Quarterly No. 80, July 1995

FoMRHI Quarterly

BULLETIN 80

BULLETIN Supplement

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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 80

July, 1995

REPEAT OF ADVANCE WARNING: Can you please get things in sharpish for the next Q and Bull? As I told you last time, I want to go off on holiday on October 1st, so it would be good to have everything finished a week before that.

An unusual beginning to a Bulletin, but it’s an unusual time for me — it’s the first time I’ve retired! In fact, working here is the first time I’ve ever had a regular permanent job; until I cam here I was, as the old description for such riff-raff as musicians went, a rogue and a vagabond. It was an awful shock to become respectable, and perhaps it’ll be a worse one to become an old-age pensioner (especially as the pension is going to be pretty minimal — does anybody know of any consultancy or visiting professor jobs going?). People keep asking me what I’m going to do when I retire, and I always say ‘start work’. There’s a large pile of work just waiting to be done, books and articles to write, and so on. Just to remind you, you have my address, 171 Iffley Road, Oxford OX4 1EL; please send things there, now for preference, and certainly after October 1.

I was going to put it further down as BATE NEWS but as I’ve raised the matter here, I may as well tell you now who my successor is. It is Dr Hélène La Rue, who has for the last fifteen or more years been in charge of the musical instruments in the Pitt Rivers Museum. She will still be in charge of the Pitt, but she will also have the Bate to worry about. She is very conscious of the different policies and characters of the two collections, and keen to keep the Bate different. She will have an assistant based here so that there will always be access to the instruments here; that post should be being advertised at any moment, and I’ll tell you who it is next time. So, anything you want from here, write to me at the Bate up to September 30, and thereafter to Dr Hélène La Rue also at the Bate. If you have email, you will find her as pitt@vax.ox.ac.uk. When we got on to email, I tried to keep a separate address for the Bate (bate.collection@music.ox.ac.uk) but it has never worked well. So better to use pitt@vax.ox.ac.uk. She has done a terrific job at the Pitt Rivers over the years and I’m looking forward to all the improvements she’ll make here.

The Assistant’s Post will certainly be advertised by the time you get this; we are only waiting for the final details, so if you are interested in applying, get in touch reasonably quickly either with me (at the Bate) or with Julia Cousins, Administrator, Pitt Rivers Museum, South Parks Road, Oxford OX1 3PP. What I can tell you now is that the job is to Assist the Curator with the day to day running of the Bate Collection, manage the shop (ie there is financial responsibility, but there is an invigilator who deals with actual selling etc), help with visitors to both collections (Bate and Pitt Rivers), help with events such as Bate weekends, concerts etc. It is a post on the scale of Clerical 3, and the pay scale is £9,995–11,581 (under revision). It must be emphasised that the candidate should have good clerical skills, musical knowledge, and it would be a good post to experience musical collection management etc. It will require some flexibility because if Hélène La Rue is down at the Bate, then the assistant will have to be up at the Pitt, which is on two sites plus several stores in different buildings. Both Hélène and I suspect that what we are looking for is something rather beyond the post scale — someone with knowledge, enthusiasm, commitment and immense energy — a paragon! But then, with the privilege of collections like these to work in, why not? I may say that over the years I have gradually become aware that my salary is about 20% less than that of my colleagues who are also College tutorial fellows, but nevertheless, nice as a few thousands extra would be, it has still been wonderful to work here.
LOST MEMBER: Does anyone know where Alice Margerum, who was in Sauchiehall Street in Glasgow, has got to? She renewed her subscription and promptly vanished!

FURTHER TO: David Crookes in a Comm herewith (Shelly Hautbois) asks whether any other conch trumpets have fingerholes. On the assumption that Eph and I are allowed to answer questions of fact in the same Q as the query, the answer is yes. On Fiji there are two species of conch used. Charonia tritonis Linnaeus is side-blown in the normal Oceanic manner, though I'm not sure whether hand-stopping is used as it is on other Pacific islands. Bursa bubo Linnaeus is end-blown and always has a fingerhole in the parietal wall just inside the mouth. The normal rise in pitch produced by opening the hole is around a minor third; obviously it varies a bit from shell to shell, but all that I have blown in many museums have been in that area. The two conches and their use are quite distinct, though there is, in one of the Australian museums, an example of C tritonis, end-blown and with a fingerhole; as far as I know it's the only one. What the B bubo were used for, and why they wanted a finger hole, is unknown. The shells seem to have been ritual instruments (they were all found associated with cult sites), and the missionaries so firmly extirpated the native religion at the end of the last century that there are no traces left and, unlike the Roman Catholics, many of whom produced valuable ethnographic documentation (the Vatican produces one of the most important ethnographic journals, Annali Lateranensis), they were too bigoted and too ignorant to record any details of what they found. This is the only conch that I know of which has a fingerhole, other than David's of course. Hand-stopping is common over much of Oceania (certainly in the New Hebrides and the Solomons), also in East Africa (the only part of Africa where the conch is known) and lowers the pitch by a fifth or more. I made some use of this technique when recording music for the film of The Alien.

Charles Stroom in one of his two Comms herewith, Copyright, the Status of FoMRHI, and Accuracy (with his permission). He asked me about FoMRHI, copyright, and some other matters in a letter which he has, at my suggestion, turned into a Comm because it raised several points of general interest. What I replied to him about copyright was that we had quite deliberately never claimed copyright so that collegial journals, for example the Dutch Bouwbrief, would be free to reprint articles of interest to their members, and people in countries unable to export currency could translate and reprint for colleagues there. I said, too, that as a matter of courtesy authors should be asked if they were willing to be reprinted, and that in English copyright law the author retains copyright of everything s/he writes unless it is specifically assigned to someone/thing else (I didn't say that as a professional author, and a member of the Society of Authors, I am very much against assigning copyright, though I have had to do it occasionally; even then, in English law, the author still retains moral rights to his or her work). Thus everything that appears in FoMRHIQ is, automatically, legally its author's copyright. I went on 'However it would be normal practice to give a copy of anything in FoMRHIQ to a friend without any formality. I presume that Crookes is signalling that he prefers even that not to happen — his bad taste'. I am sorry if this offends David Crookes, but his action in putting a copyright notice on his Comms does go against what has been normal practice in FoMRHI up to now. However, he is certainly entitled to put that restriction on his writings if he so wishes, and you should take this as a warning that you must not make photocopies of his articles for anybody or any purpose.

Controversies etc: Denis Thomas writes 'I had decided to discontinue my subscription, as a protest against the endless spate of "scholarly" lectures to which members are subjected, but on reflection the irritation is outweighed by the excellence of the other contributions. Perhaps you could publish a supplement devoted solely to scholarly philosophy to be available to members only on request'. To which JM adds 'bravo'.

And Margaret Hood says 'I enjoy the controversial outbursts in the journal — they aren't too vitriolic or irrational in my opinion.'
She says, too, that she hopes that her Comm herewith 'will draw some comments from others who do this (does anyone hot-glue veneer still?). I also French Polish and have had builders declare it a waste of time — they use spray guns and modern synthetic varnish.'

**Advertisements:** She says 'on the back pages, like some of the scholarly journals, might work - as you say a lot of bother to manage'.

Julian Goodacre says 'Oh God, must we? It would be really good if they could be avoided. If it really is a good idea financially, then print them on yellow tinted paper and have them stapled in the centre fold of each issue. Then we can 1) ignore them, 2) leave them there and refer to them, 3) remove them and file them all together elsewhere, 4) remove them instantly and put them in the "round file". Each advert should be a set size, basically "visiting card" size. I don't want to see adverts with loads of prices and exaggerated claims; I just want to to know who, what, and where I can contact them. I don't want richer/more desperate advertisers to be able to buy bigger spaces. As these adverts will all be camera ready and in a set increment of A4 you just have to glue them into the set place in the yellow page and cash the cheques (I imagine £20-£30 an advert). If response is poor then you can space them out and make a plea for more and finally abandon the idea (or lower price, but don't tell us that at this stage of the scheme!). If the takeup is good then you may have to hold back some to next issue. 1st come, 1st served. Say each A5 size has 6 adverts. 4 A5 sides x 6 = 24 adverts = £250 revenue for each issue. You're in the money!! But these schemes always have drawbacks. Let us keep our standards and stave off the rising tide of commercialism.

**AWARDS:** I'm sure you will be pleased to hear that The New Langwill Index has been awarded the C.B.Oldman Prize for 1993 by the UK branch of the International Association of Music Librarians. The prize is awarded for what they consider to be the outstanding work of music bibliography, music reference or music librarianship published by an author resident in the UK. Certainly the award is well-deserved, and whole-hearted congratulations go to the author Bill Waterhouse and the publisher Tony Bingham, both members of FoMRHI. The Index is still available, still costs £64 in UK (£66 abroad) including postage, or £60 if you buy it at the RCM Early Instrument Exhibition or from Bingham because you carry it away instead of the postman. See the reviews (Comms 1209 & 1331 in Qs 74 & 79). I use the book all the time, of course, in this job, and the more I use it the better I find it.

Also good news is that Bob Barclay's The Art of the Trumpet-Maker won the American Musical Instrument Society's Nicholas Bessaraboff Prize for 1995. Bob is one of our non-member readers (his institute pays the subscription). Unfortunately his book has sold so well (reviewed in Comm 1103, Q 68) that OUP seem to have run out of them but I have hopes that they are going to print some more copies.

**MICROFICHES OF INSTRUMENT PLANS:** While we're on the subject of books and reviews, in Q 71, Comm 1154, I reviewed Rob van Acht's Checklist of Technical Drawings of Musical Instruments in Public Collections of the World. Without actually checking through the list I'm not sure whether all of these drawings, but at any rate the bulk of them have now been published in microfiche format (you'll find a note about this from the publisher elsewhere in this Q). As one of the participants, the Bate Collection was offered a special price if we wanted to buy the complete set. I dithered about this for a while because normally we only spend this sort of money (most of £1500) on an instrument, but eventually I decided that it was such an important resource that it was worth having here, so there is a set here for consultation. It lives in the Music Faculty Library (all part of the same building), partly because the Library put up a third of the price, but chiefly because the Library has microfiche readers and the Bate hasn't. So if you want to consult these, you need to make appointment with the Librarian (Faculty of Music, St Aldate's, Oxford OX1 1DB; 01865-276146, 276147, or 276148; email libmus@vax.ox.ac.uk.
I'm afraid that the Library will be closed throughout August, but otherwise they're normally open normal hours.

Members elsewhere would probably like to know where else they can consult this set, so please let me know if you know of other institutions which have bought them. It is the ideal way to decide which plan of any instrument you are going to buy — look at all the microfiches and see which you like the best.

EXHIBITIONS, ETC: Doubtless you know about the London Early Instrument Exhibition, which is in early September this year (not such convenient dates as in the past), from 8th to 10th. Remember that it's back at the Royal College of Music, where it began, behind the Albert Hall, and don't go chasing down to the back end of Victoria Street. We shall be there and I look forward to seeing many of you. You'll be welcome to renew your subs there as in the past (saves forgetting to post them!). The amount will be the same as this year; our new printer is so much cheaper than Beeprint that the financial crisis is over and we can hold the price.

The Utrecht Early Music Exhibition runs during their Festival; it seems to be just at the weekends, 26/27 August and 2/3 July they say but I think that must be a misprint for September (7 whereas it should be 9).

Peter Holman is running the Suffolk Villages Festival, August 25–29, which includes a makers' exhibition in St Mary's Church, Stoke by Nayland; enquiries to 01206-211359.

MEETINGS: The American Musical Instrument Society is meeting next year (16-19 May) at the Shrine to Music Museum in Vermillion. Abstracts for papers (250 words maximum, with 100 words or less of autobiography) should be sent to John Koster at the Shrine, 414 East Clark Street, Vermillion, SD 57069 by 15 November.

FoMRHI 20th Anniversary: This falls this autumn and I hoped that we would celebrate it with a conference. This was Lewis Jones's idea, and I asked him to follow it up, but there have been problems finding anywhere suitable (wait till Hélène La Rue has settled in before I start asking if I can use the Bate!), so we thought that perhaps if it was mentioned now, people might have time to think up a really good programme for our 21st Birthday next year. So think of subjects for a weekend of discussion (or a week if you have the stamina), and a good place to hold it, and let me know so that I can pass suggestions on in the next and future Bulls.

THINGS AVAILABLE: Trevor Robinson writes: 'In response to a question in the Quarterly about steam bending, a useful book is Wood Bending Handbook, published by HMSO for the Forest Products Research Laboratory. There are several other useful publications from the FFRL that instrument makers should know about. I cannot find my list, however, and perhaps someone in the UK might follow up on this and publish an up to date list in the Quarterly.'

Condit Manufacturing Co. Inc (29 Philo Curtis Road, Sandy Hook, CT 06482, USA; (203) 426-4119 offer 'an illustrated catalog (sic) of specialized tools for making and repairing string instruments and bows'.

DEADLINE FOR NEXT ISSUE: 18th September, please. I know it's a bit early, but things are liable to be hectic round then, doing all the things that should have been done but haven't been, so please be kind and give me a margin.

Jeremy Montagu
Hon.Sec.FoMRHI
Birthdays and anniversaries are important for the young to celebrate milestones in growing up. They are important for the very old to celebrate the fact that they have survived that long. They are less important for the healthy and mature, who just get on with doing what they have been doing well. So a quick congratulations to all of us, and let's get on with it.

**Nuts and bolts Comms.**

The maturity of the field we are in is an important reason why Nuts and Bolts Comms are less frequent than they used to be. Those who used to write them were mostly self-taught, sharing their discoveries of how to do it with the rest of us. They now know how to do it, and don’t have much new to share. The new makers in the field have mostly been taught how to do it by others, and they can easily feel that it is not appropriate for them to write up the methods of their teachers.

The maturity of FoMRHI is another factor. If someone discovers a new useful gimmick, a major inhibiting factor in writing a Comm. about it is the chore of going through 80 issues of our Q to find out whether it had been thought up before. The Permuted Index helps, but titles don’t tell all, and there is lots in Bulletins that are not included. It could be embarrassing if one hadn’t checked and it had been mentioned before.

As with our bodies getting older, we just have to live with the disadvantages of our maturity.

**The names we call each other**

In Comm. 1334 Peter Spohr writes ‘I feel that I am expected to write “Segerman” now though FoMRHI members were once told that they are using first names’. I don’t remember any decision made by anyone about using names. We decided to be informal, which usually means that we do not have to abide by any rules. Fully formal addressing (like Dr. Segerman) would probably only be used if one was feeling angry or sarcastic, while the last name (Segerman) is neutral about feelings. The first name (Ephraim) shows friendliness, and the nickname (Eph) more so. At least that is how I feel about it. Others might feel differently, and those feelings should be respected. Ideally each person should be consulted as to how they want to be addressed. What they call themselves helps. When they write Comms with initials before a surname, I would stick with the surname, but I could use the first name if it is mentioned.

**Peter Forrester on string tension and other matters**

Peter writes: ‘My experience suggests that equal “feel” implies unequal tension. I imagine the usual test is to push the string down towards the soundboard. On my just finished 4 string mandore this gave a range $2^{3/8}$ Kg to $1^{5/8}$ Kg. Height at bridge is one obvious cause, also psychological (?) effect of inharmonicity in the thicker bass strings. For what it’s worth, except for citterns, everything I make has had unequal tension for some years & none of my customers have complained yet!’

In the same letter he enclosed a picture postcard of an English painting from around 1570 entitled ‘Death and the Maiden’ that has just been restored at Hall’s Croft in Stratford-upon-Avon. It depicts a man who looks like Lord Burghley holding a skull in one hand. In the other hand he holds a mirror in front of a lady who looks like Queen Elizabeth and is playing a small 6-course lute. What is curious about the lute is that the neck has 7 equally-spaced frets on it but its dimensions relative to the body would allow 12 frets. Possible errors in the painting are wrong fretting, a neck too long or a body too short. Peter intends to write about it, probably in The Lute, most likely making more of a fuss about it being an English treble lute (with reference to the Consort repertoire) than I would.
In another letter, Peter mentioned coming across the quote 'a once-white lutestring negligée' in an 1818 novel. Notes to the book give 'a silk fabric'. A recent Chambers Dictionary has lutestring - a glossy silk cloth, also lustrine, lutestring. He adds that his 19th century Afghan dutar has silk strings (cord/rope construction of 3 strands). He encloses these tidbits with orders and cheques. Getting them is one of the especially nice things about being in the string business.

Accuracy of Detail

Alec Loretto’s Comm. in this Q about the anniversary of Arnold Dolmetsch’s invention of the modern recorder is mainly about the accuracy of details. One detail in it states that the Editor has the sources of the various conflicting statements. This is not (yet?) true.

NRI Design Service

Many years ago, Ian Theakston, who was a keen lute maker, was in Manchester for some time, supervising the installation of a new computer system for British Rail at one of the main line stations. Out of one of our enjoyable discussions came the NRI Design Service. I gave Ian a load of the instrument plans I had, and he agreed to run a lending library or rental service with them, with income used to buy new plans. It seemed to run well for a few years, and then I started to receive complaints of Ian not responding. I looked into it, and indeed found that Ian had disappeared, the family moving out with no forwarding address or phone number.

Very recently, Ian’s wife phoned, saying that Ian had just died in an accident, and that I could arrange to pick up the plans. The plans are back now, and my wife Yvonne has volunteered to catalogue them and revive the Service. There probably will be more on this in the next Q.

Occam’s Razor

There are a half-dozen Comms in this Q that mention or allude to Occam’s Razor. It means different things to different people, and has become the subject of some lively discussion. Some say that the subject will be banned. While I am editor, NO subject will be banned as long as it is interesting to some, it doesn’t take too much space, something new is being said, and it is relevant to our purposes. Those purposes include promoting instrument scholarship.

There are many members whose approach is limited to doing things with musical instruments, collecting ‘facts’ about them and speculating about their history. To them, if the ‘experts’ agree on a speculation, it becomes a ‘fact’, and that is all that historical scholarship is about. To them, any further discussion about what scholarship is about is a bore. They have a right to this opinion. I also have a right to my opinion that though a lot of good scholarship is done their way, it leads to some that is not good, and a lot more good scholarship would happen if they knew and followed the basics of what scholarship is. Neither they nor I have the right to try to suppress the approaches of others.
The Project
For many years the construction of musical instruments was a secret that only instrument makers knew. The technical complexity and skillful craftsmanship necessary for the making of musical instruments limited the group of experts in this field. Now for the first time the information in technical plans and the details of construction and decoration of instruments from twenty-six major collections around the world have been brought together and made available on microfiche thanks to the efforts of the Music Department of the Haags Gemeentemuseum (Municipal Museum of The Hague), under the editorship of Dr. Rob van Acht, curator of musical instruments. The project has been carried out in cooperation with the International Council of Museums and the 26 participating institutions.

Contents
The drawings were made for various purposes, but usually include detailed information about important aspects of the instruments, such as technical construction, materials used, details of interiors, carvings and decoration. The information also includes the name of the instrument and its maker, the collection number, the person(s) who made the drawing and the number of sheets and microfiches.

Many different kinds of musical instruments are represented:

- flutes
- oboes
- trumpets and cornets
- violins
- viols
- lutes
- guitars and citherns
- organs
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(for more music from MMF, see reverse)

Specifications
Order no.: M390

Size: Drawings of some 519 instruments on microfiche (format: one to six images per fiche)

Finding aids: Eye-legible headers on the fiches and printed guide compiled by R. van Acht.

Special prepublication price: Dfl. 3,725 (valid until 1 October 1995 thereafter Dfl. 4,865)

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Subsets available (with prices before and after 1 October 1995)

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Back in 1989 we reviewed what one might call the first half of this subject, James Tyler & Paul Sparks, *The Early Mandolin*, which was, of course, well within our remit. This present book, which takes the story on from the early 19th century up to present day, is really rather further from our province, especially since the main emphasis is on the rise of the popular style and of the mandolin orchestras and societies which thrived from the 1870s to the middle of the present century. Nevertheless, there is much here to interest anybody who has a general interest in musical instruments and their history and especially anyone who is interested in the sociology of music and of musical performance.

Dr Sparks has undertaken a great deal of research and has discovered many interesting details, which he recounts very easily and readably. He is, too, an example of what one always hopes from one's students (no, he was not one of mine, though I was able to be of a little help in his student days), for his doctoral thesis was the subject of the first book and he has gone on with his research (so many people say 'OK that's it, now I can do something else') and produced an even better book on the further development of his subject.

The book is divided into periods, the first, from 1815 to 1878, detailing the decline of interest in the instrument from its heyday as an instrument of classical music, with enough reference to the main repertoire for it that those who have not read the earlier book will still have at least some idea of its importance in those days, now largely forgotten save in the recesses of the CD racks and among those who still have an affection for the instrument in its solo rôle.

Thereafter, in three sections, Sparks describes the rise of the amateur mandolin movement, led of course by professional virtuosi, and the enormous popularity of the mandolin orchestras throughout Europe, America, North and South, and Japan.

Then again we see decline, from the second World War to the present day, as we have in every aspect of amateur music-making, with the changes in society, the rise of effortless 'let someone else do it while we listen', and initially, of course the ravages of the war.

Finally we have a very useful section on 'Practical Information for Players and Composers' which does not attempt to replace a proper tutor but nevertheless provides a good deal of useful information, followed by an extremely useful appendix of Brief Definitions of Mandolins and Related Instruments -- how many of us know what a mandoliola is, or a mandolinole? And last a good bibliography.

As a pleasant change, we have footnotes on page, rather than endnotes. One does get so tired of having to keep one finger in a place in the back of the book while reading, and it becomes a real menace when the phone rings or some other interruption means that one has to put the book down open in two places. It is, though, unusual to have abbreviations in the notes such as 'pub. in 1885', or 'without giving a full ref.', or 'will be found in the App.' Still, these don't cause any problems.

I wouldn't say that this was essential reading for many of us; it is essential, of course, for anyone interested in the mandolin, and to those it is highly recommended. It is also, as I said at the beginning, recommended to anybody who is not so blinkered that they can read only books on their own speciality. If you are aware enough of musical instruments to be open to those in which you do not specialise, then you will gain from reading this.
Review of:  Richard Burnett, Seven Broadwoods; the Evolution of an English Piano, CD SCD 0623, Kunitachi College of Music, Collection for Organology, 5-5-1 Kashiwacho, Tachikawa-cho, Tachikawa-shi, Tokyo, 190 Japan

I am not certain whether this CD is normally available or not; Kunitachi College sent it to me and asked for comments on it, and a good way of commenting seemed to be to tell you about it, too. You ought to know about it because it is fascinating to listen to, and one really does hear the evolution of piano sound.

The instruments are three squares, c.1791, c.1815, and c.1830; one high upright cabinet piano, c.1850; and three grands, c.1820, c.1850, and c.1900. The music played on them is, in the same order, J C Bach Sonata in c minor, 17:2; John Field, Nocturnes 2 & 5; Mendelssohn, two Songs without Words and a Barcarole; Schumann, seven Kinderszenen, Beethoven, Sonata 27, Chopin Valse and Berceuse; Debussy, two Préludes and Clair de Lune. So not exactly contemporary in all cases, but not far off, the Bach and the Beethoven furthest from period — by 1820 wasn’t Beethoven back on the Viennese piano? One has to allow for the fact that this is a museum collection and that what we are hearing is the instruments that are in this collection, and for this we should be, and in my case certainly am, grateful.

The booklet with the CD has a photo of each piano, very small but enough to recognise the type, with a drawing of the action and a more detailed drawing and description of the hammer (those of the 1820 grand and the cabinet are reversed, as an insert points out).

The booklet contains a brief and interesting account by Richard Burnett of ‘Piano Music and the Broadwood Piano’. One statement that I wondered about, in connexion with the 1791 square, is that ‘there is no mechanism for sustaining the sound’. My ignorance perhaps, but I’ve never seen a square without the damper lift in the box at the side, and often at this earlier period with a pair of levers, one for the bass up to middle B and the other for the treble from middle C up, thus allowing something which became impossible with pedals, a singing treble with a more staccato bass, or of course the reverse. There is also a very informative article on the mechanism by Dr Sumi Gunji, the curator of the collection, and the author of many of those invaluable handbooks on different families of instruments which I have commended here in the past. Both the articles, and all the other written texts, are bilingual, Japanese and English; this one is rather less English than it might be, and I was sorry that it had not been referred to Burnett, or some other English colleague, for revision. However, the information is all there, even if one has to work some of it out.

Except for the 1900 barless grand, which is at 440 Hz, all the pianos are at 415 Hz. From the conservation point of view, this is probably a good idea; bringing them up to original pitch would have been a considerable strain on the frames. However, if the strings are either original, or the correct materials and gauges, they must be far too slack. By 1791, London pitch was around 437, and by 1850 it was a semitone higher. The 1830 square, the 1843 cabinet, the 1850 grand are all noted as unrestored, and the 1820 grand as ‘stringing partly restored’, and to have tried to take them all up to the pitch they were built for would certainly have led to broken strings and possible other damage. Thus one should regard the sounds as approximations or indications of what they once were.

On that basis alone, this CD is very valuable; one does get at least an idea of the different sound worlds of these various composers, and to have all the pianos the same make, all from the same city, and all played by the same pianist eliminates many variables which otherwise plague museum recordings, and makes this one all the more valuable.

And finally one should add that the performances are excellent, intensely musical, and all ideally played for each instrument as one would expect from a collector and expert such as Richard Burnett. The disc is a delight to listen to in that respect also.

Brian Harvey sets himself a monumental task in his aims to describe Britain's eminent makers, their work, and its context from their beginnings to the present day in a format appropriate for players, dealers, collectors, students, other enthusiasts and makers alike. As Harvey outlines in his preface, his goal is to help people associated with British violin-family instruments answer the following questions: who made this instrument? what is the maker's other work like? who and what influenced the maker? what prices were/are charged for instruments like this?

A useful reference work, Harvey's Violin Family is divided into two main sections: the first containing the text and the second the directory of makers, photographs of instruments, and facsimiles of labels. For the most part, both sections are much more a critical synthesis of earlier resources than they are the presentation of new, ground-breaking research on the violin family in Britain. This is especially true of the first seven chapters covering the development of the violin in Britain. Here Harvey quotes extensively from out-of-print sources. Given that many works on this topic are no longer easy to find, Harvey's Violin Family provides readers access to these through many well-chosen excerpts. Similarly, the directory is an expanded and annotated compilation of previous dictionaries of makers. Perhaps the most significant improvement Harvey has made, other than updating the other sources with new or newly discovered makers, is the inclusion of auction prices and catalogue numbers of specific exemplary instruments where available. Harvey modestly provides the disclaimer that the 'Directory tries to be comprehensive but inevitably cannot fully succeed.' In looking up over 80 British cello makers, there were only five makers I could not find, and one of those was a nineteenth-century maker of a newly patented type of cello and, as such, perhaps should not be counted.

The inclusion of label facsimiles and auction catalogue photographs greatly enhances the directory. Harvey rightly warns readers to be cautious of labels. (See review of Harvey's Violin Fraud in Quarterly No 70, Jan 1993.) For instance, the third one Harvey includes, from the William Baker cello, is possibly a reproduction of a facsimile made by the Hills. The photographs, arranged alphabetically, provide a broad sample of the works of British makers. Again, referring to the Baker cello (Plate 34), it is unfortunate that a post-restoration photograph was not included, especially as this is the earliest known extant British cello. Even so, these visual aids, both the labels and the photos, provide a useful reference of exclusively British works.

It is a shame that Harvey's Violin Family is so costly, especially since there are proof-reading errors throughout. There are some typographical errors and a few repetitious passages. Perhaps the most disturbing are the misnumbering of four of the plates. It is disconcerting for the reader expecting to see a cello, to turn to an illustration of a violin. Numbers 62 to 65 should be as follows: the description of 62 is actually of 63, of 63 is of 64, 64 is of 65, and 65 is of 62. These can be easily corrected in a future edition.

Harvey presents a good introduction to the violin family and its makers in Britain for the non-specialist, and a useful reference for all, especially those without access to earlier British dictionaries and violin histories, in his directory. Harvey answers the questions he poses in his preface, and is particularly good about putting violin-making into economic and political context. Another question Harvey could have set in his preface, which he discusses at length, is — who has written about the violin family in Britain and what points do they emphasise? Another point Harvey somewhat surprisingly stresses is the cello's unique role in Britain as an aristocratic instrument. Overall, Harvey has done well to present a well-rounded picture of violin-making in Britain. Perhaps his greatest challenge was attempting to produce a work equally useful for specialists and non-specialists alike, but he succeeded admirably. While much of this work presents technically basic explanations for the non-specialist, it also compiles information from difficult to obtain sources, as well as new biographical information about makers and writers on the subject.
REVIEW


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The 1993 Magnano Clavichord Symposium, instigated by Bernard Brauchli and Christopher Hogwood, will be the first of a biennial series bringing together players, makers and scholars with an interest in the instrument. Undoubtedly part of the success of this first event was due to the setting, a secluded village in beautiful surroundings in the foothills of the Alps, which helped to produce in everyone a feeling of well-being and led to an atmosphere of goodwill and comradeship which is not always (I believe) to be found in conferences like this.

This book is the record of proceedings and the first thing to be said is that it has been efficiently edited and promptly and economically produced. It is exceptionally good value for money, and no FoMRHI member interested in the clavichord can afford to be without it. Inevitably it reflects primarily the formal papers and public comments. These formed only one aspect of the conference, alongside public and private performances; the exhibition and trial of instruments brought to Magnano by makers; and, of course, much animated discussion, sometimes late into the night. As we shall see, however, echoes of these other activities are to be found in these pages.

A glance at the contents list (which continental-style is at the back of the book) reveals that the great majority of the papers presented are primarily about instruments rather than repertoire or technique[1]. Some consist merely of descriptions of particular instruments hitherto uncatalogued, which is obviously useful as far as it goes. Dr Gerhard Stradner, for example, Director of the instrument collection at the Kunsthistorisches Museum, Vienna, gives brief details of six small clavichords recently acquired. It is heartening to know that instruments are still being found in unexpected places, as some of these were.

Luigi Tagllavini, in contrast, gives an exhaustive description of a multiple-fretted instrument in his possession. It is a member of that small group of surviving 16-th century Italian clavichords which includes nos. 1 and 2 (and possibly no. 3) in the Leipzig University collection[2], the Boston Museum of Fine Arts instrument formerly attributed to Onesto Tosi[3], and an instrument in the Paris Conservatoire collection[4], and it bears an obvious family relationship with these. Like the others in the group, it has a series of short, straight bridges placed at right angles to the

1. For Magnano 1996, the balance will be reversed.


spine, four in this case, one more than any of the others. Unusually for this date it has an original over-rail or damper board: is this the earliest surviving? [6]

Although Sr. Tagliavini's article, like several others, is illustrated with black and white plates, there seem to be no references to them in his text, and since all the plates of the book are grouped together between pp. 136-7, it is easy to overlook them, which is a pity.

Another group of papers consists of surveys of documents and iconography relating to the clavichord. Angelo Mondino gives an account, with measurements, of the clavichord depicted in the well-known 16th-century intarsia at Urbino, a striking case of an instrument depicted with meticulous realism that also has a complex symbolic meaning.

Bernard Brauchli attempts a comprehensive list of iconographical documents on the clavichord, apparently extending from its beginnings to the present day. Here the plates prove their value. Of the eight items illustrated, two are specially intriguing: a detail from a triptych by Gossaert (c.1500) which clearly shows a small clavichord taken out of its case to play, with the discarded empty case lying nearby in the grass; and a surprisingly late depiction (c.1600) by Rubens of a small fretted instrument, the performer being St. Cecilia. Both demonstrate the symbolic significance of the clavichord as representative of the divine harmonic order. But how one longs for illustrations of every item, in the style of Edmund Bowles' Checklist [6], to facilitate comparisons. This might be possible in the format of a single short book.

Ed Kottlick surveys clavichord decoration through the centuries and makes some interesting suggestions on the relationship between the décor of the instruments and their musical and social rôle.

Renato Meucci and Benjamin Vogel both concentrate on the varying significance of the word 'clavichord' (or its equivalents) and the other names used for the instrument, in Italy and in Poland respectively. Both studies may help prevent wrong deductions being made from documentary evidence in future, such as the idea that Frédéric Chopin's first keyboard instrument was a clavichord, which turns out to be the result of a mis-translation: klavikord in mid-19th century Poland meant a square piano.

Vogel finds ample evidence of genuine clavichord making and use in Poland, but not a single surviving instrument of Polish origin [7]. Discoveries are, however, now being made in various hiding places in countries of the former Eastern bloc, and my wish for Mr Vogel is a real live Polish clavichord for him to discuss at some future Magnano conference.

Jean Tournay's curiously unfocussed paper combines reflections on the well-known

5. Michael Praetorius's woodcut of a clavichord 'Italienischer mensur' (Theatrum Instrumentorum 1620) apparently shows an over-rail.


7. Georg Woytzig fl. 1688 was Polish-born, but his surviving clavichord was made in Sweden.
manuscript of David Tannenberg (c.1780)[8] and on an article in Peter Sprengel's encyclopedic work Handwerke und Künste in Tabellen (1773) with descriptions, too brief to be of much use, of clavichords by Gellinger (1670) and Kintzing (1752) and a reference to yet another hitherto unknown 16th-century depiction of the clavichord. Despite strenuous efforts to establish links, none of this holds together. However, there is one insight which may help understanding of Tannenberg's MS. This consists of a set of instructions on how to make a clavichord, accompanied by a drawing. It had been thought incomplete; however, Tournay points out that Tannenberg describes only those details which are hidden. Clavichords were in regular use in the community to which his letter was sent, and details such as strings and fretting would be familiar from daily observation and would not need to be described.

Alfons Huber's extraordinary paper entitled 'The Hexagram as a String Partition Canon for Fretted Instruments' might (just) be described as iconographical: its relationship to clavichords is, so to speak, tangential. He begins by referring, undoubtedly with justification, to the importance of number and proportion in the world-view of those who devised the clavichord in the early 1400's. It is certainly true, as he says, that those who thought in this way were bothered by the fact that pure, numerical concords could only be incorporated into music by way of a series of unduly compromises. At this point Huber leaps into speculation, suggesting that tempered intervals could have been rendered somehow more acceptable if derived from geometric figures: specifically, from the elongated hexagram painted on the parchment belly of a folk instrument from the Balkans which is now in the Vienna Kunsthistorisches Museum collection. This little diagram is surrounded by a frame, enmeshed in various construction lines, and compelled to reveal its secrets in the form of various musical scales (provided you measure the right lines). The trouble with numerical speculation like this is that the assertions put forward can neither be proved nor disproved (as Mag. Huber indeed admits). It is all done here with charm and grace, and is really entertaining: but I do not see how knowledge can be advanced in this way.

Coming down to earth (but only somewhat), two papers offer reconstructions of instruments known only from descriptions or depictions. Nelly van Ree Bernard presents a 'hypothetical reconstruction' of the keyed monochord. Did this instrument really exist? The evidence is tenuous, but it ought to have, since it represents the Darwinian missing link between the clavichord and its monochord ancestor. Ms. Bernard's reconstruction was made to her design by Koen Vermuel. It is by no means the first[9]; it has many speculative details, such as a plucked drone-string, but is undoubtedly capable of musical results, as participants at Magnano could hear.

John Barnes' first paper is an ingenious attempt to reconstruct a practical clavichord of the 1660s from the description published by Claas Douwes in 1699[10]. Where Douwes' instructions are doubtful or incomplete, Barnes finds a sound reason for every choice he makes, avoiding the rather engaging leaps into speculation which Ms. Bernard permits herself. The result is an extremely plausible design: anyone wishing to hear it is invited to make the instrument (a working drawing can be obtained direct from the author).


Barnes' second contribution is one of several papers concerned with examining the structure of clavichords and the function of various components. It is entitled 'A Theory of Soundboard Barring': this is applied specifically to 18-century German clavichords. It distinguishes two types of bar: those whose function is primarily structural, i.e. to help support the soundboard against the various loads placed upon it, and those whose function is acoustic, i.e. to distribute vibrations across the board.

I must admit to being uneasy about the approach adopted here. No clear distinction drawn between (1) what the makers themselves thought they were achieving in placing bars in various positions and (2) what mechanical and acoustic functions bars actually fulfill, according to the best understanding we can achieve using modern scientific methods of investigation. Statements about makers' purpose, such as 'Sometimes ... makers tried to improve the acoustic response of their soundboards by bars which acted as extensions to the bridges' or 'Hubert was not content with relying on the stiffness of the soundboard in the direction of its grain ... and used acoustic bars to extend the bridge vibrations ...' need to be backed up with evidence of what the makers actually thought their bars were for; otherwise one risks attributing to them views which are really one's own and which they may never have held. On the other hand, statements about the actual behaviour of soundbars need to be based on experimental evidence. Of course, both kinds of evidence will be extraordinarily difficult to obtain.

The rigid distinction between structural and acoustical bars reminds me of the distinction between protein and energy foods made by proponents of the Hay diet: not to be eaten at the same meal! Intuitively one feels matters are more mixed-up and complicated than that.

Two contributors, Alan Caro and Clifford Boehmer, both by coincidence from the USA and practising architects, attempt to analyse the behaviour of the clavichord structure under the stress imposed by the strings, and to suggest ways in which its performance might be improved. Boehmer's approach is experimental, and is based on mechanical tests done on a copy of a Hubert clavichord at various stages during its construction. His paper has very much the character of 'work in progress', suggesting the lines on which future experiments might be conducted, and making only tentative proposals for modifying the traditional design.

In contrast Alan Caro strides boldly out along the same path, guided this time not so much by experiments as by pure reason and calculation. Like others before him[11] he proposes a radical new design: it has a structure based on two I-beams, within which the action and soundbox are freely suspended and around which a case is wrapped. The bridge is 'flown' some 6 mm above the soundboard, to which it is anchored by a series of fins: this permits the strings to pull upwards on it rather than bearing down. It is an anti-climax when one realises that Mr Caro has not actually built the clavichord described here. This is urgently needed to test the validity of his reasoning: perhaps it will be available in time for the next symposium[12].

What is curiously refreshing about these two American contributions is that both assume that the clavichord is capable of - indeed requires - further development (I hesitate to say improvement) for the 20th century, just as it developed between the


12. Mr Caro is also credited with the discovery and translation of a remarkable document bearing on the early history of the clavichord, printed for the first time in this volume.
16th and the 18th centuries. More of this later.

The selection of tonewood is of vital importance, of course, to all makers of stringed instruments. Jorg Gobeli's paper — largely presented in the form of tables and diagrams, which can be bewildering for those more comfortable with text — discusses the use of ultrasound testing to help in this task. Actually the equipment involved is far too elaborate and costly to be practical for most makers, but the suggestion is made that some experience of its use might be helpful in view of the decline of traditional wood selection skills (ie looking at it and tapping it in various places). To come to any conclusion about this one would need a far more detailed exploration of the subject.

The best of the organological papers are those which, based on evidence collected from a number of instruments, achieve new understandings through careful deduction. The process has much in common with criminal detective work (at least as portrayed in fiction) and has some of the same fascination.

There are four outstanding papers of this kind. First, Thomas Steiner's Investigation of the origins of those two odd characters, clavichords nos. 2 and 3 in the Leipzig collection, which irrefutably demonstrates that, although apparently so similar, they must have had different origins; and that there are clear connections between no. 2 and a harpsichord in the Castello Sforzesco, Milan, both of them probably of Neapolitan origin. Then Koen Vermey's detailed examination of the surviving clavichords of C. G. Hubert, which succeeds in establishing their chronology, despite the misleading evidence of some false labels.

Grant O'Brien next, who from a detailed examination of clavichords by Gerlach and by the Hass family not only establishes a close working link between these two workshops, but also finds evidence to support the important conclusion that harpsichord string-scaling practice — the relationship between the length and pitch of the strings and the choice of string material — was also applied to clavichords: a fact which, as he says, might seem obvious, but actually can't necessarily be assumed and requires proof. O'Brien's conclusion depends on the evidence of a few old 4-foot strings on one instrument: I hope this isn't too slight a peg to hang the argument on. Of very great help to makers and restorers is his other achievement here: to deduce a clear relationship between the 18th-century continental gauge numbers (so widely found on old instruments) and wire diameters.

Finally, Lance Whitehead's work on Hass clavichords is a particularly impressive demonstration of the power of this method of rigorous deduction based on meticulously collected evidence. He has, with much labour, examined and measured all 23 surviving clavichords from this workshop, and by considering which elements remain constant, which vary in a seemingly random way, and which are subject to apparently deliberate change, he has been able to work out in what order the task of laying out a clavichord was approached, what rulers were used, and how they were positioned. The whole argument is not presented here, but it seems that he can show that similar methods were used by other 18th-century makers throughout Europe. As Whitehead justifiably claims, his analysis enables us 'to understand the working methods of these makers, and to enter into their minds as they developed their designs, in a way that has been impossible hitherto.' An impressive achievement, and well worth the effort required to follow the rather dense reasoning.

One organological paper remains to be discussed, but I propose to leave that till the end for reasons which will appear. Before leaving instrument studies, though, one small quibble about the book: how helpful it would be if, in addition to the Index of

13. See note 2 supra.
Names there was an Index of Instruments listing all references to particular clavichords. Comments directed to one instrument may occur in many different papers, and an index would reduce the time needed to hunt them down.

Now for those papers which focus on history, repertoire and technique.

Beverley Sing, an authority on the German philosopher J.G. Herder (1744–1803) shows how thoroughly the clavichord, and images derived from its perceived gentleness and expressive powers, permeated Herder's ideas and the culture of his time. Derek Adlam's interesting paper discusses the place of both fretted and unfretted clavichords in 18th-century German musical culture, and shows how the clavichord ideal crucially influenced the development of the Viennese fortepiano. Later, it seems, influence went in the other direction. Some very late clavichords belong to the sound-world of the fortepiano, by then well established, and approach it in tone and touch.

Frances Bedford’s account of 20th-century clavichord music was given by way of a 'taster' for her forthcoming book, now published[14]. She shows how compositions for the instrument have reflected in miniature the changing fashions of modern music, from pastoralism to serialism to music theatre (not much minimalism - yet) like gales blowing through the window of a tiny room. But there are so many pieces listed - almost all unknown - that the reader's attention is taxed. A frank catalogue, with dated and publishers, might have been a useful supplement to this paper, and music examples might have helped to bring it alive: but perhaps copyright proved too big an obstacle for this.

Christopher Hogwood, in a paper notable for its wealth of quotes from primary sources, is concerned with attitudes towards the clavichord in the 18th, 19th and 20th centuries. Part of his purpose is to define a repertoire for the instrument. Which keyboard music ought to be considered 'first preference clavichord' material, in Ralph Kirkpatrick's phrase? For this Hogwood suggests a number of criteria. He goes on to protest at the marginalisation of the instrument in present-day musical culture. It has become, he says, 'a lap-dog in the early music menagerie - entertaining, charming, portable, but miniaturised.'

One reason for this sad state of affairs, it seems, is the course taken by clavichord makers following the revival of the instrument in the 1890's. John Barnes has described[16] how Dolmetsch, beginning by making copies of a large 18th-century clavichord, changed course in the 1920's, creating a small unfretted instrument of a type which was, in principle, unknown historically.

There is no doubt that these instruments were highly valued at the time: one may even speak of their popularity, at any rate in England, among the small elite who took an interest in the clavichord. The type was produced by many other makers such as Lambert and Goff, and compositions, such as Howells' two well-known sets of pieces[16], were written for them. Joan Benson, in her short but thoughtful paper on 20th-century clavichord technique, describes how their yielding touch encouraged a mannered style of performance in which almost every note was subjected to *Bebung*.


15. FoMRHI Comm 961.

From 1920 until about 1965 'a clavichord', to most people, meant this type of instrument. Now we have, as with the harpsichord, a reaction in favour of copying historical models: yet some contend that the Revival clavichord continues to have an influence - allegedly pernicious - over modern clavichord making, encouraging tensions which are still too slack and touch which is still too squishy.

It was inevitable, with so many newly made clavichords from different countries available at Magnano to hear, touch and compare, that differences in the makers' approach would form the focus of many of the 'out of class' discussions, and some of these are reflected in comments appearing in this volume.

Grant O'Brien, for example, in a note sent to the organisers following the Symposium, takes makers to task for emasculating their instruments by stringing too lightly, thereby curbing their volume and, even more important, their dynamic range. He admits that heavier gauges result in a harder touch and make the instrument more difficult to play because of the tendency of the keys to judder and bounce off the strings, causing the well-known (and feared) 'chucking' or 'spitting' instead of a clear note. Players must jolly well learn the technique required to control such instruments, and not complain.

Yet Derek Adlam, in a statement here printed directly after his paper, criticises instruments in which loudness is achieved at the expense of subtlety and control:

We must remember that the works of the great school of clavichord composition of the second half of the eighteenth century are extraordinarily subtle and technically demanding. Many, such as Haydn's sonatas, form an important part of our standard, modern keyboard repertoire... If these works can be performed adequately only with unreasonable difficulty, then we must look to the instruments to see if the fault lies there.

It is true that Adlam's criticisms are directed principally at too great a depth of touch, excessively firm listing, and heavily weighted keys, rather than at heavy stringing. But it is clear that there is, as yet, no consensus about what kind of touch a clavichord ought to have, and still less as to the means of achieving it.

How firmly should the strings resist the finger? How should this resistance be achieved - by thick gauge strings, by stiff listing, perhaps by an over-rail? How deep should the keys go? How heavy should they be? And what about their leverage? Can loudness and sustain both be maximised, or is there a trade-off between them? If so, what is the proper balance? Does firmness of touch, by itself, give rise to 'chucking'? If not, what other factors tend to produce it? How hard should the player have to work to avoid it?

Clavichord makers need answers to these questions, and it is the great virtue of John Koster's paper that he takes the first steps towards enabling them to be answered. He aims to cover the same ground as Grant O'Brien, the stringing and pitches of historical clavichords, but approaches his conclusions about this by a very different route.

Koster dares to consider the difficult question of how clavichord action actually works. Starting from the insight that in this instrument, uniquely among the keyboards, the strings themselves are part of the action, he identifies some of the factors which affect the touch, and shows that they are amenable to measurement and calculation. The ease of obtaining a Bebung on any particular instrument, for example, can in principle be quantified: it depends on a number of measurements of the strings and keys which can be put into a formula which will yield a definite answer.
To my mind, this is a ground-breaking achievement. With further work, it should be possible to find out how other vital aspects of the action, such as the likelihood of producing ‘chucking’, depend on the construction and setting-up of the instrument. The formulae will be complex, but in the computer age that perhaps need not daunt us. To be able to produce a design that will, with certainty, give the results in touch and tone that we want will be a very great step forward in clavichord making, not only enabling us to satisfy players, but also helping us to answer some more of the questions posed by surviving historical instruments: perhaps even to produce a (dare to say it) better clavichord for the 21st century.

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FoMRHI Comm. 1361

Gillian M Alcock

Dulcimers in Public Collections

I’m currently working on a checklist of dulcimers and related instruments in public collections throughout the world. The idea is to further knowledge about the different types of dulcimers in use in times past and now. I started this project in 1988 when travelling in USA, Britain and Europe and continued it in Germany and the Czech republic in 1993. I am a dulcimer maker, teacher and player, in case you are wondering.

For a definition, I’m using the Grove Dictionary of Musical Instruments in the entry under dulcimer, so it includes hackbretts, santurs, salterios, cimbals, cimbaloms etc. It does not encompass all box zithers though and the mountain dulcimer of the USA is not included. This comes from a different family I think, with its fretboard etc.

Without wanting ever to compare the outcomes, Boalch’s work on the harpsichord and clavichord was an inspiration for the idea.

So if you have instruments which you think qualify in your collection or in other collections, please let me know. If you are willing to contribute I could send you a datasheet to fill in, as consistent input would be valuable. Of course all contributions will be properly acknowledged.

I’d be grateful for more consideration than the Delete key on this, as you may imagine!

I will be in Europe again later this year, specifically to perform in England, Germany and Slovakia.

Thanks in advance
Gillian Alcock <Gillian.Alcock@anu.edu.au>
Making Dulcimers in Australia
That word 'scholarly' may well call up the image of a pontifical know-all, but the genuine article is different; not self-important, dispassionately applying the discipline of fact and logic to monitor the elaboration of hypotheses which too easily become articles of faith and thus inhibit the further research which might well overturn them.

In Comm. 1350 I find "As known, the earliest mention of overspun strings dates back to J. Playford's in 1664...". In a valuable recent book I find "Umsponnene Saiten...werden zum erstenmal von John Playford (Musick's Recreation on the Viol, Lyra-Way, London 1661...) erwähnt..." – "overspun strings were first mentioned by John Playford (etc)". These statements, new to me, led me to spend some weary hours (and more money than my sub. to FoMRHI) in travelling to London to scrutinize "MR o t LV" (editions of 16572, 1661, and 1682) and "Introduction to the Skill of Musick" (editions of 1655, 1660, 1662, 1664, 1666, 1670) in the original. I found nothing whatever about overspun strings in any of them. A mention of overspun strings in London in 1659 has indeed recently been found, not in Playford but in the correspondence of the polymath Samuel Hartlib. Were Playford in 1670 and Mace in 1676 really ignorant of them? possibly, but it would seem surprising. They may have taken them for granted, or a matter of no great importance in the context of what they were writing.

Comm. 1350 further states "In fact...T. Mace (1676) and J. Talbot (c. 1696) recommend all-gut strings for the lute: Lyons or dark red Pistoys." The reader will construe this as meaning that Talbot's 'recommendation' is independent of Mace's. But 'in fact' it is nothing of the kind. Talbot's note is a paraphrase of Mace; one of many such paraphrases which he made of earlier writers. His ms notes are an untidy collection of working notes of 'Work in progress', not a carefully revised final text. It is unsafe to use such a note as evidence that it applied to Talbot's own time.

While dealing with Talbot, I note his comment "...all Lutes [are] more proper for slow and grave lessons than for quick and brisk by reason of the continuance of sound when touched which may breed discord". Why? because overspun strings were used? or because all-gut strings gave the same problem?

And does Mace 'recommend all-gut strings'? I know many people assume that he does, but he doesn't say so (neither does Talbot, who probably knew about overspun strings, it would be surprising indeed if he did not); the assumption is based on a chain of argument which is (I submit) far from conclusive; let any reader who demurs at this try summarising the argument for himself.

All of us, to be sure, have to make much use of secondary sources and can be tripped up by the mistakes of others. Exact, 100% reliable work can be very tedious. But I suggest that contributors citing sources they have not seen should always state the secondary source on which they rely.

1. "The Oxford Book of Literary Anecdotes", ed. Sutherland; 1975; pp. 124-6. The last word of advice of the nonagenarian President of Magdalen to a young student, ca. 1850, was "Always verify your references, Sir".
5. " " p. 58.
Response to Comm. 1337 on Wood Science

First, I must express my appreciation for having the honour of debating with a scientist of such eminence as Professor Beament.

Prof. Beament writes that my underlying science (in the ‘Wood Absorption’ section of the Bulletin Supplement of Q 78) is ‘wrong’, and proceeds to offer his explanations for what I had discussed. In Comm. 1337, he has not addressed himself to the basic science of what I wrote, so I can’t see where I have erred.

At one point Prof. Beament wrote that ‘Methane is almost insoluble in water’, and at another point he wrote ‘Methane is ... without attraction for ... water’. The first is more accurate than the second. At 20°C, 9 cm³ of methane will dissolve in 100 ml of water (1). This is as much as 10% of its solubility in ether, the common organic solvent. Beament can call this ‘almost insoluble’, and with equal right I can call it ‘quite soluble’.

I was explaining that methane is more soluble in water than larger non-polar organic molecules because it has less effect on the entropy of bulk water, which is why those molecules are so insoluble. The fact that liquid non-polar organic materials spread on the surface of water indicates that the organic molecules have more attraction to water molecules than to one-another. As early as 1869, A. Dupré realised that the condition for spreading is that the total surface energy must be decreased by the spreading process, implying these relative attractions(2). Water molecules are more attracted to other water molecules than to non-polar organic molecules, so water forms a lens and does not spread on such materials.

It is the attraction between oil and the mainly polar molecules in wood which causes the contact angle between oil and a wood surface to be less than 90°, and this is the reason for the capillary force. That force comes from the lowering of total surface energy by the oil spreading over wood surfaces. The surfaces are channels which are the inside walls of cells connected through pores or pits to other cells in chains, and the spreading is penetration. Thus I cannot see how Prof. Beament can write ‘you create oil-lined holes which will suck in more oil and cause the wood to swell by capillary forces’. Once the holes are lined (by oil-wood attraction) and filled with oil (by oil-oil attraction), there is no additional force to suck in more oil that would swell the holes.

There must be another mechanism for the swelling of dehydrated wood by water or oil, and the swelling of a dry sponge by water, after capillary action has filled each channel. That has to do with the liquid entering and swelling the solid structures themselves, which in wood are the cell walls. These structures are wet when grown, and when initially dried, water loss contracts and stiffens them. The stiffening has to do with strain energy in the structure because the contraction is not homogeneous. So on rewetting or adding oil, the hydrophilic molecules in the structure are provided with the liquid which they can absorb and thus swell, and strain energy is relieved.

There are two mechanisms for the suction of a fully-swollen sponge that has been squeezed. The squeezing distorts the structure, collapsing channels, and suction results both from relief of the strain energy and capillary action pulling the water that had been squeezed out back in.

The above suggestion is supported by the structure of wood. It’s dry weight is roughly made up of half cellulose, a quarter hemicellulose and a quarter lignin. Cellulose molecules are long polysaccharides, hemicellulose molecules are shorter branched polysaccharides, polyuronides or copolymers involving these, and lignin molecules are phenolic polymers. Cellulose molecules pack together in a regular pattern forming microfibrils that include bound water that does not take part in any changes involving dehydration or hydration. Lignin is largely non-polar. Hemicellulose is the primary material that associates with the mobile water in each cell.
Most of the cellulose microfibrils spiral around the cell walls in several layers at different angles. These spiralling microfibrils give wood its strength. The hemicellulose mostly fills in gaps between the microfibrils, making it a continuous structure. It contracts during drying and expands during re-adsorption, thus affecting the structure of the cell walls. Most of the lignin molecules are in the regions between cell walls, apparently involved in their sticking together. In heat bending of wood, the cells slip relative to one another because of softening of the lignin.

I believe that woodwind instrument makers would argue with Prof. Beament’s last paragraph. For them, covering with a filler and varnish is not good enough to protect their instruments from dimensional changes resulting from repeated soaking in saliva and drying. Moisture does eventually get through filler and varnish, and the strains in repeated contracting and swelling promote splitting. When capillary action soaks in a non-drying oil, and the surface is wiped dry, the oil does not bleed out for years. There is no reason to do so, since it is held in by the attractive forces that sucked it in initially, keeping the wood in a continuously swollen state. I am not aware of any claims that there is any significant expansion of a drying oil as it polymerises.

This is my understanding of the science of the materials and processes we have been discussing. I would be very happy to learn from Prof. Beament where I might be going wrong. A scholarly debate is for participants to learn from one another, and I certainly want to improve my understanding of the subject.

References:

(1) Handbook of Chemistry and Physics, Chemical Rubber Publishing Co. 30th edition (1947)


Some Thoughts on Advertising

As editor, I would not tolerate any attempt to influence the content of our Q by an advertiser. I am sure that there is no such influence by advertisers in the Galpin Society Journal, and I doubt whether there is such influence in other journals like Early Music. There are some journals in the field that have a policy of promoting the early music movement in all of its commercial aspects as well as its supporting scholarship, and so some of the discussions about historical accuracy that appear in our Q's would not be welcome, but that is general editorial policy of advocacy and is not dictated by individual advertisers.

Consequently, I have no objection to advertising as long as we are not financially dependent on advertisers. With our current printing arrangements, costs of running FoMRHI are well under control and we don't need advertising. But if someone else is interested in generating advertising and collecting the fees, I wouldn't mind editing adverts into the Q's.

Personally, I would like to see more Comms about the commercial side of making historical instruments. This could include the models we make and why we make them. If the 'why' is because the musicians want them, why they want those and not others would be very interesting. Since this is not historical scholarship and is in the commercial world, deviations from historical considerations because of the desires of the musician cannot be criticised. If someone compiled a list of makers of a particular type of instrument, the models they make and prices, I would find it very valuable for people asking about what is available, and would be very happy to include it. Comms on pricing policy would be most interesting.

Back to advertising, but from the point of view of the maker. Occasionally I've had a customer who saw an advert, said 'Ah! That's what I want', and ordered it. But most of the time, new customers are generated by contact with our products in the field and recommendation by their users. Then the main purpose of adverts is to provide the details about contacting us. The advertising campaign I most admire was that of Zuckerman, who advertised his harpsichord kits in the 'personal' column of a widely available literary magazine. Then the verbal recommendation would be to write to the address in that ad that can easily be found in any issue of that magazine.

Advertising so cheaply is not easy any more. The magazines don't think that it is worth their while to sell advertising space that is small enough for small-income makers to be able to afford. But new makers need to advertise to make their names familiar, and established makers without enormous waiting lists need to advertise to remind people that they are still in business. An idea I had a while ago but never did anything about is getting a consortium of instrument makers to share a regular advert in a prestige magazine (that all libraries get) to keep costs down while maintaining some visibility in the market place. I wonder what our Fellows might think if we proposed to head the advert with 'FoMRHI makers'?

On Restricting Types of Comms:

Rawson suggests that the editor should restrict the higher intellectual-level Comms because they intimidate people who would otherwise write Nuts and Bolts Comms. Since I am a major culprit here, I am not the man for the job. There is a need for a place informally to air ideas and research on the history of musical instruments and how they were used, within months, when they are still new and fresh, and not years. Our Q is the only such place available, and if we stop offering that, I will have to start up another Quarterly to offer it. If that is what the members (and particularly the fellows) want, such a separation of the M and R in our name can be arranged. Aside from the question of possible intimidation, Nuts and Bolts Comms of relevance to historical instruments have always been fully welcomed, irrespective of quality.
On the Upsetting Power of Occam's Razor

Our membership is quite diverse in its interests, and I am sure that most members don't bother to read some Comms. that are outside their own interests. They don't complain about those unread Comms. being included in our Q's because they respect the rights of other members with different interests. The problem is therefore not a matter of disinterest.

We all are interested in research into historical musical instruments, not only workshop tips. We all accept that the interpretation of research is scholarship, and that it is important to tell the difference between good and bad scholarship. The problem is thus not a matter of irrelevance.

So what are they complaining about? By 'they' I mean Jeremy and the other members that are angry with my Comms. about the methodology and quality of scholarship. Let me suggest two possibilities of what it is.

One is that many people expect progress to be a rolling consensus with the respectability of our field marred by controversies. Truth should be obvious and acceptable. Arguments are ugly, and people shy away from groups where there is disunity. This clearly shows itself in politics when parties that have obvious internal strife lose votes. People are attracted to certainty and are repelled by uncertainty. I haven't discussed this with him, but I suspect that if Jeremy was editor, he would have by now banned further Comms on the subject, feeling that it would be better if we got on with less controversial topics. Those who are out of step with the rest of us should be allowed briefly to say their piece and then shut up.

Another is that we have always determined whether scholarship is good or bad by intuitive judgment in the light of our experience (taking the evidence into account, of course), and have never had reason to question that judgment (and are really quite proud of it). And now I come along and apparently am saying that this is not good enough for good scholarship. This seems to be an attack on ones intellectual credibility, and that is enough to get anyone angry. I am apparently trying to set up my own thought police, and am trying to deny people the freedom to interpret research their own way - a freedom that is cherished, and will not be given up to anyone who thinks he knows better.

I have respect and some sympathy for both of these views. As for the first, I don't think that I can say more that can further clarify my position, and so after this Comm. I will not respond to more criticism unless it raises new issues. As for the second, I very much doubt whether anyone would disagree with how I define 'good' scholarship. What is upsetting to some is that I seem to say that what is done in the same way that they do things is not 'good' scholarship. This is rarely true, and I'll make a last attempt to dispel the misunderstanding.

Our intuitive judgment is the basis of all our decision making. It is very rarely wrong and deserves our trust and pride. It got that way by learning from the evidence of our experience. In that learning, we treated the evidence with objectivity and fairness, and adjusted the judgment accordingly. As we increased our intuitive understanding of what is going on, we found less need to adjust our judgment, and have become more reluctant to do so. My discussions of scholarly method are addressed to the rare circumstances when strong confidence in intuitive judgment is in direct conflict with some evidence that cannot be objectively faulted. When this occurs, most scholars will either put the problem aside for further research and consideration, or discuss the findings without making any conclusion. A minority will suspend intuitive judgments and explore the possibilities of what the evidence implies. A different minority will distort, malign or ignore evidence that is contrary to its intuitive understanding of truth, and then claim that the evidence supports that understanding. It is only this latter minority that I criticise and call 'bad' scholars. If one of that other minority comes up with a counter-intuitive explanation that fits the evidence better than any others, we don't have to believe it. New evidence could change that choice next year. But it might not, and their explanation deserves to be considered with as open a mind as can be mustered.
Once more, Scholarship and Disputes, (further to C-1335)

I assume, that every reader of FoMRHI wants to advance the knowledge on early music and profits therefore from an open and honest discussion, appraisal or criticism. Although Ephraim Segerman believes that the current dispute is largely about his insisting that standards of scholarship are maintained (C-1335), this is only partially true. Firstly, the dispute should, and does, include the question "what, or maybe even, whose standards?" and secondly, non-scientific and personal arguments do appear in the dispute as well.

Segerman supports the idea of "standard" methods in scholarly reasoning. I will defend the position that, although scientific methods exists, they are not uniquely defined, nor as simple as suggested. I will do so by giving alternative propositions with some quotes from other authoritative sources. My intention is not to start a debate on scholarly and scientific methods (indeed, that would not be the subject of FoMRHI), but only to show that not agreeing with Segerman's is not necessarily a sign of a non-scholarly approach or, even worse, dishonesty. And, perhaps, Segerman could be convinced.

The statement "between two hypotheses the one that is simplest is to be selected" (C-1276) does NOT logically follow from Occam's principle "In a single hypothesis, do not include unnecessary premises".

"The rules of scholarship should be the same for all fields, be it scientific, historical, literary, theological, or whatever" (C-1218). Natural sciences (physics, chemistry, etc) are concerned with the abstraction of general laws, which have the widest possible applicability and which can be tested by endless repetition of experiments. They can predict the outcome of an experiment. History, however, is concerned with unique events that have happened, that cannot be repeated nor experimented with. Some (many?, most?) scholars state that "the logical structure of the concepts and explanations required in human history is fundamentally different from the logical structure of concepts and explanations in the natural sciences (E. Nagel, the Structure of Science, ch.15, 1961, Routledge & Kegan Paul). Exit Occam into history?"

"The validity or quality of an hypothesis is based on its fidelity to all of the relevant evidence,..." (C-1335.). Bunge (M. Bunge, Causality, p.286, 1959, Harvard UP) writes that "a factual (scientific) hypothesis must have the quality of consistency, but consistency with the WHOLE corpus of available knowledge is NOT required" and he continues to say that "Socrates is said to have discovered that the advancement of knowledge chiefly proceeds through the clash of (internally consistent!) ideas or system of ideas".

The idea of "truth" as to why entities exit or exhibit behaviour as observed, is philosophical. Newton's law "Force equals the product of mass and acceleration (F=m.a)" predicts that a body with a constant force applied to it, will exhibit a constant acceleration in a straight line. Equally well, we will conclude that a body, which is not in a straight-line constant-acceleration type of movement, must have a force applied to it. Thus "F=m.a" becomes the definition of "force". We don't know WHY "F=m.a" and therefore, although "F=m.a" very adequately describes and predicts the outcome of experiments, it does not explain. Then, what is the truth of "F=m.a"? Some (many?, most?) scientists do not care about the truth value of "F=m.a" (objective or not), but all accept it as an adequate description (which is, of course, the ultimate consequence of Occam's razor: remove unnecessary premises). How this applies to the science of history, I have no idea.

Science and the rules used in scientific reasoning have many more aspects than suggested by Segerman, and accusations of dishonesty, unscientific methods and personal feelings have no
place in scientific discussions. The base should and must be the strength of arguments, no more, no less. But the strength of an argument is not easily determined and has to be judged.

There are also quite a few personal statements, often as asides, in several comms from Segerman, which are given as opinion without substantiation. Examples:

"... like all the other directors of period instrument ensembles, he (Goodman) is not interested in ... historically accurate equipment." (C-1147). Where is the proof for the statement concerning ALL directors?

"What they (restorers) don't think about is that after they ... are dead, bassoons should live on ..." (Bull suppl 72). Again, where is the proof for a generalised statement on restorer's motivations?

Did Brueggen become a conductor because of the decline of his recorder career, which was due to the incorporation of historical practices into his playing (C-1194)? I would be interested to hear of any substantiation for such conclusion.

Such remarks are presented without evidence and can easily be interpreted as a biased opinion which does not add to the credibility of the other statements. Or in Occam's terms, they are unnecessary premises, which should be cut away.

To paraphrase Segerman (C-1315), I don't recognise a subjective view of music history in myself. But I do observe two types of musicians interested in music history. One is primarily interested in using the most faithful copy of the 'period' equipment because (s)he believes that no other approach to an authentic performance is possible. The other is more concerned with the sound quality, the feeling, the response and the touch of the musical instrument and the intentions and objectives of the instrument maker and how that would fit with the composer's intentions (and maybe some more considerations) and accept the available evidence or indications (see last paragraphs of C-1227) that there is more in a copy than just a copy.

I agree with Segerman that words like 'authentic', 'period', etc are used sloppily in many brochure-like texts. But so is the text of many technical articles in daily journals. Dishonest? I believe that they are in a situation as explained under "1)" of C-1335, but come to a different conclusion on what and how to present. And popular scientific journals (Scientific American, Science) come to yet another conclusion on how to present scholarly results for a non-scholarly readership. Moreover, even between experts, the opinions vary (see for example Elste, Reflections on the 'authenticity' of musical instruments', in CMCIM publication No.3: 1994, available from the MICAT mail server, and the Editorial of the most recent Early Music).

Just for the sake of clarity, I have no position re any previous controversy and it is not my intention to undermine anybody's (including mine) credibility, why should I?
Copyright, the status of FoMRHI and accuracy.

The Crookes articles has brought a specific point to my attention. It is one of the first comms which carries an explicit copyright statement, and there are a few more. I seem to remember that Jeremy has mentioned some time ago that in order to reprint old comms, the consent of the authors was required which was not always that easy (not a member any more etc.). So, what is the copyright status of comms in FoMRHIQ? Am I allowed to make a copy of something and send it to someone? If FoMRHI does not have a right to copy, how can it issue the Q in the first place? Questions I have never given any thoughts to, but what about it? Should copyright not automatically be given to FoMRHI, who ever FoMRHI is legally? A question to the Fellows?

And indeed, what about the Fellows? I knew they existed, but did not know they were supposed to be the leading writers. Having the Q-index on the computer, I could check how often the Fellows (there are 20 of them) indeed have contributed over the last 5 years (comms, no reviews, starting at 1989):

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Compare this list with the top-scoring writers (over the same period), who are:

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and it seems that maybe there should be a little movement in the Fellows. Maybe something like the football league: the last, say, 3 Fellows will be replaced by the top 3 authors of the year, who are not yet Fellows, excluding of course the Fellows who are a Fellow *qualitate qua* (Jeremy, Eph and Barbara, who surprisingly is not a Fellow, but this may be because apparently "female fellows" are a rarity in this elect company. Because the Fellows have nothing to do anyhow, except when Jeremy calls them, it can not do much harm:-). But, now seriously, coming back to the copyright and legal status of FoMRHI, maybe this ought to be sorted out (if it is not) and FoMRHI should be at least registered, or something like that. I know that a careful financial management is carried out, but I hope that some people are not personally liable for any problems FoMRHI might come to. That would certainly not be appropriate. Maybe something for the Fellows, like Lloyds?:-(

I would like to add a comment to C-1325. With the introduction of the electronic calculator, it has become very easy to present results of calculations in large number of digits as presented by the calculator. Calipers can be bought with a digital readout in micron i.e. 1/1000 of a millimeter. Measuring the thickness of a piece of wood could give a reading such as 25.021 mm, which is, however, absolute meaningless: no piece of wood would even remain stable within 0.001 mm. Dimensions in measurements, drawings or calculations, should be given in as many digits as is meaningful, no more. If no explicit accuracy is given, for example such as +0.00/-0.05 mm, it has been good practice that the least significant digit gives the expected precision and thus a dimension given as 1 mm is different from a dimension given as 1.000 mm.
The first means 1 mm +/-0.5 mm, while the second is to be interpreted as 1 mm +/-0.0005 mm. C-1325 violates these principles: a chord length of 10.95445115 mm is meaningless, when working with dividers on a piece of wood. A more acceptable presentation would have been 11.0 mm (the next rounded value to within +/-0.1 mm). It than also appears that the two dividers settings are actually equal (11.0 mm). It is important that in presenting numeric results, measurements or calculations, the author gives some thoughts on the meaningful precision of the presented numbers, and either explicitly state the precision, or imply the precision in the manner indicated above. Some smaller remarks: the result of a calculation cannot be more accurate than the least accurate component of a calculation (because in the last formula .008727 (+pi/360) is only given in 4 significant digits, the result can never be more accurate than 4 significant digits) and the accepted abbreviation for millimeter is "mm", not "m/m".

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Many readers will agree that a couple of words that have appeared rather too often in recent issues of this Quarterly, are Occam's and razor. The suggestion that these words be forbidden for a year or two might prove popular. But before they do disappear could we farewell them by noting what they mean, and where they came from? Not far from Haslemere, where Arnold Dolmetsch set up his workshops, lies the small settlement of Ockham. From that place of birth one of the greatest Franciscan scholars came to be known as William of Ockham. Exactly when he was born is uncertain but most authorities agree it was some time between 1280 and 1300. He studied at Oxford and later at Paris. He was a radical thinker whose ideas were never popular with the Church, and he clashed more than once with Pope John XXII. Indeed he was summoned to appear before the Pope in Avignon in 1324. Four years later Ockham was excommunicated and fled to the more liberal Court of Emperor Louis in Munich, where in 1349 he died and was buried. His most famous principle which is known today as Occam's razor, he expressed thus — essentia non sunt multiplicanda praeter necessitatem — which translates as entities are not to be multiplied beyond necessity. Roughly translated it can mean hypotheses are not to be multiplied without necessity and even more roughly, don't jump to conclusions without concrete evidence, otherwise you're going to waste a lot of people's time in pointless arguments. And he wasn't even a FoMRHI member! Nevertheless, in spite of the over use of these words they are very relevant in the world of making and restoring musical instruments from times long ago. Too often, when we work from incomplete information, it is easy to create musical instruments built on very slender evidence using unproven hypotheses. In case readers need a concrete example of what happens when Occam's razor is ignored, may I give one, not from the world of music, but relevant all the same in this year, 1995, when we remember World War 2. British Intelligence Services (BIS) knew that the nazis were working on a pilotless bomb - the V1. Adjacent to the launching ramps were to be constructed large buildings comprising two rooms, each having no connection whatsoever to the other. To move between rooms required going outside, around a protruding wall, and then into the other room. It was assumed by (BIS) that these rooms were to store the V1 fuel, which comprised two components that, for important safety reasons, must be kept well separated. They further assumed that components readily available to the nazis at that time could well be hydrogen peroxide and sodium permanganate. In turn this led to agents being asked to locate...
the production centres of these, which in turn would lead to bombing raids to destroy them. It was also possible for scientists to roughly calculate the size of a missile propelled by hydrogen peroxide and sodium permanganate and from that could be approximately calculated the weight of the explosive charge, from which in turn could be calculated the amount of damage and possible casualties. Quite a complete picture was obtained from the initial evidence of the two-roomed construction. All, alas, to little avail. The V1 was in fact driven by an Argus Motor, fuelled by a crude kerosene. It was the launching mechanism, the Walter catapult, which was driven using hydrogen peroxide and sodium permanganate, and, like the "atmospheric railways" used on aircraft carriers to hurl the planes skywards, these catapults hurled the V1 missiles into the air at flying speed. The fuel storage building was in fact a perfectly correct starting point, but all other assumptions had been built on false hypotheses - a classic example of what can happen when we ignore you know what! It is over fifty years since I heard the sound of the V1, its raucous noise never fading from my memory. But I hope that for a time at least, and much more quietly, those two words fade from the pages of this publication and that we can return to gentler times.
FoMRHI Comm: 1370
Alec V Loretto

Recorder Writings

I once heard the recorder defined as the easiest of all Western woodwind instruments to play badly. Matching the level at which the recorder is played must surely be writings about the instrument. When false information appears in print, there is a danger that it be taken as gospel. Failure to challenge and correct it, leads other writers to quote it, which in turn leads other writers to do the same and so on. It is not long before totally spurious information assumes a respectable air and is soon accepted as the truth. The following article by me highlights this phenomenon - it appeared first in Continuo of June 1993. I approached many of the writers who made the following statements and asked from whence their information came. I was not at all surprised to learn that most was copied from other publications without any research to verify the truth!

Happy Birthday, Whenever That Might Be! or Is There a Musicologist In the House?

Ninety years (almost) have quickly passed since an important event, which certainly started the 20th Century Recorder Revival, and which, it could be argued, triggered the entire Early Music Revival. I refer of course to the purchase by Arnold Dolmetsch of an original, very old alto recorder. He taught himself to play the instrument, which was then used in numerous concerts before being lost by his son Carl. This loss sparked Arnold Dolmetsch’s interest in making a replacement (at which he succeeded admirably) which in turn led to the production of sopranos, tenors and basses, and the appearance of recorder consorts in early Haslemere Festivals. These Festivals led to the attendance of interested parties from Germany, which in turn, led to German fingering etc etc. And somewhere along the line the lost recorder was found, and returned to an ever grateful Arnold Dolmetsch. Good stuff this - clearly documented and well worthy of a 90th birthday celebration! But readers must ponder very carefully before accepting a position on the Birthday Organising Committee. It will be necessary to rule on a multitude of contradictory statements concerning people, dates, happenings, and instruments. Consider the following few extracts from a wide variety of sources, all of which have appeared in print, in highly respected publications.

1. The date on which Arnold Dolmetsch obtained the original, old recorder
   a) In 1903 he had acquired an old treble recorder ...
   b) ... when, in 1905 ... a boxwood and ivory alto ...
   c) 7th June, 1905. ... bought a beautiful old English recorder

2. The maker of this recorder
d) He acquired a Bresson alto ...
e) The old Stenesby ...
3. The date the recorder was lost
f) ... after a concert during the 1914-18 war.
g) ... its temporary loss in 1918 ...
h) ... loss of this recorder ... in 1918
i) But on 30 April of that year (1919) an incident occurred ...
4. The place the recorder was lost
j) ... on Waterloo Station in front of platform No 5.
k) ... it was left on platform seven of Waterloo station ...
5. The year Arnold Dolmetsch started recorder making
l) 1918. Dolmetsch, at 60, ... (born Feb 24, 1858)
m) ... began recorder making in 1919.
n) ... started making recorders in the early 1920's.
6. The date Arnold Dolmetsch completed his first recorder
o) 1919 - ... builds the first new recorder
p) ... completed ... during August 1920
q) ... (after its loss) spent the following year experimenting ... on August
Bank Holiday (1920), when he finally solved ...
7. The person who identified the missing recorder
r) ... Hunt saw the missing recorder ...
s) His (Geoffrey Rendall) suspicion that this was indeed the lamented
lost recorder, having been shared by Robert Steele ...
8. Today's whereabouts of the lost recorder
t) ... preserved among us (Dolmetsch family) as a historical exhibit
u) ... traded it to Frans Bruggen ...
9. The year the two Germans visited the Haslemere Festival
v) ... visit to the first Haslemere Festival in 1925
w) ... visit to Haslemere in 1926, made with ...

It is clear that if Birthday Cards are to be printed carrying an interesting
selection of photographs, to say nothing of a chronological list of events
and dates, some very thorough sorting of fact from fiction will have to
be done. And while looking into these matters, other dates could be
announced, such as when the lost recorder was found, and when it was
returned to Arnold Dolmetsch. Having written at length on a variety of
recorder related matters, I know how easy it is for slight and not so
slight inaccuracies to creep in. But it would be comforting to be certain
that when the ninety candles have been blown out, and the sounds of
'Happy Birthday To You' are resonating in the highest corners of the
banqueting hall, it is at least being sung during the correct year and not
some year(s) too soon or too early. So hearty thanks to Arnold Dolmetsch
hearty thanks to the young Carl Dolmetsch who lost it. What happened so many years ago has since provided countless millions with a great deal of happiness. Enjoy the 90th Birthday Celebrations. But a word of caution to the Organising Committee. 'Happy Birthday to You' composed by Mildred J Hill (1859-1916) with the words added in 1935 by Patty Smith Hill, is still subject to copyright. In theory at least, permission should be obtained to perform this work at a public function. Have a nice party!

Finally, readers should know that the Editor has received full details of who made the above conflicting statements, and in what publications they appeared. There is no need for such details to appear in this publication - it might get the Birthday Celebrations off to a sour start.

But if among readers there is a musicologist who would care to look into this matter, she/he would be doing a service to the literature devoted to the recorder. If the research could start soon, then it could be published to co-incide with the 90th Birthday Celebrations. Readers should immediately reserve their places for 1993 or 1995. Or should that be 1994? Whenever!
Praetorius’s and Surviving Nuremberg Sackbut Lengths & Playing Pitches

Before the recent growing awareness of the need for conservation of evidence for future instrument historians, the only reason why one might refrain from indulging in the natural desire to try to make music on antique musical instruments has been the possibility of damaging them. Modifications to apparently healthy instruments to conform to current playing requirements have been readily made as long as ‘essential integrity’ (usually meaning appearance) has not been seriously altered.

Consequently we have every reason to expect that surviving early Nuremberg sackbuts were readily modified to the current pitch standards of ensembles they could play in during the time since they were made. Since musical usefulness in later periods has been a primary factor in the statistics of original instrument survival, and since many early sackbuts survive, we can expect that they were extensively played in later periods.

The most noticeable aspect of the appearance of these instruments is the decoration, and that would not be disturbed in the relatively simple modification of shortening lengths of plain tubes to raise the pitch. Adding length involves replacing tubes, and that requires a much higher level of craftsmanship, would be much more expensive, and the new bits would look new, devaluing the antique look. But slides do wear out, and replacement could be with a longer one. Therefore, it is much more likely that the pitches of surviving early Nuremberg sackbuts with original slides are indications of the highest pitches at which they were subsequently used, than indications of the pitches when made. Even if we could identify the cases where no alterations have been made, we do not know for whom each instrument was made, so we cannot relate the pitches of the instruments with pitch standards in particular areas.

Though these instruments do not provide reliable direct information on early pitches, their relationship between length and playing pitch should be consistent with that relationship in Praetorius’s sackbuts. As explained in Appendix A of Comm. 1327, the shape of the bell affects the relationship between length and pitch. That shape has most probably not been modified since the sackbuts were made, and so we should expect that the relationship between pitch and length in Praetorius’s sackbuts should be the same as that in the surviving sackbuts made in the same tradition.

From Table 1 and Figure 1 of Comm. 1327, we can deduce that in the size region of the Gemeine sackbut, length changes by a factor of 1.062 for each equal-tempered semitone. Then, for a length of 2,743 mm, a semitone is a length change of 170 mm. So a two-fingers floating first position of 40 or 45 mm (which is a change of length of 80 or 90 mm), corresponds with a pitch difference of 0.5 semitone. Thus, with the slide all of the way in, Praetorius’s Gemeine sackbut played at half a semitone sharper than his Cammerthon. If his Cammerthon was at a' = 430 Hz (as deduced from his Pfeifflin diagram), at a' = 440, that same pitch would be 10/25 or 0.4 semitone flatter, resulting in a pitch that is 0.1 semitone (10 cents) sharper than a' = 440 Hz. This assumes that the lowest note is called A, as Praetorius did. If we call it Bb, then the pitch is a semitone lower (a' = 415 Hz, apparently ideal for a modern trombonist playing in a modern baroque orchestra).

The Catalogue of the Edinburgh Collection (see Comm. 1211 for details and a review) lists, as No. 2695, a sackbut made by Anton Schnitzer dated 1594. The entry gives the total length of air column as c 2,583 mm with slide in and without mouthpiece. It states that with the mouthpiece that came with it (Cat. No. 2696), with the slide in, assuming that that note is an A, it plays at nearly 200 cents above a' = 440 Hz. When inserted into the instrument, Arnold Myers (curator of the collection) tells me that this mouthpiece adds 60 mm to the total length (on all of Praetorius’s sackbuts the mouthpiece adds about 70 mm).

With the total length of the Praetorius instrument measured in Comm. 1327 being 2,743 mm,
and the total length of the Edinburgh instrument being 2,643 mm, the pitch difference in semitones should be the length difference of 100 mm divided by 170, which is 0.6 semitones. The difference between the playing pitch of the Edinburgh sackbut and the calculated pitch of an equivalent Praetorius sackbut is 1.9 minus 0.1, or 1.8 semitones. The discrepancy of 1.2 semitones needs to be explained.

Possible contributions to the discrepancy include the following:

1. The pitch reported for the Edinburgh instrument may be too high. On a sackbut type of instrument, with the same length of vibrating air column, there is a range of pitches that one can play just by lip control. In the following Comm. in this Q, I explain why early sound expectations and lipping traditions are likely to have been different, and the pitch level played at was most probably lower.

2. The measured lengths on the Praetorius plates may be too short. If the scale on each plate was laid out using the same setting of the proportional compass (applied to a full scale rule) as when applied to the instruments on it, then the actual setting of the proportional compass would be irrelevant. It is possible though that the scale was generated differently from the drawings on the plates, and they were subject to different errors, resulting in a systematic error that would equally affect all measurements.

Evidence that could support this was given by Gwynn in Comm. 342. He discussed the work of Bunjes, who had pipes made for him with dimensions both from the scaled plates of organ pipes and from the Pfeifflin diagram. The former led to an average pitch of a' =445 Hz, and the latter to an average pitch of a' =430 Hz. The difference is 15/25 or 0.6 of a semitone. Gwynn pointed out that the scale on the plate appeared to be 1 to 12. This can suggest that the scale was laid out directly from a ruler, and a proportional compass was used only for the instruments. Thus an error in setting up the proportional compass, or a systematic error in how it was used, could possibly explain why the organ pipe diagrams were 0.6 of a semitone shorter than needed for consistency with the Pfeifflin diagram, and could similarly explain some of why the sackbut diagrams were shorter than they should have been.

The trouble with this evidence is that it is unlikely that meaningful pitch information can be extracted from Praetorius's plates of organ pipes. These are the only plates in the book where each foot on the scale is only subdivided into two, all of the others being subdivided into four and twelve. This could well indicate that the intended accuracy was lower, with the objects shown depicted in a more schematic way. This interpretation of Praetorius's intention is supported by the pipe mouths being shown at an angle in a kind of perspective to show the structure around them, and by the ends of open pipes being shown in perspective and covered pipes not. This is also supported by the lengths of the Principal pipes, with the 8ft one being 8 ft long and the 2ft one being 2 ft long. But the 4ft one is 5% shorter than 4ft (if it was a full 4ft long it would bump into another pipe), and the 3ft one over 10% shorter than 3ft (for no apparent reason). The scale was obviously just to provide an order of magnitude.

In the early 17th century, it seems that an English 10ft organ pipe was made 10 English feet long, and (as mentioned in a following Comm.) Mersenne wrote that an 8ft pipe would accurately be made 8 (French) feet long before tuning. If we similarly calculate the pitch of Praetorius's 8ft pipe from the length of his Brunswick foot, we get about 67 Hz, implying a' =445 Hz, which is inconsistent with his Pfeifflin diagram pitch. Praetorius was probably intentionally being ambiguous about whether the scale on the plates of organ pipes referred to the pipe-makers foot or his own foot. The most probable explanation is that organ builders in England and France only encountered one length standard, and that was what was used on their measuring equipment. German organ builders encountered different length standards in each principality they worked in, and the foot they used was probably at the standard of the principality in which their measuring equipment was made.

3. The pitch standard derived from the Pfeifflin diagram may be higher than the reported a' =430 ±5 Hz. (or ±0.2 of a semitone). This possibility has been considered necessary by
various researchers wanting to support simple models of pitch history based on the measured pitches of surviving wind instruments. That apparently was the intent of the study by Bunjes, and why the conclusion he presented gave the pitch derived from the pipes in the scaled plates rather than the pitch derived from the Pfeifflin diagram. As Gwynn pointed out, this was clearly an error of scholarly judgment. If Praetorius felt that the plates were accurate enough for specifying pitch, there would have been no need for him to add the Pfeifflin diagram at the end of the book, which he stated was for this purpose. Bunjes and Gwynn would have preferred Praetorius’s Pfeifflin pitch to be higher. There is ambiguity in the Pfeifflin evidence, but none of the possibilities supports such an interpretation.

In summary, there is an inconsistency of 1.2 semitones in the relationship between pitch and length when comparing the lengths of Praetorius’s drawn Nuremberg sackbut, and pitch derived from his Pfeifflin drawing, with modern playing of the surviving Nuremberg sackbut in Edinburgh, and probably similar surviving sackbuts elsewhere. A systematic error in the drawings in the plates, as Bunjes’s report implies happened with the plate of organ pipes, is possibly involved. A difference in how high one lips the note is probably the major contributing factor, with modern criteria differing from early ones. If there has been an error in interpreting Praetorius’s pitch standard from the Pfeifflin diagram, it has not yet been suggested.

It is understandable for those who believe that modern early music performance style is at least similar to that which pertained in the early baroque to easily accept the pitch evidence of modern playing of early instruments and thus consider that the Pfeifflin evidence must be wrong. But in historical or any other kind of scholarship, it is necessary to give a reasonable explanation for how evidence that is apparently contradictory to one’s favoured theory can fit into it. In the following Comm. 1 present such an explanation for what could be wrong with the playing evidence. A similarly objective and reasonable explanation for what is wrong with the pitch-pipe evidence will be necessary for the modern playing pitch theory to qualify for consideration for the choice of scholarship to add to our historical knowledge.

FoMRHI Comm. 1372  
Ephraim Segerman

On Measuring the Pitch of Early Wind Instruments

What a player does with his or her mouth and/or breath can vary the pitches of notes on all wind instruments. There is always a range of possible pitches for each note on each instrument. A major criterion in measuring the pitch of an instrument with fingering holes is that an in-tune scale can be blown with a smooth variation in wind pressure and loudness as the notes of the scale are fingered. With instruments without fingering holes, it is the various modes of air vibration approximating harmonics that are blown. This smooth variation is required of an in-tune instrument today, and it is assumed that the same requirement was made historically.

As a result of this requirement, some of the pitch range possible on each note is considered not usable because it cannot take part in playing in-tune scales. There usually remains a range of pitch levels at which such scales can be played. Occasionally there is only one pitch level at which this can be done, and often there is none. Instruments in the last category, according to the above assumption, are judged to be defective and unplayable.

When, as is usual for instruments judged to be playable, there is a range of pitch levels at which scales can be played, a second assumption associated with quality in modern instruments comes into play. That assumption is that the pitch at which the sound is most focussed is the proper playing pitch. Focus in wind instruments increases as the number of higher modes (approximating harmonics) in the tube resonance are excited by the sound-generator part of the instrument (operated by the mouth). The more energy delivered to the tube by the generator, the more higher modes of the tube are excited. Since higher energy generated by the mouth usually sharpens the generator resonance, and the final pitch is a
compromise between the generator and tube resonances, the chosen pitch level for the instrument is usually at the top of the range in which scales can be played. The range and the preferred top of it is sometimes likened to a 'keyhole'.

The latter assumption follows from modern instrument use and tuition having the objective of public performance to an audience of hundreds. The greater projection of a more focussed sound is considered necessary for audibility, and the particularly quiet notes that the instrument is capable of making are considered to be unusable because of inaudibility and poor tone quality.

The first assumption follows from modern aesthetics of intonation and phrasing. Phrasing usually involves long sweeping shallow waves of loudness covering many notes, with minimal space (time) between them. Individual notes involve very little variation in loudness or pitch (shaping) beside that produced in vibrato. So getting a note safely 'spot on' in tune quickly is of the highest priority.

This is most easily accomplished by the sound generator having considerably broader resonance characteristics (ie. less variation with variation in frequency) than those of the tube, so that tube resonances dominate pitch control. It is easier to quickly get around the notes in tune with the fingers on the tube than with mouth pitch control. The mouth can affect pitch, but over a small range, and one has to really want to do it for it to happen. Modern wind instruments are essentially 'factory tuned'.

Both of these assumptions are challenged by the evidence comparing baroque bassoon reeds with modern ones given in Comms 1223 and 1282. Modern reeds are as expected from the above, while baroque reeds were much stiffer, having narrower resonance characteristics than the tube, making the reed dominant in pitch control. This implies that baroque players were willing to sacrifice easily playing in tune in favour of greater flexibility of pitch, tone colours and dynamics controlled by the mouth. It also probably implies that it was more acceptable then than now to start a note out of tune and to slide to the in-tune pitch. In my youth this was acceptable in opera singing even when the in-tune pitch was never reached. If that was also the case in the baroque, the actual pitch level played at could have been outside the range determined nowadays by playing smooth even scales. Then some instruments that we now judge as unplayable could have been so then.

In conclusion, the way that the pitch level of an early wind instrument is measured is not an objective determination of the pitch at which it was originally played at, as has generally been thought. The measurement method assumes modern criteria for instrument quality and note production associated with modern intonation aesthetics and phrasing, and evidence exists which indicates that criteria then were different, associated with different aesthetics of intonation and phrasing. The dynamic range used originally by players apparently was greater on the soft side than in modern playing of the same types of instruments. This strongly implies that reported pitches of instruments are sharp with respect to the pitch levels that the same instruments would have been blown at originally.

Appendix: Background to the controversy on Praetorius's pitch

There has been much speculation, mostly in wind-instrument circles, about Praetorius's Cammerthon pitch standard (often extrapolated to a general Renaissance standard) being up to a semitone higher than modern. The earliest statement of it that I have noticed is in the classic Woodwind Instruments and their History (1957) by Baines, who wrote 'Recorders at Verona, identical in shape and size with Praetorius's scale drawings at 'chamber pitch' sound a good semitone above modern pitch; say about a'=470'. The latest is Mitchell's article in this year's GSJ. On the other hand, Praetorius intentionally communicated his pitch by giving the dimensions of a set of pitch pipes (Pfeifflin) in a diagram, which has been analysed by Ellis (1885), Bunjes (1966) and Thomas & Rhodes (1971), with Gwynn (Comm. 342) comparing the previous analyses. The conclusion from the pitch-pipe evidence is that a'=430 ± 5 Hz.
Mersenne's Sackbut and Pitch, Plus Playing with Shawms

Mersenne (1636) wrote that the sackbut was used as the bass for all kinds of ensembles, so it's pitch is relevant to general pitch standards in France. It had an extension coil that lowered it by a fourth to play the bass with shawms. He said that without the extension coil and with the slide all the way in, its total length was 8 feet 1 inch, apparently including the mouthpiece. That is 265 cm, which is about 3% or half a semitone shorter than the length of Praetorius's Gemeine sackbut as reported in Comm. 1327. From Mersenne's description of playing the slide positions, it is reasonably clear that there was no floating first position. So, from the length of the instrument, we can deduce that Mersenne's sackbut with the slide in would play at B♭ in Praetorius's Cammerthon, and consequently at C in Praetorius's preferred Chorthon (used 'at Prague and at a number of Catholic chapels elsewhere'), a tone lower. In my model of pitch standard history, the latter is the same as the major French standard at that time (as discussed in the following Comm.), so we would expect that the fundamental note of the harmonic series of Mersenne's sackbut with the slide in would be called C. Mersenne indeed did so, tying the evidence of the difference in lengths with the evidence of the difference in pitch standards. Sackbuts in C could have been the most common in areas using this pitch standard, which could have survived in bands when orchestral standards rose there in the 18th century. This could relate to the evidence for C trombones being used in the 19th century.

Appendix: Sackbuts Playing with Shawms

In Prop. XXXIII of the Fifth Book, Mersenne published two strains of a Pavanne in 6 Parts by Henry le Jeune stated to be transposed for shawms (Haute-bois). The 'Seconde Taille ou Basse Taille' (5th) part is labelled pour la Sacquebute, has the same transposition as the others, and has a range of c to a. This range is readily playable on a sackbut without the coil extension, which raises the question of what the purpose of the extension might be in the context of playing with shawms. The other parts are played by three sizes of shawms sounding, with all holes closed, d', g and G, the biggest shawm having 4 keys. The lowest notes of the parts for the two larger shawms are f and F, apparently lipped a tone lower.

When the sackbut played with the shawms and had the extension coil in, one possibility is that the player read the notes as if the coil wasn't there, which would mean that the piece had been transposed down a fourth from Mersenne's usual pitch. For the lowest shawm to remain the true bass, it had also had to be down a fourth, but the other shawms could have had the alternative of being up a fifth. But Mersenne's shawm body lengths (without reed or pirouette) of the two smaller shawms are within a few percent of those with the same lowest notes given by Praetorius (the largest one is 20% less), and it seems unlikely, even with the difference in pitch standards, that the playing pitches were so different.

An alternative possibility is that the sackbut player transposed up a fourth to compensate for the coil, and the band played at the normal French pitch. The only advantage of this arrangement (over not using the coil and not transposing) that I can think of is that the player would find it easier to play fast divisions when his turn came because he could play it all in the nearest four semitone positions of the slide, while otherwise he would need six.

Another possibility is that the sackbut player transposed up a fifth, which would mean that the band played at a tone higher than the French standard, i.e. at the same pitch as Praetorius's Cammerthon. This seems the most likely because of the shawm sizes.

Praetorius wrote 'Most schalmeis [keyless shawms] are pitched a tone higher than cornets and sackbuts'. This would make perfect sense if Mersenne wrote it, but not in this context. Praetorius's pitch tables imply the same pitch levels. Bairnes, without specific reference to Praetorius, suggested that the shawm band transposed up a tone to avoid an unsatisfactory F' on the treble. Mersenne's treble had an extra hole for this note, apparently played with the right thumb as on a flageolet. Any other ideas?
Mersenne’s Pitch Standard

At the beginning of his Third Book, Prop. XVIII, Mersenne wrote: ‘this proposition is one of the most beautiful in music’. It’s beauty was that anyone anywhere would know the intended pitch of a piece of music if one specified the frequency of the first note, and would know the intended tempo if one specified the length of a string with a weight at the bottom (a pendulum) that swings and returns in the time of a measure.

In the previous proposition (XVII) Mersenne gave a table of frequencies (the number of vibrations per second) for all of the notes in eight octaves. In one of the octaves, the frequency for A is 480 vibrations per second (Hz). This table was calculated from the statement he made earlier in that Proposition that ‘the string which is in unison with a four foot open organ pipe makes 48 vibrations in the space of a second’. In the table, and in other versions of this statement, that note was called F.

An immediate problem with this statement is that the frequency (calculated by Ellis (1885)) for an open organ pipe that is four Parisian feet long is 113 Hz, not 48 Hz, leading to a ‘standard’ of a'=563 Hz. It is easy to be out by an octave when comparing notes of different timbres, but this still leaves his frequency between 2 and 3 semitones too low. We will discuss how he measured frequency and how he could have erred in the Appendix.

In Prop. XLIV of the Sixth Book (of the Organ), Mersenne mentioned that the ordinary organs of churches were built with 8 ft. pitch. When he used the term ‘ton de Chapelle’ as equivalent to 8 ft. pitch in Prop. VIII, it is most likely that he was saying the same thing. In Prop. XV he presented a diagram of pipe lengths and widths for all pitches for 4 octaves of pipes, saying that organ makers couldn’t go wrong if they followed this diagram accurately. Four octaves was the total compass of the keyboard. The longest pipe, called C, was one foot long. To use the diagram, one should multiply the given lengths by integers to give the lengths of pipes in the different ranks. The integers mentioned were 2, 3 and 4, with 16, 24, and 32 being possible as well.

The implication above is that the pitch of the 8-ft. open pipe was called C. In Prop. XXIII on keyboards, Mersenne wrote that keyboards could start with any note name, but starting on C ‘conforms more to the practice of today’. The conclusion of a'=563 Hz, above was on the assumption that the 4-ft. pipe was called F. Now that we know that such a pipe would usually be called C, we can conclude that the usual pitch standard would be a fifth lower, or about a'=375 Hz. This is about a tone lower than Praetorius’s Cammerthon standard, as expected from other considerations.

Of the keyboards that Mersenne illustrated to display various temperaments and added keys to improve intonation, all started on C except one, which started on F. It thus seems likely that in France at that time, if one were going to assume a starting pitch for a keyboard other than C, it would most likely be F. This could mean that the transposition practices discussed in Comm. 1127 applied to France as well as England, and probably Flanders, Germany and Italy too.

This solution to the problem of Mersenne’s absolute pitches seems straightforward and conclusive, but there may be an anomaly concerning vocal ranges. When discussing the table of frequencies in the Third Book Prop.XVII, and applying it to the notes of a four-part air called Divine Amarvllis by Boesset, he wrote: ‘The lowest note or voice of the bass is on F ut fa; and because those who sing the bass in a hall do not ordinarily go lower than a four-foot open organ pipe, which is in unison with the thickest string of the spinet three feet long, it follows that the lowest note of the air mentioned above corresponds to... the number 48 [Hz]’.

If Mersenne called the 4ft. open organ pipe at about 113 Hz, F, this implies a pitch standard four and a half semitones higher than modern (French quire pitch), and the lowest note of the
bass sounded between A and B♭ at modern pitch. The highest note of the highest voice is d"; which is between f" and g" at modern pitch. Modern judgments of vocal range are historically unreliable, but comparison with Praetorius’s ranges could be relevant. In Praetorius’s Cammerthon, the total range of the piece is from B♭ to g". The highest pitch is one tone higher than Praetorius’s ‘natural’ limit for the highest voice, requiring a specialist to sing it. There is evidence that Mersenne was inconsistent about what octave he was in since in Corollary II Prop. XVIII of the Third Book, he assigned the frequency of 48 Hz to the first note of the bass of the Boesset air, rather than the lowest note, an octave lower, as quoted before. But assuming that it was sung an octave lower doesn’t help because the bass lowest note then exceeds Praetorius’s ‘natural’ limit by a tone, requiring a specialist to sing it. We must either assume that Mersenne was referring to his usual pitch standard a fifth lower (where the range presents no difficulties), or that traditions of vocal training and use then were very different in France and Germany. We should keep the latter possibility in mind, but we should remember that Mersenne was foremost a theoretician, experimenting to understand and then to explore possible applications. He was not a reporter of current practices which, to him, were arbitrary. He offered no way to practically realise the purpose of that Proposition. This was just a feasibility study using assumed data that seemed reasonable, without giving it full care.

Appendix on Frequency Measurement

In the Third Book Prop. XVIII Cor. VII, Mersenne wrote that he had stretched a spinet or lute string of 100 or 120 feet (33 or 39 metres) so that it vibrated once a second. This would not be an audible pitch, but if one vibrates one foot of that same string with the same stretching force, he realised that it would vibrate at 100 Hz, (i.e. that the frequency multiplied by the length was a constant, as happens when one fingers or frets an instrument like a viol). He did not use this experiment to get frequency numbers, probably because to get a frequency of 1 Hz, he needed such a low force that the string (most probably horizontal) sagged terribly, and he was not sure of the effect of sagging (he discussed the conditions for sagging elsewhere, without getting very far in understanding it). I calculate that to get this frequency with the thickest spinet string (0.55 mm), the tension would have been about 0.7 Kg.

The experiment to determine frequency was on a string 60 or 72 feet long stretched to vibrate 2 or 3 vibrations a second. One person counted the vibrations and another the seconds. Mersenne wrote that this could be done horizontally between two bridges, or vertically between the weight and the supporting nail. How he related this frequency to an audible higher frequency on a shorter length was not mentioned, but most likely it was done by putting the same string with the same weight on his monochord, and moving a bridge on it to be in unison with the organ pipe. Mersenne would then have calculated his number 48 by multiplying the ratio of numbers of vibrations counted to the number of seconds counted by the ratio of the long string length to the monochord string length.

His monochord, as illustrated in his First Book Prop. XII, had a nut on the end with the string then going vertically through a slot. The slot had a peg in it for tensioning the string if attached, or if not, the string could go through the slot to a hanging weight. When using the weight, there would be friction in the nut, making the tension in the horizontal string less than than the weight, making the pitch lower than if there were no friction (see Comm. 82 point 11). Then to be in unison with the organ pipe, the bridge setting would make the length shorter than it should be, leading to a calculated frequency that would be too high. But the frequency he calculated was too low, so this mechanism is not an explanation for his error. Mersenne was aware of friction in the nut of his monochord, as indicated by the correction of adding a sixteenth to the weight that he said was necessary in the Third Book Prop. VII Second Rule.

There is no reason to question the accuracy of counting vibrations or seconds, so suspicion of where the error arose focusses on the length of the long string. It could have been shorter than it should be in the horizontal case by neglecting sag (measuring the direct distance between bridges rather than along the string), or in the vertical case by neglecting the stretch (measuring the string’s length before or after it was hanging, but not during it).
UNDERCUTTING SMALL DIAMETER FINGER HOLES ON WOODWIND INSTRUMENTS: GOODACRE'S RAZOR.

'Undercutting' is one of the techniques that one can use to raise the pitch of an individual note on a woodwind instrument. It can also improve the tone of the note.

The subject was concisely covered by Geert-Jan van der Heide in Comm 505- an excellent Comm I often re-read or refer others to. However there remains the practical problem of swiftly, accurately and neatly removing a small amount of wood at the bottom of the fingerhole (A). My experience has been entirely with bagpipes- where on a smallpipe chanter the fingerhole may only be 5/32" with a bore and wall thickness each of only 3/16".

I experimented with various knives for carving and also tried files, but all were hard to use, inefficient and could easily scuff or damage the visible outside of the fingerhole.

I now use a small upward cutting knife which I have immodestly called 'Goodacre's Razor'. It is made from a rollpin; a tube made from hard spring steel. These are used in light engineering for fixing collars and pulleys onto shafts; the seam of the tube is not joined and thus it can be driven into a slightly undersize hole where it will stay put. An engineer uses it much as a woodworker uses a nail. They come in a variety of diameters and lengths- I bought 1½" long ones from an agricultural engineer for 10p each. They also feature in washing machines and similar. There are roll pins made like little Swiss rolls- these are useless for making a Goodacre's Razor.
Though a rollpin is made from spring steel it can be filed with fine needle files. To start with you need to file out a triangle (B), when viewed from the side. Then round off some of the back heel (C). You should end up with something that looks a bit like a hollow crochet hook, but which is not hooked. Now you can get a fine round needle file through the end and file the edge (D) from inside, so that the bevel is on the inside. Final honing can be done with a small rolled up screw of the finest wet and dry paper.

All that is now needed is to turn a nifty boxwood handle with the correct diam hole in it and epoxy the roll pin in place. Bingo!

You can make a series of these knives in different diameters, though I only ever use one which is 3/32" (2.5mm) and use it on holes from 1/8" to 3/8". I made it 8 years ago and it has never needed sharpening. Think of Goodacre when you use yours. Without it I could not be held responsible for the temper of myself or my chanters.

JULIAN GOODACRE.

SUPPLIERS FOR OCCAM’S RAZORS

Lately a fair amount of wind, both fair and foul, has blown through these pages concerning Occam’s Razor. I admit much of it has blown over my head- I am pretty hazy about the finer details. One thing, however, appears quite clear- any instrument maker who does not have one and wield it fiercely is going to be considered a complete dunderhead by those makers who do.

A frequent problem for instrument makers is finding a supplier. Personally I myself never shave and consequently have a moustache and beard. However I enquired at my local Boots The Chemists and got into all manner of embarrassing misunderstandings. The lady assistant behind the counter thought I was making an indirect request for 'Gentlemens Requisites'. The misunderstandings got worse when talking to the male assistant, who I fear thought I was preparing for some kind of self vasectomy! (Perish the thought- I have been an ardent anti-vivisectionist all my life.)

British Telecoms Directory Enquiries have been of no help either.

Can any one supply names and adresses of suppliers? I cannot wait to get my hands on one. I assume it will be just the very thing for scraping reeds. Though I feel far too young to be considered an 'elderly man', I long for the respect I will instantly recieve for my wisdom, knowledge and judgement.

JULIAN GOODACRE.
How to make a shelly hautbois

David Z. Crookes

And loud with quavering Sounds on shelly Hautbois
Tritons shall sing.

[D’Urfey, Ariadne II:i]

Some time ago I made a number of trumpets from shells of Strombus gigas, intending to use them in an ensemble as one-note instruments. I cut off the tip of the first one rather laboriously with a hacksaw; after that I caught myself on, supported each tip on the anvil, and nipped it off with a cold chisel. Once I had carved half-civilized mouthpieces and got the shells sounding I ran into a problem: they were all pitched around c#, and I wanted them to play d. It was going to mean drilling a tuning-hole in each one. How could that be done without spoiling the look of the shell? The obvious thing to do was drill down through one or more of the spines—the final spines, the ones nearest to the “bell”. I did the drilling in stages with a Black and Decker (small bit, medium bit, large bit, and finally a conical file-bit). It’s best to do the drilling outdoors. Hold the shell on the lawn between your knees, and dip it in a bucket of water every so often, for it gets extremely hot—hot enough to burn you if you touch it. Cool your drill bits in water as well. Above all, don’t let the drill damage the inside of the shell. Take your time and don’t press too hard. Grind the spine down with a rasp once you’ve finished drilling.

Anyway. By drilling a half-inch hole through the final spines of the shells, I managed to bring their pitch up to d, with no palpable loss of sound-quality. Now I had a number of instruments that could play c# and d. At once it occurred to me to drill more holes through more of the spines—not tuning-holes this time, but fingerholes. After two hours (which weren’t great fun for the neighbours), I was playing the start of the Orfeo overture on a “shelly hautbois” with the chromatic range of a fifth. The upper notes are less than brilliant in sound-quality—they have to be sweetened with the embouchure. I’m inclined to think now that the optimum range of a conch-cornett is approximately that of a major third. Tritons, maybe: but tritones, no.

On an unpierced shell you can lower the pitch by a semitone or a tone by respectively half-stopping or stopping the “bell” in the French horn manner.

Here’s what I’ve been wondering. Is the pierced shell a new invention, or has someone been there long ago?
Upon the harp with a solemn sound

David Z. Crookes

In the AV verse 16 of Psalm 9 (= verse 17 of the Hebrew) reads as follows:

"The Lord is known by the judgment which he executeth: the wicked is snared in the work of his own hands. Higgaion. Selah."

The title of Psalm 7 (= verse 1 of the Hebrew) reads in the AV as follows:

"Shiggaion of David, which he sang unto the Lord, concerning the words of Cush the Benjamite."

The eight Hebrew words of Psalm 9.17 which precede the two words transliterated as Higgaion and Selah add up in 400 alphabet gematria to 2284, and so do the six Hebrew words of Psalm 7.1. That fact makes you wonder whether the two rhyming words Higgaion and Shiggaion, with their Greek-sounding endings, are related in some way. The word Higgaion (= He-Gimel-Yodh-Wau-Nun) appears in the Psalter three times: in Psalm 9.17 (AV = Higgaion), in Psalm 19.15 (AV = meditation [of my heart]), and in Psalm 92.4, which as verse 3 in the AV reads as follows:

"Upon an instrument of ten strings, and upon the psaltery; upon the harp with a solemn sound."

How should you construe the word Higgaion, which in both Psalms 9 and 92 is clearly a technical musical term? You recall that the word is derived from the root He-Gimel-He (= meditate, speak, moan), and at once a harmonic possibility suggests itself. If you express the word He-Gimel-He in the musical alphabet you get a major-third version of This old mart

An a, an f, and another a. That's the musical translation of He-Gimel-He. So does the derivative word Higgaion mean accompany the tune at the third below? If it does, and if the tune doesn't exceed the range of an octave, then the solemn sound of Psalm 92 is going to need an instrument of ten strings. You look at the Hebrew text of Psalm 92.4. Literally rendered, it runs as follows:

"With ten and with psaltery; with Higgaion on harp."

It seems to you that only two instruments are involved: a ten-string psaltery (which could play octave-range melodies in thirds), and a harp able to play Higgaion. Does the parallelism of the verse involve psaltery and harp of parallel capacities? If so, then the musical term Higgaion is most likely to denote what the notated root-word He-Gimel-He suggests—an accompaniment of each tune-note at the lower third.

And now you wonder whether the last two words of Psalm 9.17 ("Higgaion. Selah." in verse 16 of the AV) mean, repeat with an accompaniment at the lower third, or repeat in thirds. The word Selah occurs 71 times in the Psalter, and its three Hebrew letters Samekh-Lamedh-He have been taken to stand acrostically for the
direction = da capo (see J. Taylor, ‘Selah’, in Hastings’ Dictionary of the Bible (Edinburgh, 1904). Following that acrostic construction, you’re inclined to see the word Selah as a musical repeat-mark, applying to either part or all of a particular psalm-verse. You’re not completely certain about it, because you haven’t yet met any such acrostics in the Psalter, but for the moment you’re happy to understand “Higgaion. Selah.” as, “In thirds: Repeat.”

So much for Higgaion. What is Shiggaion? Is it Higgaion plus something, or Higgaion on top of something? You read the first verse of Psalm 7 (AV, title):

“Shiggaion of David, which he sang unto the Lord, concerning the words of Cush the Benjamite.”

The word rendered concerning is Ayin-Lamedh. Ayin-Lamedh can be translated upon, as for example in Psalm 45.1 (AV, title: “...upon Shoshannim…”). You have already construed Shoshannim as a recurring bass-line set below a tune played in parallel sixths (see comm. 1342). Is the bass-line of Psalm 7’s Shiggaion set below a tune played in parallel thirds (= Higgaion), and does the bass-line somehow consist of “the words of Cush”? If so, what are “the words of Cush”? You don’t know. Verses 4 and 5 are rendered in the AV’s verses 3 and 4 as follows:

3. O Lord my God, if I have done this; if there be iniquity in my hands; 4. If I have rewarded evil unto him that was at peace with me; (yea, I have delivered him that without cause is my enemy:)

David cites the false charges of Cush, but he speaks in the first person, so you think it wrong to look in these verses for the actual “words of Cush”. You wonder for a moment whether the bass-line consists of the titular Hebrew words represented in English by Cush the Benjamite, and then suddenly your eye is caught by a group of three words in the Hebrew text of the psalm’s final verse, verse 17. Cush in Hebrew is קְשֵׁב, Kaph-Wau-Shin, and the third, fourth and fifth words of verse 17 read from right to left as follows:

Mem-Shin He-Resh-Mem-Zayin-Aleph-Wau Wau-Qoph-Daleth-Tzadhe-Kaph

Three words, whose initial letters are respectively Kaph, Wau and Shin. Are these “the words of Cush”? They transcribe musically as follows (the fourth character of the second word functions as a double letter):

That strikes you as a perfectly credible bass-line, but at 26 beats it’s much longer than the 14-beat Shoshannim of Psalm 45, and when you try to identify the overlying melody you run into problems. First, no verse or group of verses transcribes as a plausible melody in its own right. Secondly, however you vary the Cush-acrostic’s pitch and time values, no verse or group of verses overlies it euphonically. What then? If Cush is concealed in three initials, may the melody be concealed in a large number of initials? And if so, where does it start and finish?

As you ask that question you notice something. The second word of Psalm 7 is the authorial word דָּיוֹד־לֶדֶת, Lamedh-Daleth-Wau-Daleth (= of David), which in 400 alphabet gematria has a value of 44; and after the first 44 words of Psalm 7 comes the word Wau-Yodh-Shin-Gimel. The psalm’s very last word is Ayin-Lamedh-Yodh-Wau-Nun. And in Hebrew the word Shiggaion is spelt Shin-Gimel-Yodh-Wau-Nun. Is the melody of David’s Shiggaion constituted by the initials of all the words including and between Wau-Yodh-Shin-Gimel and Ayin-Lamedh-Yodh-Wau-Nun?
You transcribe these initials (there are 98 of them), and write out below the emergent melody an accompanying part at the lower third (= Higgaion). Then you try to set the Cush-acrostic under the two melodic parts as a recurring bass-line. But however you vary its pitch and time values, it won’t work euphonically. And now you wonder about the matter of recurrence. Is a Shiggaion a ground in the same class as Shoshannim, or is it something more like a Browning? You remember that the word Shiggaion is derived from the root Shin-Gimel-He (= to wander), and you ponder. Your initial-based melody is 182 beats long, and the Cush-acrostic is 26 beats long. 182 = 7.26. What happens if you treat the Cush-acrostic as a vagant bass, with variable time-values like those of Shoshannim, and play it seven times, once on each degree of the scale? After an hour in your contrapuntal laboratory, you arrive at the transcription set out below. It employs a thirteen-string ‘bass’ harp and a ten-string ‘treble’, on the putation that David sang the tune in the 864-432 octave but played the two Higgaion parts in the 432-216 octave. Is Psalm 7 the only Shiggaion in the Psalter? You can’t believe it. You even wonder whether the Hebrew word Shiggaion (= a vagant chaconne) survives in the Spanish word chacona, for Spain had a substantial Jewish population until 1492. The idea strikes you as attractive. Then you remember the famous musicologist who suggested that Cabezón and Cavazzoni were the same person, and you decide not to overstate a mere possibility.
On Some Early 17th Century Discussions of Gracing

Robinson and Dowland

Robinson (1603, p. Cr) mentioned gracing notes either by passionate play (involving alternation between playing loudly and softly) and relishing. On the next page (Cv) he wrote 'a strong relysh for loudness, or a mild relysh for passionate attention'. He then mentioned (p. Cijr) a 'fall with a relish, or a fall without a relish'. When describing the former, after details on how to play the fall, he wrote 'and the relish continued (with the little finger)'.

It appears that 'relish' had two meanings, the choice made obvious by the context. One was any grace in which pitch (rather than loudness) varied, and the second was a category of the first that did not include the fall. That category must have included shakes for two reasons: One is that Robinson did not use the term 'shake' and it is much more likely that he referred to it under a different name than that he did not use it. The other is that shaking seems to be called for when he wrote (p. Cr) 'a relish will help both to grace it [a note] and also it helps to continue the sound of the note his full time'.

The above quote is followed by 'but in a quicke time a little touch or jerke will serve.' A touch involves one finger movement and so probably means a quick appoggiatura. A jerk, like a twitch, usually involves a movement and return, and so it probably means a quick mordent. These obviously were graces, but Robinson did not name them.

The only information on grace execution given by Robinson was that the fall was a lower appoggiatura and that the relish following (or the continuation of it) involved the main note plus the upper auxiliary. Only the fingering was mentioned, not how fast the fall went. A possible reason for his two meanings of 'relish' is that the lower appoggiatura was a grace newly imported from Italy, before which 'relish' meant all graces. Then the two meanings would be the old one of being all-inclusive and one that distinguishes between the old familiar graces and the new one. Only the new one needed to be described because of its unfamiliarity.

In the translation of Besard's instructions in *Varietie* (1610), Dowland mentioned 'sweet relishes and shakes' which 'cannot by speach or writing be expressed'. The repertoire of graces referred to would probably not have been significantly different from Robinson's, and it is unlikely that the meaning of 'relish' was different from Robinson's all-inclusive one. So this statement apparently distinguished between shakes, which were considered less sweet, and all of the other graces. This is consistent with what Christopher Simpson wrote in *The Division Viol* (p. 12) half a century later: 'Shaked Beats and [Shaked] Back-falls' are 'more rough and Masculine' while the 'Close-shake [i.e. vibrato] and plain [i.e. unshaked] Graces' are 'more smooth and Feminine'.

Mersenne

Most of what Mersenne (1636) wrote about gracing is included in his section on gracing on the lute (Second Book of String instruments, Proposition IX). He invented his own special characters for each grace (he wrote that most people did). Most used a comma after the note letter in the tablature to indicate a grace in general, the word for which was *tremblement*, meaning 'shake'. He described the proper shake first, as an alternation between the main note and an auxiliary a diatonic note above. The description was of the placing of the fingers, where the finger on the higher-pitched fret made the *tremblement*. He did not specify which was the starting note.

In the second grace, called *accent plaintif*, there was no *tremblement* made by the left hand. Mersenne was aware of the inconsistency of a *tremblement* (i.e. a grace) having no
tremblement (i.e. shake), but he explained that this was common usage. The description indicated that this grace was a lower appoggiatura, just like Robinson’s fall. The finger fell on the main note (from the lower auxiliary) when the sound of the string had half passed away.

The third and fourth graces as Mersenne numbered them was the martelement played with one finger on an open string and with two fingers on a stopped string. It started with the finger at the higher-pitched main note, which then shook with the lower pitched note, and ended firmly on the main note. Since it is a shake with a lower auxiliary it can be called a lower shake, and since it started on the main note, it can be called a repeated lower mordent. Mersenne’s marking was a small cross (given for the one-finger version of it, but according to its usage in his music, it applied to both).

The fifth grace was the verrecasse, where one takes the left thumb off the neck and violently swings the left hand towards and away from the bridge (with the finger continually stopping the string at the appropriate fret) after the right-hand pluck. This was a more violent vibrato than that produced by a second finger at the same fret as the stopping finger, as reported previously by Vitali (in the Capirola Lute Book, c. 1517) and later by Simpson (called ‘close shake’). Mersenne reported that in the past, players used it almost all the time, and because it was over-used, it had since been shunned, too much in his opinion. Mace also described the violent vibrato (calling it ‘sting’), and also regretted its under-use. That time of great popularity of this grace in France can be estimated by looking at Nicolas Valet’s works, where it was not mentioned in his Secretum Musarum of 1615 but was mentioned in his Pieta Royalle of 1620. It was also described in Italy by Piccinini (1623), implying that gracing fashions may have tended to be rather international.

Mersenne’s sixth grace, called battement, started with an upper appoggiatura onto the main note (described but not named), followed by repeated playing of the main note by the right hand, filling its full time. He said that this grace was more appropriate on the violin than on the lute.

The seventh grace was a combination of the second (fall) and the sixth (battement). After making the fall one immediately falls onto the upper auxiliary and makes the upper appoggiatura, after which the right hand continues the grace. The unarticulated part of this grace is equivalent to a turn from below with the second note faster than the first.

The eighth grace was a combination of the second (fall) and the fifth (violent vibrato). The example given was with the graced note a major third higher than the open string. The fall started on the open string and included a finger on each tone before the vibrato commenced. Thus an accent could include two moving fingers and an interval of a third as well as one moving finger and an interval of a second, as defined. Mace called this grace a ‘whole’ fall (with ‘half fall’ being with one moving finger).

In addition, Mersenne mentioned other examples of the slurring of notes or successive graces with the left hand. The first was an upwards tirade (scale passage) of seven notes over two strings, with the first note on each string plucked with the thumb and the others slurred with the left hand. The second was a shake slurring into a shake on the semitone lower, then back to the note, and then the note above, which was then given a violent vibrato.

As examples of lute music, Mersenne presented an accompanied song, a courante and three allemandes. In all of these tablatures only two grace signs appear: the comma (104 times) and the cross (6 times). Of the comma signs, 15 are on notes shorter than quavers, and three of these are on demisemiquavers (all in the last allemande by Basset, with two of the three on open-string notes - no doubt very quick upper appoggiaturas). This is the only piece I know of with grace signs on demisemiquavers, and obviously is a tour de force in grace notation. Mersenne wrote that many grace signs were missing in the printed tablatures to play them the way Basset did (Basset composed only that last allemande), and readers would be welcome to copy the others from fully-marked copies that he and Basset had.
Mersenne's approach to the graces he described was very similar to Robinson's. The old familiar graces with an upper auxiliary were not discussed (and only one general sign was used for them), while the new, less familiar graces, mostly starting with lower auxiliaries were discussed, and individual signs assigned to them. The repeated lower mordent was not a new grace, but it had to be included because it was very different from the other old graces and a different sign was used for it.

Discussion of the Fall

The fall is the only unshaked grace mentioned by Mersenne or Robinson. The extended version of the fall, with two passing notes starting a third below, was called the same name as the fall by Mersenne when he described a complex grace starting with it. The origin of this type of fall seems to be in late 16th century Italian vocal practice. Caccini (1602) in Nuove Musicke described some aspects of this. A translation appears in Playford's Introduction to the Skill of Musick (1664 edition onwards) in the chapter called 'A Brief Discourse of the Italian manner of Singing ... Written some years since by an English Gentleman who had lived long in Italy, and being returned, Taught the same here'. In this translation Caccini wrote: 'There are some ... that in the Tuning of the first Note [(presumably) of a point (motive)], Tune it a Third under: Others Tune the said first Note in its proper Tune, always increasing it in Lowdness, saying, that this is a good way of putting forth the Voice gracefully. Concerning the first: Since it is not a general Rule, because it agrees not in many Cords, although in such places as it may be used, it has now become so ordinary, that instead of being a Grace (because some stay too long in the third Note under, whereas it should be but lightly touched) it is rather tedious to the Ear; ...'. He goes on saying that he 'would chuse the second for the Increasing of the Voice', and that he prefers a diminishing of the voice (as in an exclamation) to the above-mentioned swelling.

This is almost certainly the origin of the version of the fall with two passing notes. It was notated as a grace in 1594 by Bovicelli (Regie) with a dotted first note (as implied by Mersenne and apparently disapproved of by Caccini), called 'elevation' in the Coleman table and called Whole-fall by Mace. Recently, Apel introduced the word 'slide' for it (the term was used in the Board book for what we call a 'slur' and Mace used it for a descending slur, the ascending slur being called 'slur').

It is clear that the simple fall and what is now called a slide were related, but the historical relationship between them is not clear. Perhaps it was the alternative to the slide when the third below was not in the harmony, or the intention was to momentarily make the harmony ambiguous. Mace called the simple fall a Half-fall, i.e. a curtailed slide. Its popularity clearly grew simultaneously with the slide, but its origin was probably earlier. It seems to have been described by Sancta Maria (1565), but used in a different melodic context than in the 17th century (see Comm 408).

When grace signs were included in English, French-influenced English and French tablatures in the first half of the 17th century, two signs predominated in most examples: One was a cross and it was almost never on a note on the open string, implying that it involved a lower auxiliary. The other (the dominant one) was marked by a double cross (♯) or a dot or a comma, and it had no such restriction. Virginals music had single and double strokes across the note stems, in parallel with the single and double cross in tablatures. Initial naive modern interpretations of these signs assumed that each was an individual grace, and which ones they were was argued. Virginalists, who have always been more advanced in appreciating and applying gracing than others in early music, used more variety.

In the early '80's, I suggested that the signs signified families of graces. Following Robinson, these families were the falls, which were the new graces starting below the main note, and the relishes, which were the old graces starting on or above the main note. I didn't seriously consider Mersenne's grace categories, which are different.

Mersenne wrote that the cross sign signified a repeated lower mordent or lower shake, and his
fall sign was the upper shake sign plus an added dot in front. In the music he published, where all added signs are missing, it would be indistinguishable from an upper shake. Thus the fall was in the non-cross category in Mersenne, and in the cross category in the English repertoire of the time. Of the compound graces, Robinson discussed the fall plus an upper shake, and Mersenne discussed the fall plus every type of extended grace except the upper and lower shakes. The missing combinations were probably more used than the ones described, but they were so well known that he didn’t bother mentioning them. Following Robinson’s advice, we can expect that the fall would have been used as the beginning of a compound grace on longer notes, and on its own only on shorter notes. Then the difference between Mersenne and the English sources becomes much less serious, only affecting shorter notes where the cross rarely appears. The compound grace Robinson described would be in the non-cross category (dominated by upper shakes) in both cases, and the fall combined with the lower shake would be in the cross category in both cases.

New hypothesis on the meaning of grace symbols

Thus the new categories proposed here are: The cross represents a shaked grace with a lower auxiliary on longer notes. It usually starts with the lower auxiliary (a fall), but other possibilities include beginning with the main note, an upper appoggiatura or a turn from below. On quite short notes, it can be a fall in England and a lower mordent in France. In the non-crossed category the sign represents a shaked grace with an upper auxiliary on longer notes. It usually starts with the upper auxiliary, but other possibilities include the main note (especially if held), a fall, a lower mordent or turn from above. On quite short notes, it can be an upper appoggiatura, a fall (in France), a lower mordent (in England) or an upper mordent (on alternating notes, mostly in downwards scalewise passages). On notes of in between length in both categories, there may be additional unshaked graces involving the appropriate auxiliary note.

In common time, longer notes are defined as a quaver or longer. Quite short notes are defined as a quaver or shorter for mordents and turns, and a minim or shorter for appoggiaturas and the other non-shaked graces mentioned. The note sizes between a minim and a quaver are most numerous in undivided music, and are in most need of variety. That variety is enhanced by the choices including both shaked and unshaked graces. In this range of note lengths, the passing notes in unshaked graces are usually not played as fast as when played on faster notes. It is assumed that graces (such as mordents) that only apply to fast notes could have been considered to contribute to texture only, not providing harmonic or melodic tension as proper ornaments do, and so were not discussed.

The above hypothesis is consistent with all of the evidence from the 17th century on the assumption that writers never attempted to be completely comprehensive. Mace was the most comprehensive, and his not mentioning mordents or turns probably means that they were truly well out of fashion by then.

Mersenne indicated that the gracing notation in the music included in his book was far from comprehensive. This was most probably always the case to some degree. Quickly passing graces were rarely notated. We can never say with confidence that the graces notated in any piece are all that were played by a competent player.

In her Leading Notes article on recorder tablatures, Marianne Mezger wrote that on average (in undivided music) there were two graces per semibreve bar, and one or two graces per bar in triple time. Except for the slur, these were all shaked graces. This grace density was probably quite typical throughout the 17th century, at least in playing arrangements of popular repertoire.

Michael Morrow once said to me about interpreting early music “You might not know what to do, but do something”. When I suggested that he should apply this principle to gracing (which he felt was superficial and effeminate), he changed it to “When you don’t know what to do, do nothing”. He was less in tune with the original spirit of the music than he felt he was.
Mrs Liz Greenberg has just written to me of the sudden death of her husband on May 27th of a ruptured aortic aneurysm. Members will remember the careful and informative account he wrote which was in the Quarterly previous to this one, dealing with his restoration of a Keene spinet of 1704.

He was a builder with a fine reputation who had exhibited at the Boston Festival. I never met him but we corresponded during the last year. We have lost a member who, I am sure, would otherwise have made many other valuable contributions to our society.

Comm. # 1381  
Withdrawal of Ascription  
Robert Greenberg

A year ago I sent to this journal a piece describing the restoration of an Italian harpsichord now in Berkeley, California. Having got interested in the study of mouldings of such instruments as a basis for giving instruments without known makers their proper place as members in lists of instruments of known makers, I happily set about making impressions of the mouldings and key arcades of the instrument, in hopes of finding a match in the published and unpublished recordings of these decorative details. I thought I might have found such a match and suggested that the instrument could be held forth as having been made by Giovanni Giusti, near the end of the 17th Century, in Lucca.

Perhaps so, but not proven so on the basis of the evidence I advanced. The moulding outline was not close enough to be considered identical and is probably not a match to similarly placed mouldings on other instruments by Giusti. I too forcefully stated the only other point I raised in favor of the conclusion that Giusti made the instrument: I sent a profile of the arcade on the end of one of the keys to Denzil Wraight. His response, in a personal letter a year ago [1 August 1994], was that "it is conceivable that your arcade might be a sharpened version of 721" [that is, number 721 in a listing of all Italian keyboard instruments being assembled by Mr. Wraight, number 721 being named as by Giusti].

Mr. Wraight has made clear to me in a further letter that he did not state that the arcade was made by Giusti, but only that, given what could have happened, it would not have been impossible for Giusti to have made it. Thus his opinion, which I seized on a year ago as saying that I might offer the arcade as having been made by Giusti, was misrepresented and misstated in my report. I apologize to him here for misrepresenting what he had said to me.
Some Comments on Veneering with Hot Hide Glue

I have been veneering pianos with hide glue for a number of years now, and finally feel relaxed about the process. I will record my method here in the hope of getting some response from others who use hide glue, and perhaps we can all add to our knowledge and learn some new tricks for coping with the hard parts.

Hide glue has many advantages over other adhesives. (I am not discussing liquid hide glue, the ready-made stuff in the plastic bottle, which has serious problems.) There is no 'cold flow', but a glass-like hardness once set which can hold wood together under a piano’s stresses. This also means the veneer will not tend to check and crack along the grain with time, and burls and other figured parts will not curl or gap. Whatever disasters occur, you can always reheat and moisten, and remove and reglue the piece. Repairs can be made to old instruments by the same means, since the glue can be re-melted. It does not seem to be too vulnerable to normal moisture, however. An 1860’s piano I have had been standing on its spine in a flooded basement over a summer, and the spine and its veneer had to be reglued, as did all the action parts at the submerged end of the keyboard, but what remained out of water was fine. As Mike Frederick remarked to me, if you don’t take it in swimming with you, there will be no problem.

While using hide glue is not exactly easy, it is possible. You need to find a supplier of dry glue, to buy a thermostatically-controlled glue pot and brush for applying the melted glue, a sponge and water container. Beyond this my method is less usual. Instead of an iron veneer hammer such as the catalogs offer, I use a maple slat, about a quarter inch thick and two inches wide, a foot long or so. The iron will stain light woods with black streaks when it contacts the glue. The maple does not. For most of the pressing-on however I use a household iron. Not your only good household iron of course, as it gets extremely cruddy; also a steam iron with holes in the bottom is not much use. Prowl the used goods sales until you find an old plain bottom one. The edge-banding irons sold by the woodworking catalogs are fine too but small for piano cases. The metal in ‘irons’seems not to be iron; it does not stain the wood.

The actual process: Melt the glue granules in the glue pot with water to more than cover the granules. Let them soak overnight before turning on the heat; add a bit more water when the heat goes on. Stir from time to time. When it’s ready to use it will be smoothly melted. Timing varies. The consistancy is critically important. You need to be able to spread it on the veneer (and the core) but only just. If it is a honey-like consistancy, as books often advise, it’s a bit thin unless you’re used to awfully thick honey. If too thin, let it sit for a while and some moisture will evaporate out. Too viscous, add water. Leave the pot turned on the whole time you work, and add small amounts of water at need.
Preparing the veneer: have your core (the structure to be veneered) as smooth as possible, free of bumps, protruding endgrain etc. Most books handle this part fully. Many use a grooving plane to put fine glue channels in the surface, for a bit more mechanical bond. I use one-sixteenth inch veneer; thinner is fine and goes down easier but you might need to do an underlayer to cover core flaws. Grain should be in a different direction than the wood (or veneer layer) under it. Pieces can be pre-joined and taped or trued on a jointer and butted as you go. This is what I do. (But if the grain is irregular, uneven expansion will make gaps.)

To get to the actual doing, the idea is to spread enough hot glue on the veneer piece and then on the core, place the veneer promptly while it’s all hot, and squeegee out any excess glue while making firm contact between veneer and core. The glue cools quickly and then won’t flow out. The iron (set to cotton) heats it all up again, while also being the instrument to push out the glue. To make the iron or wood tool slide on the veneer surface (The iron particularly will want to cook fast to any dry veneer) wet the veneer with the sponge, and also brush some glue on the top side of the veneer. Mop up any excess water with the sponge. A bit of cooling makes the veneer stick down, so I switch to the maple implement off and on, pushing the excess glue to the edges, back toward the joint with the previous piece, while pushing them firmly towards each other (things can get hectic at this stage) and if the glue is right the veneer will go down and grab quite nicely. Reheat anyplace where you feel a bulge of excess glue, and push it out. If the piece, or a part of the piece, will not stick, there is probably not enough glue, or it is too thin. (The same thing) Lift the veneer and add glue with the brush, and re-stick. When the piece is down, clean up excess surface glue and water with the sponge, particularly along what will be the edge joining the next piece. Clean up the glue squeeze within three hours, while it’s still soft. If you leave in the middle to have lunch, clean the edge which will join the next piece carefully because any old hardened glue here will make trouble for the next piece later. And if a place stays up, makes a hollow whisper after a day, iron it to melt the glue again and clamp the spot. To insert more glue after all is dry is a lot of trouble but can be done if necessary. I’ve only had to do this a couple of times in ten pianos; as complex as this all sounds, with a little practice it goes along fine, and I can do the outside of a whole case in a day’s work.

I have not touched on cutting and matching veneer, nor finishing, but there are lots of good books and articles on this. I may have omitted some details. I’d love to hear how others veneer. I should add, I use a vacuum veneer bag to do my lids, and find it fabulous, all the lid pieces in one day.
The British Ten Foot Organ

In Comm 1347 Eph has put a scholarly mind to most of what I had written about in Comm 1329 concerning the tricky subject of pitches, transposition etc. in pre-1700 England. I can say that we are happily in complete agreement on two points:

1. It is very difficult for each to understand what the other is writing.
2. It is possible that no one else can understand us.

As a builder with (at the moment) no spare time, I am sorry that I cannot go through Eph's analysis with the care it deserves, though I will try to comment on the few points where I don't have to think much. There are a couple of items which came to mind after sending off the last Comm., but I guess this happens all the time. I would also like to move the discussion towards the instruments and their use, and may attempt 'voicing' next time.

The arguments can be summarised as follows:

1. I reported that 'choir pitch' at Magdalen College up to 1690 was between a semitone and a tone above a = 440Hz. I proposed that the general level of choir pitch in England in the seventeenth (and possibly the sixteenth) century might be close to this pitch. I would not be happy to accept a pitch level higher than about a tone above a = 440Hz. I also think that this pitch was known as 'church pitch of f'.
2. Eph would like a precise figure for choir pitch, which he gives as 2½ semitones above a = 440Hz, with 'church pitch of f' a tone lower than this.

Concerning the pitch and use of the term '10 foot (Open Diapason)', it invariably refers to the rank of pipes that is on display, and in many cases where the organ is on a screen this can mean two stops, one facing one way and the other on the opposite side facing the other way. These display pipes would normally be made overlength and cut shorter at the back for tuning. I mentioned this in Comm 1329, giving a mid eighteenth century example. Eph insists that the earlier ten foot pipes were actually ten foot. I can think of no example from any period or country where these display pipes are cut to length.

Eph considers the organ pitch at Magdalen College, Oxford, to be unusually low compared with other organs of the seventeenth century. There is a considerable amount of music extant that was written for that chapel during this period. It would be interesting to see whether there are any signs in the music that a low pitch was allowed for. It might also be interesting to examine Magdalen music written before the Dallam organ was installed to see if there was any change in the pitch range. The investigation should also take in music written after 1690, when the organ was evidently converted so that one keyboard was GG/BB - d¹ at 8ft pitch and the other remained as C - d³ at a 10ft 'transposing' pitch.

The large number of performances of pre-Restoration British church music on CD's play/sing at whatever pitch they like, and there are some very fine examples at both high and low pitches. For unaccompanied singing there is no reason to fix the pitch as precisely as either Eph or me, and there seems to be no move away from this style of performance, though I would have thought that there was more evidence for accompanied singing than unaccompanied, e.g. see I. Harrison, Loud Wind Instruments in England, part 2 (M.A. thesis at City University, 1986-7), where he has found references to cornetts, sackbuts etc. in fifteen cathedrals and chapels between 1565 and the Civil War. Detailed accounts from Canterbury and Durham cathedrals indicate that four part (at least) wind ensembles may have accompanied the choir daily. Pre-Reformation evidence for organ accompaniment is elusive, but why have a low C# in the keyboard if it doesn't play with the choir? The recent discovery (and rediscovery) of two early sixteenth century soundboards from Suffolk will. I hope, stimulate interest in the British organ of this period, leading to a reconstruction.
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