inside when the template is folded will make for a thin line when printing the rib-shape. Care will then be needed to avoid cutting away any of the actual area of the belly shape. To do this would distort the eventual rib shape.

Space-filler : the end of a letter to Jeremy –

Whenever I see mention of Occam's razor I always think of a quotation from the American humorist H. L. Mencken "For every complex problem there is a simple solution - which is wrong."

Yours sincerely,

Donald S. Gill

German baroque lutes and overspun strings

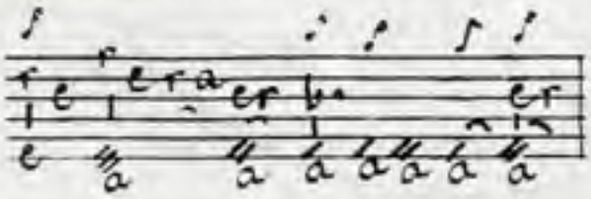
by Mimmo Peruffo

Introduction

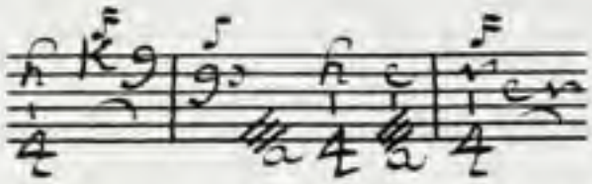
In his introduction to *Comm. 737 (Q44)* M. Hodgson put forward the hypothesis that also in the 18th century lutes may not have used overspun strings. In fact there are serious difficulties to render with clarity bass lines of authors like Hagen, Falckenhagen, Kohaut and many others, because of the long acoustic persistence typical of such strings.

Today it has been necessary to develop a special technique, suitable to the need of damping out the excess of sound, to the point that we might say that a lutenist spends more time stopping the basses than actually playing them! On the other hand, without such a solution passages like these:

F. Daube (1773 - 1797): Allegro



K. Von Kohaut(1726 - 1782): Allegro



would be hardly intelligible and as regards the last five basses of an extended 13 courses lute the problem becomes unbearable. Both the way of composing (e.g. the 9th course is never fretted, unlike the 8th) and the portraits of some lutenists from the 18th c. (Falckenhagen, for example) seem to confirm that it was the latter version of the d-minor lute which characterised the 2nd half of the 18th c., all the way to its final extinction. Today's lutenists use overspun strings, but did they do it in the 18th c. too?

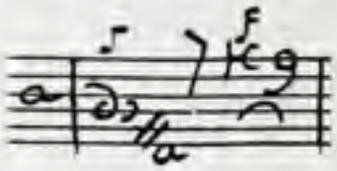
Some elements seem to support such a hypothesis: when in 1664 J. Playford announced the existence of the new type of strings he included also the lute like a potential beneficiary. On Raphael Mest's 12 courses lute (*Comm. 156*) survive some fragments of overspun-strings, but whose dating is uncertain. Nevertheless several various hints favour the opposite hypothesis, that is overspun strings where not employed on the lute.

Musical documentation

The association *overspun strings - sound damping* technique is today a must, the penalty being a great harmonic confusion in florid bass passages (so frequent in late baroque tablatures) but curiously enough, in spite of this rather obvious consequence, no 17th or 18th c. source available to us today suggest that any damping technique may have been used, with the exception, maybe, of Radolt's lute instructions (*Comm. 737*).

We do find pause signed in some tablatures:

K. Von Kohaut: Allegro



but meant as musical effect rather than as indication of playing technique. It is in any case hard to believe that the big harmonic confusion caused by the excessive acoustic persistence of overspun strings could be tolerated by the lutenists of the past, unless damping the basses while playing was generally taken for granted, to the point of not being bothered to mention in the treaties, or simply was not required since overspun string where not used.

For the eleven course lute this supposition holds well considering the abundance of written and iconographic evidence. As known, the earliest mention of overspun strings dates back to J. Playford's in 1664, followed some decades later by other documents which fully confirm (C. Perrault's "Oeuvres de Physique" 1727; D. Speer's "Grund Richtiger Unterricht" 1687, also J. Talbot, 1696 c.a.) but the eleven course lute did not exploit this novelty. In fact both M. Burwell's lute tutor (c. 1670), T. Mace (1676) and J. Talbot (c. 1696) recommend all-gut strings for the lute: Lyons or dark red Pistoys.

Iconographic sources, too, leave no room for doubt and among the many examples available I shall only mention the famous portrait of the French lutenist C. Mouton in the Louvre, painted towards the end of the 17th century. Right at that time, in any case, the first examples of overspun bass strings begin to make their appearance in

iconographic sources, especially as violin's or cello's 4th string, or as seven strings gamba's last three basses (e.g. F. Puget's "Luis XIV's musicians", 1687?, in the Louvre Museum). They are clearly distinguishable because of their "white" colour (i.e. the silver wire) from the higher strings, which are "dirty yellow" natural gut colour.

String vibrating lengths: treble pitch

According to Segerman, with the advent of overspun strings, the necessity to tune the trebles close to their breaking frequency in order to safeguard as best as possible the acoustic performance of all-gut basses till then in use, could be dispensed with. By tuning the trebles in such manner it was possible to reduce the working diameters to the minimum possible, thus gaining in elasticity and therefore in sonority.

The same rule was essentially applied also to bowed instruments (see Playford and Mace). On the other hand, the new type of bass strings gave such remarkable acoustic performance as to allow the top strings to work at last under conditions less critical to their integrity. The practical consequence was a general reduction of the strings vibrating length (at equal pitch, that is) which Segerman estimated at around 10 ÷ 15 %. This favoured also a more agile performance (*Comm. 65* etc.).

But, surprisingly enough, all this did not happen for the d-minor lute, whether 11 courses or the two types of 13 courses (with peg box bass rider or with neck extension). Surviving instruments built in Germany, Austria and Bohemia in the 18th century have strings vibrating length, for the major part, between 70 and 74 cm, against the 62 ÷ 66 cm one would expect following the criteria applied to other instruments which adopted overspun strings for good (plucked ones included).

Assuming a treble tuned in Kammerton "f" (Baron, 1727), half a tone lower than today's standard pitch of a'=440Hz (*Comms. 891, 1098*), we obtain a break index for gut between 231 and 244 Hz/m with an average value of 237 Hz/m, for a string vibrating length of 72 cm. This corresponds at about two semitones lower the breaking critical frequency for a gut string possessing a breaking point value of 34 Kg/mm^2, a coefficient which, from a historical point of view, I consider more likely than the 32 Kg/mm^2 so far proposed by Segerman. The latter, in relation to actual evidence, that is surviving d-minor lutes but also Renaissance ones present in Museums, appears to be surely underestimated.

On such basis the working frequencies would almost coincide with the theoretic breaking frequency, leaving practically no safety margin (incidentally 34 Kg/mm^2 is also the mean breaking point of modern commercial gut strings). The conclusion is that d-minor lutes trebles worked close to breaking frequencies, which is exactly the same criterion applied both to Renaissance and Baroque lutes which, as we know, aimed at the safeguard of the sound of the basses, which, at the time were not overspun.

Finally, it should be noticed that the transition from 11 to 13 courses did not modify the usual strings vibrating length, fit for all-gut basses of the traditional types (Lyons, Pistoys etc.).

Open strings ranges

The transition from 11 courses lute, with an open-strings range equal to a 8 courses Renaissance lute's, to the 13 courses lute with the last two carried by a bass rider (for which the earliest surviving tablatures date back to after 1715) did not change things as regards the early 17th c. lutes. The overall frequency range between first and last open courses is the same, in fact, as that of a 10 course's with Renaissance tuning (8 courses, according to Scipione Cerreto) and all course lying on a single neck (i.e. at equal string length). To be picky, the 13 course's overall open strings range is half tone lower, but this excess is counterbalanced by the slightly longer string length of the last two courses due to the bass rider which approximately equals a semitone.

So, since there is neither real expansion since the early 17th c. (which seems to has always been a direct consequence of some technological progress in the manufacturing of the bass gut strings), nor any noticeable reduction of string vibrating lengths (as was the case for the other instruments) it seems legitimate to query whether d-minor lutes even employed overspun strings.

Bridge holes

An interesting element of recent acquisition regards the bass strings bridge holes measurements. Diameters of 18th c. German lutes bridge holes (without extended neck, see my *Comm. 1288*) so far recorded are never less than 1.6 mm and mostly fall in the 1.7 to 1.9 mm Ø bracket. This is too small, in fact, to allow plain-natural gut strings (if not loaded-weighted) to reach a sufficient working tension but certainly too large for overspun strings.

Maybe it is worth noting that the lute makers choice of holes size was not haphazardly done. The diameters recorded are generally between 1.1

and 1.3 mm Ø for the highest strings and, for the octaves coupled to the basses between 1.2 and 1.4 mm Ø. The ancient lute makers may have exceeded in drilling the bass string holes of course, but why make the whole operation more complicated (and pointlessly weaken the bridge, too) using various "drill-bits" if every thing could have quite simply done with just one bit, the same one used for the higher courses? After all an overspun string on lute rarely exceeds 1 mm in diameter and this is certainly true for extended necks.

But here things become interesting. In Tab. 1 I have recorded the VIII course (the lowest fretted) and XIII course's bridge holes sizes of some extended neck 13 courses lutes. It can be observed that the diameters are consistently larger that the ones recorded on 11 and 13 (not extended) courses lutes, so large in fact as to allow all plain gut strings to reach sufficient working tensions (pitch a'=415Hz).

More over d-minor lutes with extended neck maintains the use of coupled octave strings, whose function may certainly depend on the aesthetic tradition pertaining to lute, but also on the need to add the high harmonics which the basses were lacking: exactly what an overspun string (in general, but especially if on an extended neck) does not need.

A constructional trait common to the instruments listed in the table is the fan bracing below the bridge, which seems to favour a dynamic enhancing of the lower registers (see *Comm. 334*), if compared to the "classic" J-bar disposition in the same area of the belly.

This topic actually needs deeper examination. Still the amount of information gathered so far allows us to attempt to formulate an interesting working hypothesis: After the early decades of the 18th c. it became impossible to find any supply of traditional all-gut bass strings (Lyons, Pistoys etc.) because bowed instruments, which in the 18th c. must have dictated the market trends, were by then quite satisfied with overspun strings. The terms Lyons, Pistoys, Catlins do not appear at all in the otherwise accurate description of gut string manufacture both in Diderot's "Encyclopedie", 1754, and DeLalande's "Voyage", c. 1765. This probably changed the string makers' manufacturing attitudes, with negative consequences for the lute which was, by now, less important and in use than bowed instruments.

So, in order to be able to use normal (not loaded-weighted) plain-gut, the remedy was to extend some of the bourdons and related octaves, to recuperate sound. At the same time a fan bracing system below the bridge was adopted, disposing with the J-bar, favouring the acoustic performance of the low registers. Naturally the bridge holes were sized to accommodate such strings. Should they not have been the case, the only alternative, in the middle of the 18th c., would have been overspun strings, which, however in conjunction with the other measures just described, would have acquired such an acoustic exuberance as to heavily impair the instrument's balance in terms of dynamics, tone and sound persistence.

Iconography

Some iconographical sources show with no possible doubt the use of all-gut basses, in support of the above exposed hypothesis. I shall mention here the portrait of E. von Kayserlingk (1742) playing an extended 13 courses lute whose basses are all dark red (Pistoys?) and out of Germany, in Italy, a portrait of Count Giovio (after 1770) playing a 10 courses lute "Arcileuto alla francese" (Dalla Casa's type), were the use of natural gut is absolutely clear, both for fretted and extended strings in the bass ranges.

Other interesting examples of 18th c. lutes string with all-gut are: Kohaut portrait, by Carmotelle at Chaintilly, musee Conde, and (by anonim) "Die familie Mozart" (?) c. 1770, in Staatsarchiv, Augsburg. Does anybody know of other good examples?

Conclusions

What so far exposed can certainly put into question the use (or at least the generalised use) of overspun strings on lutes in the 18th century. The feature pertaining to such strings would appear to be a concentrate of all the negative qualities which lutenists of the past always tried to avoid:

a) serious tone dishomogeneity from the higher all-gut registers

b) excessive acoustic persistence of the basses, such as to cause harmonic confusion in flond bass-line passages

c) heavy dynamic unbalance in relation to top strings.

These feature are obviously present all at the same time, but each one alone goes in opposite direction to what always expected from the lute (I do not consider here the chitarrone or tiorba): dynamic balance and homogeneity of tone. On the other hand, are there alternative arguments in defence of overspun strings?

Appendix

The "Arcileuto alla francese", in use in Italy in the 2nd half of the 18th century, probably represents a case of adjustment of the lute to the use of plain-gut strings. We know from DallaCasa's tablatures (1767) that the instrument had ten courses (but Stradivari in his drawings shows 12 courses), six of which were fretted and the other four were on the extended neck. DallaCasa has left us a drawing of the instrument which allows us to determine the relationship between the two string lengths, and that turns out to be in a 1:1.5 ratio. This means the 10th bass, at equal working tension, has the same diameter of the 6th fretted string.

From the portrait of DallaCasa playing the Arcileuto (1759) it is clear that the strings were all gut and since they are coloured red or blue (I think, here, for aesthetic purpose only), it can be inferred that the 6th course and the extended basses had coupled octave strings, while the higher courses were unisons (the treble being single at nominal g'). The tuning was the same as the Renaissance lute's. (At Museo Civico di Bologna there is an archlute labelled "Hans Frei in Bologna 1597". This instrument was modified by adding an extended neck in the half of 18th century. The proprietor in the 1st decade of 19th century was the Liceo Filarmonico, were DallaCasa worked and gave his archlute's manuscript. Is this instrument his "Arcileuto alla Francese"?).

Even without the iconographical evidence, it can not escape us that the arcileuto alla francese was to use all plain gut strings. It had only six fretted courses, the last of which with an octave coupling, exactly like the lute of the 1st half of the 16th century, a time when basses did not yet benefit from any of the technological improvements which took place after c. 1570. The extended neck on this instrument grants the last bass string's diameter to remain within limits where a plain gut string can still give a satisfactory acoustic performance. Such limit, exactly like for its 16th century forefather's, was the diameter of the 6th course's bass string. Thus, taking in due account the advantage of the extended neck in terms of diameters reduction and the overall open string range, it becomes evident that the arcileuto's reference is, in actual fact, the Renaissance 6 courses lute. It could not really be otherwise, since the strings seem to have the same acoustic limitations that they had in the 16th c. (high twist?).

What so far said is also valid for the 13 courses d-minor lute with extended neck (here again, taking in due account the effect of the extension on the bass strings' diameters), although it has a semitone wider open string range in comparisons to the arcileuto alla francese. In fact, all in all, the diameter of the 13th course's bass string falls within the limits of a Renaissance 6 courses lute which, let us bear in mind, exploited to the highest degree the mechanical and acoustic characteristic of all plain gut strings, corresponding to an open string range of two full octaves,* with a 6th course's bass string of 1.5 mm more or less.

* Which is the same open string range of the first eight fretted-courses on a d-minor lute with ex.neck.

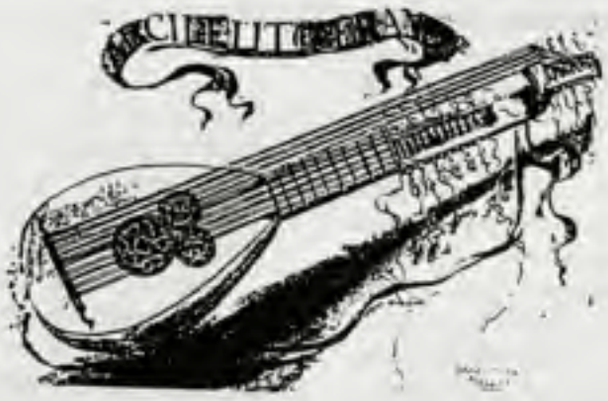


Table 1

LUTEMAKER & PROPRIETOR	DESCRIPTION	COURSE	HOLE Ø (mm)	TENSION (Kg)	NOTES
"Laux Maller" MI 54 Germanische National Museum Nuremberg, Germany	Thirteen courses lute with extended neck (2x1, 11x2) v.l. 0.700 m 0.950 m	XIII	2.00	4.05	Modified by Widhalm in 1757 Fan bracing
"Leopold Widhalm ... in Nürnberg fecit anno 1755" MIR 903 Germanische National Museum Nuremberg, Germany	Thirteen courses lute with extended neck (2x1, 11x2) v.l. 0.738 m 0.995 m	VIII	1.50	3.46	Fan bracing
		XIII	1.85	3.02	
"Sebastian Schelle ... Nürnberg, A.1721" MIR 902 Germanische National Museum Nuremberg, Germany	Thirteen courses lute with extended neck (2x1, 11x2) v.l. 0.705 m 0.903 m	VIII	1.90	5.07	
		XIII	1.75	2.80	
"Christoforo Hoch lauter... " MI 55 Germanische National Museum Nuremberg, Germany	Thirteen courses lute with extended neck (2x1, 11x2) v.l. 0.700 m 0.952 m	VIII	1.40	2.71	Modified by Widhalm in 1757 Fan bracing
		XIII	2.07	4.36	
"Martin Hoffman in Leipzig, 169..." MI 245 Germanische National Museum Nuremberg, Germany	Thirteen courses lute with extended neck (2x1, 11x2) v.l. 0.690 m 0.960 m	VIII	1.40	2.64	Modified in the II half of 18th century? Fan bracing in the treble side?
		XIII	1.70	2.99	
"Hans Frei in Bologna, 1597" nº 1814 Museo Civico Medioevale Bologna, Italy	Ten courses lute with extended neck (6x2, 4x2) v.l. 0.584 m 0.930 m Reinassance Tuning *	VI	1.60	3.20	Modified in the half of the 18th century Arcileuto alla Francese ?
		X	2.00	5.64	

* Top string two semitones below its breaking frequency (breaking point of gut 32 Kg/mm^2)

Fa

More on Ramelli, loaded gut strings and gansars

by Mimmo Peruffo

To my great surprise I found two original copies of Ramelli's "Le Artificiose Macchine ..." (Paris 1588) in my own town's library. This enabled me to verify the bilingual text, French and Italian, and more precisely Chap. 190 where a type of strings for bowed instruments is referred to. The relevant passage reads: "... una doppia e grossa corda fatta nella maniera che si fanno le grosse corde dei bassi di violoni: e che la sia ben ritorta ..." and, in French: "... une grosse corde et double, faicte en la façon des grosses cordes des basse-contres des gros violons; qui soit bien retorte ..."

The author suggests to follow the manufacturing method used for making the bass strings of big violoni (that is, he does not refer to bass gambas). In Chap. 191 Ramelli states "... e facendosi le corde della medesima materia che s'è detto nel capitolo precedente saranno molto miglior che di qualunque altra sorte di materia." The modern edition of Ramelli's work quoted by J. Downing in his *Comm. 1318* unfortunately makes the mistake of translating this passage as though it were from modern Italian: "... if the ropes are made of the same material described in the preceding chapter." But in Chap. 190 there is absolutely no mention of what sort of material should be used to make strings. It is, in fact, an old Italian form of expression meaning: "... if the ropes are made in the same *manner* as said in the preceding chapter, they will be better than in any other system". Downing, rightly, wonders why Ramelli should take, as an example for catapults, only the bass strings of one specific musical instrument and not strings for musical instruments, plucked and bowed, in general.

The only logical explanation is that only those specific strings made use of a rope construction system. The just as logical conclusion is that the basses of lutes and most bowed instruments did not make use of such system. For the violoni, the largest of bowed instruments, the reason seems to be simple: to increase the strings elasticity as much as possible because of the thick diameters involved; elasticity which is of great importance also for the ropes which must absorb the blow coming from the arm of Ramelli's catapult.

In Preatonous drawings, such as the fifth for example, the last four strings of the five string "groß contrabaß" seem to indicate just such rope twisting method. I say "seem" because at such thick diameters, also a high twist string clearly reveals its twist.

One should never overlook the fact that, no matter what twisting process is employed, it never leads to any density increase of the material (if anything the opposite is true). But the increase of gut specific weight is the only logical explanation that can justify the small diameters of bass strings bridge holes on extant historic lutes (*Comm. 1288*), this totally independent of other elements supporting the loading of gut, such as the colour of lutes and bowed instruments bass strings in the 17th century iconographic sources, bearing also in mind that it is possible to load gut without changing its natural colour in any noticeable way.

The colour of bass strings in 17th century iconography deserves a deeper investigation. Their colour, although in paintings by different artists and from different times and geographical regions, remains surprisingly constant: very dark red or various shades of brown, down to black.

If the colouring of those strings was meant as merely aesthetic, why do we never find them depicted with more aesthetically pleasing colours, like green, blue, or rosy, which are the only examples mentioned in treatises of the time, with an obvious aesthetic goal in mind?

Another point to bear in mind: the coloured strings appear on the instruments where we, today, employ overspun strings (e.g. 3rd and 4th violin and bass violin strings, 4th 5th and 6th viola da gamba and violone strings and, on lute, all basses from the 6th down): a pure coincidence, common to all the iconography examined?

And now some observations on the transparency/translucence of strings. From Mace's and Dowland's treatises it is clear that the concept of "transparency" is only applied to trebles and mid-range strings. About bass strings we know nothing beside the fact that one commercial type was dark red; but about their transparency nothing. Iconographical evidence shows also strings of lighter or darker shades of brown: can a thick string of such chromatic characteristics be at the same time "transparent", when even a rope structured string, because of its high twist, is not transparent? For the sceptics I suggest to take notice of whether a well twisted harp-string of a certain thickness stained black or dark red can be "transparent". An increase in twist always reduces the degree of "transparency" of any string, and a bass string is always high twisted.

On the other hand, if the colouring of bass strings was actually consequence of a gut loading process, why were they never green, blue etc.? The answer may be a technical one. I have ascertained that in 16th and 17th centuries, just like today, there existed no inorganic/organic pigments, natural earths, etc. of a green, blue or rosy colour possessing so high a specific weight (and therefore apt to be used for the purpose of gut loading) as can be obtained from the inorganic compounds of mercury and lead, such carbonates, oxides and sulphides, or of partly oxidable impalpable metal powder such copper, for example, whose technique of preparation was widely known since the early Middle Ages. These heavy pigments has a density of some 7.0 ÷ 11.0 gr./cm^2 and a chromatic aspect which spans from white and light yellow (practically the colour of natural gut) all the way to black, via all shades of yellow, orange, red and of course brown. No traces of green, blue or rosy colours. Any other compound produced with other metal salts than mercury or lead compounds do absolutely not guarantee a comparable specific weight: in practical terms could not be used to load gut to the level which must be inferred from the bridge hole sizes on historic lutes.

To confute the hypothesis of gut loading, the only other possible way is to maintain that the basses of lutes (whose bridge holes diameters were recorded) worked at a tension between 1 and 2 kilos (Segerman's *Comm. 1307*). I leave it to the reader to judge the credibility of such a theory, maybe after trying to play a lute with basses set at such tensions, even playing with the right hand as close to the bridge as it will go; this seems to be, all in all, Segerman's proposed remedy to the "rubber band effect" hinarmonicity and to the extreme poorness of sound which gut strings at such tension values unavoidably give (1).

One could stop here, but there are still other elements worth considering. As is well known, the universal custom of the 16th-17th centuries entailed aiming for a feeling of equal stiffness through the strings of an instrument, which can be assured, broadly speaking, as a situation of equal working tension. I say broadly speaking, because strings of different diameter have different longitudinal elastic displacements index, strongly dependent, as well as the diameter, also on the type and degree of twist: maximum in a [possible] rope construction, minimum in a low twist. Such a difference in elastic displacement obviously affects the feeling of stiffness. Then, considering a condition of equal tension between strings, Segerman worked out the diameters of a lute's treble: 0.24÷0.32 mm a (1).

[illegible] in Rome (which, I wish to remind, was an important official document, regulating the profession of the city's string makers quite strictly over a period spanning from 1599, when the first statute was undersigned, to the end of the 18th century, when the string makers guild was suspended) disposed absolutely, with severe penalties against transgressors, that 2 whole lamb guts must be employed and forbid explicitly to make trebles out of a single gut (I assume here 7 to 9 months old lambs, which has always been considered to be the best age for cooking the "abbacchio", the typical Easter delicacy, in Rome).

Segerman estimated (I believe quite correctly) a diameter of about 0.42 mm for such trebles (2). Thus, in the light of this important historic and practical evidence (it is easy to verify today too, that with two whole guts from young lambs it is impossible to obtain diameters smaller than 0.40÷0.50 mm), the only other viable path is to assume that lutes of the past worked under conditions of very unequal tension from string to string: low for the basses (to fit to the bridge holes diameters) and much higher for the trebles (as consequence of the minimum diameter obtainable from two whole lamb guts)(3).

But how can such a statement be justified when confronted to all the 17th century's treatises, where, in any case, the search of feeling of homogeneous stiffness among strings is recommended, if not quite the same working tension (see Mersenne, for example, although his scope was probably didactic)? Mace (Musik's monument, 1676) is absolutely definite: "... another general observation must be this, which indeed is the chiefest, ... and here note, that when we say a lute is not equally strung, it is when some strings are stiff, and some slack, ...". The title of the chapter where he makes this statement read, quite eloquently: "The very principal observation in the stringing of lute".

But there is more: some sources referring to the violin suggest even that one should apply the same weight to the strings, experimenting with different diameters, until the fifths are perfectly in tune (Serafino di Colco 1690, and Leopold Mozart in his violin tutor, 1756).

All this to reaffirm, once more, the historical credibility of gut loading, in other words, it does not justify itself only the basis of a name given by the ancients to a commercial type of bass strings in the 17th century! In fact the contrast between the gut loading and the rope construction never existed, and this apart from the fact that modern reconstruction take only high twist into account: may I remind that we are trying to carry out a research based exclusively on historical elements

here, free, and to, from my ... constructive) reference, which should be foreign to the spirit of the bulletin (and to this spirit definitely I intend to remain true). These two techniques are all but incompatible and if used simultaneously they enable us to obtain the maximum acoustic performance from a bass gut string: was this the case for the violone basses of Ramelli's time?

Both Capirola (c. 1517) and Dowlands mention "ganzer" or "gansars" strings. This is what I found in Diderot's Encyclopedie (c. 1754) under the entries "cor" (p. 176) and "gal" (p. 422÷423):

* GANSE, (*Manufact. en ſoie*) petite poignée de gavaſſines auxquelles les lacs ſont arrêtés, & que la tireuſe attache avec une corde. *Faire les ganſes*, c'eſt arrêter la même poignée de gavaſſines, afin que tous les lacs ne tombent pas ſur la main de la tireuſe.

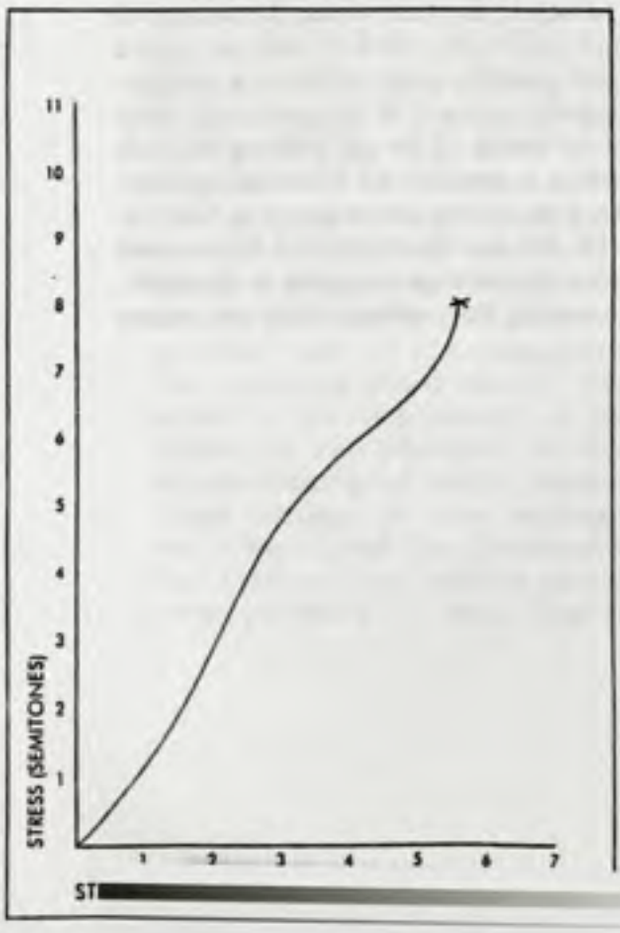
GANSE, ſ. f. (*Rubanier*) eſpece de petit cordonnet d'or, d'argent, de ſoie ou de fil plus ou moins gros, rond, & même quelquefois quarré, qui ſe fabrique ſur un oreiller ou couſſin avec des fuſeaux, ou ſur un métier avec la navette.

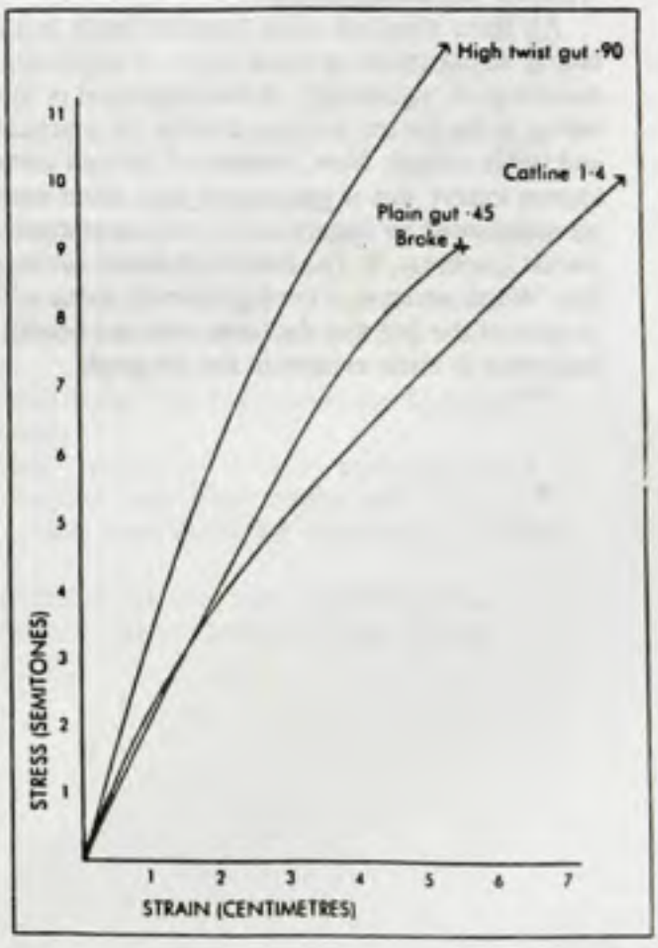
Les *ganſes* ſervent de boutonnieres pour arrêter & boutonner les boutons; on en décore auſſi les habits, ſur-tout aux environs des boutonnieres.

Les Chapeliers s'en ſervent pour retrouſſer les chapeaux, & les femmes pour lacer leurs corps & corſets.

On fait un commerce aſſez conſidérable de *ganſes* en France: les marchands Merciers les vendent; mais ce ſont les Tiſſutiers-Rubaniers & les Paſſemétiers-Boutonniers qui les fabriquent.

CORDE, (*Gazier.*) Le gazier ayant à-peu-près le même métier que l'ouvrier en ſoie, a preſque les mêmes *cordes*. *Voyez ci-après* CORDES (*Manufact. en ſoie.*)





Notes

1) In his *Comm. 1307* p. 55 Segerman states: "A combination of roped basses and plucking lower basses closer to the bridge could explain Guzman's 3 octaves" About strings, we must tackle the problem of even justifying its actual existence. Bermudo states that it is a normal six courses vihuela with one more course added one fourth above the chantarelle (and that makes two octaves and one fourth and not three octaves). How that can be is a mystery, when we bear in mind that on the vihuela, just like on the lute, the top string was already tuned as high as it would go, short of breaking. It is impossible, of course, to go any further.

2) This datum find confirmation in the values given by Mersenne: Segerman (*Comm. 325*) has calculated for each single gut a cross section area of 0.054 ÷0.0675 mm². Thus the diameter of a treble string made out of two guts turns out to be 0.37÷0.415 mm ø; 0.41 mm ø according to M. Namreges (*Comm. 343*, 53 guts = 2.10 mm ø).

3) If anything, it should be the other way round, on the basis of both ascertained historical elements and experimental ones. The style of playing which strongly established itself as from the beginning of the 17th century and lasted till the end of the 18th century prescribed that the right hand thumb be outstretched towards the rose. Thus, on merely physiological ground (and unlike what was common in the Renaissance) the fingers engaged with the higher strings find themselves close to the bridge, where one has a feeling of higher stiffness as opposed to the basses, which are plucked further away, where the amount of "give" is noticeably higher.

Like D.Van Edwards made evident graphically ("Lute strings and angled bridges" in "The Lute", XXV 1985, Fig. 1), a treble string made of a whole gut, when close to its breaking frequency (that is the normal playing conditions for lutes and bowed instruments trebles in the 16th and 17th centuries) loses almost completely its stretch capacity (exactly the opposite of modern trebles made out of split gut strands, which close to breaking begin to show clear sign of fatigue).

Furthermore, it is quite probable that the trebles were of the low twist sort and therefore possessed a no high stretch capacity: in his "Varieties", Dowland states that the end of a good treble must be hard and sharp to the thumb, and break neatly when cut with the teeth. Such features are totally alien to a good high twist string but typical of low twist ones. Mace, on the other hand, states that lute basses go more easily out of tune than the higher strings, and this is a typical predisposition of high twist strings. Another characteristic of basses is that they work at a small fraction of their break point, thus retaining a certain amount of "reserve" stretching capacity.

All these elements taken together result in a feeling of stiffness (which is, in other words, the amount of lateral displacement at equal point of application and amount of force employed) which in order to reach a condition of "equalness", as recommended in the 17th c. treatises, quite probably made necessary a compensation in the basses working tension (in practical terms, a certain diameter increase) in comparison to mean and treble strings. Now, because of its high stretch capacity (the highest among all the gut twisting methods known today), this is particularly true about rope structure strings, which is precisely the historical hypothesis maintained by Segerman (!). An experimental verification of what I am saying can be found in Van Edwards' graphic n. 2. The diameters tested correspond roughly to the 1st, 4th and 6th strings of a Renaissance lute. Worth noticing it the high stretch index of the thickest string (rope structure) as compared to the treble, in spite of the fact that the latter does not benefit from the advantage coming from using a whole gut, whose behaviour is made evident in the 1st graph.

FoMRHI Comm. 1352 John Downing

More on Roped Strings and other Knotty Problems

Following Comms 1318, 1319 and 1320, Mimmo Peruffo sent me photocopies of part of the original texts from Ramelli and Diderot which reference roped string construction.

Ramelli's work of 1588 was published in Italian and French. In chapter 190, Ramelli compares the massive double rope of his trebuchet war machine to the contra bass string not of a bass viol, as reported in Comm 1318, but of a bass violin. The relevant passages read "...fatta nella maniera che si fanno le grosse corde de i Bassi di violoni..." and "...faicte en la façon des grosses cordes des basse-contres des gros violons..."

In Comm 1320, I commented that there appeared to be no evidence to confirm that gut (instrument) strings were ever made in any way other than by simply twisting gut fibres together as shown by Diderot in his engraving of the gut string maker's workshop. Mr Peruffo advised me that Diderot, in another volume of his encyclopaedia, does in fact refer to gut strings of roped construction and suggested that the spinning wheel shown in the engraving of the gut string maker's workshop, which is furnished with two spinning hooks, might have been used for making strings of roped construction.

Following his description of gut string making in general, Diderot goes on to state "C'est de la meme maniere que se préparent les grosses cordes a boyau, avec cette difference ..... qu'on les tord & file comme le chanvre ...." which I take to mean that large (diameter) gut strings are made in the same way (as small strings) except that they are spun and twisted like hemp fibre.
Although Diderot is clearly saying that the large gut strings were made like a hempen rope, he does not say at which diameter strings were made in this fashion or, indeed, if these large strings were intended for use on musical instruments (I only have part of the original text, so perhaps this is made clear elsewhere?). Note that Diderot's gut string maker manufactured strings for a variety of end uses - not just for instruments. He does, however, also say that the large strings were made from ordinary (quality) gut and that less attention was paid to the cleansing of the guts compared to those used in small strings. This might suggest that the large roped strings were not made for instrument use?

The spinning wheel illustrated in Diderot's engraving (see Fig 4, Comm 1320) is fitted with two twirls or spinning hooks one of which is being used to twist a single gut string. In his general description of gut string making above, Diderot also states that the spinning wheel of the gut string maker is like that of the manufacturer of cordage.
The spinning wheel could, therefore, have been used in two ways as Diderot shows in his engravings of cordage manufacture.
There are two distinct branches of the cordage industry - that specialising in the making of small diameter cordage or twines and that involved with large cordage or rope making (by the 19th C, rope was cordage measuring greater than 1 inch in circumference).
Fig 1 shows the way in which the basic component of cordage manufacture (known as yarn) is spun from hemp fibre. Worker B is attaching hemp fibre

from a bundle (chanvre) wound around his waist, to one of the nine twirls on a large spinning wheel. A second worker, at the same time, is paying out the fibre into a spun yarn as he walks backwards. This wheel, therefore, was able to accommodate up to nine workers spinning yarn at the same time in the interests of increased productivity.
Fig 2 shows the manufacture of twines where spinning wheels with four twirls are being used to make twine by twisting together two yarns (bitord) and three yarns (merlin). A four yarn twine could also be made on this wheel. Hence, the two twirl wheel of the gut string maker could have been used to twist two gut strings at the same time to improve output or to twist two strings together to make a two element twine.

So what was the construction of the large diameter gut strings described by Diderot as spun and twisted like hemp fibre? Were they simply two element twines or something more complex like a cabled rope?
Gut instrument strings, by virtue of their relatively small diameter, are not ropes but twines - if we are to use hemp cordage terminology. It is probably better, in fact, to avoid reference to rope constructions for instrument strings as,for example, a cabled twine is of less complex construction than a cabled rope due to the difference in scale between them. Twines may be made from two, three, four, or five individual yarns. A cabled twine or cord is made by twisting or laying together three twines.
A rope, on the other hand, is made from strands or bundles of yarns twisted or laid together to form three or four strand hawsers. A cable laid rope is made by laying together three or four hawsers.
In Comm 1320, I described the Chinese silk instrument strings as being made like a miniature hawser. As these strings are made from three or four bundles of silk filaments individually twisted and laid together like a rope, I equated each filament bundle to a rope strand and hence the complete assembly to a hawser laid rope whereas these strings would be more accurately described as three and four element twines.
A gut string is a special case in that each simply twisted gut string, made from a single gut, is a bundle of fibres equivalent to a filament bundle of a silk string or a spun yarn in hempen rope terminology.
Diderot's large gut strings were most likely twines, laid up from two, three, or four (perhaps five?) simply twisted strings or, cords made by laying up three of these twines.
His gut string maker's workshop was, however, only equipped to make a simple two element twine.

Before moving on, it is ,perhaps, interesting to note that 'yarn', 'cord', and 'rope' are all derived from words in Old English and Greek meaning gut or entrails (Shorter Oxford English Dictionary, 1959).

Ramelli's Ropes

Following his description of gut string making, Diderot then talks about "Des cordes de tendons" which, he says, were used by the ancients on their machines of war. This type of rope was made, not only from tendons, but also from veins, arteries, ligaments and nerve fibres - the properties of each depending upon the type of animal they came from.
This again raises the question about the materials Ramelli used on his machines which he implies were the same as the material used for the counter bass strings of a bass violin. Is it possible that these instrument strings were made from tendons as suggested in Comm 1318? Does this material have proper-ties, such as greater density or elasticity that would make it more suitable for the very largest strings?

If the bass violin strings were made just like the huge rope on Ramelli's trebuchet, then they would indeed have been extraordinarily complex for such a relatively small diameter instrument string. Perhaps, Ramelli only meant that they looked like a trebuchet rope just as I thought my silk instrument strings looked like tiny hawsers?
My guess is that this type of string was made like a four element twine, each element being a simply twisted gut or tendon or fibre bundle of whatever material was used. Such an assembly would have looked like a downsized version of a trebuchet rope.

Mersenne's Knots

In his second book of Harmonie Universelle of 1634, Marin Mersenne, in his corollary to proposition 2, lists various knots used by instrument makers and lute players to tie frets, strings to bridges and horsehair to bows. With the exception of the latter application, Mersenne does not say how each knot was made or what was its specific application.
His list includes "le noeud du Marinier, dont il nouë ses cables" or the Marinier's (Bargee?) knot which he uses to tie his cables (ie to tie to something rather than together). Could this knot have been used to tie bass strings of roped construction to a bridge or tailpiece and is this further evidence of the existence of roped instrument strings in the early 17th C?

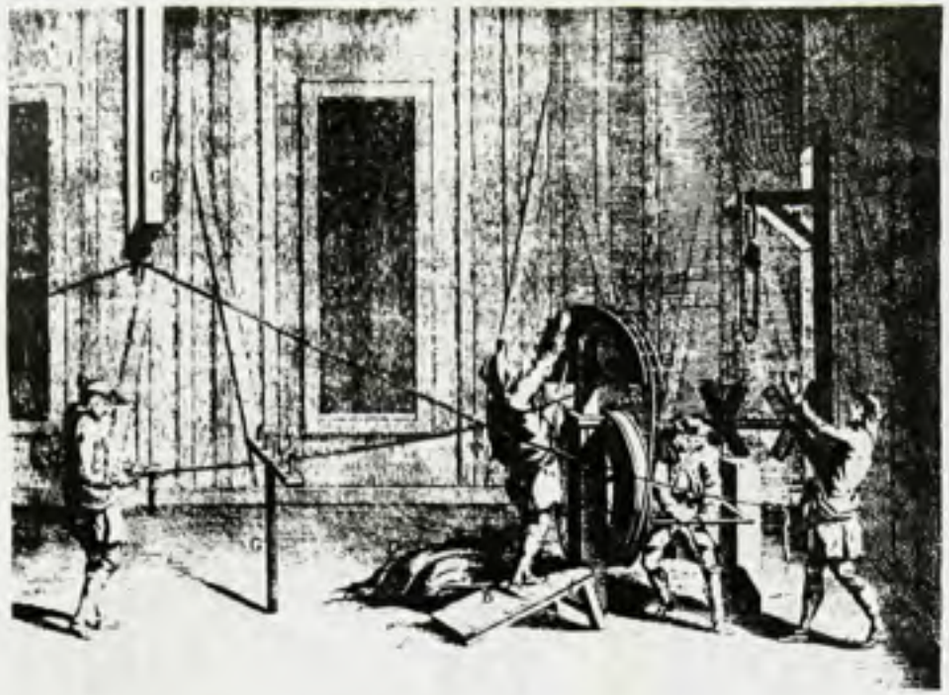


Fig 1 - Spinning yarn for cordage manufacture (Diderot)

B - Fileur..... attachant son chanvre à la plus haute des molettes, pour commencer un fil.

1 - Chanvre à terre.

(B - Spinner .... tying his hemp fibre to the topmost twirl in order to start spinning.
1 - Hemp fibre on the ground.)

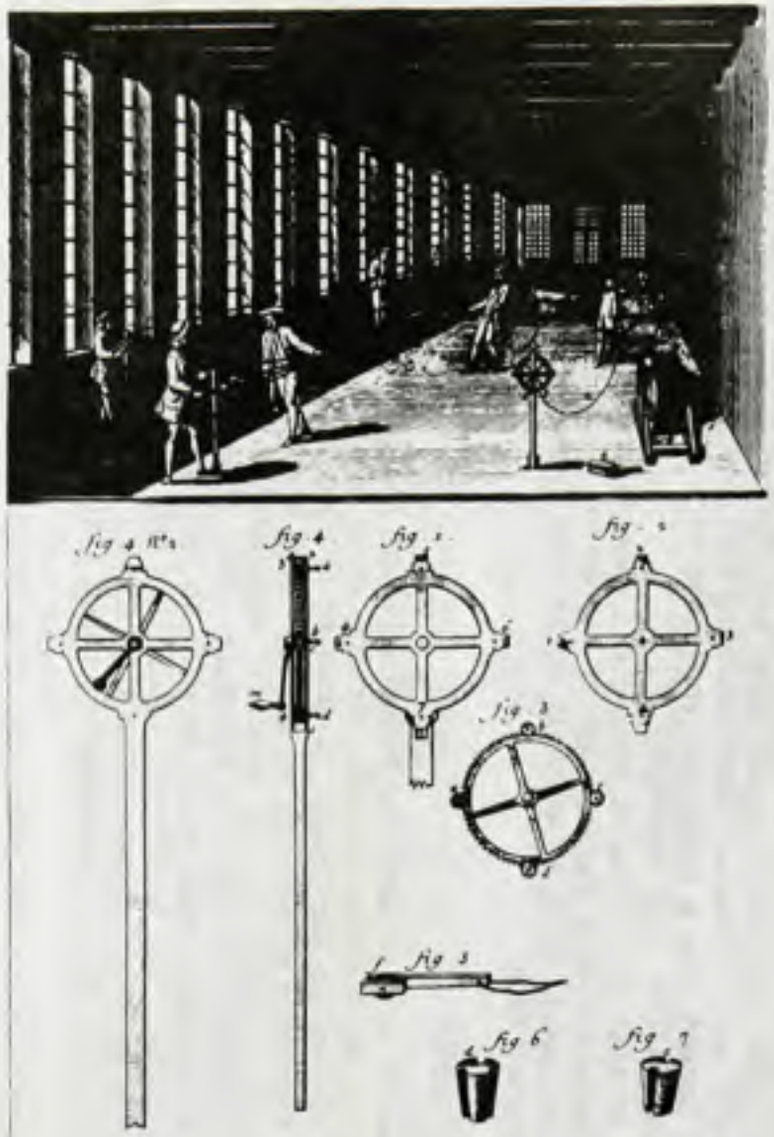
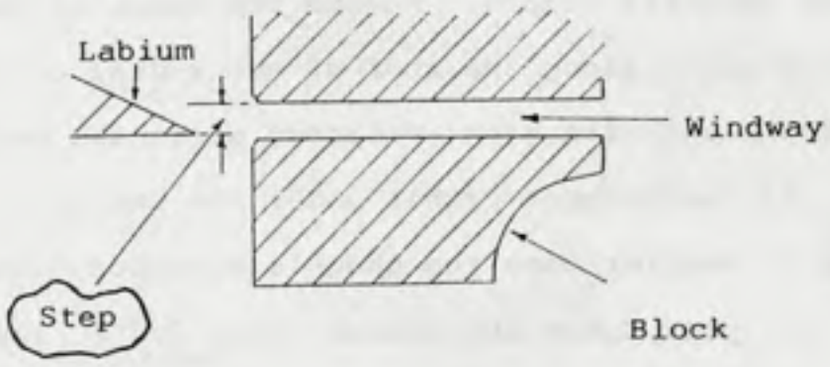


Fig 2 - Twine making (Diderot)

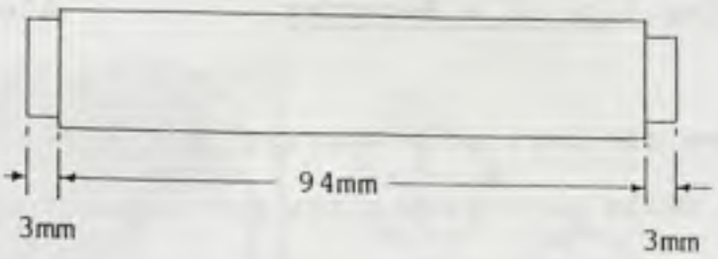
Determining the Step Size of a Recorder

Alec V Loretto

GO and NO-GO gauges are part of measuring techniques in precision engineering. And while such gauges may not accurately indicate exact measurements, they can define sizes to extremely close tolerances. For example, if one has a steel plate with a series of accurately machined holes from 10mm diameter to 15mm diameter, in steps of 0.05mm, one can determine that a particular steel rod has a diameter bigger than, say, 11.4mm but smaller than 11.45mm. By having the holes in even smaller increments, say 0.01mm, it is possible to be even more accurate. The above mentioned steel rod is bigger than 11.4mm in diameter but smaller than 11.41mm. And so on. This same principle can be used in manufacturing a set of gauges to measure the step on any given recorder.
First it is necessary to define what the step of a recorder is. The following cross section of a recorder windway, block and labium makes things clear.



The gauges themselves are quite straightforward and can be made from 10mm diameter brass or mild steel. Each gauge has an overall length of 100mm. Notice the 3mm long reduced diameter spiggots at each end.



If the gauge has been made from 10mm diameter stock, and if the spiggot at one end has a diameter of 8mm, it is clear that the size of each shoulder is 1mm. If the spiggot at the other end has a diameter of 8.1mm it is clear that the size of the shoulder is 0.95mm. By making a number of these gauges and by carefully turning the diameters of the spiggots, it is possible to have a set of gauges to measure from 0.6mm to 1mm in increments of 0.01mm. Or even smaller if necessary. Any sharp edges should be carefully relieved without destroying the accuracy of the gauges.

To develop measuring skills it is wise to use discarded wooden recorders or plastic ones from which the blocks can be removed. The use of the gauges is quite simple. Remove the block of the recorder and carefully place along the roof of the widway one of the gauges. Very slowly and with great delicacy slide the gauge towards the labium. If the spiggot passes under the labium (see fig 1) the step is smaller than the gauge's shoulder size. If the spiggot does not pass under the labium (see fig 2) the step is bigger than the gauge's shoulder size. By carrying out these procedures with a variety of gauges one can with great accuracy determine the step size of a recorder.

The size of the step is one of the numerous variables a recorder maker has to be able to control. If all other measurements are correct and the geometry of the head is also correct then -

a) if the step is too small the recorder will sound pure and sweet, but will lack depth, reson

b) if the step is too big the recorder will sound breathy and diffused and the sound will lack focus.

There are times of course when the above characteristics might be quite desirable and may lend the music a special and individual charm. But generally speaking the majority of players be they the top performers or the most modest amateurs, require a recorder that lies comfortably between the above extremes.

While teaching on recorder making courses throughout the world many of my students have learned how to use these simple step measuring gauges without causing any damage whatsoever to recorders, which have varied from the discarded to the very valuable.

It was in fact one of my students, Josep Bartomeus from Spain, who introduced me to these gauges.

A brief account of these gauges appeared in FOMRHI Bulletin 40, page 69, with a subsequent correction by me in a later issue, number 43, page 5.

Fig. 1

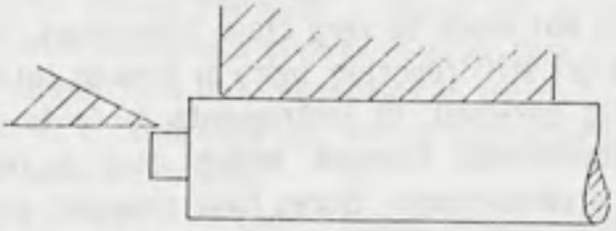
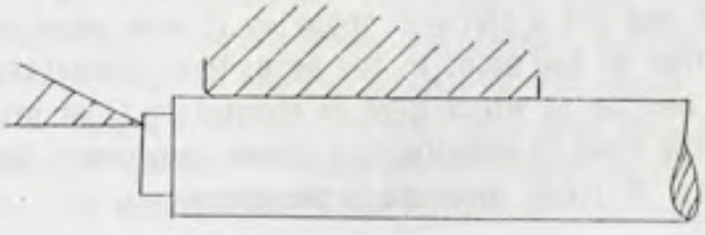


Fig. 2



Finally, make some sort of rack with a cover to protect the gauges from damage, dirt and dust. And to be sure which gauge is which it is worthwhile marking the end of each gauge with its shoulder size.

FoMRHI Comm. 1354

Recorder Bore Measuring

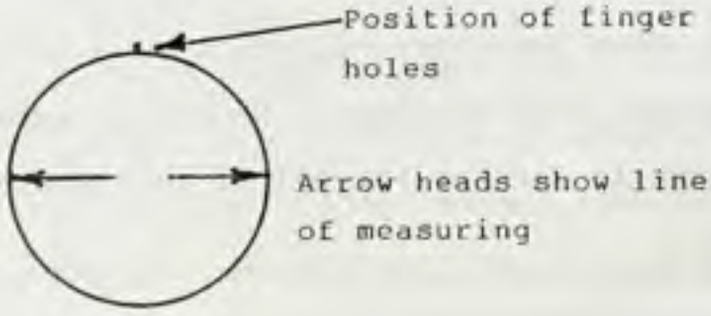
Alec V Lorello

Before making the reamers to replicate the bore of an instrument being copied, certain measurements need to be taken either by the instrument maker or by some other party. As with all problems in recorder making there are a variety of approaches. At least three concern the measuring of the recorder's bore -

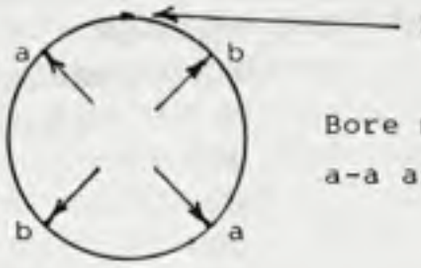
a. What to measure
b. How to measure
c. How to record the data

What to Measure

If one is measuring a plastic recorder it is reasonably safe to assume that the bore is much as it was when it left the factory. But with wood, it is impossible to make the same assumption with any degree of safety. From my own experience (and certainly from my own instruments!) the most stressful experience that wood can undergo, is to be transported from the northern hemisphere to the south, or vice versa. It is difficult enough for a thin walled tube of wood to be made, say, in a dry part of Europe and then to be sent to a wet part of the USA. It is even more difficult if, in addition to the above problem, the instrument undergoes a change of season - from winter to summer. The recorders that survive best these climatic changes are, perhaps not surprisingly, the cheap mass produced wax impregnated models. This latter feature lends considerable dimensional stability to the wood, and as the windway/labium geometry is not made to very close tolerances, things can warp and distort a little yet still function, more or less as intended. Untreated wood, particularly boxwood, in instruments made to close tolerances can undergo dimensional changes which have a marked influence upon the recorder's performance. Bores have changed, and the cross section of the instrument is now some sort of oval shape with a major and a minor axis, and not a circular shape as it was when made. This means of course, that at any point in the bore, two "diameters" at least can be obtained - neither of which give an absolutely clear picture of the one diameter at the time of manufacture. Some measurers, with a "plague on both your axes" attitude, measure in one plane only -



Others prefer to find both "diameters" at various points in the bore -



Bore measured in two planes - a-a and b-b

Others measure the "distance of maximum insertion". They set the measuring device to a known diameter and gently jiggle it this way and that, to see how far into the bore it can go. A little thought will show that this method produces a bore of greater volume than the one being measured, producing an instrument slightly lower in pitch than the original.

All three above methods (measuring in one plane only; measuring maximum and minimum "diameters" at the same point; measuring maximum insertion) gives the maker a starting point - somewhere to begin the journey of making a copy, yet none give the exact dimensions that could have been obtained had it been possible to measure the instrument the moment it was originally reamed.

How to Measure

Certain specialist tools are required, and many makers produce their own.

a) Discs of known diameter - these are often made from thin sheet plastic with diameters clearly marked. One needs to be aware that an ovalish shaped hole is being measured with a circular disc. The discs are attached to a calibrated rod, which indicates how far into the bore the disc has been positioned.

b) Ellipse shapes with known major axis - again made from thin plastic and fixed to a calibrated rod. These can be used to measure in one plane, measure in more than one plane, and measure maximum insertion.

c) Measuring rods of known length - as per above, but made say, from plastic of square cross section with both ends domed.

d) Telescopic gauges - these are standard engineering tools, modified for use. The supplement following this article makes clear the various stages of modifying this over-the-counter piece of equipment.

e) Non manual measuring devices - these include sophisticated electronic and sonic equipment some of which give printouts and bore graphs.

How to Present this data

There are two main methods. Both show a length column (at what distance into the bore the measurement is being taken), and a diameter column (the diameter at a certain distance into the bore).

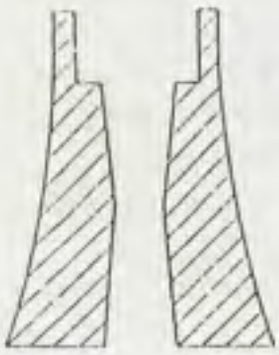
Length	Diameter
0	19mm
5	18.9
12	18.8
	18.7
	18.6
	etc

Length	Diameter
0	19mm
10	
20	
30	
40	
etc	

The above example (left) shows that the bore diameter is being measured in increments of 0.1mm. It also tells us that the measuring device has been set to a definite size, then passed into the bore to determine the insertion length. Or maybe measuring rods of known lengths have been used. The above example (right) shows that the bore is being measured at points 10mm apart, along the length of the recorder. The right hand example assumes the use of an expanding gauge that is passed into the bore to a known depth, adjusted carefully to the diameter at that point, the locking device operated to prevent the measuring lugs changing position, the gauge removed from the bore and finally measured. If one is measuring in two planes the data might look like this -

Length	Diameters max	Diameters min
0	19mm	18.83mm
5	18.9	18.79
12	18.8	18.7
	etc	

In many recorders it is necessary to measure the bore from the north and then from the south - this bore of a recorder foot makes matters clear.



Note the socket to accommodate the string or cork covered tenon.

It is clear, that with the narrowest point being where it is, measurements must be taken from north & south

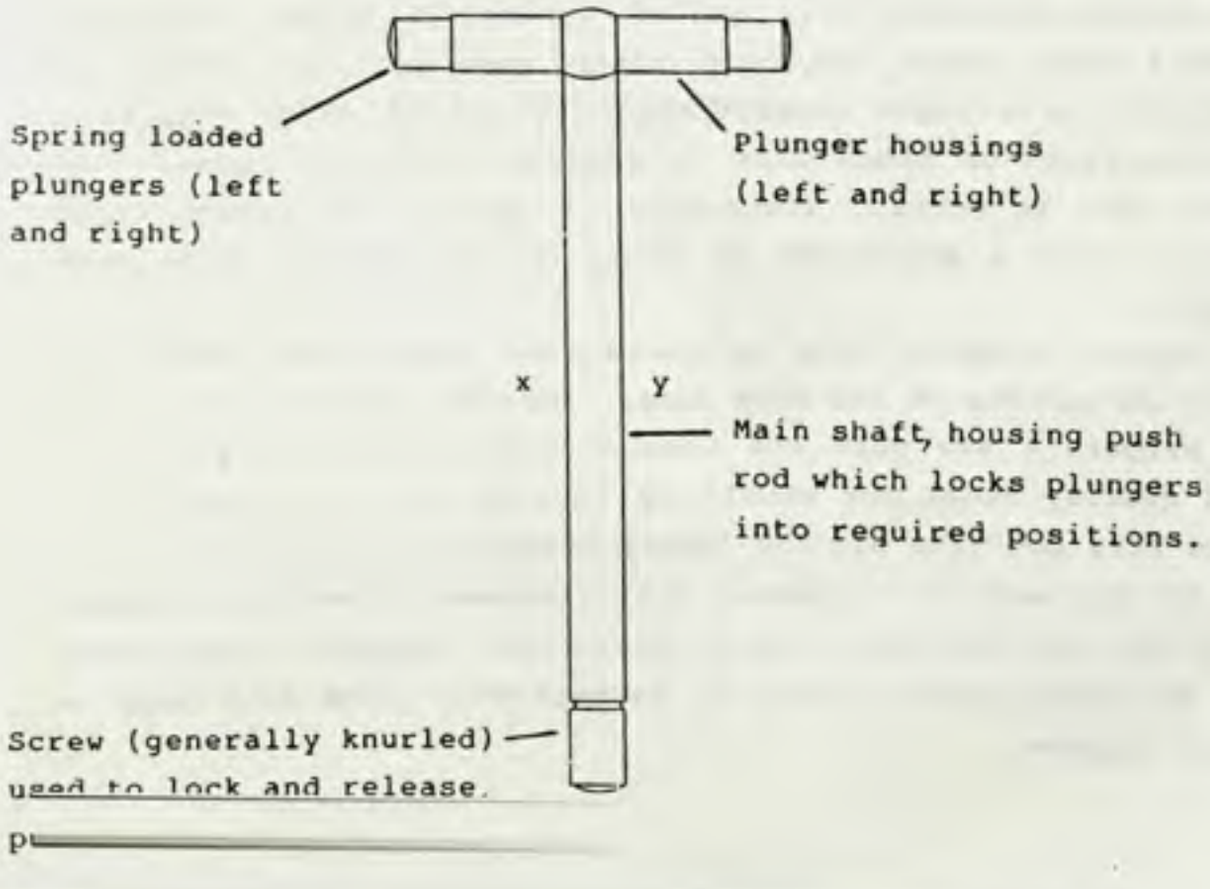
Many of the 'old timers' in the recorder making world spent long hours with original instruments measuring and playing them - not always under the most comfortable conditions. Today there are literally hundreds of plans available from a variety of sources, which is just as well because there is an almost total museum ban on the playing and measuring of

instruments. In the late 1960's and early 1970's almost anybody could ask to play and measure whatever recorders they wanted and the museums generally obliged. No-one was asked to prove they had measuring skills and nobody had to show that they understood how to play these old instruments. With just one person having measured an instrument and just one person having played it, the damage inflicted even by quite unsuitable people was minimal. But by the time this happened over and over again the evidence of damage began to show. At this point, and it happened at different times in different museums, the rules were changed and no measuring/no playing became the order of the day.

Which means at least two things. First, there will be generations of recorder makers who will never be able to measure an original, and who will have to rely on the dimensioned drawing made by others. Some of these drawings are first rate while others leave much to be desired. Second, many makers will never enjoy the quite overwhelming experience of playing some of the superb surviving originals. Apart from being a very satisfying experience, it is at the same time, daunting and humbling. But in spite of restricted access to museum instruments, it is worth having in one's tool-kit some well made bore measuring equipment. Who knows - the second hand shop down the road just might one day have for sale a pristine set of renaissance recorders!

FoMRHI Comm: 1355 Alec V Loretto

RECORDER BORE MEASURING - using modified Telescopic Bore Gauges



The above type of bore measurer is available in a variety of designs. I prefer the symmetrical model illustrated, with both plungers active. Others prefer a non-symmetrical model with only one active plunger. Whichever type one prefers they all share a common problem - namely that while five or six different sizes which make up the usual set cover, via their telescopic plungers, an enormous range the main shafts are never long enough to measure a bore more than about four inches long. Before embarking on any modifications one must decide to what depth one wishes to measure. It is a mistake to think that because one intends measuring the huge sub-contra recorder in Brussells that all six bore gauges should be extended to a length say, of three metres! A three metre gauge with a diameter measuring range from 7mm to 12mm becomes a trifle unwieldy when measuring the ex-Hunt Bressan foot when next visiting the Bate Collection, and could give even greater problems when checking in at the local airport! The range of each gauge must obviously be related to its length.

Having settled that problem, proceed as follows -

a) remove screw and push rod

b) with a fine toothed hacksaw cut the main shaft in the area of xy making sure to remove all wire edges and roughness

c) obtain suitable thin walled tubing (I prefer stainless steel) into which the main shaft must fit. It should if possible be a tight interference fit and it might require a little lathe or bench work to achieve this. For those whose bête noir is working accurately in metal, the modern super glues cover a multitude of sins not to mention wide open spaces!

d) select prepared tube of convenient length and insert the main two pieces of the bore gauge. Ensure a strong join!

e) prepare a new push rod (the original indicates what size and quality steel one should be looking for) and test it to make sure you have got the length correct!

f) if you wish to calibrate the extension to see at a glance how far into the bore one is measuring, prepare a long strip of mm graph paper marking it perhaps every 10mm with easy to read figures.

g) fix it to the extention with a touch of glue here and there making quite certain that the zero mark is suitably aligned with the plungers

h) obtain some transparent heat shrink tubing of suitable diamter and carefully slip it over the extension without dislodging the calibrated strip of graph paper

i) apply enough heat to contract the heat shrink tubing, which not only holds the measuring strip in place but protects it from grease, oil and other workshop dangers.

j) trim the heat shrink tubing to length to enable locking screw to operate freely

For those who have aleady made such modifications to their telescopic gauges and unthinkingly made them all three metres long, and are reluctant to shorten them don't give up hope! Many museums sell dimensioned drawings of their treasures, and it is possible to buy plans of the ex-Hunt Bressan!

This Table is Fig 1 of the following Comm 1356 by Bolton

f=1984.00 Hz

```
Fichier : alt-hot          Doigté n° 3        Pavillon Ouvert
Trous ouvert : 1 2 3 4
Précision du calcul : 0.20 Hz              Acuité minimale 1.00
          -- Tapez 'Fin' pour interrompre le calcul --
     Fr= 440.96              Ye= 310.66
     Fr= 870.32              Ye= 139.21
     Fr=1245.14              Ye=  58.77
     Fr=1539.93              Ye=  44.86
     Fr=1913.32              Ye=  42.88
```

Fig 1

Table of resonance frequencies and admittance peaks for a 440 Hz alto recorder (after Hotteterre). The fingering calculated here is bottom a with the 2 lowest (double) holes open.

FoMRHI Comm. 1356

RESONANS

Since 1988 I have been using a software programme called RESONANS as an aid to developing new recorder models.

RESONANS was developped in France by IRCAM and the Acoustics Department of the UNIVERSITE DU MAINE in Le Mans.

The programme, which works on PC compatible computers, calculates input admittance (for flute type instruments) or input impedance (for reed and brass instruments), and gives a good picture of the the resonant frequencies for the different fingerings.

These can be presented in tabular (fig 1) or graphic form (fig 2). The position of nodes and antinodes can also be visualized, which can be useful for checking hole positions (fig 3).

If used before making the first reamers and building the first prototype, much time can be saved as possible defects can be detected at this stage, and corrections envisaged and tested.

It can also serve to test different possible solutions to a given problem on an existing instrument, avoiding wasting time and pieces of wood or metal on physically experimenting these different hypotheses in order to find the answer.

Another possible application of the programme could be research on original instruments that no longer sound. From their external and internal dimensions, hole sizes and positions etc. their sounding frequencies, and, to a certain extent, their sound spectrum can be calculated.

I have even used it to check 2 different originals of a similar type of recorder to detect which would be the best to copy.

As the programme can be used for any type of wind instrument, a little research may be necessary to adapt its use to the exact characteristics of one particular type of instrument. For instance, the thumb hole of the recorder must be keyed in as a double hole (one large and one small) to be able to simulate the "pinching" technique used for the upper registers of this instrument.

Let it be quite clear that RESONANS will not do the research work in the place of the maker. No solution is suggested by the programme, but any solution proposed by the maker can be tested, with economy of time and materials.

An English version is now available.

For more information, contact :

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20 rue Thalès de Millet
F-72000 LE MANS
FRANCE

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(Figure 1 is on previous page)

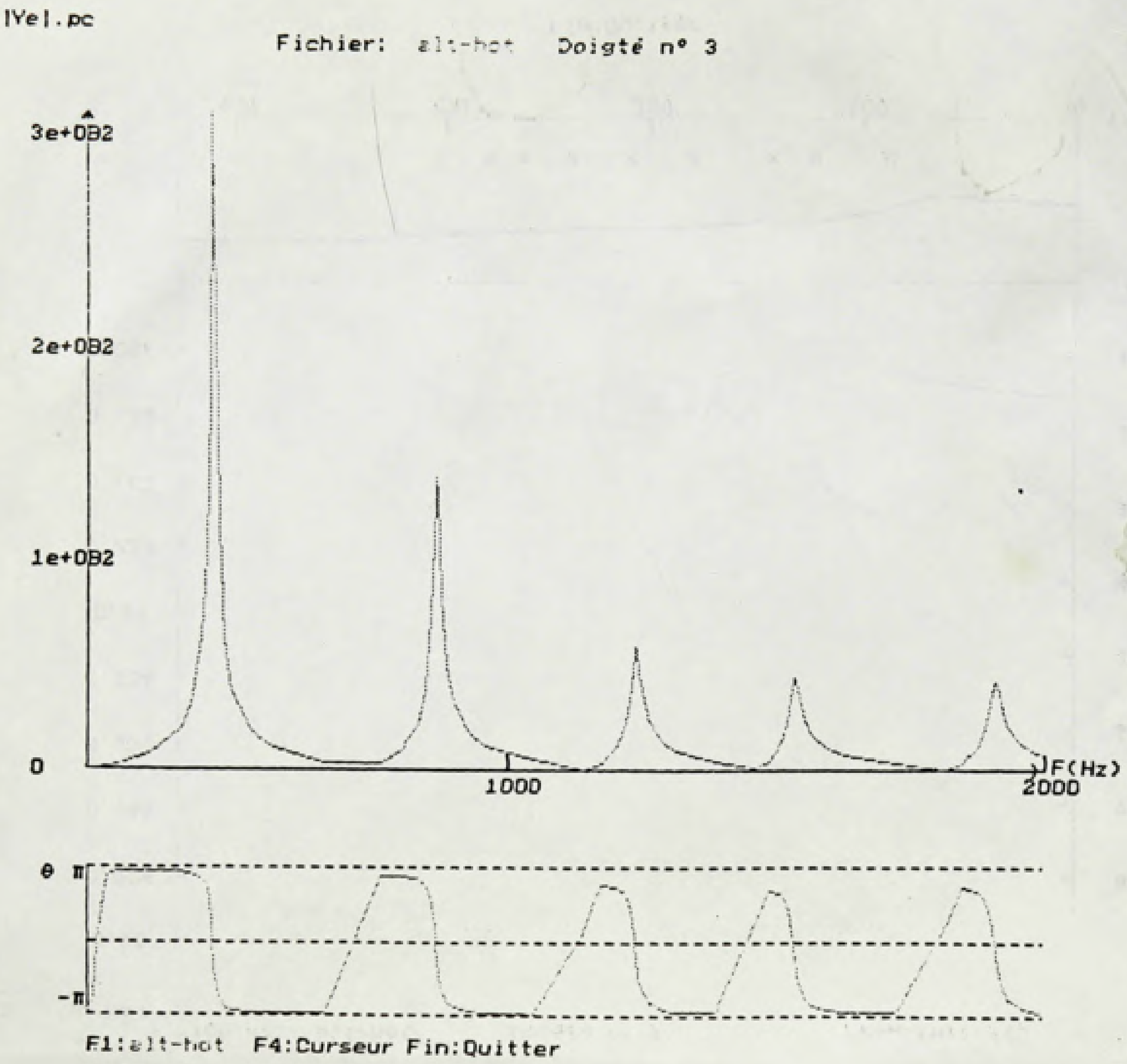

Fig 2

Admittance graph for the same fingering (bottom e) on the same instrument (alto recorder at 440 Hz after Hotteterre) showing resonance frequencies and admittance peaks.

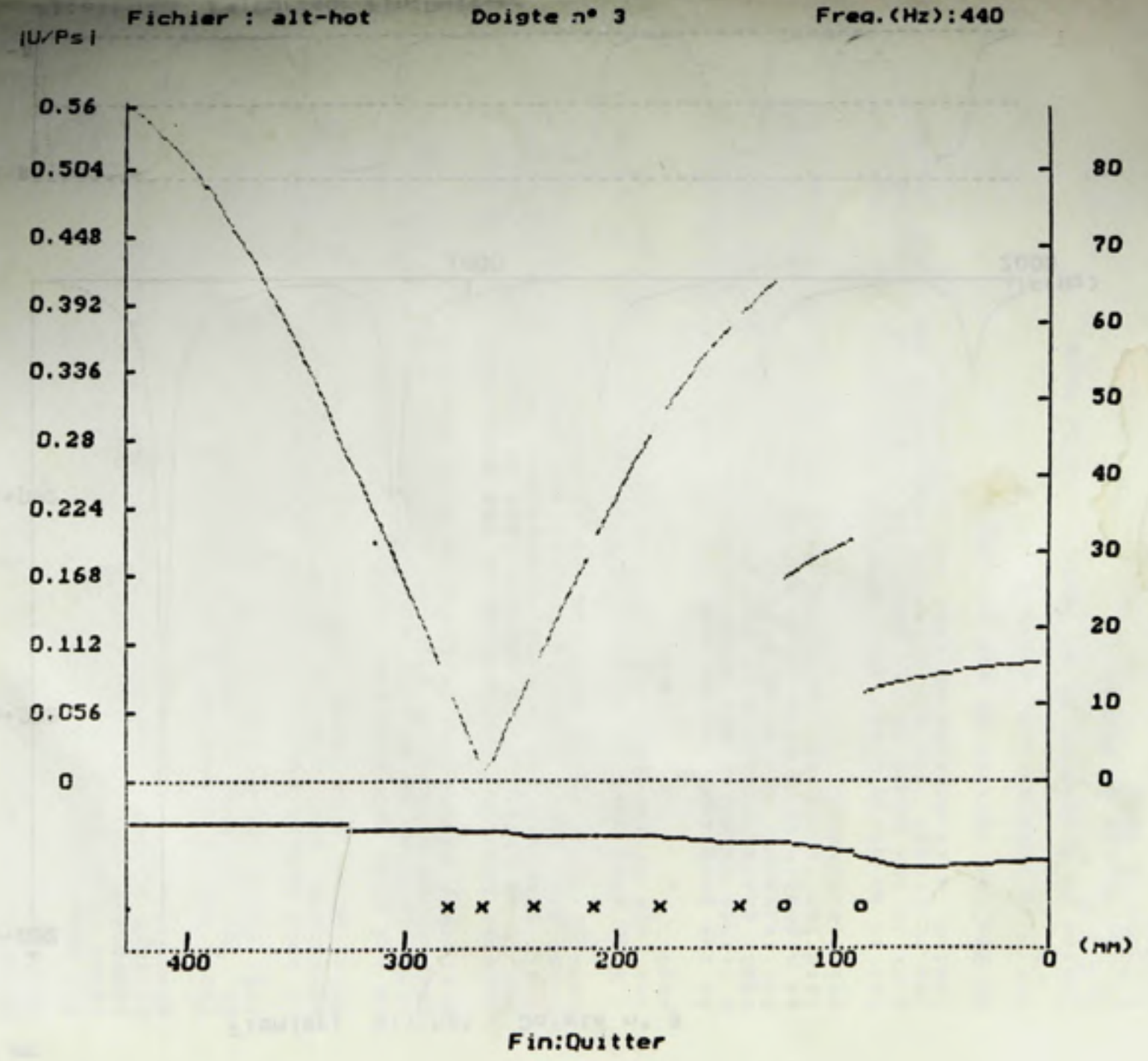

Fig 3

Velocity graph for the same fingering (bottom a) on the same instrument (alto recorder at 440 Hz after Hottetette) showing node and antinode positions (upper part of the graph) in relation to bore profile and open and closed holes (lower part).