Quarterly No. 78, January 1995

FoMRHI Quarterly

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FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS
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FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS

Bulletin 78

January, 1995

A HAPPY NEW YEAR to those of you (about 50%) who have renewed your subscriptions (and in due course to many others of you who haven’t yet), and in particular to those who have sent us kind messages of thanks and good wishes. May we reciprocate and say the same to you, for while Barbara, Eph, and I are spending some of our time working for you, if you were not working for us by writing Comms, etc, there wouldn’t be anything to send you except the front cover and this page! So keep it up, and together we may take FoMRHI into the next millenium.

In which connexion, as some of you will know, I retire as Curator of the Bate Collection at the end of September next. It was in my mind to retire as Hon Sec FoMRHI at the same time, but when I suggested to Eph that I’d done twenty years and perhaps it was time to pack it in, he was horrified at the idea. It then occurred to me that under our Rules the Sec, Treas, and Ed were supposed to be elected every three years, and it was a long time since I’d bothered to do anything about this. So I sent a Ballot form round the Fellows, and the response was unanimous, that they wanted the three of us to carry on. So while not (if I remember to do anything about it in 1997) to the millenium, but at least for the next three years, you’re stuck with us.

However, you should start getting used to a separation between me and the Bate, and I have therefore asked Eph to change my address on the front of the Q, and when he has to get a new stock of envelopes, to my home address 171 Iffley Road, Oxford OX4 1EL; I shall get round to getting a new Printy, too, and to changing the letterhead stuck in the computer in due course (we’ve got nine months so there’s no rush, but it takes time to get used to a change which is why I mention it now). I would ask you, as before, to stick to letters and e-mail (jmontagu@vax.ox.ac.uk — I shall keep that one when I retire, the University is generous to us in that way, but probably I shan’t keep the one @music.ox.ac.uk), and not telephone on routine FoMRHI matters, since I’m usually head-down in the middle of something, and letters and e-mail can be dealt with whenever I’ve got time. I don’t have fax at home. Remember that e-mail can be plugged straight into the Bulletin (there are examples here), or be printed out here as Comms.

LIST OF MEMBERS: Only fifteen people responded on this one. Ten said that every two years was often enough; five preferred annually. Depending on finances you probably will get a new one this year because all the UK telephone numbers are changing. This will happen before you get the new List, on April 16. With six exceptions (people who live in places where changes are greater, all of whom are noted in the Supplement herewith), a 1 will follow the present 0, so that Oxford will become 01865 instead of 0865, and London 0171 and 0181. This allows them in due course to introduce 02..., 03..., and so on, and thus double, triple, etc the capacity. The new numbers are working now, in parallel with the old, so you can start getting used to them already.

You’ll find quite a few new e-mail addresses in the Supplement herewith; anyone else who has one and who would like it to be included, please let me know between now and the beginning of April. Also any other additions and changes, of course. E-mail is spreading fast, and it is so useful for rapid communication that one can expect it to grow even faster. Whether those of you who have it will get this on e-mail will depend on whether I’ve got time to sort out the mechanics — there were problems last time I tried, and there was a suggestion that I should have sought permission at this end. Cary Karp pointed out that I should have sought permission at your end also, since some services charge you for mail. From my point of view, the reverse is easier. So if anyone does NOT want to get any...
FoMRHI material through e-mail, please let me know. I don't have a scanner, so all that you can get is what I have on disk already.

ADVERTISING: It has been suggested that we should take paid advertisements in the Q. The idea has come up before, but now rather more strongly in view of rising costs, etc. In the past I was against it, partly on the basis that it makes work for someone — one needs an advertising manager to go out and get advertisements, and more important to see that the advertisers are invoiced and actually pay up — and partly on the basis that we are more independent if we don't have paid ads, and I can always tell you about anything that I hear of and think will interest you. So, what do you think? Would you like to see paid ads? More important, who would like to act as advertising manager? You won't get the one without the other! If enough of you seem in favour of advertisements, we'll put it to the Fellows, for they will have to decide on such a radical change of policy. Maybe I should put it to the Fellows first, but it seems to me that a straw poll round the membership as a whole is some necessary guidance for us. Above all, perhaps, don't keep silent now, and then, if we eventually do decide to accept them, scream in horror. Speak now!

METHODS OF PAYMENT: Torsten Björling has written saying that paying his sub cost him 32% over the odds for sending the money, and asking whether there isn't any cheaper way. He asked if he could pay by VISA and the answer to that is no. One has to be a very much bigger operation than we are to get onto that sort of network, and you have to pay through the nose for it; subscriptions would have to go up to cover the percentage that they take off. He asked, too, if he could pay for more than one year, and the answer to that is yes. Some people have sent a convenient sum, a hundred dollars or whatever, and we gnaw at it little by little till it's gone and then shout for more. Others have sent a multiple (usually double or triple) of the sub, and we then have to hope that we aren't going to put up the rate while it lasts and so lose out. At least one year the Dutch members got together and sent a joint payment for a group of them, since the exchange fees are usually much the same whether it's 15 quid or 150. Others (sh! you aren't meant to do this) get hold of a £5 and £10 note and stick them in an envelope. Others use our GIRO account or Eurocheques, both of which seem usually to be free. You've got a year now to sort out whatever will be the cheapest way for you. It makes no odds to us how you do it, so long as the right amount winds up here. What annoys me most is that things seem to have got worse within the European Community, rather than better, and if you try to pay by ECUs, the European international Currency Unit, the banks swindle you out of even more than any other way of paying!

SENDING COMMS: It's not just the banks screwing us, the Post Office are at it, too. One of the Comms herewith was overweight (I won't embarrass the chap by saying who's), but instead of delivering it and asking for an extra 19p on arrival, a card arrived, saying that it could be collected from the Sorting Office if I went and paid for it! So to save me a half hour walk, my wife went round on her bicycle and collected it. Apparently it is now policy not to deliver such things any more. So please be careful; if you think it may be on a border line between weight limits, check it. Exercise is good for me, but I don't need it that much! and I don't have a car any more — it got abolished by a drunk just before Christmas (I wasn't in it — it was parked).

FURTHER TO: Comm.1277: Gillian Alcock (whose e-mail address is Gillian.Alcock@anu.edu.au) says (this is an amalgam of four e-mail messages): Re John Rawson's article in the latest newsletter, maybe it should be pointed out that the list for which he gave an address (battle® etc) is defunct and the instrument makers list is now run on rec.music.makers.builders. It isn't an email address, it is a newsgroup name. I use the newsgroup reader nn, and it appears along with rec.music.early,rec.music.celtic.alt.music.makers.woodwind and thousands of others. I agree with him that it would be good to get stimulating
discussion going on that group, but as in so many things people have to actually put finger to keyboard for discussion to happen. Maybe he could start topics which other members would respond to. There are a few individual instrument lists operating. I subscribe to one on dulcimers and one on harpsichords. The former is lively, the latter is big but not stimulating to me as a builder. HPSCHD-L is its name. One sends a message to LISTSERV@ALBNYVM1.BITNET, blank subject line, message to read SUBSCRIBE HPSCHD-L <your full name> eg SUBSCRIBE HPSCHD-L Jeremy Montagu. Then you don't add anything else like a signature. One can get lists of the articles in their archives too. It is heavily oriented towards people who recommend sheet music editions and recordings that they like. I got knocked off because their mailer was having trouble getting through to my server. I must confess I don't miss it. Maybe it is early days yet. The hammer dulcimer list is hammerd@mcs.com and one simply subscribes by sending a message to that address without copying the message to the entire list.

Another message relevant to that arrived from MICAT-L the day before yesterday from Justin Renquist, addressed initially to: Multiple recipients of list HPSCHD-L <HPSCHD-L@UACSC2.ALBANY.EDU> saying that: I am going to be making the Early Music WWW (world Wide Web) page which will be accessible both by graphical browsers such as NCSA Mosaic and Netscape, Cello etc (for those with graphical systems with direct IP connection like SL/IP etc.) and by character based browsers like Lynx for those still using "antiquated" terminal sessions.... I would like to include full colour scans of instruments and make catalogues of instruments available to browsers. No one on the net anywhere in the world seems to have any real interesting representation of early music instruments (I've only found one place in Japan...mostly recorders, shawms etc.) I'm also going to putting sound files out (for those with multimedia capabilities on their pc's/workstations) to be able to listen to short snippets of music. I do not intend for the page to be only for harpsichord and clavichord - so if you know of people that would have input for other areas such as voice, woodwinds, strings, organ, hurdy-gurdy etc. have them contact me. I'd mentioned this before...but didn't get really any sort of response at all, and I've been using the time to get better at HTML & now I'm ready, are you? I'm in the process of getting a really good colour scanner so I basically would just need photos etc. to scan to start... Justin Renquist (justinr@sirus.com).

JM adds: this and Gilllian's remarks above give you a pretty good idea of what HPSCHD-L is like.

RESIGNATIONS: Two resignation letters that we who write for FoMRHIQ might note and think about (and you who don't write might also like to consider and perhaps start writing): "I do not intend to rejoin FoMRHI this year. The information on the practical making of instruments is being smothered by philosophical argument; Ian Abernethy". And "I will not be renewing my subscription for 1995. I have enjoyed the Qs over the years but I'm afraid it's become a bit too rarified and esoteric for me...I find most of what is published now goes over my head; A R Abraham." I have had other similar comments recently. Can I (yet again) appeal for more nuts and bolts Comms? Surely we've not printed everything that can be said about workshop practice? Surely there must be more basic information floating around among you on how one makes instruments? I can't write it — I'm not an instrument maker — you are, you can.

PERMUTED INDEX: Charles Stroom has produced a new edition of this invaluable tool. We hope, if finances permit (wait till this Q has gone through the pipeline) to publish it, but if anyone wants it sooner Charles writes: I have been thinking on my index and whether there was an easy way to send it to people as an electronic mail. However I checked the postscript file and it is roughly 4 Megabytes, thus probably not feasible. I could compress the postscript file (which is much smaller but becomes then a binary file, and thus cannot be mailed directly) and convert the
compressed file into an ASCII file again, which would reduce the file to slightly less than 2 Mbyte, still a lot. A compressed/encoded file would need the utilities 'uncompress' and 'uudecode' at the receiver's end, in order to restore the original file. If anybody is interested, I could give it a trial. charles@ye.estec.esa.nl or CSTMROOM@ESTEC.BITNET European Space Technology Centre, Noordwijk, the Netherlands Phone: +31 1719 84014, Fax: +31 1719 12142, Telex: 39098

A QUERY: Paolo Barattini (c/o Teresa Gallo, Via San Francesca da Paola 6, 10123 Torino, Italy) is researching the evolution of bows for the violin family and gamba, and the Dragonetti style bass bow for his thesis for the violin restoration course in Milan. He would like to measure and take pictures of bows other than those in museums so he wants to contact bow-makers, luthiers, musicians, and collectors. He would also be glad of suggestions and iconography, excluding Baschenis and Becchera, and offers eventual information in return. Since he stopped subscribing to FoMRHI after 1987, I think this appeal is impertinent, but since our aim is to disseminate information, I am printing it nevertheless.

AN APPEAL: Surrey County Council Library in Kingston has the latter part of the Broadwood archive (the earlier part is in the Bodleian here), and much of it needs conservation. They are appealing for help. They have raised £7,500 so far, and, the County Archivist has told me today that the National Manuscripts Conservation Trust has matched that sum. The Trust has also said that if they come back in a year's time, they may well match whatever they have raised in the meanwhile. Much of the Archive is unusable until it has been conserved, but it will then allow the sale and other details of individual pianos to be traced, and to do the whole job will cost most of £58,000. They'd be grateful for help, and donations should be sent (made out to Surrey County Council) to The Broadwood Archive Conservation Appeal, c/o Dr David Robinson, County Archivist, Surrey Record Office, County Hall, Kingston upon Thames, Surrey KT1 2DN.

OTHER SOCIETIES: The British Clavichord Society has sent us a report on their progress so far (see elsewhere here). You may like to know that they will be meeting here, in the Bate Collection, on Sunday, June 18 for a day devoted to the Hieronymus Hass of 1743; it will be played and Lewis Jones will talk about its restoration, which he supervised. If you want to come I'd suggest getting in touch with Peter Bavington (new address in the List Supplement herewith) as it's their meeting, not mine.

The Bagpipe Society is having a BLOWOUT '95 at Courtyard Arts Centre, Great Linford, Milton Keynes, 2-4 June. Information from Ian Claburn, 6 Greyfriars Road, Daventry, Northants NN11 4RS; 01327-705263; please send a large sae. There are camping sites and boat moorings available. For information about the Bagpipe Society, write to Tim Garland, 25 Baden Road, Evington, Leicester LE5 5PA.

COURSES: I am not at all clear whether West Dean College has summer courses or not; I told you of their Easter courses in the last Q. Now they have sent me information about their Early Music Performance course (15-21 July if you're interested in that), on which there is a note: Renowned instrument-making workshops (lutes, viols) on site in the College, but nowhere in the accompanying bumph is there any information about them. Their address is in the last Bull. They do have many general woodworking courses, including turning, silver-smithing, black-smithing (whether that includes reamers as well as spades I don't know) and so forth; so there is a lot of basic skills tuition available there at summer schools and probably other times as well.

We've arranged another Bate Collection Bow-Rehairing Weekend for May 13/14, run as usual by Andrew Bellis. Places are strictly limited to 15, so if you want to come don't delay -- book now. £20 as usual (£15 stud-
Alec Loretto has asked me to thank everyone who has supported him by coming to his Bate Weekends over the years. I'd like to thank him, too, and everyone else who has either taught or learned at any of our courses — one of the many things that has made this job such a happy one to have done.

**OPHICLEIDE CONCERT:** Barbara has sent me a flyer for a concert on February 18 at 8 pm at Longsands Community College, St Neots: *Sam and the Sleeping Beauty*, a musical entertainment featuring the ophicleide recently discovered at the Royal Opera House, Covent Garden, given by Tom Winthorpe, accompanied by Mark Packwood. Apparently this is yet another of Sam Hughes's ophicleides (we've got one in the Bate and there's at least one other). She thought that anyone within reach of St Neots might be interested in this example of people bringing an early instrument back into the mainstream.

**THINGS AVAILABLE:** Chevalets Despiau make bridges for bowed instruments of all sorts and periods. They would like to send any of you interested their full catalogue; I was reluctant to send them a list of members, but do write to them for one, saying you're a FoMRHI member: Catherine Capelle, Sales Department, Chevalets Despiau, 32200 Gimont, France. Their baroque bridges look a bit late to me, and they don't seem to include viols.

Camwood (Unit 2, Staden Business Park, Staden Lane, Buxton, Derbyshire SK17 9RZ) have sent their current price lists for boxwood and ebony, and also an interesting note on ebony in general. I'll send them up to Eph on chance there is room and he thinks them suitable for the Q. I'll send him the Despiau catalogue, too; he can tell you more of what the bridges look like than I can.

**Editrice Turris di Spotti Giancarlo e C, Via Bertesi 1, 26100 Cremona, Italy, have a large list of books on string instruments, many of them at what seem to me quite reasonable prices. They have offered the Bate a 20% discount and post free, so it might be worth mentioning that you got the name from FoMRHI at the Bate. They also have a considerable list of Disegni - Drawings; whether these are full maker's drawings, I don't know, but they may be.

**DEADLINE FOR NEXT Q:** April 3rd I'd think. I'm a bit vague because I'm not sure whether I'll get away at all in that vacation - I stayed here this vac, keeping the Bate open all over Christmas since I believe that museums should be open when people have time to get to them.

**CODA:** Thanks to those of you who've got your renewals in by now; latecomers for the next week or two will still get their Q in time (though, if they've moved, without note of their new address in the List herewith); the rest of you will get it with the April one.
**BULLETIN SUPPLEMENT**

**Ephraim Segerman**

Bent-Stave Idea: I recently followed up a reference in *Early Music* Oct. 1978 issue. During a browse I reread Pringle’s article on John Rose, and on p.506 found a rejection of the theory that the central stave on early English viol soundboards was bent and the other staves carved (I argued against Pringle’s view in Comm. 289 in the July ’80 Q). I had forgotten this, and so withdraw my claim in the Bulletin Supplement of being the first to mention bent staves in print. I still claim that, to my knowledge, Stephen Gottlieb was the first to think of it. I am sure that no one at the time thought about who postulated it first.

Wood Absorption - relating to Comms. 1227, 1228 and 1285: If we take a piece of figured sycamore of the thickness of a violin rib (about 1 mm) and give it a liberal coating on one side of light rosin oil or raw linseed oil, one will soon see that the oil has penetrated right through to the other side. This will not occur if the rib is made of split straight-grained wood. The reason is that the oil is absorbed by end grain, along the channels that sap originally moved in.

That absorption can swell the timber. I have been able to close a wide crack in a teak bowl permanently by repeated application of tung oil. This swelling is apparently what Kirkpatrick observed in Comm. 1227. I can imagine that the swelling that she reports on playing-in could be caused by the deposition of solids from saliva that remain in the channels after playing and drying.

The advice that Simmons gives in Comm. 1228 seems to be sound, but his underlying science is too elementary. All attraction between molecules is electrical in nature. Water molecules are dipolar (having a plus charge on one side and a minus charge on the other), and they are attracted to (not repelled by) hydrocarbon (oil) molecules (which are electrically neutral) by dipole – induced dipole interaction. The question of solubility of other molecules in bulk water depends on whether water molecules are happier (i.e. having a lower free energy) surrounding other water molecules than surrounding the other molecules. A small hydrocarbon molecule like methane is quite soluble in water because it doesn’t much inhibit the dance of the water molecules around it. That dance is important for the entropy component of the free energy. Larger hydrocarbon molecules inhibit the dance more, and so are less soluble. So our experience of oil (which is called a hydrophobic material) not mixing with water or other water-liking (called hydrophilic) materials is a bulk property, and has no relevance on the microscopic level when there is no fluid water available.

The molecules in wood that are in contact with the sap in the living tree are mainly cellulose and hemicellulose. They are hydrophilic because they have charges and dipoles on their surfaces, but after drying, with little water around, they can be quite attracted to oil molecules. An unsaturated oil molecule has a carbon-carbon double bond in it, which is a dipole, so it is attracted more strongly to the surfaces of the wood molecules than a saturated oil. (Incidentally, there is an H atom too many on the carbon atom on the right of the double bond in Simmons’s Diagram 2).

The oil penetrates along sap channels, coating the inside surfaces of the channels, and can get into the adjacent cells that are ruptured, but probably not into the intact cells. Others follow, filling spaces. When the oil molecules get in between the molecules of the wood that it interacts with, swelling of the structure occurs, mostly by opening up the channels that are partially collapsed. This is the same mechanism of adsorption and swelling as with water. If it is a drying oil, the oil hardens, usually by polymerisation, and if it is a non-drying oil, it still acts as a physical barrier to the subsequent passage of water down the channels in humid conditions.

Despiau Bridge Catalogue: Hardly a month goes by when we don’t get a phone call from a violin specialist seeking to buy a baroque violin bridge. My response has been that they are not available and makers do the design research and make them themselves. The Despiau catalog says that they have spent ‘the last 10 years devoted intensively to acoustic and historic research’. That research hardly shows in the baroque bridge designs, of which 7 are offered for the violin, 4 for the viola and 2 for the cello. Most are classical and there is no indication of the origins of the designs. I will recommend them only with great reluctance.

**Hardwoods:** The Comm. on Ebony by Camwood (UK) Ltd. came with a price list covering West African ebony and boxwood from the French Pyrenees. They are at Unit 2, Staden Business Park, Staden Lane, Buxton, Derbyshire, SK17 9RZ, Tel: (01298) 77407, Fax: (01298) 71156.
One can really only begin this review by saying 'Wow!' They do things differently in Holland. Here is an art dealer in the Hague which decides to put on an exhibition. They publish a magnificent book, which will stand on library and scholars' shelves for very many years; they persuade two major instrument museums to lend them priceless instruments which complement the subject of the exhibition. They get the leader of an important early music group to write some of the material in their book (and I suspect that they may have laid on concerts by the group during the period of the exhibition). They persuade Phillips to publish not only a CD of that group playing appropriate music, but also a photo CD which includes a guided tour of the exhibition, pictures and all, if you have a multimedia computer to play it on (which unfortunately I haven't). All this for an exhibition that ran for two months in their own premises in The Hague and three months on loan to a museum in Antwerp. OK, some of the paintings were for sale and doubtless there was a profit margin when they were sold, but London is full of art dealers, and so is New York and so are other cities, and they put on exhibitions and sell paintings, but they don't do anything like this that I've ever come across. Wow indeed. Let us take our hats off to Messrs Hoogsteder & Hoogsteder!

What we have here is, centrally, an illustrated catalogue of 47 paintings, ranging alphabetically from Dirck van Baburen to Philips Wouwerman, most of them early to middle 17th century, one conspicuously right at the end of that century. Save for the one or two black and white engravings, all are illustrated in colour, several of them on fold-out plates, some with additional details either in colour or black and white, and all of them reproduced to the highest standard. What is more, when one of the paintings is mentioned in one of the introductory essays (to which I'll come in a moment), there's seldom any of the 'see page xxx' business; no, there's another illustration. Also, of course, both in these essays and in the catalogue there are many other illustrations of parallel material, other paintings from museums all round the world that bear on these. That is why I said in the header 'innumerable illustrations' – more than I can count.

After a brief description of the relevant buildings, Hoogsteder's premises, a historic house in The Hague, and the three relevant museums, the Hague Gemeente and the Vleeshuis, which both lent instruments, and the Hessenhuis in Antwerp, where the exhibition was also shewn, Magda Kyrrova provides a note on Music in Seventeenth-Century Dutch Painting, covering a rather wider field than one might expect from that title, and also discussing symbolism, a subject which is unavoidable in this context. Louis Grijp, the leader and lute and cittern player of the ensemble Camerata Trajectina which provides the music for the CD, then gives short biographies of the composers of the Dutch Golden Age, from Sweelinck through Jacob van Eyck to Huygens. Eva Legène has an article on musical instrument collections, which mainly consists of inventories, with a certain amount of iconography, except, rather curiously in this context, for those of the Danish Court, of which a number of instruments are illustrated – is Denmark one of the Low Countries? Or is this because that is where Dr Legène is working? Anyway, let us not complain, for we have the benefit of photographs of a number of instruments which we should otherwise be without. Then another article by Grijp on the Exhibition itself and its interpretation, including some interesting statistics of what instruments and who plays them.

Then comes the main catalogue; this hundred and twenty-six pages with all its illustrations that I've been describing is merely the hors d'oeuvres. The main course describes in considerable detail, as in any art-historical catalogue, the 47 pictures. They all show instruments, some of them Vanitas still lifes, but many of them with instruments being played,
so that there is a vast amount of information here for us all. What do they look like, how are they held, how are they played, in what social contexts are they played, who are they played by, and so on and so forth. Much of this, of course, we know from Valentijn Denis and many other books, but this is the first book that I've met where the reproductions are so good that I seldom feel I really must go and look at that picture. Most of them are early baroque, what we usually call renaissance (i.e. Praetorius-type recorders or even earlier) until suddenly one reaches a chap whom I must confess I'd not heard of, Pieter van Roestraten, who was born in Haarlem in 1630 and died in London in 1700, and provides here a typical baroque treble recorder, ivory beak and ferrule, either decoratively stained boxwood or quite possibly tortoise-shell-veneer (we saw one like that in the salerooms recently), not a Bressan, more like an Hotteterre, a baroque lute, baroque violin, and a bass recorder rather vague in the background, and the keyboard of what looks like a single-manual harpsichord, with the lid shut but it has a frontboard that folds down like a virginals, so that the ebony naturals and ivory sharps may be seen. The treble recorder dates this to the very end of the seventeenth century, whatever the keyboard instrument may be.

The next course, so to speak, is a catalogue, illustrated of course, of eight instruments which were loaned to the exhibition, three Andreas Ruckers harpsichords and two Jan Ruckers virginals (imagine any of us lending instruments like that, even if we had them, to an art dealer - 'a dealer?' our museums would say), and then rather oddly a Rombouts gamba of 1706 and a Sellas theorbo (why Sellas?, and why a theorbo? - there wasn't anything like that in any of the pictures), and an anonymous fretted clavichord said to be c.1730 but looking rather earlier.

Finally, as if this were not enough, there is a detailed survey of all the instruments illustrated, with brief descriptions, many of which are best ignored, but illustrated from contemporary engravings etc, Praetorius, Mersenne, and the like.

There is, of course, a bibliography, mainly art-historical, and all the hors d'oeuvres essays have copious annotations.

You may think 130 guilders quite a lot (around £50), but not for something like this, it isn't. And then of course there's the CD. This is called Jacob van Eyck & Dutch Songs of the Golden Age, and it starts with some of Van Eyck's carillon music played on one of his own carillons in Utrecht. I had not known that he was primarily a carillonneur and that he had an ear so precise that it was he who led the Hemony brothers to learn to tune bells properly and thus to found the true art of the carillon. It then consists of songs and Van Eyck's recorder versions of the same tune, sometimes the one after the other, and sometimes with the recorder version interspersed between the vocal verses. Mostly well done and a pleasure to listen to, though sometimes, for my taste, the recorder player, Saskia Coolen, pulls the tempo around too much, as does the soprano, Suze van Grootel, in the well-known Nightingale - that one is an English text and while she sings it well, it is not quite so well as her Dutch (doubtless much better, though, than an English singer would manage the Dutch texts!).

The book and CD were of course the catalogue of the exhibition, but now that the exhibition is over, they remain available through the normal book trade, and of course direct from the publisher whose name and address I gave at the beginning. They warned me that the quoted price does not include postage (nor bank conversion) and it is a very heavy book, so if you are ordering direct, I'd suggest writing first to get a final figure. And it is worth ordering; we can all learn a great deal from it and we should all thank Messrs Hoogsteder & Hoogsteder most sincerely for initiating so magnificent a project and carrying it through so superbly.
At least I assume the price to be seventy dollars, high as this seems for such a book — the piece of paper that came with it had the word Price printed on it and 70- written in, without any currency being specified, but it did come from New York, so it's likely to be dollars; it's just that it does seem a great deal of money for a book of this type and size.

The book itself reminds me of the well-known little girl 'who had a little curl, right in the middle of her forehead. / When she was good she was very, very good, / and when she was bad she was horrid'. It is a very comprehensive index indeed, with brief summaries of most of the entries, sometimes amounting to quite detailed abstracts. It is well divided by subjects, which makes it very easy to find material, and well-indexed (with one or two errors of entry number) which makes it equally easy, if somewhat laborious, to find an entry by a known author, or from the reviewer's point of view to check whether a certain entry is included.

Here we come to one of the adverse points. There are quite a lot of entries from our Q, but there are some that one would expect to be in that are not, and it's difficult to see why they are missing. Incidentally, as well as a number of Comms being in the body of the book, there is an appendix entitled Articles in *FoMRHI Quarterly,* a similar appendix covers the *Bouwbrief.* And in the Collections of Historical Instruments section, at least three major omissions jump to one's mind, one of them the Dayton Miller Collection which it is hard to imagine any American scholar of the instrument missing ('Flute' was a very wide term to DCM and several very important recorders are included in it).

An aspect to which I have a strong aversion is the constant use of the term 'fipple flute', a term which would be more useful if either the authors, or we, knew what a fipple was. In the hope that Dr Sadie will not object, I quote from my own entry under this head in *The New Grove Dictionary of Musical Instruments:* "A word, uncertain in derivation and varying in meaning from one authority to another, associated with some part of the sound mechanism of the duct flute. To Marcuse (1964) and others it represents the whole head of the instrument: 'it contains the flue and the mouthpiece'. To Hunt (Grove 5) it 'is the plug which stops the end of the tube except for the narrow channel...One meaning of "fipple" is "lower lip" — in this sense it forms the lower part of the instrument's "beak".' To Blom (1946) it is an obstructive block. To Sachs (1913), however, it is the origin of the word 'pipe', deriving from Latin *fibula* and progressing through *fib* and *pib* (presumably as in 'pibcorn'). The *Oxford English Dictionary,* on the other hand, derives it from the Icelandic *flip,* the lip of a horse, and cites as a cognate the Swedish *flipa,* to weep, whimper, whine, slaver, or dribble, all of which might have been deemed appropriate to the duct flute by unsympathetic hearers. The earliest English usage, according to the *OED,* is by Bacon, whose meaning is obscure: 'Let there be a Recorder made with two Fipples, at each end one'. ... in the 1972 supplement [of the *OED*] ... another meaning is cited: 'the sharp edge of the lip or fipple (Schlesinger, 1911). Galpin (1910) used the word in the same sense, but the *Concise Oxford Dictionary* (1982) defines the fipple as the flue (presumably meaning the windway). This confusion over the meaning of fipple was pointed out by Welch as early as 1911. The word, however, continues in use, interpreted as the windway, the lip, the block, or the whole head. To avoid further confusion, the use of the word should be abandoned."

Sorry to take up so much space with this, but I do feel quite strongly that we should not use words whose meaning we do not know, and worse whose meanings differ in different hands. Duct flute is a far better term to use, for all such flutes must have a duct, a pas sageway of some sort, whether between block and roof or, in the case of tongue-duct flutes,
between player's tongue and wall, or in external-duct flutes such as the *suling*, between leaf etc and outer surface of the wall, to lead the air at the correct angle to the lip.

The weakest chapter is that entitled Miscellaneous Fipple Flutes, and my reason for saying this is not because of the use of 'fipple' but because it is an entirely random selection of articles on duct flutes of other types, some of them from our culture (eg flageolet, tabor pipe, gemshorn) and others from other cultures. The selection is so random, and so small, that it is really rather misleading. If you are interested in world duct flutes, there is infinitely more than this, and much of it more important than some of the rather trivial articles cited here. It also tends to be inaccurate. Two examples of this are the description of the *czakan* as a cane recorder; it is neither made of cane nor is it a recorder (it is the Austro-Hungarian version of a flageolet and it is always made of lathe-turned wood). The other is Bob Marvin's Comm 'A Double Recorder', which is not based on 'a 14th-century work of art' but describes the well-known double pipe found at All Soul's College, published by Galpin (*Old English Instruments of Music* -- referred to as 1910 above -- plate 35) and others, and now on loan to the Bate Collection.

These all, however, are fairly minor considerations, and the book is laid out to be an invaluable research tool for anybody working on almost any aspect of the recorder. As I said at the beginning, 'when it is good it is very good', and that is certainly true.

Where it is really horrid is in its revival of the controversy which raged from 1987 about the Ganassi Recorder, and which one had hoped was now safely dead and buried. It was initiated by Lasocki, as he recounts here (p.153ff), and the reason that I call it horrid is that he uses this opportunity to revive it and, by being highly selective in his quotes, to prove himself correct, whereas the documents of the period prove the contrary. He does cite those documents, but unless one looks them all up and reads them again, one is not aware of this, which is why I say he is selective of his quotes.

It is a pity. It was all mostly forgotten, save by those most closely involved, and it would have been far better, far more tactful, just to have cited the relevant articles (for they are relevant to a book of this nature) without comment, and certainly without writing a special two-and-a-half page article on the affair. May one suggest that if this book were ever to be reprinted, those pages could with advantage to all sides be excised, and some of the comments to items 460, 461, and 462 also be removed. For one of the compilers of this book to take advantage to represent his own side in this, or any other, controversy is, let us say, unfortunate, and I am sorry that his colleague in carrying out this task, which on the whole is valuable and a worthwhile addition to any library, especially to anyone working on any aspect of the recorder, did not manage to dissuade him from doing so.
Gerhard Stradner telephoned me yesterday and pointed out that in the front of the CD was the usual leaflet that one gets in CD boxes, and in that leaflet I would find all the information the absence of which I regretted at the top of page 20 in the last Q. So last night, when I got home, I had a look, and he was right. I suppose that I was so concerned with the main booklet, with the text and the illustrations, and the CDs themselves that I never thought to look at the thin one in the front of the CD box. There is no cure for stupidity save to apologise to Gerhard and here and now to continue the review.

For clarity let us call this booklet the CD booklet, to distinguish it from the main Guide booklet. It has the details of every track on the CDs, with their timings as is now usual, and their numbers. It also gives the name of every solo performer and the members of most small ensembles and details of their instruments. The break-down is quite interesting.

On Disc One we have 23 instruments from the museum (including 2 belonging to the Gesellschaft der Musikfreunde), 5 from other museums, 15 from private collections (usually the players' own, one assumes), 9 from early music commercial recordings, some of them purely vocal, and 5 from ordinary recordings. On Disc Two we have 8 instruments from the museum, 2 from other museums, 10 private, 4 early music commercial recordings, and 9 ordinary recordings. I have not counted duplicates (the same instruments used on different tracks), and I may have put some of the recordings into the wrong camp due to ignorance over whether they were early music or ordinary modern instrument.

The commercial recordings include the Hilliard Ensemble, Chris Hogwood and the Academy, Roger Norrington and the LCP, and Nikolaus Harnoncourt and the Concentus. The one ordinary recording which might be considered historical is Richard Strauss conducting his own Till Eulenspiegel with the Vienna Phil. And the one museum instrument which is also a commercial recording (but not here counted as such) is Anton Karas playing the Harry Lime Theme on a zither belonging to the museum and published by Decca.

As I said last time, to hear some of the museum instruments is wonderful. I am considerably less certain of the value of hearing some of the other recordings, other than for musical pleasure. Vocal music is of marginal relevance to a collection of instruments. So is a bit of Schubert's Unfinished played by Norrington and his gang on the usual hotch-potch of more or less contemporary instruments and reproductions from a number of different European traditions. Is a Brahms Hungarian Dance played by the Vienna Phil under Claudio Abbado relevant at all? Or the English Colin Davis with the Bavarian Radio Orchestra accompanying the Israeli Gidon Kremer in Berg's Violin Concerto? At least the Vienna Phil is local and some of the instruments bear some slight relationship still to the historic Viennese tradition.

Every museum has its own idea of what a sound-guide should be, and mine is that it illustrates the sound of the instruments in the collection. If I want to hear Bruckner 4 played on modern instruments, or the Hilliard Ensemble singing Dufay, I can do it at home.

Nevertheless, let us be grateful for the 31 instruments from the Kunsthistorisches Museum. It is wonderful to hear them and let us hope that other museums will follow this example and not only produce good sound-guides but publish them like this, with good texts, well-illustrated, full details (my apologies again over missing this), and beautiful music.
The British Clavichord Society is truly under way. At the first general meeting in Oxford, the society was established, and on the 29th October, at the Friends Meeting House in Blackheath, the second general meeting took place.

Paul Simmonds gave a lecture-recital on the development of the structure of the clavichord and the place at which the Pisaurensis clavichord stood in time. He illustrated his lecture with structural drawings. Beginning with the monochord, he explained how the clavichord gradually developed from this basic structure. He ended with musical illustrations which covered the expanse of repertoire of the 16th century and firmly established the versatility of the Pisaurensis clavichord of 1543.

The recital was followed by a meeting chaired by Peter Bavington who reported on the plans and the progress of the interim committee. It was agreed that free corporate membership should be given to the Dutch Clavichord Society reciprocating their kind offer, and that this offer would also be extended to the German Society. The committee's aim was to organize events for the future, to write the Constitution and to prepare for members' subscriptions.

It was announced that future events would include an illustrated talk by John Barnes under the auspices of the Open University, at Milton Keynes on the 11th March 1995, and a lecture given by Dr. Carl Dolmetsch, at the Haslemere Museum on the 2nd September 1995. The society's newsletter will be published early in 1995 to keep members informed of all future activities.

Thanks were expressed to the various committee members for the work that they had done and in particular to Paul Simmonds who has given so generously of his time and expertise.

If you are interested in the clavichord as a player, a maker or simply as a listener, we welcome you to join the Society.

To join please contact the Membership Secretary:

Sheila Barnes
3 East Castle Road
EDINBURGH EH10 5AP

Susan Alexander-Max
PR officer, interim committee, the British Clavichord Society
As with other social animals, humans seem to need repeated confirmation of their status in the social group. Good manners means acting in such a way that reassures others that one accepts the relative status between oneself and them. When there is a difference in social status, the person with lower status must include appropriate verbal and/or physical signs of servility whenever interacting with a person of higher status. It is taken as an offense if this is not done.

In Europe, the traditional physical signs of servility are initially to adopt a physical position of status that is lower in the position hierarchy than the position that the other person happens to be in at the time, and subsequently to adopt a position of higher status only if given permission by the other person. The sequence of positions from low to high are: flat face down, kneel, bow, stand, lean against wall or high seat, sit and flat face upwards. These can be illustrated as follows:

LOW STATUS

HIGH STATUS

Children are usually exempt from these rules. This position hierarchy helps considerably in interpreting the social relationships between people depicted in realistic early art.

In modern life where clear differences in social status are usually absent, much of this still survives. One plays the lower-status servile role showing deference when entering another's territory, such as their house or office unless this is reversed by social status (like the visitor is the boss). In neutral territory, competence is the usual area of sensitivity amongst social equals, where manners is associated with reassurance that competence is not being put into question.

I've heard a famous leading authority in his field give a lecture on his work with enormous modesty, implying that it was all luck and anyone in the audience would have done at least as well given the same circumstances. He had good manners. Everyone thought that he was a wonderful person even though he was obviously a liar. His position at the top of his field (being on all of the powerful national and international committees) had at least as much to do with his manners as with his brilliant research. Lying in the name of modesty is socially necessary for success as long as there are enough other successful people to sing one's praises.

Good manners is usual in scholarship. Most scholars in each field seek to discover new evidence that can be interpreted in ways that their scholarly colleagues can readily accept as new knowledge. They see themselves as contributors to the rolling consensus in growth of knowledge. Most have never been involved in a controversy in which one party claims that some work of another is not good enough. Most research applies uncontroversial methods in collecting evidence and interpreting it. The results are generally accepted without close scrutiny because they too are uncontroversial. Colleagues are nice supportive helpful people and friends, so one thinks very carefully about pursuing a line of research that could lead to results that would upset any of the others. Like other people, most scholars want to be popular and have a distaste for conflict.

Controversies arise though whether one likes them or not. A number of scholars (like myself) welcome scholarly debates. We want to test and improve our ideas but don't have the confidence to do it well enough ourselves. So we invite criticism from others to help. Useful criticisms bring in ideas and information that had not been considered (or considered seriously enough), and point out errors in logic and judgement (e.g. not being objective enough). We learn both from the criticism and from formulating responses to the criticism. We are objective about our ideas, and if they turn out to be poor, we have confidence that our imaginations are fertile enough to come up with better ones. We are concerned with ideas that can help lead to truth, and the personalities of the others involved are essentially irrelevant.

When we apply the dictum 'do unto others as you would have them do unto you', and offer constructive criticisms (like those we welcome ourselves) to help them to sort their ideas out, we often get into trouble. We are considered by some to be impolite and ungentlemanly, or lacking of manners, etc. Many sensitive people can't be objective about their own ideas, and can't help
considering any criticism or offer to teach them something as a slur on their competence and thus an insult. For self-respect, they seem to need to maintain a public image of consistent faultlessness. Having it written in public that one's idea is inferior to that of another would be upsetting to them. Good manners is to avoid any such upset. This is probably why employing Occam's Razor is currently out of fashion in scholarly publications. The disputes that do occur between colleagues with manners are usually conducted with each participant presenting the virtues of his or her case without overtly showing why it is better than that of the other.

When writing in FoMRHI, I consider that the pursuit of truth in scholarship is what the membership is interested in, and that is of greater importance than personal factors. In this respect I admit to occasional bad manners when I feel it is necessary. It is not that I don't care about the feelings of others, it is just that it is more important for scholarship to advance much more efficiently than to protect the feelings of the sensitive, and for me to be liked and popular.

And now to specifics of Comm. 1301. As Spohr's English is clearly good enough to realise that my comment 'tear him viciously to pieces' was the joke that it was, I wonder what he means by calling it 'revealing'. If he is implying that I enjoy hurting other people, he is very wrong indeed. I've noticed that those who are most sensitive about the possibility of others being unjust or hurtful to them are often very much less sensitive about the possibility of themselves being unjust or hurtful to others.

According to Spohr, most of the inconsiderate and hostile dealing with fellow researchers in FoMRHI (not caring much for their labour, engagement, intelligence and feelings, and treating them like inexperienced schoolboys) can be blamed on Jeremy and me, and we repeatedly inform the readers that we are the true experts, the important personalities and the superior scholars. If this Comm's criticism were directed at a member other than Jeremy and me, I would probably have sent it back to the author because of its unprecedented hostility, attacking people's characters rather than debating issues. He writes about politeness, which from the structure of his Comm. apparently means that one can be as condemning as one likes as long as one minimises the expected hurt by mixing a good portion of praise in with it, spreading the condemnation to as many others as one can (including Jeremy when I am the obvious target), and trying to minimise the importance of the condemnation ("this is only a matter of style"). Politeness seems to be a kind of packaging to give the victim straws to grasp on to that could save face. I deplore hostility irrespective of packaging.

Perhaps Jeremy and myself are a little less modest than most, but that only is because we are that much more honest. That does not make us arrogant or egotistical. We know what we know and we gladly share it with anyone who is interested. We do not stand on authority (i.e. expecting others to accept what we say because we say it). That is something that more sensitive people do. We are always willing to go into how we know what we know, and to admit what we don't know and our errors. We are as honest about our weaknesses as with our strengths. We are willing to debate differences, and when we feel that we have a strong case and show it, that does not mean that we are condescending. Confidence is not arrogance. We are interested in the status of issues, not people, and the status posturing we are accused of is only in the eyes of the beholder.

So what is the reason for this outburst? I suspect that Spohr is upset about the same thing Cronin was: my writing that the Haynes version of pitch history is not viable scholarship. They want to continue believing and convincing others that it is, and so want to consider the question unresolved. They do not want to debate the question of viability, so they get at my lack of courtesy in bringing it up. I admit that it wasn't the best of manners, but it was necessary. After I published my Comm. 442 in 1983, I was being gentlemanly about Bruce's 1985 JAMIS paper by not mentioning it specifically in my 1986 Comm. 683, and not even responding to his 1988 Comm. 891 arguing against my Comm 683. That proved foolish, since when I referred to my Comm. 683 in my Comm. 1063 (on English pitch standards), Bruce wrote in his Comm. 1082 (1992) that 'before he can again convincingly cite Praetorius as an authority for his historical pitch hypothesis, Eph must provide reasoned, specific answers to these arguments [in Comm 891]'. I answered his challenge comprehensively in Comm. 1098, pointing out that valid scholarship had to offer reasonable explanations for all of the relevant evidence, and showed that mine did and that his didn't. Bruce has shown no inclination to come back into the debate. I very much doubt whether his model can be defended objectively. Yet he is still promoting it. I doubt whether Cronin and Spohr question Bruce's manners. Is all fair in love or war or anything else one is committed to?
The World according to Segerman

Many communications published lately by Ephraim Segerman have been devoted to authenticity and scholarship and in all very strong views were expressed on good or bad, superior or inferior, authentic or fraud, integrity or commercial. Segerman's opinions are his good right, however they tend to get the effect of brainwashing and include damnation of other opinions, if not coinciding with the Segerman view. It is therefore humbug to see Segerman write about "Bavington's absolutism" (C-1192): I do not know of many scholars, who express themselves with such absolutism and certainty as Segerman himself.

Segerman seems to define an authentic performance as a performance on historic (in Myers terms, C-1245) instruments and played according to all (his) evidence provided on tempi, loudness, embellishments, etc. If no historic instruments are available, at least a faithful copy must be used. I guess, a tape recorder in Leipzig during the Matthaus passion in 1735(?) would have been best. Right? Well, maybe, it is a definition, but I do agree with Bavington (C-1174) that "all attempt at historical constructions are doomed to failure". In my humble opinion, what count are the composer who wants to tell something to an audience, using the language of music, and two out of the three (the performer and audience) are certainly never authentic! Authenticity is, at best, an ambiguous term, but in my view Bart Kuiken came close when he remarked (recorder symposium, Utrecht 1993), that "one should be honest to the composer", which is not necessarily the same as an attempt to copy a performance of 1735. Would I get an authentic performance if I listen to the above mentioned tape recording of the Matthaus Passion, seated in a comfortable chair, headphones on, glass of port, open fire? I doubt if that was really Bach's intention with this music, composed for a religious text and audience. I am interested in an "authentic" expression of the composer's intentions in the musical language he has selected. To quote Zaslaw: "that is not to say that 'anything' goes. But given the knowledge, taste and ability to keep within boundaries of a specific style, quite a lot goes." Similarly, in my view the instrument maker quoted by Segerman in C-1147, who "started with a basic historical design and then made the best instrument that he could", uses a far more 'authentic' approach than the one with objectives to make a copy accurate to the micron, apparently Segerman's ideal.

I believe that Segerman totally ignores these aspects and that his crusade to keep the word "authentic" reserved for his own private interpretations, becomes rather out of touch with reality, which allows for other views as good as his own.

Did I apply Occam's razor to reach my conclusions and interpretations? Of course not, for Occam has nothing to do with it, nor with many advances made in science since he died. Thus am I not a scholar? No, I am not, but not for the reasons given by Segerman. I would nearly say, in the contrary. According to Segerman's interpretation, Occam's razor must be used to "maximise objectivity and minimise judgement". While my dictionary (remember, I have to look up such things to be sure, being non-English), says that a scholar "is a usually elderly man noted for wisdom, knowledge and judgement". Now my dictionary may well be wrong (it does not accept a female scholar!), but it confirmed my own intuition, and as such I am in the good company of Mr Occam who considered intuitive knowledge as primary and fundamental. But I am really taken aback by the arrogance, expressed by Segerman (C-1276) that "there are also inferior scholars .... whose conclusions are not justified by objective comparison ...." (i.e. do not apply Occam's razor). Fortunately, I am somewhat comforted when he admits that not many scholars share his views.
However, also Occam deserves a honest treatment and Segerman’s attribution of the razor, that “with everything else being equal, one chooses the hypothesis that is simplest” is certainly simply, but also rather simple-minded and subjective. Occam used his razor quite differently, however: he used it “to get rid of unobservable entities the existence of which was not demanded by the data of experience” and in that sense he indeed simplified his world view. Reason, why his razor is also called the “economy principle”: do not assume more entities than necessary (entia non sunt multiplicanda sine necessitate). Segerman’s razor is not used in modern science, simply because it does not provide what Segerman claims it provide (objective truth, whatever that means), nor does it take away the necessity for judgement, the base of all proper science. Occam’s economy principle is used by many in their quest for a knowledgeable base for judgement.

I do not intend to spend more than the one page spent so far on these subjects which get my blood up but have no direct bearing for makers and researchers of musical instruments. It would be a sad day for FoMRHI, however, when Segerman would start to use his razor to cut away “inferior” scholarly Comms.
This year is the 300th anniversary of Henry Purcell's death. It will be marked by an unusually high number of performances of compositions by him and his contemporaries and scholarly papers on these and related topics. One such topic is performance practices at the time. My paper on the evidence on tempo standards should appear in Early Music this year. Purcell himself was responsible for some of that evidence, and what appears concerning this period in that paper is an improved version of Comm. 1129. Another aspect of historical performance practice that deserves more attention is gracing.

The clearest explanations of gracing on stringed instruments in the second half of the 17th century that I am aware of are given in Thomas Mace's Musick's Monument and Christopher Simpson's The Division-Viol. The latter includes a table of graces in mensural notation attributed to Charles Coleman. The same table appears unattributed in John Playford's An Introduction to the Skill of Musick in the Treble-Viol section, and is referred to in the rudimentary explanation of gracing in the Bass-Viol section as applying to that instrument as well.

I first became aware of gracing practices on the flageolet and recorder from Marianne Mezger's article "Performance practice for recorder players" in the Spring 1994 issue of Leading Notes. They seemed very strange to me, and I wrote to her for more information. She sent me duplicates of the explanations of graces on the flageolet in Thomas Greeting's The Pleasant Companion (1652) and the anonymous Youth's Delight (between 1683 and 1691), and on the recorder in Humphry Salter's The Gentle Companion (1683). She also kindly sent me a copies of two tables of graces explained in various recorder sources that she compiled for the edition DOL 250 (Sonatas by Paisible) that she published.

The meaning of 'to shake'
The Coleman table separates graces into two categories: 'Smooth' and 'Shaked Graces'. The verb 'to shake' was quite distinct from the grace name 'shake', since it was used for describing a part of the execution of a variety of graces with different names. It is clear from the descriptions of Simpson and Mace, and the notation in the table, that the verb meant an oscillation between two notes (irrespective of which is a main note or an auxiliary note), where each is played more than once. I know of no early source that clearly includes one oscillation in the execution of a shaked grace. This evidence clearly excludes it.

Many sources, including the flageolet and recorder ones, use the verb in a way that the number of oscillations is not clear. Mezger, not knowing the string evidence, assumed a single oscillation in her interpretations as the simplest. A single-oscillation interpretation of shaking has become a modern tradition amongst some lute players. They have been encouraged in this by the introductions to lute facsimile editions by Robert Spencer. Excellent quality of scholarship in repertoire all too often does not extend to performance practices.

Whether the auxiliary note is above or below the main one, and grace names
It is astonishing that the string sources differ from most of the flageolet and recorder sources by the auxiliary note being in the opposite direction from the main note for shaked graces of the same name. Thus the string shake and the wind beat have an upper auxiliary, while the string beat and the wind shake have a lower auxiliary. This is surprising since many of the leaders in the flageolet and recorder movement were string players, members of the royal fiddle band. They include Thomas Greeting, John Banister and Robert Carr. The only exception amongst the 17th century flageolet/recorder book writers, who defined graces in the strings way, was Carr.

Sancta Maria (1565, but publication was delayed for 8 years because of a paper shortage) discussed several graces but did not include any that involved shaking with the auxiliary lower in pitch than the main note. His contemporary Bermudo (1555) criticised theoreticians who ignored such shakes, saying that it could be very effective at times. Praetorius (1619) mentioned shaking with the auxiliary in both directions, but commented that the one with the lower auxiliary was not as good as the other. It is apparent in the repertoire of the flageolet and recorder late in the 17th century that shaking with the lower auxiliary became dominant. With strings, shaking with the upper auxiliary was
always dominant. This may explain the difference between string and wind grace names, with the name 'shake' tending to always go to the dominant shaked grace.

How such a difference in dominance could have developed is another matter. There may be some psychological or physiological basis for the difference. If it felt most 'natural' to shake on the auxiliary note with a weaker finger while the stronger finger held the main note, this would automatically generate the difference.

The main shaked graces ('shake' and 'beat') and the starting note.

There is some variation in the string sources concerning what the name 'beat' (with a lower auxiliary) meant. The Coleman table shows it amongst the 'Smooth Graces' as a lower appoggiatura. In Simpson's explanation before the table, this is called a 'plain beat', while a proper beat is shaked (the 'shaked beat' is amongst the 'Shaked Graces' in the table). In contrast, Mace's beat starts on the main note and is shaked, in effect a repeated lower mordent. His description indicates that the auxiliary is always a semitone below the main note. This happens to be the case for the 'shaked beat' in the Coleman table, and Simpson says that this was usual.

The difference between Mace and the others on what the 'shaked beat' was illustrates an inconsistency in whether one starts with the main note or the auxiliary note when shaking. It could be a matter of player's choice, and ambiguity in another source could be deliberate. The starting note of a grace could also be neither of these.

Mace called the shake the 'chlefest' of graces, notated 'with a prick before it'. He defined it as starting on the main note. That importance seems to be contradicted by that prick sign hardly ever appearing in the music given in his book. Yet the backfall (upper appoggiatura) sign appears in profusion, and it applies both to the plain and shaked backfall. Since the sign appears mainly on the longer notes, we can expect that the shake was 'chlefest' mainly as a component of the shaked backfall. This is supported by the Coleman table, that does not include Mace's shake.

The flageolet/recorder evidence is that one holds one note and shakes with another finger. This is ambiguous as to which note the shaking starts with. A degree of arbitrariness in this is likely.

Other graces that are related between the string and wind evidence

From the point of view of our expectations, plain (unshaked) upper and lower mordents are conspicuously absent from any of the explanations of late 17th century English graces played on stringed instruments. The string evidence is clear. The ambiguity in the number of oscillations in the wind evidence is not a justification for assuming any importance for them in gracing practices at this time.

The 'close shake' in the Coleman table seems to be a pitch vibrato between the main note and a sharp version of it. According to Simpson's explanation, the shaking finger is as close as can be to the fret that is being stopped for the main note. He wrote that an 'open shake' is the normal one where each finger has its own fret. In some later recorder sources we find these terms used, but referring to whether the finger hole of the lowest-pitched note is left closed or open at the end of the grace. The specific result from the instructions for playing them is that the wind closed shake is a shaked backfall and the wind open shake is a pitch vibrato between the main note and a flat version of it. Another name used in the sources for the latter grace is 'sweetening'.

The 'slur' is mentioned in both the wind and string sources. Its meaning was the same as nowadays, with the wind sources stating that the written notes affected are played on the same breath without the use of the tongue, and the string sources stating that the written notes are played on the same bow or pluck. Thus the written notes are related in the same way as the unwritten notes in a usual grace are. The other graces mentioned in these sources seem to be different for different types of instruments.

Conclusion

Gracing music while playing involves choosing where to use it, how long a grace to use, and what specific grace to use. The above comparison of wind and string graces seems to indicate that, from the point of view of musical interpretation, the specifics of the graces used were less important than the other factors, and whatever falls comfortably under the fingers will often do.
On the Scholarship of String Names – Reply to Comm 1308

Catch’s approach in Comm. 1308 is a very ubiquitous and understandable one. It rejects a presented theory, pointing out that evidence support for it is very tenuous, and calls for more research to find more evidence to support a better theory, and advises readers not to accept the proposed theory (or, to be fair, any other) unless there is more support from evidence. Of course, if he liked the theory, he wouldn’t be making any fuss.

This approach is the one we all use in our normal lives to make judgements of what to accept and reject into our subjective-reality picture of the world. In that picture, there are hypotheses that we accept since we feel they are proven true, and others that we reject since we feel that they are proven false. We feel that other hypotheses are not sufficiently supported by evidence, and we tentatively accept those that we intuitively feel should be true, and reject as unproven those that we intuitively feel should not be true. There are also areas of potential knowledge that are mysteries, and we don’t expect any evidence to appear to support any resolving hypothesis, and we think what we like about them.

Scholars, being people, have the same feelings about what they accept and reject. But scholarship, as formally performed and reported, involves the discipline of a very different approach. Objectively, there is no proven truth, just the best we can now do to approach truth. The process involves, as objectively as possible, to pick the hypothesis that best fits the evidence available. There is no non-arbitrary objective criterion for distinguishing between an hypothesis that is supported by mountains of evidence and one supported by one piece of tenuous evidence. All are chosen as the best scholarship can do at the time to approach truth.

Scholars should be trained to recognise the existence of a formal objective scholarly choice of an hypothesis for the truth in spite of a differing judgement of what the truth is most likely to be. Such differences provide the motivation for research to find or generate new evidence with the object of changing the scholarly choice. They cannot claim that there is not enough evidence for a scholarly choice to be made. There is no criterion for ‘enough’. They are free to claim that there is not enough evidence to convince them of the scholarly choice’s truth, and hope to collect (or that others will collect) evidence to change it to one that they find acceptable.

Etymologists are scholars, and their choices of word histories are often (perhaps usually) based on very tenuous evidence. They can only include hypotheses of word origins in their dictionaries that are based on the evidence they are aware of. If the future writers of dictionaries become aware of the discussions of string names that we have been indulging in here, the choices of our scholarship will appear in their work.

For instance, the dictionary writers apparently never read Dowland (1610). If they had, they might have included the spelling ‘calline’. Also, they would not have chosen the hypothesis that the origin of ‘minikin’ was the obsolete Dutch word ‘minneken’ meaning ‘little loved one’. The evidence they worked with apparently was that minikins were very thin strings, the words seemed similar, and ‘minikin’ was occasionally used for other things that were small. If they had read Dowland, they would have noticed that he did not use precisely this term, though they would have expected him to use it since it was apparently in continuous wide use from well before to well after him. Consequently, when Dowland wrote that a type of string (that minikins were) came from ‘Monnekin’ in Germany, they would have concluded that ‘Monnekin’ and ‘minikin’ are sufficiently alike to be the same, and that according to a contemporary source (and not modern conjecture), the word’s origin was a German place name, most probably Munich.

Dowland was trying to be comprehensive, and apparently to him minikin was a string from Munich. But as with gut strings being made from the intestines of sheep, those less well informed thought that the meaning was more generic. In May 1622 a consortium of musicians (including Orlando Gibbons) applied for a monopoly (not granted) on string making. In the application the strings were ‘called Venice or Romish minikin and Catlin strings’ (see Holman’s 24 Fiddlers, 1993, p.49).

Dowland stated the places where strings came from whenever he could. When he could not
associate a place of origin with a type of string for sale, he said 'we call them ...'. This applied to 'Gansers' and the 'Catline' part of Venice Catlines. If these names were reasonably new at the time, they most likely would have been place names (since all others were). But they were very old, and if they were place names originally, that was forgotten (as minikin was by many).

The earliest source that distinguishes between string types that I know of is the manuscript of lute music by Vincenzo Caprilia written by his student Vitale, dating from about 1517. A modern edition (1955), was edited (with the Venetian dialect translated) by Otto Gombosi. Vitale mentioned that there were 'corde da ganzer' which were thicker on one end than the other, and 'corde da monaco' that were of uniform thickness and give more ('le camina plu'). Gombosi translated 'ganzer' as 'hooker', but wasn't sure about it, while he translated 'monaco' as Munich with confidence.

So we know that Munich was a source of lute strings then, and all sizes appear to have been made there (since the uniformity particularly required of minikins and the elasticity particularly required of catlines were both mentioned). This seems to still have been the case in 1542 when the Earl of Rutland (Woodfill, 1953, p. 268) bought 'menekyns' for lutes and 'bressell' strings for viols. But by 1553, Sir Thomas Chaloner (Woodfill, p. 256) bought 'Mynynks' strings and 'Catlyns' for lutes. This seems to imply that bass strings from Munich were no more available, and they were replaced by 'Katlyns'. My hypothesis is that either the Spanish contracted to buy all of the bass strings made in Munich (see Comm. 1289) or the specialist bass string makers from Munich transferred their manufacture to the Barcelona area.

Since the name 'menekyns' was in use before 'Katlyns' appeared, and 'menekyns' was a place name, this is evidence favouring 'Katlyns' being a place name. The model presented in Comm. 1289 is consistent with the Spanish historical situation and so is evidence in favour of 'Catalan' being the original meaning of catline. Other associations with the term meaning 'unusually flexible thickish string' or 'kitten' ('catling') could easily have developed later when Catalonia ceased to be where these strings came from.

Similarly, the fact that Brussels is a place name is evidence supporting the word similarity for it to be the meaning of 'bressell' strings (Woodfill had no doubt about it). Only the origin of 'ganzer' or 'ganser' remains undetermined. Vitale wrote that they were thicker on one end than the other, and Dowland wrote that they were not strong enough to be first strings. One may be tempted to consider that it meant 'whole' in a Germanic language, perhaps implying that they were made of whole guts. But according to Perulfo (Comm. 1307), Roman treble strings were made of whole guts, and Dowland didn't complain about them. This, and it not being a place name, are evidence against this meaning. Gand is an alternative name for Ghent or Gent (in Belgium). I wonder what someone or something from Gand was called in the 16th century. Any further ideas?.

Catch (or anyone else) is free not to accept any of my hypotheses about string history and names. I claim that each is the scholarly choice, being more consistent with the evidence than any alternative that has so far been offered (in some cases none other has been offered). The appropriate procedure for those that don't like any particular hypotheses of mine is to offer an alternative one that at least equally well fits the evidence. Finding more evidence would be very constructive. It could easily change the scholarly choice (because we have so little evidence now), and we would then be closer to historical truth. Though a scholarly choice is particularly vulnerable if it is based on little evidence, this does not detract from its validity at the time. That vulnerability decreases as research attempting to change it fails to do so.

At the end of Comm. 1308 is a quote from Howard Mayer Brown: "What I should hope to teach is how little we know and how difficult it is to know it. One should be exceedingly sceptical about whether we know anything and how we know it and of the way dogmas arise". It is very true since it refers to the 'proven' knowledge that we accept in subjective reality. This has nothing to do with the objectivity of scholarship where all scholarly choices are knowledge, so we know something about everything we have objectively studied. Instead of being unhappy about how ephemeral scholarly knowledge can be, we should rejoice that changes in scholarly knowledge are closer to truth. Brown argued against the arrogance of our believing that we already know the truth, and because of this belief, dogmas arise because we will not look at challenging evidence or valid alternative hypotheses.
Roped Gut Bass Strings - A 16th C Reference

Recently, while reading through an English translation of a 16th C technical treatise about machines of war and other mechanical devices, I was amazed to come across a direct reference to the construction of thick strings used for bass viols. This work was originally published in 1588 by Agostino Ramelli, military engineer in the employ of Henry III of France at the time. It contains many detailed engravings of ingenious machines such as pumps, mills, winches, coffer dams, fountains as well as military machines (1).

In chapter 190, Ramelli describes two versions of a trebuchet, a war machine designed to propel heavy objects a considerable distance. The trebuchet is simply a strong beam pivoted in a support frame with a massive counterweight on one end and a cradle to hold the projectile at the other. An essential part of the machine, apart from a release mechanism, is a resilient buffer which is employed to arrest the downward motion of the beam and counterweight after release causing the projectile to be forcefully ejected and also to prevent the force of the impact destroying the frame.

This buffer is made from a double loop of rope stretched across the frame and tensioned by twisting the ropes together torniquet fashion, thus:

This part of the machine is described in the text as - "a thick double rope made in the same way as the thick strings of a bass viol ....... it should be tightly twisted and made taut."

I am no string maker but, as I understand it, the roped gut bass strings manufactured today and called catlines are made the same way as a multi-strand rope, the individual strands being bundles of fibres that are twisted before being wound together as a rope - the resulting assembly of twisted strands being stable with no tendency to unwind. Ramelli's observation, if the English translation is true to the original text, implies that the thick gut strings of bass viols were made up from the equivalent of four modern catlines simply twisted together to form a complex rope which I imagine presents no problem to manufacture today if a little costly.
On the other hand, Ramelli's statement may mean that the ropes of the machine were made in the same way as the strings of a viol i.e. from gut and constructed like a rope.

Whichever way is chosen to interpret this passage, I believe that it is positive evidence confirming roped string construction was being used, at least on viols, before the end of the 16th C. Historical information describing string construction from this period is sparse and ambiguous and I do not recall having read about any references until now that clearly support the proposition that roped gut strings were in use during the Renaissance in Europe.


POST SCRIPT

In my hasty preliminary reading of the Agostino Ramelli book, I had overlooked some additional information concerning the above.

In chapters 191 and 192, Ramelli describes two other machines of war designed to throw projectiles. The motive power for these machines, unlike those described in chapter 190, is an arrangement of twisted bundles of rope providing a powerful torque to an actuating arm which, in turn, propels the projectile.

At the end of both chapters 191 and 192, Ramelli adds the following notes: "If the ropes are made of the same material described in the preceding chapter, they will be much better than if made of any other material" and "Note that if the ropes are made of the same materials as that described in Chapter 190 they will be much better and will have a greater effect than if made with any other kind of material".

As far as I can determine, the only reference to the ropes themselves in chapter 190 is that they were "made in the same way as the thick strings of a bass viol" unless, of course, some information has been left out of the original text relating to rope material.

Therefore, the twisted ropes used on Ramelli's machines, were made of the same material as the thick strings of a bass viol of the period.

Does this mean then that the ropes of the machines were best made of gut? If so, why does Ramelli specifically mention the similarities to thick bass viol strings rather than gut instrument strings in general? It would seem that the thick bass viol strings of the late 16th C were quite different from other instrument strings of the time. The question is, what made these strings different from other types? Is it possible that thick bass viol strings were made of a fibrous material other than gut - sinews (tendons) for example. (I have vague recollections of reading in the past that the early war machines of the type described by Ramelli were powered by twisted bundles of sinews).

While Ramelli seems, in his statements, to be primarily referring to the material of his ropes, I still believe that his analogy extends to include the similarities of the construction of the rope to a thick bass viol string. Indeed, if it were the case that gut was the preferred rope material then the similarity between the rope and a viol string would be in the way they were constructed.

More ambiguity I'm afraid!
A number of years ago, on a visit to London, I purchased some silk instrument strings from a shop selling traditional Chinese musical instruments. As I recall, the strings were for the Chinese lute or pipa, a shallow bodied fretted stringed instrument played with a plectrum. This instrument is, apparently of ancient origin and was originally strung with silk strings but is now strung with nylon overspun strings. I assume, therefore, that the strings that I bought were of traditional construction and most likely had been made in this form in China for centuries. The strings have been sitting in my workshop, unused since the day that I bought them and I decided the other week to take a closer look at them.

The strings were made up in coils of about 50ft maximum length with string diameters of 0.51mm, 0.56mm, 1.07mm and 1.1mm, these being the only sizes in stock at the time of purchase.

The strings were well made and even, a pale amber in colour (similar to gut strings) and springy yet flexible. On examining the strings under low magnification, it could be seen that they were made like a multi-strand rope i.e. they were the silk equivalent of a modern catline string.

By twisting each string in the opposite direction to the way in which it had been wound, it was possible to readily separate the individual strands for examination. The smallest diameter string was made from three strands twisted together and the largest from four.

Each strand was made from a multitude of individual silk fibres gathered into a bundle and formed into the strand by twisting. The twisted strands were then wound together to form a rope which was the form of the finished string.

Separating each strand into its individual fibres required some force as the fibres appeared to have been combined together with some kind of glue or varnish as a binder. Under high magnification, small particles of an amber coloured material could be seen adhering to the fibres. (This could also have been debris remaining on the raw silk after processing?)

If the strings in my possession are of traditional construction, then it is possible that the catline string had been invented by the Chinese many centuries before the Europeans began using similarly constructed strings in gut for their instruments and that the whole idea originated in China. Indeed it may be that roped silk strings from China were used on European instruments prior to the 16th C and that the gut version came about in an attempt to make a string more suitable to the climate of Europe or, perhaps, a more durable or cheaper string than the silk import.

Just a thought!
All at Sea with the Ship’s Cat

Re: Comms. 1284, 1289 and 1308, and some other speculative observations concerning Catlines and instrument strings made like ropes.

It is likely true that musical instrument strings made like cords (small ropes) were being used in Europe by the 16th C. and that it is possible Catlines might have been strings of that type but, associating the rope of a ship’s cat tackle with a Catline string as evidence of rope like construction of the latter, may be a bit of a red herring although, as is proposed later, the connection may not be entirely irrelevant.

Anthony Deane, Master Shipwright at Portsmouth, in his treatise on ship design of 1670(1) included a comprehensive tabulation of the names, sizes, lengths and weights of all cordage used to rig a British warship of the period. Rope sizes (stated in the usual convention of circumference in inches) measured from 7/8 inch to 17 inches. In Comm. 1254 it was suggested that ropes called lines were restricted in size to less than one inch. It is apparent, however, that no such demarcation was known to Deane for the smallest rope size is given as 7/8 inch and lines (Clewlines, Buntlines, Leechlines and Bowlines) ranged in size from 7/8 inch to 4 1/2 inches.

Deane listed Cat Ropes, ranging in size from 2 1/2 inches to 5 3/4 inches, but made no reference to “Cat Lines”. The cat rope was part of the block and tackle (the cat) used to hook and lift an anchor from the waterline to the cathead (a short beam projecting outboard) so that the anchor might then be swung alongside the gun ports and secured before putting to sea (see Fig 2). The upper pulleys of the cat were built into the cathead and the end of the beam was often decorated with the carved mask of a cat. To “cat the anchor” was to hook and raise the anchor to the cathead.

As Deane did not discuss rope construction, we do not know, from his account, if cat ropes were specially made to be resilient (extra elasticity being the reason for the proposition that gut instrument strings might have been of roped construction). The technology of making hemp cordage for marine applications, however, is well documented elsewhere and the information readily accessible. From this information and knowledge of the duty required of a cat rope, we can be fairly certain how a cat rope was constructed. To digress for a moment:

Hempen ropes were were built up from a basic yarn spun from the short, hemp fibres which were then twisted together to make a uniform, coherent bundle of fibres known as a strand. Three or four strands twisted or laid together formed a basic rope. The direction of twist of strand lay was counter to that of the strands themselves in order to create a stable assembly with no tendency to unwind. The resultant rope was pliable (easy to bend), strong and, to a certain extent, resilient (able to stretch elastically) by virtue of its construction.

The pliability of a rope could be varied during manufacture by adjusting the degree of compression of the fibres and degree of twist in the lay — known as hardness. A hard rope was less pliable than a “soft” rope but was able to resist absorption of water better. Hard ropes were, therefore, used for duties such as anchor cables, where the rope was subject to water immersion.(2)
A rope made from three or four strands was called a hawser-laid rope or simply, hawser. Hawsers were rarely made from more than four strands and did not exceed 10 inches in size.

Larger ropes, up to 24 inches in size, were made by laying three or four hawsers together (again, with counter twist). These ropes were called cable-laid ropes or simply cables. A basic cable made from three hawsers therefore, contained nine strands. (see Fig 1).

Cables were made very hard and compact and lacked pliability but were resilient (3), a useful, shock absorbing, feature for anchor cables and the like, subject to impact loadings. As cable construction was water resistant, cables were also made in sizes down to an inch.

Four strand hawsers and cables were often also made with a centre core of fibres laid straight which acted as a non load bearing filler. (3). (see Fig 3). These were known as shroud hawser-laid or shroud cable-laid.

To return to the cat rope, such a rope would have to be pliable (to negotiate the relatively tight radius of a pulley) and strong (to lift an anchor and its cable weighing a ton or so). The cat was not subject to water immersion. This duty was suitable for a simple three strand hawser which, if it was specially made at all, might have been made softer than usual for extra pliability and strength (but reduced resilience). (4)

Remaining unconvinced that "catline" was anything to do with "cat rope", I looked for other words in nautical terminology that incorporated "cat".

"Cat holes" - were holes in the stern of a sailing vessel through which cables were passed to draw the ship astern or to secure the vessel.

"Cat's paw" - was a double looped hitch made to receive a lifting hook.

"Cat-o-nine-tails" - was a 'rope's end' used to flog errant sailors. Its name suggests that it may have originated from an unwound, nine strand cable.

"Cat" (meaning the feline animal) appears in old English and is also to be found in the Celtic, Slavic, Arabic and Finnish languages but is of obscure origin. (5)

The use of "cat" in the above words, seems to have an association with ropes (particularly cables) or with cordage being pulled, stretched or otherwise put under great tension (as opposed to ropes used for, say, tying up a sail) or perhaps for hooking or catching something with a rope (e.g. cat tackle). If these are obscure meanings of "cat" they may have originated from the observed stretching or extending behaviour of a cat or its ability to catch and secure its prey with its claws?

Other examples might be:

"Cat's cradle" - an arrangement of cords stretched tight between the hands.

and, of course:

"Cat gut" - an instrument string stretched to breaking point, made from gut.

If "cat gut" means an instrument string made from gut, as it surely does, then "cat line" (or "catlin" or "catling") might mean an instrument string made from flax fibre? ("line" being an obsolete word meaning flax or flax fibre - "lin" in French or old English. "Ling" is Scots for line (the rope) e.g. Bowling v.s. Bowline). (5)

I am not qualified to say if a successful instrument string might be made from flax but, if it could, then it would have been made like a hemp rope (hemp and flax fibres being relatively short and having similar microscopic features). As spun flax thread, presumably, does not have the same inherent
2.7

elasticity of gut (or silk, that other instrument string material), the cord could, of necessity, have been made like a cable for maximum resilience. Such a cord would also have been hard and springy like a gut strand.

Flax grown in the favourable climate of Northern Europe could yield fibres as fine as silk filaments and spinning of flaxen threads was an ancient technology by the time of the Renaissance. The manufacture of instrument strings like tiny cables would have presented little difficulty and such strings may have been an economically viable alternative to gut or silk for certain instrument string applications.

Furthermore, such a string construction might easily lend itself to the manufacture of loaded instrument strings - fine annealed wires being incorporated as a core of a shroud cable-laid cord or incorporated into the strands themselves. The spinning of metal wires into fancy cords for decorating clothing was an established industry described by Diderot. (6) European fairy tale traditions include stories of flax workers spinning gold thread ...

To return to Deane, his inventory of ropes includes "viol cables" or "viols". These cables were used to engage the anchor cables on the largest 1st and 2nd Rate warships to aid in the release of an anchor stuck fast to the bottom. "Viol" could be a corrupt spelling of some other word with no connection to the musical instrument but the possibility that a viol cable described the construction of a Viol string seems to echo the observations of Agostino Ramelli concerning the huge, shock absorbing, ropes (as thick as a man's thigh) fitted to his catapults (no! no connection with cats!) and other war machines. (7)

In conclusion, it would appear that there is some evidence to support the view that instrument strings of roped construction were in use by the 16th C in Europe (for bass viols at least). We do not know if these strings were made from gut or some other material and we do not know if Catlines were strings of this type. Dowland (8), does not say what his strings were made from although his use of adjectives such as "clear", "old", "fresh", and "rotten" implies a material of animal origin - probably gut. Perhaps it was not always possible, short of tearing a string apart, to say what material it was made from, particularly if the strings were dyed different colours?

If it could be confirmed that Catline strings were used for the largest strings on bass viols then we would know not only that Catlines were made like a cable (Ramelli) but that roped basses were also used on lutes (Dowland).

The problem is that there seems to be no evidence to confirm that gut strings were ever made in any other way than by simply twisting the gut fibres into strands (see Fig 4). Silk strings, on the other hand, were made like miniature hawsers by the Chinese and silk strings were used both by the Chinese and Arabs on their lutes from very early times (9). In Europe, however, there does not appear to be any record of silk strings having been used on instruments although silk filaments were used later as a core for overspun strings.

Of flaxen strings or loaded strings made from flax there seems to be no record.

The proposition that early gut strings were of much greater elasticity than we can imagine today - the breed of sheep, its age, its food, the climate and its physical condition and the way gut was prepared - all being critical factors that might have resulted in a very elastic gut fibre - has its attractions.
For a string made from such a material, it may not have been necessary to use the expedience of roping the gut strands together to gain additional elasticity. The expertise on how to produce this special gut, unlike rope making technology, could easily have been kept a closely guarded secret that has now been lost. Indeed, it may now be impossible to replicate the early gut strings because the breed of sheep providing the gut, like many other old domesticated breeds of animal, could well be extinct - victim to changing demand and economic conditions. Such is progress.

Notes:


(2) "...as cable-laid ropes are very hard and compact, ropes of no very great size are made in this way, if intended to resist the action of water" Tomlinson's Cyclopaedia of Useful Arts, London 1866.

(3) "In old worn out ropes the core is always found to be broken in consequence of the stretching of the strands: for the strands being twisted spirally, and the core straight, the strands will give more under load than the core, which cannot therefore be relied upon for adding strength to the rope; but it assists materially in keeping the strands in position during manufacture of the rope by hand" Dictionary of Engineering by Byrne and Spon, London 1874.

(4) A rope on a 19th C pulley system in my barn is a 3 inch, three strand hawser-laid rope, with 16 yarns per strand. This rope is very pliable and used with 6 inch diameter pulleys.


(7) Under CATAPULT in the Byrne and Spon "Dictionary of Engineering" 1874 is a description of a war machine taken from the German edition of Ramelli's work published in Leipzig in 1620. The relevant passage relating to viol strings is rendered by the 19th C translator as "The cords should be made of the same material and in the same manner as the counterbass strings of a violoncello". It should be noted that Ramelli's ropes were simply twisted together - an unstable assembly that would unwind if not restrained at the ends. It would be possible to use such an arrangement on an instrument but, as it would require that the string be twisted up on the instrument, it is not very likely that this exact arrangement was a practical proposition. Ramelli's rope would have looked just like a cable made from four hawser - perhaps this is what Ramelli meant?


(9) The Chinese lute strings in my possession are of three and four strand construction - just like a tiny hawser-laid rope. Presumably, the inherent elasticity of the silk filaments did not require these strings be made
There are many references to the use of silk lute strings by the Arabs in "Studies in Oriental Musical Instruments" by Henry George Farmer, The Civic Press, Glasgow, 1939 - translated from texts dating from the 9th to 14th Century, for example:

"With silk strings, they should be white, smooth, of equal gauge and well finished. These are boiled in water and ashes, and are then washed two or three times in pure water and dried in the shade. The strings are then twisted into the following gauges ... made from 64 threads, 48 threads, 32 threads, 24 threads and 16 threads...A paste of moderate consistency is then made of gum and a little saffron. This is rubbed on the strings with a piece of linen until it has penetrated into all the parts, when the string is dried",... "silk when stretched taut, is finer in tone than gut"...

"these strings require a tautness, on account of their high pitch, which one or two strands of gut are not capable of sustaining".

Tomlinson's Cyclopaedia of Useful Arts, 1866, describes gut string manufacture in 19th C.London. It is stated that Italian (violin) strings were much better than the English strings and is suggested that this was due to the different way in which the sheep were fed - emaciated sheep being better than fat.

Also, the following quotation from Otto's 'Treatise on the Violin' is given:

"The best strings which have come under my observation are those from Milan. The Milanese strings are as clear and transparent as glass. The third string should be equally clear as the first. They must by no means feel smooth to the touch, for they are not ground or polished off by any process, as other manufactured strings are. If a good string be held by one end in the finger and opened out, it will recoil to its former position like a watch spring ......; those which are of dull and opaque appearance are useless. Their elasticity is after all the best criterion, as no other strings which I have tried have that strength and elasticity for which the Milanese are so much esteemed".
**Fig. 1**
Construction of a cable-laid rope made from three, three strand hawsers.

**Fig. 2**
Sketch of ship's anchor and anchor cable suspended by the cat from the cathead and swung into position for stowing.
Fig. 3 above
The manufacture of a cable-laid rope from three hawseres and a shroud cable-laid rope from four hawseres. (Diderot)

Fig. 4 left
Gut string manufacture. Twisting and polishing the gut strands. (Diderot)
In the course of designing an "English Theorbo" with its stepped nuts on an extended neck, as described and illustrated by Thomas Mace in Musick's Monument (1676), I have re-examined the measurements given for this kind of instrument in the Talbot MS rather more closely than before. (Christ Church Library Music MS 1182, reprinted in an abridged form in GSJ XIV pp.52 - 68 by Michael Prynne). As a result I believe I have discovered an anomaly which sheds a different light on the problems of reconstructing this type of lute. Previous discussions of the string length of Talbot's English Theorbo, such as Bob Spencer's articles in Early Music [Oct 1976] and FoMRHI Comm.337, have given the length of the fingered strings as 88.5 cm [Prynne wrongly calculates it as 91 cm], which is really rather long for the repertoire and which, when extrapolated to the other half of Mace's lute dyphone, gives a string length for the twelve course two-headed lute of 75.6 cm, which is far too long for all the available tunings or repertoire for that instrument. Indeed it would turn that lute itself into a theorbo, since 75 cm is reckoned by most makers to be the point at which theorbo tuning starts to become necessary. This would be especially odd as the English two-headed lute measured in Talbot is given as having a fingered string length of only 59.7 cm. However the string lengths from the Talbot MS are themselves extrapolations, because he does not give this measurement directly but instead gives a series of running measurements from which the string length can be deduced. The problem is that, because of this arrangement, any doubt or ambiguity about any single measurement or measurement point creates doubt and ambiguity for all the other measurements in the chain.

Talbot usually ends his running series of measurements by adding them all together and giving that total as the total length of the instrument, however in this case he has left this space vacant, and this was what first raised my suspicions that all was not quite right. I therefore drew out all the measurements full size on a large sheet of paper and it became apparent that the body of the lute was exceptionally long and the neck so short that only seven frets could be fitted to it, even though he specifically notes that English Theorboes have nine or ten frets to the neck and indeed this is required for the music of the period. All the other measurements looked very plausible. However the way this part of the manuscript is laid out implies that the diameter of the rose is included in the running total, whereas in some of the other instruments he measured, such as the eleven course lute and the angel lute, he has given the rose diameter as a separate measurement not included as part of the total length.

If we now recalculate the measurements for the English Theorbo on this basis, leaving out the 11.4 cm diameter of the rose, everything falls rather neatly into place. The string length comes down to 77.1 cm, an entirely normal small theorbo size, the body of the lute resumes a normal proportion in relation to its width, 9 frets on the neck become possible, as Talbot says is the case, and, by extrapolating this new string length onto the two-headed lute half of Mace's dyphone, we get 65.7 cm, which is much more in keeping with the repertoire and other paintings of such lutes. That this is a possible mistake for Talbot to have made is reinforced by
the many crossings out and alterations throughout the rest of the manuscript. His failure to complete his running total for this instrument could, on this view, point to a realisation that the measurements did not add up properly and, maybe, to an intention to sort it out later. Three hundred years later, perhaps it has been done!

I have now made a theorbo for Lynda Sayce based on these re-calculations, with a fingered string length of 78 cm and stepped nuts along an extended neck and it has proved to be an entirely workable instrument, with, as we had assumed to be the main point of this type, a very smooth transition from the fingered strings to the extended diapasons.

**FoMRHI Comm. 1322**

Geometrical Design of the Lute Belly ....... Roy Chiverton

Whenever Eph Seegerman goes on about Ockham's razor, I (mentally) stand up and cheer. Call me a minimalist if you will (I don't at all mind flattery) but I have never seen the fun in having to have a variety of hypotheses in order to reach a single conclusion. Particularly am I reluctant to follow those who find several different inches in a single simple outline. Does the following help?

To make an ellipse, as every schoolboy knows, you stick two pins in a board, put a loop of thread around them and run a pencil around inside the thread......If you put three pins in a board in the shape of an isosceles triangle and a thread round them, - you get the outline of a lute belly.

Vary the pin positions and thread length to get a great range of shapes.

We tend to think of lutes as coming to a point at the neck end - but was this really the case? I can't do the maths for this outline yet, having had to leave maths behind when I went into the Sixth Form but I'm consulting an expert. In the meantime, it seems to me that the shape the pencil point traces out doing as I suggest is so complex you'd need a lot of different inches to explain it away in what has become the usual geometrical manner.

I can't do violin (less corners) and guitar shapes unless I hypothesise four pins and small captive rings on the thread so that you can press inwards as well as out, or alternatively having the pins actually piercing the thread, but this is multiplying hypotheses, so I'm not keen anyway.

Comments, anyone?
FAMILY: Ebenaceae

Four genera (Diospyros, Euclea, Royena and Tetraclis) make up the Ebenaceae family with over 300 species. The main genus is Diospyros from which nearly all the ebony timber is cut. Widely distributed in tropical and some temperate forests around the world, some of which are cultivated for their fruit (eg. date plum, kaki plum and persimmon), their timber is often of little importance.

We generally think of ebony as being black, but most Diospyros species produce only a streaked or mottled brown heartwood, often narrow in comparison to the sapwood.

TRANSITION OF SAPWOOD TO DARKER HEARTWOOD: This occurs as the trunk increases in diameter and the older wood no longer functions in conduction and storage. The living cells in the wood die and in many diospyros species results in a deposition of a darker-coloured substance; thought to be the transformation of lignin into ulmic acid which afterwards becomes decarbolixized, a process similar to fossilization.

AFRICAN EBONY: Diospyros crassiflora (West Africa)

Though there are many species of Diospyros in Tropical West Africa, we concentrate on two species; Diospyros crassiflora and closely related Diospyros dendo.

THE TREE: An understorey species found growing among sapele, utile, bubinga and many other large West African trees. Widespread and found in the southern forests of Nigeria through Cameroon and Zaire into Gabon and the Congo. Small to medium-sized tree with a height of 25-50 feet and a relatively straight trunk, with a diameter of 16-36", occasionally 24 feet before branching. A fairly quick growing Dicotyledon (Hardwood) tree, reaching maturity in approx. 60-80 years.

THE WOOD: Heartwood varies from jet black to primarily black with very fine short dark grey-brown flecks to mottled grey-brown with broad bands and streaks. Sapwood varies from 3" to 7" wide, being white when fresh felled and changing to bluish or pale reddish brown after exposure to light and air.

Straight to slightly interlocked grain, occasionally curly. The texture is fine and even. Growth rings are present but faint with between 4 to 8 per inch. Mineral deposits are rare in African ebony and more common in Indian and Sri Lankan.

DRY WEIGHT: 1030-1200 kg/m3, 64-74 lb/ft.3, 61b/BF

Specific gravity: range 1.03-1.20, average s.g. 1.10
EBONY

HARVESTING: Ebony has been exported from West Africa in the form of billets for over a century, with the only advancement being the use of chainsaws rather than an axe. Usually a team of four local men would work together in an area, with one chainsaw. The men walk in to the forest and locate a mature tree (young trees do not contain enough colour to be worth felling) and once felled, it is cut into manageable pieces (billets), which are then carried by hand to the nearest track or road. This is the main reason you will not see large sections of West African ebony, as it is not possible to carry these by hand. There are no permanent tracks made for the extraction of the ebony and in the space created, the dense forest shortly regenerates. We have now instructed the teams that cut for us to seal the ebony with PVA glue shortly after cutting, so as to protect the ebony from cracking in transit and avoid waste. It is not a common tree but is by no means rare, found growing over very large areas, it is difficult to extract any large quantities in a short time span, ensuring in West Africa that large companies cannot over exploit this species of ebony.

SEASONING: The wood dries fairly rapidly, naturally, though it is recommended to slow down this process to avoid surface checking. Air drying should be done with reduced air flow and in cool conditions, with no direct sunlight. Both the temperature and air flow can be allowed to increase, as the moisture content reduces in the wood. It is always recommended that the wood be cut close to its finished size for drying, as degrade is more likely in larger sections or billets. Kiln drying can be very successful when a mild schedule is used. However, avoid high temperatures and low humidity in the latter stages of drying. It is advisable to seal the end grain with either hot paraffin wax or PVA glue. Once seasoned, ebony is reasonably stable in use.

WORKING PROPERTIES: Conversions of wet ebony causes only moderate dulling of cutting edges, dry ebony is more severe. It is fairly easy to work with sharp hand and machine tools. A reduced cutting angle of 20° on planers is recommended, an easy wood to work on the lathe. Gluing properties are good but pre-poring is required for nailing. The dust irritant factor of African ebony is low, whilst Indian, Sri Lankan and Macassar ebony are high. Dust extraction should be used for sawing of all ebonies, especially when dry. The wood is capable of an excellent finish, though fine grade paper is required to avoid visible finishing marks.

USES: The best quality black ebony is used for guitar and violin family fingerboards, saw cut veneers and some woodwind instruments. Striped ebony is popular for brush backs and woodturning. All types are used for snooker and billiard cue butts but black is usually more popular. It is also used for very high class cabinet making, piano keys, walking sticks and inlays.
Figures for following Comm.

**FIG 1**
Bridge angled up in the bass side.

**FIG 2**
Usual bridge placement.

**FIG 3**
Experimental 'J' bar sections,

Annular rings vertical & horizontal placement.

**FIG 4**
Bars placed for treble (extant).

**FIG 5**
FoMRHI Comm. 1324

G. Mather

LUTE: ANGLED BRIDGES, ‘J’ SHAPED BARS, FAN BARS AND BELOW THE BRIDGE TRANSVERSE BARS.

...and with reference to Com 687 by Eph Segerman, and Com 1237 by John Downing.

You’ll recall that Laux Malers Lutes were amongst the most highly prized (particularly by the French) for their sweetness of tone, and that they amongst others had the low bridge position which favours a quieter purer sound.

ANGLED BRIDGES

I’m of the opinion that the angled bridges referred to in L.S.J. XXVPart1, 1985 by David Van Edwards, where positioned like that (a) to an original customers requisite, slavishly copying other makers, or (b) to a makers idea of a nicety of his personal tone preference, or (c) with the three degrees tilt of rose and bars as well, to confusticale us all!

The instruments I find generate more plum in the bass for this bridge angling up, or to put it another way, some plum would be regained, if it had been discovered lost (at stringing up) by the addition of J or Fan bars misplaced or carrying too much wood. It is easier to alter the bridge position for tone than it is to remove a soundboard and adjust the barring in the vicinity of the bridge, and perhaps certain makers of the past did just this. Michael W Pryne in 1960 noted that a wide range of bridge position occurs by certain of these old makers. (a feeling for tone here perhaps eh?)

Angling the bridge as at fig 1 here is opposite to the usual practice and requisite for the diameter of string differences from treble to bass side, ie re-mass’dia. It is usual for most instruments to have the bass strings slightly longer as at fig 2 here. However the bridge could well have been moved to a position in the vicinity of a ventral segment in the soundboards resonance deflection; See my Com 260 Fomrhi Bull No 18 page 54 1980, and intonation wouldn’t be much of a problem because the bass courses were seldom fingured high up the neck anyway.

THE J BAR

Now after thicknessing trials on several soundboards, and experiments with different barrings upon one same soundboard, I find that the curved J bar links tones, allowing the belly to flex less, restricting movement of the bass side, near the bridge particularly, to an enhancement of lighter tone! It takes away some bass response and tightens up as it were the bass upper partials. It refines the tone.

Now NB. thinner plates are pertinent to bass tones, and thicker plates will nullify bass tones. This, can be proved by cutting several pieces of the same wood say 4" x 6" but of different thicknesses eg 4" x 6" x 3⁄4", 4" x 6" x 1⁄2", 4" x 6" x 1⁄4", 4" x 6" x 1⁄16", and carefully tap toning them, or by simply throwing them onto a concrete floor and listening to the pitch relation of the different thicknesses. In each case a fundamental tone, plus some harmonics are heard but the fundamental is not necessarily always the most dominant.
The earliest Lutes do not exhibit this J bar refinement. A thicker soundboard too in this vicinity behaves similarly and this feature is also recorded on some Lutes built after Arnault of Zwolle’s.

On Ephs para 2, Com 687 I would argue and say that; the J bar is a greater contributory factor effecting stiffness where it is placed, when compared with the other bars above the bridge. This is one reason why it has to be kept small in section! Regarding its section proper however, it, (like the Fan placed treble bars) is a tone device, and not just something to prevent splitting! Its placement of annular rings in sawing section, and its mass See Fig 3 can be critical to the over tones which are allowed to develop, as it prevents the overall tone from being full in the bass. eg if it is too hard the basses go hard and more metallic. I find it does not however, prevent any hump occurring below the bridge, caused by the string tension, for indeed on soundboards which I’ve measured to be 1 mm thick, the J bar itself bends and twists in flexation along with the soundboard, this when strung experimentally devoid of other bars, to search for deflection with dial test indicators.

**FAN BARS** (standard usual types)

Concerning tone re Fan Bars - from experiment with spruce, pine and various ‘non authentic’ woods for bars and one same soundboard, in particular, I discovered that the plangent tone of the instrument was made better if I used for the bars below the bridge, a wood with a similar density, to that in the winter growth rings grain of the soundboard, or harder. That is to say: that the resonance appears to travel better in the winter growth part of the soundboard and more particularly so when these are linked together fan bar fashion See Fig 4. These thin hard bars assist in high frequency response, as the soundboard at these points is prevented from flexing slowly. The closeness of the treble bars and their radial fan setting ties together the ANNULAR grains of both summer and winter growth wood dissipating energy and so brings out those courses near to these bars on that side of the bridge. (The trebles need to be brought out some of course because their sound gives nearer to a simple tone as they climb in pitch through tension). This too appears to be borne out in that close winter grained soundboard wood, usually gives a good bright purer tap tone, and is more crisp to the touch, though not so in all cases. With Fan bars placed such as they are on the following herewith listed soundboard shapes, I would say the treble was or would have been made better for this usual type placement of Fans.

Michielle Harton 1599 No 1808
Giovani Tesler 1621 No 154/494
Hans Frei 1597 No 1755
Marckus Buedemberg 1608 No 155/490
Matheus Buechenberg 1610 No 1570
Pielro Railich No 156
Michael Hartung Padua 1602 M144
Mango Tieffenbrucker Venice 1609 No 144
Michael Hartung 1599 M156
Theorbo by Martin Hoffmann M245
The Seven course Lute drawn up by Philip Macleod-Coupe 1976
The Eleven course Lute drawn up by Philip Macleod-Coupe 1979
FAN BARS ALL ACROSS BELOW THE BRIDGE

With Fan bars all across below the bridge as are: an instrument by Cristoffolo Hock Venice M155 altered to a Theorbo by Leopold Widhalm, and particularly the soundboard of a Chitarrone by Sebastian Schelle Nuremberg 1728, I would expect all of the lower courses of strings to shout above the trebles, as was the case when I Fan bared all across an experimental soundboard in 1981. I noticed the effect beginning to happen on the Lute soundboard shaped Maler M154 which was the only outline shape my first test jig would admit. (I made some mention of this in Fomrhi Com 334.)

Now with Theorbos and Chitarrone (and any other instrument of the period with its trebles tuned an octave down) I suggest this baring would be no real problem as these were accompanying instrument anyway! NB Mersenne said that when the baring is too firm the sound is not agreeable and that the treble strings do not sound as good and must be tuned lower. (Clearly too much wood here) But NB also for us, that Michael W Prynne also said that by reducing the bars (I assume he meant generally all the bars) the reedy quality of the basses was excessive. (Parameters, conflicting parameters)

TRANSVERSE BARS

I found that with short bars placed in the vicinity of the bridge and placed at 90 degrees to the main grain, the resonance from the bridge does not appear to be dissipated as effectively, See experimental Fig 5 and also the baring type by Matheus Buechenburg Roma 1608 No 142/470, but a quieter sound prevails (PERPLEXITY ENSUES!)

Transverse Bars below the bridge going all across can mule the tone! However this is dependent upon the soundboard thickness here Ah!

So as a maker one must experiment! The seemingly contradictory parameters obviously have to be optimised: A reduction in total mass of the soundboard reduces the energy from the string that is required to move it! The soundboard moves the air -we hear the vibration. (the energy used in moving the soundboard I would say is silent) Reducing the soundboards mass reduces rigidity and the total strength! So the Lute obviously makes use of baring to increase rigidity with minimum total mass increase.

Only by careful recorded baring experiment on a given shape of soundboard, and alteration thereon can one become satisfied with one particular tone or other, eg in John Downings fig 4 Com 1237 it is blatantly obvious that the soundboard in the Bass side of the bridge is freer to move! And in his fig 3, that for the Mandolino, the opposite is the case allowing more movement to reclaim tone harmonics lost from the high pitched treble strings. The closeness to the bridge, of the below the bridge transverse bars, again would be optimised in both cases according to soundboard thickness. That is to say I agree with John in Para. 2 that the addition of this bar configuration suggests basically the same acoustical function, but the soundboard thickness is also involved!!
I agree also with John in his conclusions; 'the bridge position will influence tone colour'. But in no way however does it determine the plucking point of the strings! This is determined by ones hand, and according to Thomas Mace by ones placement of the little finger!

Re John's Para. 3 in Conclusions, well! I need to have the difference between "tone colour" and "acoustical performance" carefully explained to me.

I agree in part with Eph's last paragraph, Com 687 but as I stated in Formhi Com 260 'The bridge does work in whatever position it is placed, however its most favourable node or antinode position is dependent upon one's own idea of overall tone quality'. We must remember that the soundboard shape advocated by Merssen, giving a rounder fuller sound, is quite different to those of Laux Malers, which were predominantly for bass instruments with a 'thin', delicate and introverted tone.

Further help with com 1197

Once the angle has been calculated from :-

\[ \cos \theta = \frac{28}{30} = 0.93333 \]

where \( \theta = 21.03946978^\circ \)

the start drilling point can be marked on the instrument surface with dividers, having used:

\[ \sin \frac{\theta}{2} \times \text{pcd} = \text{chord} \]

ie \( \sin 10.51973489^\circ \times 60 = \text{chord} \)

\[ 0.182574185 \times 60 = \text{chord} \]

Or, it can be marked by using a flexible ruler, having calculated the periferal length \( L \), using the formula:

\[ L = \theta \times \text{dia} \times 0.008727 \]

eg, \( L = 21.03946978^\circ \times 60 \times 0.008727 \)

\[ L = 11.016687717\text{m/m} \]
David's three-part organum

David Z. Crookes

One of your friends regards cryptography as a work of darkness. He can't believe that the psalm-texts contain their own notation. When you point out that a psalm has to be 50% music, he disagrees angrily. He thinks that every letter of the Psalter is a purely theological entity which must be interpreted either spiritually or symbolically. What of the variant Aleph in Psalm 124, you ask? If it doesn't relate to a melodic variation, why is it there? I don't know, he says, but it can't possibly be musical—a single letter couldn't carry that amount of significance.

But sometimes "a single Alif" tells a tale. Years ago, you remember, the United Kingdom and France worked together to build a supersonic aeroplane. It was agreed to name the aeroplane Concorde. Actually it wasn't agreed at all: the British wanted to call it Concord, but the French held out for their own form of that word, and the c on the end of Concorde lets you know who won. You take solace in the thought that "concorde" is really an English word. As recently as 1509 Stephen Hawes wrote (in The Pastime of Pleasure, 16.14):

The vii. scyctcs in one monacorde,
Esche upother do full well depende,
Mussyke hath them so set in concorde.

The instrument mentioned here makes you think of a rather less agreeable poem—Dante Gabriel Rosetti's The Monochord, in which the words notes and wave are used technically:

Nay, is it Life or Death, thus thunder crown'd,
That 'mid the tide of all emergency
Now notes my separate wave...?

Rosetti reminds you that each note of a melody is really a "separate wave". And a monophonic melody is a series of "separate waves", but you've no reason to believe that the psalms were monophonic. You've already asked yourself if the language of Psalm 85 implies some kind of harmony based on contrary motion. And you've wondered whether the melody of Psalm 45 is to be played and sung upon "Shoshannim", in the manner of Purcell's Three parts upon a ground. Psalms 85 and 45 have confronted you, in other words, with questions of harmony and counterpoint. You resolve for the present to address the matter of harmony. Did David's music ever involve different parts "set in concorde"?

You recall a strange passage in I Chronicles 15. Verses 19 to 21 read in the AV as follows:

19. So the singers, Heman, Asaph, and Ethan, were appointed to sound with cymbals of brass; 20. And Zechariah, and Azriel, and Shemiramoth, and Jehiel, and Unni, and Eliab, and Maaseiah, and Benaiah, with psalteries on Almamoth; 21. And Mattithiah, and Elipheleth, and Mikneiah, and Obed-edom, and Jeiel, and Azaziah, with harps on the Sheminith to excel.

What are "psalteries on Almamoth", and what are "harps on the Sheminith"? The Hebrew word Almamoth means girls, maidens, or virgins, and has been taken by some to indicate psalteries of female-voice pitch. The Hebrew word Sheminith means eighth, and has been taken by some to indicate harps pitched at the lower octave. Thus the Good News Bible speaks in verses 20 and 21 respectively of "high-pitched harps" and "low-pitched..."
harps”. But Psalm 46 is entitled To the chief Musician for the sons of Korah, A Song upon Alamoth. No instrumentation is specified, and female-voice pitch is out of the question, since the psalm is for Korah’s sons, not Korah’s daughters. (And you’re pretty sure that it’s for the sons of Korah, not authorial by or of. You remember the title of Psalm 88: A Song or Psalm for the sons of Korah, to the chief Musician upon Mahalath Leannoth, Maschil of Heman the Ezrahite.) What then? Is Alamoth a melody? No. Whatever it is, it is something that is executed simultaneously with Sheminith. Verse 28 of 1 Chronicles 15 speaks of the Levites “making a noise with psalteries and harps”, and verse 3 of Psalm 150 says, “praise him with the psaltery and harp.” So what is Sheminith (= eighth)? David’s Psalm 6 is headed To the chief Musician on Neginoth upon Sheminith, and Neginoth means string instruments, but his Psalm 12 is headed merely To the chief Musician upon Sheminith.

In other words, Alamoth and Sheminith can have an instrumental significance (1 Chronicles 15.20-21, title of Psalm 6), but they may have a purely vocal significance as well (titles of Psalm 12 and Psalm 46). Alamoth can’t mean “at female-voice pitch”, given the male dedicatees of Psalm 46, and consequently Sheminith can’t mean “at an octave below Alamoth”. If it did then you’d half-expect to find the word Sheminith in the heading of every psalm intended for performance by males. Well! So much for the notion of mere octave doubling, or parallel organum of the octave. But might Sheminith (= eighth) have been a euphonal retrograde of the melodic scale, with first and last notes separated by an eighth, or octave, from the corresponding notes of the melodic version? Did the melodic and Sheminith lines form what Menuhin, speaking in the context of melodic repetition, calls “a double image rather like the right and left halves of a leaf when folded upon each other” [Yehudi Menuhin and Curtis W. Davis, The Music Of Man (London, 1980), 31]? Was Sheminith a contrary motion scale?

There are several hints in Scripture of the musical principle non fin est non commencevit. Isaiah 44.6 reads, I am the first, and I am the last. Isaiah 48.12 reads, I am the first, I also am the last. And the principle appears four times in Revelation (1.8, 1.11, 21.6, 22.13) in the precise context of alphabetical simultaneity: I am Alpha and Omega (= first and last letters of the Greek alphabet). There is even a cryptographic contrary motion Hebrew alphabet, the so-called “Alhbash”, in which Aleph is represented by Tau, Beth by S(h)in, and so on:

Aleph Beth Gimel Daleh He Wau Zayin Cheth Teth Yodh Kaph
Tav S(h)in Resh Qoph Tzadhe Pe Ayin Samekh Nun Mem Lamedh

Lamedh Mem Nun Samekh Ayin Pe Tzadhe Qoph Resh S(h)in Tau
Kaph Yodh Teth Cheth Zayin Wau He Daleth Gimel Beth Aleph

Jeremiah uses the contrary motion alphabet at three points in his prophecy (25.26, 51.1, and 51.41). In 25.26 he encodes the word Babel, which means Babylon, as Sheshach (Beth-Beth-Lamedh in Alhbash becomes Shin-Shin-Kaph), but in 51.41 he encodes the same word in the first part of the verse, only to decode it in the final part. Verse 41 reads as follows in the AV:

> How is Sheshach (= Shin-Shin-Kaph) taken! and how is the praise of the whole earth surprised! how is Babylon (= Beth-Beth-Lamedh) become an astonishment among the nations!

Why does Jeremiah spell verse 41’s most important word, as Purcell would say, both recte et retro? In verse 48 he prophesies that “the heaven and the earth, shall sing for Babylon”. That’s music. Is it then possible that Jeremiah’s use of the contrary motion alphabet contains a hint of a contrary motion scale? Does his coupling of Sheshach and Babel refer obliquely to Levitical organum? You decide to take an enormous synthetic leap in the dark. How might you generate a scale on the analogy of the Alhbash alphabet? If you write out the three seven-note groups of the melodic alphabet retro, you get euphony in 15 out of 21 cases.

How is Sheshach (= Shin-Shin-Kaph) taken! and how is the praise of the whole earth surprised! how is Babylon (= Beth-Beth-Lamedh) become an astonishment among the nations!
If you now move the first and last notes of each retro group one degree outwards, so as to be a Sheminith or eighth away from their melodic-scale counterparts, and ‘reflect’ the note Tau in its lower octave, you achieve a euphonal reflection of the whole alphabet.

And if you’re on target then a Sheminith harper’s instrument would have been tuned as follows:

Sheminith ( = Shin-Mem-Yodh-Nun-Yodh-Tau) means eighth or octave, and its first and last letters, Shin and Tau, may have served to remind musicians that the pitches denoted by S(h)in and Tau in the melodic alphabet—c’ and d’—are both ‘reflected’ down the octave in the Sheminith scale. You recall incidentally that on your 'personent hodie' psaltery the note c (an octave below middle c) was produced by a string of unitary length 972. Now you know too many ‘gapped’ scales from world music to feel uneasy about the ‘gaps’ in the Sheminith harp-scale. (You have a Hungarian-made folk recorder, for instance, whose six-finger and seven-finger notes are pitched a third apart. And aside from instruments, the tone-and-a-half distance between the Sheminith’s first and second notes worries you as little as the tone-and-a-half distance between the second and third notes of the falling harmonic minor scale.) But you wonder if Scripture contains any cryptic reference to the shape of the Sheminith scale. You sing the eight notes of the harp-scale:

And at once you find yourself thinking of Proverbs 24.16 (AV): “...a just man falleth seven times, and riseth up again...” Well, your Sheminith harp-scale falls seven times, and rises up again: and while there is nothing musical about Proverbs 24.16 as it stands, the eight Hebrew words of the whole verse, numerated in 400 alphabet gematria, can generate (exactly in the manner of Psalm 49.4-5) formulae for the eight unitary numbers of the Sheminith harp-scale—432, 512, 576, 648, 729, 768, 972, and 864. But is there nothing closer to home? Is there nothing in 1 Chronicles 15, for example? You turn back to the chapter in which you first encountered the word Sheminith, and start reading at verse 17 (AV).
17. So the Levites appointed Heman the son of Joel, and of his brethren, Asaph the son of Berechiah; and of the sons of Merari their brethren, Ethan the son of Kushioth; 18. And with them their brethren of the second degree, Zechariah, Ben, and Jaaziel, and Shemiramoth, and Jehiel, and Umni, Eliab, and Benaiah, and Maaseiah, and Mattithiah, and Elipheleth, and Mikneiah, and Obededom, and Jeiel, the porters. 19. So the singers, Heman, Asaph, and Ethan, were appointed to sound with cymbals of brass; 20. And Zechariah, and Azel, and Shemiramoth, and Jehiel, and Umni, and Eliab, and Maaseiah, and Benaiah, with psalteries on Alamoth; 21. And Mattithiah, and Elipheleth, and Mikneiah, and Obededom, and Jeiel, and Azaziah, with harps on the Sheminith to excel.


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</table>

Why is there no and before the names of Ben and Eliab? The omission of and (in Hebrew a prefixed Wau) is so striking that it must be deliberate. Verse 20 contains eight names, all preceded by and. Verse 21 contains six names, all preceded by and. Verse 24 contains nine names, all preceded by and. So why no and before Ben and Eliab? And at once you realize. The first eight names in verse 18 stand for the eight notes of the (falling) Sheminith harp-scale. The omission of and before Ben stands for the 'gap' of a minor third between d' and b, between the first and second notes of the scale. The omission of and before Eliab stands for the 'gap' of a major third between e and c, between the sixth and seventh notes of the scale. And now you wonder whether the eight notes of the Sheminith scale were denoted by string-length-plus-notiation numbers corresponding to the (rising) melodic scale's 933, 849, 822, 762, 711, 758, 843, and 832. The string-length numbers of the (falling) Sheminith harp-scale would be 432, 512, 576, 648, 729, 768, 972, and 864. If to those numbers you add the 400 alphabet notiation-numbers 69 (= Alph + Cheth + Samekh), 81 (= Beth + Teth + Ayin), 93 (= Gimel + Yodh + Pe), 114 (= Daleth + Kaph + Tzadhe), 135 (= He + Lamedh + Qoph), 246 (= Wau + Mem + Resh), 357 (= Zayin + Nun + Sh(h)in), and 400 (= Tau, always a three-beat note), you get eight string-length-plus-notiation numbers as follows:

\[d' = 501 \quad b = 593 \quad a = 669 \quad g = 762 \quad f = 864 \quad e = 1014 \quad c = 1329 \quad d = 1264\]

Six Sheminith overseers are named in verse 21, and the last six Hebrew words of verse 18 in 400 alphabet gematria add up to 1264. You take the sum of these last six words to denote the last note of the scale (three-beat bottom d). Then, remembering that Sheminith means "eighth", you look at the first eight words of verse 21 in 400 alphabet gematria. These eight words add up to 2295, and you take their total to denote the notes a, g and f.

\[669 + 762 + 864 = 2295\]

Finally, you add up all ten words of verse 21 in both 400 and 22 alphabet gematria. The 400 alphabet total is 3388, the 22 alphabet total is 634, and the difference between the two totals is 2754. You take this difference to denote two harmonious pairs of notes: Sheminith 432 (= d') is played against melodic 864 (= d), and Sheminith 972 (= c) is played against melodic 486 (= c).

\[(432 + 864) + (972 + 486) = 2754\]
What have you unearthed by considering verses 18 and 21 in gematria? Levitical mnemonics which cover six notes of the Sheminith harp-scale—d', a, g, f, c, and d. And although you’d like something to cover the notes b and e as well, you resolve for the moment to leave Sheminith and investigate the matter of Alamoth.

Now you can work out a certain amount from the actual word Sheminith, which means eighth or octave, but what are you to do with a word that means girls, maidens, or virgins? And what compass did Alamoth employ? Migh it conceivably have employed the same compass as the melodic and Sheminith scales? That would mean having all three parts contained by the range of a major ninth, which is perfectly credible: as you’ve already observed, the three parts of Georgian sacred music are all contained by the range of a major tenth. Another question poses itself: what note did the Alamoth scale start on? Well, if Alamoth used the same compass as the melodic and Sheminith scales, and if euphony was a consideration, then when the melody had a bottom d' and the Sheminith had a top d' the Alamoth would have had either f, g, or a—something in the middle. In the middle! Sheminith expresses the musical principle ma fin est mon commencement. Does Alamoth express the principle mon centre est mon commencement?

That reminds you. You got on to Sheminith after looking at Jeremiah’s use of the contrary motion Hebrew alphabet. But there is also a chiastic Hebrew alphabet, the so-called “Albam”, in which Aleph is represented by Lamedh, Beth by Mem, and so on:

<table>
<thead>
<tr>
<th>Aleph</th>
<th>Beth</th>
<th>Gimel</th>
<th>Daleth</th>
<th>He</th>
<th>Wau</th>
<th>Zayin</th>
<th>Cheth</th>
<th>Teth</th>
<th>Yodh</th>
<th>Kaph</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lamedh</td>
<td>Mem</td>
<td>Nun</td>
<td>Samekh</td>
<td>Ayin</td>
<td>Pe</td>
<td>Tzadhe</td>
<td>Qoph</td>
<td>Resh</td>
<td>Sinh</td>
<td>Tau</td>
</tr>
<tr>
<td>Lamedh</td>
<td>Mem</td>
<td>Nun</td>
<td>Samekh</td>
<td>Ayin</td>
<td>Pe</td>
<td>Tzadhe</td>
<td>Qoph</td>
<td>Resh</td>
<td>Sinh</td>
<td>Tau</td>
</tr>
<tr>
<td>Aleph</td>
<td>Beth</td>
<td>Gimel</td>
<td>Daleth</td>
<td>He</td>
<td>Wau</td>
<td>Zayin</td>
<td>Cheth</td>
<td>Teth</td>
<td>Yodh</td>
<td>Kaph</td>
</tr>
</tbody>
</table>

Isaiah uses the chiastic alphabet at one point in the seventh chapter of his prophecy. After calling Pekah in verses 1, 4 and 5 “the son of Remaliah”, he describes him in verse 6 as “the son of Tabeal”. Tabeal is a cryptic Albam form of Remaliah (Resh-Mem-Lamedh becomes Teth-Beth-Aleph). Verse 6 reads as follows in the AV:

Let us go up against Judah, and vex it, and let us make a breach therein for us, and set a king in the midst of it, even the son of Tabeal.

The words in the midst of it (= in the midst of Judah) may be a key to the cipher, since the chiastic Albam alphabet starts, so to speak, in the middle. Well!—Isaiah’s use of Albam may be intended to display a prophetic awareness of enemy ciphers, but has it anything in the world to do with Alamoth, or even with music in general? At first sight it seems not. Then you notice verse 14. “...the Lord himself shall give you a sign; Behold, a virgin shall conceive, and bear a son...” And you look at the Hebrew word used for virgin—Ayin-Lamedh-Mem-He (the singular of Alamoth). Is there any link between the Albam alphabet and the Alamoth scale? Does the Alamoth scale ‘start in the middle’? If you bend the melodic scale back on itself, in the manner of the Albam alphabet, you get a chiastic scale which accompanies the melodic scale in perfect fifths:

And if the eight notes of this chiastic scale were played on eight consecutive strings of an Alamoth psaltery, then the 22 notes of David’s melodic system would have been accompanied as follows:
Why would such a system be called Alamoth (= Ayin-Lamed-Mem-Wau-Tau)? Three possible reasons suggest themselves. The name may be an anagram in which Lamedh and Mem, first two letters of the Albam alphabet, combine with the word Ayin-Wau-Tau (= bend). Or it may be a nickname based on the shape of an instrument. An Alamoth psaltery tuned a, b, c', d', d, e, f, g, if fitted with strings of equal thickness, might have looked something like this:

A third possibility involves gematria. The Alamoth psaltery's highest note, d', will be produced by a string of unitary length 432, and d's of one, two, and three beats will be represented respectively by the notation-letters Daleth, Kaph, and Tzadhe. Those three letters have 400 alphabet values respectively of 4, 20, and 90: so the overall string-length-plus-notation number for the psaltery's highest note is 432 + 4 + 20 + 90 = 546. And 546 happens to be the 400 alphabet numerical value of the Hebrew word Alamoth.

The eight string-length-plus-notation Alamoth numbers will be as follows:

\[
\begin{align*}
a &= 645 & b &= 593 & c' &= 579 & d' &= 546 & d &= 999 & e &= 1014 & f &= 1086 & g &= 1048 \\
576 + 729 + 864 + 135 &= 2304
\end{align*}
\]

Once you've worked out these numbers you wonder whether verse 20 of 1 Chronicles 15, which names the Alamoth overseers, contains anything to parallel the numerical cryptography of verse 21. First you add up all ten words of verse 20 in both 400 and 22 alphabet gematra. The 400 alphabet total is 2921, the 22 alphabet total is 617, and the difference between the two totals is 2304. You take this difference to denote the D minor triad produced by three fifth notes—melodic a, Sheminith f, and Alamoth d—each of which is represented by the 400 alphabet fifth-note notational number 135 (= He + Lamedh + Qoph).

\[
576 + 729 + 864 + 135 = 2304
\]

The rest of your work is done in 400 alphabet gematra. Alamoth provides the melodic scale with an accompaniment of fifths, and the first five words of verse 21 add up to 1575. You take the sum of these five words to denote the fifth note of the Alamoth scale sounding together with the fifth note of the melodic scale. 135 is the fifth-note notational
number, and the unitary numbers for the fifth notes of the Alamoth and melodic scales are respectively 864 and 576.

\[ 135 + 864 + 576 = 1575 \]

You notice that the third, fifth and seventh words of verse 20 also add up to 1575. The last four words add up to 1296, and you take their total to denote that the top d' and bottom d strings, of unitary lengths respectively 432 and 864, sit side by side on the Alamoth psaltery.

\[ 432 + 864 = 1296 \]

The second, third and fourth words add up to 1191. You take their total to stand for the notes a and d', whose Alamoth string-length-plus-notation numbers are respectively 645 and 546. (546, you remember, is the 400 alphabet value of the Hebrew word Alamoth.)

\[ 645 + 546 = 1191 \]

The third, seventh and tenth words add up to 2085. You take their total to stand for the notes d and f, whose Alamoth string-length-plus-notation numbers are respectively 999 and 1086.

\[ 999 + 1086 = 2085 \]

The third, eighth and ninth words add up to 1215, which you construe in terms of unitary string lengths: Alamoth c' ( = 486) sounds together with melodic f ( = 729). The second, fourth, eighth, ninth and tenth words add up to 1048, the Alamoth string-length-plus-notation number for the note g. So you now have Levitical mnemonics that cover six notes of the Alamoth scale: a, c', d', d, f, and g. But you'd like something to cover the notes b and e as well, and you'd like whatever you find to indicate that b and e are respectively the second and sixth notes of both the Alamoth and Sheminith scales. How might you express that fact numerically? You start with the note b. Two string-lengths of 512 added to the second-note notation-number 81 give you a total of 1105: and the second, fourth, fifth, ninth and tenth words of verse 20 add up to 1105. (So do the first, sixth and seventh words of verse 21, the Sheminith verse.) As for the note e, two string-lengths of 768 added to the sixth-note notation-number 246 give you a total of 1782: and the third, ninth and tenth words of verse 20 add up to 1782. (So do the first and ninth words of verse 21.)

The gematria of 1 Chronicles 15 can generate many more mnemonic formulae, but that's enough to be going on with. You've managed to uncover mnemonics for every note of both the Alamoth and Sheminith scales. What you want now is some clear proof that the two scales possess exactly such forms as you have surmised. If you're right all three scales run as follows:
The melodic scale starts at the beginning; the Sheminith scale starts at the end; and the Alamoth scale starts in the middle. You ask yourself dolefully how Scripture might possibly contain any cryptic statement of these three facts: and suddenly you recall verse 26 of Psalm 68 (AV, verse 25—supplied words are italicized):

The singers went before, the players on instruments followed after; among them were the damsels playing with timbrels.

The verse's seven Hebrew words may be rendered as follows:

(2) Singers (1) went-at-the-beginning,

(4) string-players (3) behind, or at-the-rear;

(5) in-the-middle (6) damsels (7) drumming.

The word "string-players" translates Nun-Gimel-Nun-Yodh-Mem, that's to say "players of Neginoth" (= Nun-Gimel-Yodh-Nun-Wau-Tau). Where did you last read of Neginoth? In Psalm 6, which in the AV is headed, "To the chief Musician on Neginoth upon Sheminith". You wonder if the Neginoth-players in Psalm 68 are understood to be playing "upon Sheminith". Then you notice the sixth word of the verse, translated "damsels" in the AV. That word is Alamoth (= Ayin-Lamedh-Mem-Wau-Tau), and the girls whom it primarily denotes are described as having been in the middle! The Hebrew word for in the middle is Beth-Tau-Wau-Kaph: the same word is used with a pronominal suffix in Isaiah 7.6 (Beth-Tau-Wau-Kaph-He = in the midst of it). Now of course Psalm 68.26 is describing the order of a real musical procession, but you see the verse as having a secondary cryptic meaning:

Singers of the melodic scale start at the beginning,

Neginoth-players of Sheminith start at the end;

players of Alamoth start in the middle.

That is such a clear piece of evidence that you don't expect the verse to contain a numerical message as well, but you numerate it nevertheless. The seven Hebrew words have 400 alphabet values which you label A to G, as follows:

\[
\begin{array}{ccccccc}
A & B & C & D & E & F & G \\
150 & 550 & 209 & 153 & 428 & 546 & 972 \\
\end{array}
\]

Apart from 546 (the string-length-plus-notation number for Alamoth d'), one number speaks for itself: 972, the seventh number, the number "at the rear", and the only 972 in the Psalter, denotes the seventh string of the Sheminith harp, which is 972 units long and sounds bottom c. The seven numbers can generate (exactly in the manner of Psalm 49.4–5) a formula for each of the unitary numbers 432, 486, 512, 576, 648, 729, and 768, but not for 864. You take that incapacity as intended to emphasize that on the Sheminith harp the 864 string does not precede the 972 string. Is it possible to get a formula for 864 otherwise? You numerate the seven words in 22 alphabet gematria and label their values H to N, as follows:

\[
\begin{array}{ccccccc}
H & I & J & K & L & M & N \\
42 & 64 & 29 & 54 & 41 & 69 & 90 \\
\end{array}
\]
Yes—if you subtract A from the sum of G and H, you get 864. That’s pretty neat in comparison with some of your formulae based on the numbers A to G: 486, for example, needs five terms (B + C + D + F - G), and 648 needs six terms (D + E + G - A - C - F). What kind of formulae will you get if you make the two sets of seven numbers into one Davidic set of fourteen? You write out the numbers A to N in one line and work out the most economical possible formulae for all nine numbers of the three-scale system, as follows:

<table>
<thead>
<tr>
<th>A</th>
<th>B</th>
<th>C</th>
<th>D</th>
<th>E</th>
<th>F</th>
<th>G</th>
<th>H</th>
<th>I</th>
<th>J</th>
<th>K</th>
<th>L</th>
<th>M</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>150</td>
<td>550</td>
<td>209</td>
<td>153</td>
<td>428</td>
<td>546</td>
<td>972</td>
<td>42</td>
<td>64</td>
<td>29</td>
<td>54</td>
<td>41</td>
<td>69</td>
<td>90</td>
</tr>
</tbody>
</table>

That makes an total of 24 terms for nine formulae covering 24 strings. You notice that the formula for 486 involves only one word, the Hebrew word for singers, as if to emphasize that the note c’ belongs to the melodic but not to the Sheminith scale. Now while Psalm 49.4–5 gave you a 22-term formula-set for the eight notes of the melodic scale, Psalm 68.26 gives you (when you take account of the twice-appearing numbers 593, 762, and 1014, and the number 864, for which you already have a formula), a 66-term formula-set for the 24 string-length-plus-notation numbers of the three-scale system, as follows:

**Melodic scale**

**Alamoth**

**Sheminith**
501 = B + L - N, [593 = B + D - L - M], 669 = F + K + M, [762 = B + C + D - A], [864 = G + H - A], [1014 = G + H], 1329 = A + D + G + K, 1264 = C + G + J + K

And now there surfaces in your memory the strange and verbless final verse of Psalm 87, which with supplied words italicized reads as follows in the AV:

As well the singers as the players on instruments  
shall be there: all my springs are in thee.

The verse’s four Hebrew words may be rendered as follows:

1. And singers
2. like pipers:
3. all-my-springs
4. in thee.

The language is so taut that you’re sure it involves numerical cryptography, but what can you do with four numerated words? Or are you meant to numerate them in both species of gematria? It’s worth a try. First you numerate the four Hebrew words in 400 alphabet
gematria and label them A to D. Next you numerate them in 22 alphabet gematria and label them E to H. And then you write out the numbers A to H in one line, thus:

```
A  B  C  D  E  F  G  H
556 138 230 22  70  66  86  13
```

These eight numbers can generate formulae for the eight notes of the melodic scale, which was used by both singers and pipers. The words which they represent preclude them from having anything to do with either Alamoth or Sheminith, and in any case since their overall total is 1181 they can’t generate formulae of the sort you’ve been finding for the Sheminith string-length-plus-notation numbers 1264 and 1329. But they do generate formulae for all eight string-length-plus-notation numbers of the melodiac scale: 933, 849, 822, 762, 711, 758, 843, and 832. And now a question poses itself. The text says all my springs, and the cryptic meaning is all my notes. So are you meant to stop at eight string-length-plus-notation numbers, each of which denotes a three-note group? Were there not string-length-plus-400-gematria-notation numbers for every note of the 22-note system, with the crotchet, minim, and dotted minim d, for example, being indicated respectively by 865 (= 864 + Aleph), 872 (864 + Cheeth), and 924 (864 + Samekh)? If such numbers did exist they would have run as follows:

```
865 770 732 652 581 518 493 872 777 739 668 606 552 536 924 838 809 738 676 712 786 832
```

And amazingly, all 22 of these numbers—865, 770, 732, 652, 581, 518, 493, 872, 777, 739, 668, 606, 552, 536, 924, 838, 809, 738, 676, 712, 786, and 832—can be generated by Psalm 87’s numbers A to H. Another question poses itself: are these individual note-numbers ever involved in the cryptic notation of melodies? You decide to leave that question for another day.

It remains to draw your three organal strands together. You take Heman, Asaph and Ethan in I Chronicles 15.19 to have been in charge respectively of the melodic, Alamoth and Sheminith choirs. You remember the positional order of the three choirs given in I Chronicles 6 (AV):

```
31. And these are they whom David set over the service of song in the house of the Lord, after that the ark had rest. 33. . . . Of the sons of the Kohathites: Heman a singer, the son of Joel... 39. And his brother [= brother Leviite] Asaph, who stood on his right hand... 44. And their brethren the sons of Merari stood on the left hand: Ethan the son of Kushi...
```

With names omitted, the melodists stand in the middle, the Alamoth people stand on the right, and the Sheminith people stand on the left. That’s how the performers will see it: an ‘audience’ will see it the other way round. The Good News Bible leaves out the notion of middle, right and left, but makes the fact of the three choirs very clear:

```
33. . . Heman, the leader of the first choir, was the son of Joel. 39. Asaph was leader of the second choir... 44. Ethan of the clan of Merari was the leader of the third choir...
```

Finally, you observe the amazing family trinity that characterized David’s capella. You knew that all the choristers were Levites, but there’s more to it than that. Levi had three sons: Kohath, Gershom and Merari. All the melodic choristers were descendants of Kohath, all the Alamoth choristers were descendants of Gershom, and all the Sheminith choristers were descendants of Merari (I Chronicles 6.16-47, AV).
You decide now to write out all 66 notes of David’s organal system, but one of your musicologist friends tells you not to bother. Why, you ask? Because the *Sheminith* scale is on one count impossible, he says: whenever your melody has a leap of a seventh, the *Sheminith* part will have a leap of a ninth. Well, big deal, you answer—the ninth isn’t a difficult interval. The eighties hit song *Freedom* (by Wham!) is memorable mostly because of its melodic ninths. And one of the most popular hymn-tunes of the last twenty years, *Tyrol*, has no less than three ninths, three sevenths and three tritones:

Your friend is silent. He never sings a note, he never listens to the radio, he never goes to church, and then he talks about unsingable intervals. Oh, well. On with the job.

Suddenly you perceive what is really the most remarkable feature of David’s organal system. All the musicians, whatever part they perform, employ the same notation. It reminds you of when you used to play different brass instruments in a band: whatever the instrument was, you read treble clef and worked with three or four valves. You wish you had a band on the premises right now to play your three-part scorings of *The Song of the Bow* and Psalm 124. In fact you’d settle for three trombones: but since that’s not available, you decide to sing the melody line and play the other two parts on viola, with the help of a couple of tape recorders. You start with *The Song of the Bow* (II Samuel 1.19-27). There’s no mention of *Alamoth* and *Sheminith* in the text of the two introductory verses (17 and 18), but there may be something in their 400 alphabet gematria. Taken together the Hebrew words *Alamoth* (= 546) and *Sheminith* (= 810) have a 400 alphabet total value of 1356. Now you’ve already found one cryptogram in verse 18 (behold, it is written in the book = 933), so you’re prepared to find another one. The words rendered behold, it is written in the book of Jasher add up to 1448. If you subtract from that total the number of the single word rendered the children of Judah (Beth-Nun-Yodh—Yodh-He-Wau-Daleth-He = 92), you get 1356. Verse 17’s verbal numbers behave similarly: if you subtract the number of the seventh word, rendered his son (Beth-Nun-Wau = 58), from the total of the first, third and sixth words ( = 1414), you get 1356 once again. As for Psalm 124, the third, fifth, sixth, eleventh and thirteenth words of its melodic text add up (in 400 alphabet gematria) to 1356. Set out in their three organal parts—melody, *Alamoth* and *Sheminith*—the two pieces run as follows:
52

\[ \text{THE SONG} \]

\[ \text{ALAMOTH} \]

\[ \text{SHEMINITH} \]

\[ \text{PSALM 124} \]
As you play the individual Alamoth and Sheminith lines on viola, against a tape of your own voice, you find yourself reminded of two pieces of Icelandic tvisýngur (= two-voice organum). The first is Stríðlsnemn þá hófðu krossfesi Krist (IT, 801–2), which runs as follows:

There are 42 minim beats. Four of them (bracketed) involve the note e’, which is alien to David’s melodic scale. Of the remaining 38 minim beats, 34 are pure Alamoth (the unconforming four beats are also bracketed). Then there’s Ad cantus leticie (IT, 304–5), which runs as follows:

There are 26 pairs of notes. 21 are pure Sheminith (the unconforming pairs are bracketed). And many other examples of tvisýngur seem to represent a fusion of Alamoth and Sheminith. O Jesu dulcisime (IT, 163–4) contains 110 pairs of notes, of which, remarkably, only a single pair (involving the note e’) corresponds with neither Alamoth nor Sheminith. In Meliora sunt (IT, 305–6), the ratio of diverging to corresponding pairs is 12 (of which nine involve the note e’) to 103. So you’re inclined to believe that tvisýngur is a vestige of ancient Israelite practice. You recall how in the Icelandic Bishops’ Sagas you once found the verb tvisýngja (= to sing in two parts) together with the verb tripla (= to sing in three parts), and you speculate. Three parts? Melody, Alamoth, and Sheminith? Whatever the case, tvisýngur looks like a Davidic survival. There must be many other such survivals in the corpus of European organum.

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Praetorius' Brass Instruments and Cammerthon

In Comm. 1200 one of us analysed the sackbuts from Praetorius' Scuagraphia in an attempt to relate their dimensions to the pitch standard as determined from the pitchpipe diagram Pfeifflin zur Chormaß [Comm. 342]. There are significant measurement errors which means that the conclusion about relative pitches is invalid. In particular, the scale of Plate VI (reduced in the edition used [1]) is 0.0612 Brunswick feet/mm, not 0.0580, leading to a large error in scaled length of the Octav sackbut. Also, the argument of Comm. 1200 suffers from the absence of any mention by Praetorius of inserting the semitone shank (Polette) in his instructions for obtaining standard Chormaß pitch from the floating first position of a Nuremberg sackbut [2].

In this Comm. we examine Plates VI and VIII more closely and show that the instrument sizes are compatible with each other, with Praetorius’ text, and with the Cammerthon standard. If a similar analysis of the relationship between instrument dimensions and pitch were carried out on surviving Nuremberg brass instruments, it could provide valuable evidence relating to the determination of Cammerthon from the Pfeifflin diagram.

Relationship between length and pitch of brass instruments

In Appendix A the principles of brass instrument design are reviewed. For the purposes of the present analysis, it is sufficient to note that there is a range of different combinations of mouthpiece, bell and tubing designs that will make a workable brass instrument at a given pitch. Pitch is not uniquely determined from total length. Nevertheless, in designing a set of instruments at different pitches to match a known instrument that works well, it is reasonable to suppose that the maker (or school of makers) would scale each instrument from the known one according to the pitch interval ratios, and then modify each instrument to get it to work well at the same pitch standard.

If designing was done this way, we would expect the amount of modification needed to vary smoothly with size. Thus if we measure the lengths of the instruments in the set and scale these lengths back to that of the supposed original (e.g. the normal tenor sackbut, or rechte gemeine Posaun), what remains is the length modification that had to be made. If as expected these modifications vary smoothly, they can show up anomalies in the relative pitch assumptions made about the set.

Such an anomaly should be expected in the set of Praetorius' sackbuts, because whereas the Gemeine size was played with a floating first position [2], Plate VIII shows a non-floating first position on the D Quart size. Also the three larger sackbuts have tuning slides possibly for different pitch standards or for transposition, while the smaller ones had tuning bits (a semitone shank and a tone crook). There is ambiguity about whether the tuning bits had to be in or out to be in tune with the large ones as depicted.

Relative pitch of the sackbuts from their lengths

In Comm. 1200 instrument lengths were calculated by representing the bow shape as a semicircle, to try to compensate for any shortening effect due to the increase in speed of sound around the bend. That led to an underestimate of the length because the acoustic shortening effect of the bow is very small. We have remeasured the lengths, this time using the Bärenreiter facsimile editions of 1943 and 1985 with the plates enlarged.
Calculating the lengths of the instruments according to the methods described in Appendix B, and scaling them to the *gemeine Posaun*, leads to the results shown in Table 1. The trumpet shown in Plate VIII has also been included, since it works in the same way as sackbuts and was part of the same instrument-making tradition, its relevance will become apparent later. The scaled lengths $L_s$ are plotted against scale factor $S$ as the open circles in Figure 1.

<table>
<thead>
<tr>
<th></th>
<th>Fig 1 key</th>
<th>Length (metres)</th>
<th>Scale factor $S$</th>
<th>Scaled length $L_s$ (metres)</th>
<th>Scaled length increased by 3%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Trumpet</td>
<td>T</td>
<td>2.098</td>
<td>3/4</td>
<td>2.797</td>
<td></td>
</tr>
<tr>
<td>Alt Posaun</td>
<td>A</td>
<td>2.040</td>
<td>3/4</td>
<td>2.720</td>
<td>2.802 (A')</td>
</tr>
<tr>
<td>Gemeine</td>
<td>G</td>
<td>2.743</td>
<td>1</td>
<td>2.743</td>
<td>2.825 (G')</td>
</tr>
<tr>
<td>D Quart</td>
<td>D</td>
<td>4.445</td>
<td>3/2</td>
<td>2.963</td>
<td></td>
</tr>
<tr>
<td>C Quart</td>
<td>C</td>
<td>4.976</td>
<td>5/3</td>
<td>2.986</td>
<td></td>
</tr>
<tr>
<td>Octav</td>
<td>O</td>
<td>6.208</td>
<td>2</td>
<td>3.104</td>
<td></td>
</tr>
</tbody>
</table>

In the plates all the instruments are depicted with the slide fully retracted, whereas the evidence from Praetorius [2] is that at proper pitch the *gemeine* sackbut was played in floating first position with the slide pulled out two fingers' width (40–45 mm), increasing the length by 3% and lowering the pitch by about ½ semitone. Keith McGowan [3] has shown that this was necessary to allow room for the supporting hand as the instruments were held with an underhand grip. But with all the larger sackbuts the slide is moved by means of a handle, which probably implies a different method of supporting the instrument; in any event the D Quart sackbut in Plate VIII is shown fully retracted in the slide position for playing D. A difference of ½ semitone between the small and the large sackbuts fully closed should therefore be expected.

The result of increasing the lengths of the *Alt* and *Gemeine* sackbuts by 3% is shown by the black squares in Figure 1, labelled $A'$ and $G'$. From the points T, $A'$, $G'$, D, C and O we can now see here a fairly smooth variation as expected with instruments from the same instrument-making tradition. The curve in Figure 1 is a parabola that has been fitted to the
end points T and O, and accurately represents the data to within measurement error. Its equation is: \( L_s = 0.0893 S^2 + 2.747 \)

These measurements strongly support the hypothesis that the trumpet and sackbuts in Praetorius’ plates are all at the same pitch standard. The errors in Comm 1200 obscured the systematic variation in scaled lengths, and its conclusion that the smaller instruments were pitched at a standard 1/4 tone higher than the larger ones (based on the difference between the Gemeine and the D Quart) is invalid. Therefore the speculation that Nuremberg sackbut makers normally produced instruments as depicted in the plates at the highest pitch standard in Europe (Venice) and provided tuning bits to cater for other standards is unsustainable. The makers must have supplied sackbuts of different lengths for markets with different pitch standards; there is no technical difficulty in doing this, in contrast to woodwinds.

Some writers have inferred from the few surviving 17th century tenor sackbuts, which sound between bb' and b' with the slide fully retracted, that Praetorius’ Cammerthon was about a semitone higher than modern (i.e. a' = ca. 465 Hz). For the surviving instruments to have any bearing on the question of what Cammerthon was, their total lengths have to be carefully measured and compared with Praetorius’ diagrams. Only if the sackbut lengths are the same, but their pitch differs from the Pfeifflin pitch, will there be conflicting evidence that needs resolving. The measurement techniques used here for Praetorius’ diagrams can be applied equally well to surviving instruments.

The relationship between Chormaß and Cammerthon

Bruce Haynes [Comm. 891, and private communication to SH] has suggested that identifying Cammerthon from reconstruction of the Pfeifflin zur Chormaß may be misleading if what Praetorius meant by Chormaß (or rechte Chorthon) was not necessarily the same as what he meant by Cammerthon.

On pages 15 and 17 of Syntagma Musicum II Praetorius mentioned that Cammerthon was the normal pitch to which organs were being tuned. In Prague and a number of Catholic chapels Cammerthon was the same but Chorlon was a tone lower. He would have preferred to change organ pitches to this tone-low standard, but realised that this was impractical in Germany “and therefore one must retain the usual Cammerthon (which is now referred to and taken as Chorthon in most places)”.

There was only one Cammerthon standard: it was the usual standard pitch, otherwise referred to as unser Thon, unser jeziger (gewöhnliche) Cammerthon, unser jezige gewöhnliche Thon, rechte Thon, rechte Cornetten Thon, and Chorthon. But there was more than one Chorthon, and the one which was a tone lower (that was preferred but not promoted) was often mentioned in the context of instruments. So to specify the Chorthon that was the commonest standard a qualifying word was required, viz. the ‘proper’ or rechte Chorthon. The term Chormaß meant the measure of a Chorthon (choir pitch), and probably related to organ pipes since Praetorius used it only in the context of organs. It was necessary to specify which Chorthon, and in the Pfeifflin discussion [2] he did so, consistently calling it rechte Chorthon. The purpose of the pitchpipes was to define a standard pitch named Chormaß or rechte Chorthon, but there is no explicit statement that that standard was the same as Cammerthon. To make such a connection, we have to rely on our understanding of Praetorius’ motivation for standardisation. But it would be better to have independent evidence.

Such evidence can be found in Chapter VI of Syntagma Musicum II, where Praetorius states unambiguously that trumpeters were at Cammerthon, and to obtain Chorthon a crook was
inserted to lower their pitch by a tone (Plate VIII shows such a crook). Since the Alt sackbut as shown in fully retracted position is ½ semitone above the trumpet, and the Gemeine and Alt sackbuts are at the same pitch as each other, the gemeine Posaun is ½ semitone above Cammerthon. The sackbut referred to below the Pfeifflin zur Chormaß can be assumed to be the gemeine Posaun since the reference note is a, and to obtain Chormaß one has to pull out the slide 2 fingers' width, equivalent to ½ semitone. So Chormaß and Cammerthon are both ½ semitone below the pitch of the gemeine Posaun fully retracted, and consequently Chormaß = Cammerthon.

References
2. M. Praetorius, Syntagma Musicum II pp. 231-232 (see translation elsewhere in this Q.)
5. G.S. Brindley, Nature 246, 479, 1973

Appendix A: The design and pitch of brass instruments

The pitch of an organ pipe can be determined theoretically with some accuracy from its cross-sectional shape and length of the air column between the top of the mouth to the top end. Praetorius gave this information for the set of pitchpipes. The pipe vibrations closely approximate those of a tube with both ends open. Other factors that influence pitch do not need to be known accurately and are restricted by known contemporary organ-making practice, and corrections for those can be calculated. Such factors include the height and width of the mouth and the air pressure from the bellows, variation of pitch with temperature can also be calculated.

The pitch of a brass instrument cannot be determined accurately from the information given by Praetorius. Even if we had all the design information on the mouthpiece, cylindrical tubing and bell flare, the calculation would be formidable. We can model each of these components acoustically, but the interactions between them are complex. A simplified model is provided below, but the pitch predictions are not likely to be accurate to better than a semitone.

A cylindrical tube with a closed end works like a closed organ pipe, in which the harmonics have only odd-numbered multiples of the fundamental frequency. At the closed end the air velocity along the tube is zero (i.e. a velocity node) and the amplitude of pressure oscillation is a maximum. At the open end the situation is reversed, having a velocity antinode and a pressure node. If part of the tube is replaced by a bell, the pitches of all harmonics will rise owing to the increase in wave velocity within the flared section. When the closed end is replaced by a mouthpiece, the puffs of air from the lips create a minimum of air flow and a maximum of pressure variation, so the situation is close to that of the closed end. The lowest modes of vibration are essentially no different from the tube with a closed end, but the higher harmonics are flattened in pitch by amounts depending on the design of the mouthpiece cup and particularly its tube. If the combination of design of the mouthpiece and bell flare are just right, one ends up with a set of frequencies for the harmonics which include all multiples (not just odd-numbered ones) of a fundamental frequency which is not the same as the true fundamental. The true fundamental, called the ‘pedal tone’, is flat relative to the harmonics that are generally played; this makes it hard to sound in a stable manner, and it is seldom used.
We can estimate pitch from the acoustic theory of horns, which represents the above-mentioned influences mathematically. Benade [4] has suggested that trumpet and trombone bell shapes from 1600 to the present day can be modelled quite accurately as Bessel horns, for which the diameter D at distance y from the open end is given by $D(y) = \frac{K}{(y + c)}^m$. In this formula K and c are numbers determined by the horn diameters at both ends, and $0 < m < 1$. Horns with m near zero are basically cylindrical with a very sharp flare at the end; horns with m nearer 1 broaden more gradually. For trumpets and trombones m has tended to decrease from 0-6-0-8 in Praetorius' time to 0-5-0-6 in more recent instruments.

Benade showed that for Bessel horns the resonant frequency of the nth mode is given by

$$f_n = \frac{v}{4(L + c)} \left\{ \frac{(2n - 1) + 0\cdot637m(m+1)}{(2n - 1)} \right\}^{1/2}$$

where $v =$ velocity of sound in open air and L = overall length (for $m > 0\cdot8$, the numerical factor 0-637 becomes 0-707). This formula accurately represents the higher resonant mode frequencies of a Bessel horn, but they are only an approximation to those of an actual instrument. With higher modes the increased length caused by the mouthpiece and mouthpipe brings the resonances closer to the proper harmonic relationship with the low-frequency modes, the instrument behaving like a horn of $m = 1$. However the lowest resonant modes ($n = 1-3$) of the instrument are essentially unaffected by the mouthpiece, and the application of the formula to mode $n = 2$ does yield a reasonable prediction of the pitch of modern instruments. For a Bb valve trumpet, assuming $L = 1\cdot3$ m and $m = 0\cdot5$ yields $f_2 \approx 235$ Hz with no valves open. Similarly for a Bb 9' trombone with the slide fully retracted ($L = 2\cdot7$ m and $m = 0\cdot6$), $f_2 = 116$ Hz. These frequencies are consistent with a pitch standard of $a' = 440 \pm 3$ Hz.

In Table 2 $f_2$ has been calculated for Praetorius' instruments and the nominal pitch found by scaling the frequency of the mode 2 resonance to the note $a'$. The value of m for the bell of each instrument was calculated from enlargements Plates VI and VIII, by plotting $D(y)$ on log-log graph paper and finding by trial the value of c that gave a straight line fit. The slope of that line then yielded the values of m in the table. They show a progressive broadening of the horn shape on going from the smallest to the largest sackbut. But the differences in m between instruments do not by themselves change the relative pitch by more than $\frac{1}{2}$ semitone.

<table>
<thead>
<tr>
<th>Length L (metres)</th>
<th>Trumpet</th>
<th>Alt Posaun</th>
<th>Gemeine</th>
<th>D Quart</th>
<th>C Quart</th>
<th>Octav</th>
</tr>
</thead>
<tbody>
<tr>
<td>2-098</td>
<td>2-040</td>
<td>2-743</td>
<td>4-445</td>
<td>4-976</td>
<td>6-208</td>
<td></td>
</tr>
<tr>
<td>Bell correction c (mm)</td>
<td>22</td>
<td>7</td>
<td>9</td>
<td>17</td>
<td>17</td>
<td>52</td>
</tr>
<tr>
<td>Flare parameter m</td>
<td>0-76</td>
<td>0-60</td>
<td>0-63</td>
<td>0-68</td>
<td>0-68</td>
<td>0-76</td>
</tr>
<tr>
<td>$f_2$ (Hz)</td>
<td>151-6</td>
<td>152-3</td>
<td>113-9</td>
<td>70-9</td>
<td>63-4</td>
<td>51-3</td>
</tr>
<tr>
<td>$f_2$ note</td>
<td>d</td>
<td>d</td>
<td>A</td>
<td>D</td>
<td>C</td>
<td>A’</td>
</tr>
<tr>
<td>Equivalent $a'$ (Hz)</td>
<td>455</td>
<td>457</td>
<td>456</td>
<td>426</td>
<td>423</td>
<td>411</td>
</tr>
</tbody>
</table>

(*allowing for the floating first position)

Since the acoustic corrections make only minimal difference to the relative pitch, they do not account for the 11% variation in scaled length in Figure 1, and there remains an apparent trend from the largest to the smallest instrument. Therefore a more sophisticated model is needed to explain the trend. The usefulness of the present model is limited to its prediction of a pitch standard of ca $430 \pm 20$ Hz.
We can also estimate the pitch for the D Quart sackbut with the slide pulled out to the position for playing A'. In Plate VIII the slide positions for this instrument are shown, and the A' position is at a distance of 655 mm from the fully closed D position. With the slide pulled out to A' the overall length becomes 4.445 + 1.310 = 5.755 m, from which $f_2 = 55.0$ Hz, equivalent to $a' = 440$ Hz.

**Appendix B: Measurement and accuracy of instrument length**

In making his drawings, Praetorius probably used a reductional compass [Comm. 704] to transfer important dimensions on each real instrument to the reduced scale of his Plate, and drew the rest by eye. The scale for the Plate would be transferred the same way from a full scale ruler. By measuring the variation in length of each Brunswick foot on the scales relevant here, we can estimate the accuracy to which he was working from those dimensions in the drawings that he transferred. The Brunswick feet varied from the average by a maximum of 1% of a foot or 30 mm full scale, but was usually less than half that.

Our measurements of a parameter on the drawing involved setting the zero of the ruler at one end and estimating the fraction of a millimetre on the other. Errors can easily arise here, as well as in estimating where the centre of the tubing is or the edge of a line if we are dealing with a mouthpiece or a bell. In measuring the scale, the fact that the ruler’s lines are much finer than the lines in the drawing requires special care since we are trying to measure to a higher accuracy than the width of a line in the drawing. Repeated independent measurements indicated that the average difference per measurement represented 2 mm full scale, and the average difference in the sum of measurements to give the length of the instrument was about 10 mm full scale. Because of the variations in length of each Brunswick foot in the drawings, the scale factor from drawing to full scale has an estimated accuracy of about 0.2%.

A particular source of error concerns the treatment of the bows in the tubing. In Comm. 1200 the bow shape was modelled as a semicircle (of radius D/2, where D is the distance between the tube centres along the straight sections) and hoping that the errors would cancel out. The shapes in the drawings are generally more square than this, and for most of the bows a better representation is a ‘handle’ shape (consisting of two quarter circles of radius D/3 connected by a straight piece of length D/3), which gives a correction factor of $(\pi - 1) D/3 = 0.71D$ rather than $(\pi/2 - 1)D = 0.57D$.

As far as the acoustic length is concerned, the bow has a shortening effect because when air pressure waves go round a bend in the tube their speed is increased. According to Nederveen’s theory a tube of bore diameter $d$ bent into a semicircle of radius $D/2$ is equivalent to a straight tube of length reduced by a factor $a = (2^{1/2}D/d)(1-(1-d^2/D^2)^{1/2})^{1/2}$ which has been shown experimentally [5] to be an underestimate by a factor 1.2–1.8. For a typical sackbut $d = 13$ mm and $D = 80$ mm, therefore $a = 1.005$. This means that the acoustic length of a bow is less than 1 mm shorter than its geometrical length, and we cannot measure to that level of accuracy anyway. So although the larger sackbuts have six bows in the tubing while the smaller ones have only two, the effect of pipe curvature is too small to be significant.
FoMRHI Comm 13 28

Praetorius' Pitchpipe *Pfeifflin zur Chormaß*

As they are particularly relevant to the Comm. "Praetorius' Brass Instruments and Cammerlhoir" I have provided here a translation of pages 231-232 of *Syntagma Musicum* II.

NB. In this Volume II I have frequently made mention of the *rechten Chor-Thon*. I have found that in many places, even in large and famous cities and in the splendid organs to be found there, that there is no *rechte Chormaß* to which human voices and instruments should tune, but that their pitch [*Tonus*] is either too high or too low, and this is one of the main faults with the organ. I have thought of all possible ways of rectifying this, so that the *rechte Tonus* and *Chormaß* would be known to everyone including organ builders and organists, and to which an organ builder would be able to adhere in tuning new organs or renovating and adjusting old ones. Therefore the correct diagram of the *rechten Chormaß* is shown below, the *c"* of which gives the organ maker's ½ foot measure (when the large C is 8 foot).

\[
\begin{align*}
C & \quad 8 \text{ foot} \\
\text{c} & \quad 4 \text{ foot} \\
\text{c'} & \quad 2 \text{ foot} \\
\text{c"} & \quad 1 \text{ foot} \\
\text{c"'} & \quad \frac{1}{2} \text{ foot}
\end{align*}
\]

From this measure several pitchpipes can be made precisely to the *rechten Chormaß* over a complete octave, which can be used by organists and singers as well as organ makers to tune to.

In a forthcoming treatise on *Organ Supply and Maintenance* it will be shown how one can bring an organ, harpsichord and [other] instruments which are a semitone or tone higher or lower to the *rechten Chormaß* with minimal trouble and expense.

All of this will be understood by any organ builder (whom I wish well and for whose art I have the greatest respect) to be for the best intentions and not to be taken as antagonism on my part. For the things I wrote about elsewhere, especially on pages 159-160 [about organs that rapidly deteriorate] apply in no way to the honest organ builder but only to butchers and amateurs who try too quickly to play at being the master builder and who have not yet learnt how to set up a pipe properly. I am well aware of what this art is all about, and what counts in reality even with lesser crafts; which perhaps should be discussed at greater length elsewhere, if I get the chance.

**Pfeifflin zur Chormaß**

[diagram \( b = \text{pipe length for rechten Chormaß; the width is 'a' doubled}

Square section for wood, round for metal]

Also, in my humble opinion I know of no better instrument for obtaining the proper pitch [*rechten Ton*] than a sackbut, especially those formerly and still now made in Nuremberg. By pulling the slide out 2 fingers' width from the end one gets an exact tenor \( a \) (alamire) in *rechter Chormasse*.

Because cornets especially, and also flutes, are easily overblown, and curtals and dulcians when they are handled can sound one moment lower and the next minute higher, one cannot rely on them. Therefore on the regal that I happen to have available I set the correct tuning for \( c, f \) or \( g \) from a pitchpipe, to which one can always tune a regal or other *Instrumenta pennata*. This is infallible because one can never retune as precisely by blowing a pitchpipe with the mouth as by means of the bellows of the regal, which are capable of maintaining constant wind [pressure] at all times.
I must apologize for inconsistencies and ambiguity in my brief question and answer article on this subject (Comms 1252 and 1261): it was written primarily for organists, appearing in the British Institute of Organ Studies Quarterly, and from the FoMRHI membership list I assumed that not many would bother to read it. I am always aware that most of my writing needs editing to make it intelligible to others, and I don’t always see that some things that are clear to me are nonsense to others. Thus, when I wrote that the organ now at Stanford on Avon was ‘the chair to the Tewkesbury case when it was at Magdalen College, Oxford’, there may have been many readers having some difficulty in trying to visualise where the cushions might have been placed. I should explain that the Chair organ was the second keyboard division of a two keyboard, or ‘double’ organ. It was housed in a separate case behind the player’s back (Rückpositiv), and first appears in English organs just after 1600.

Having begun my apprenticeship with a Dutchman, I have always used the convention where c\textsubscript{1} is middle ‘c’ of a keyboard, the one two octaves below (normally the bottom note of modern organs) is C, and the one in-between is c, with the higher ones c\textsuperscript{2} and c\textsuperscript{3}. I subsequently trained under a traditional English organbuilder, where these notes were called (from bottom to top) bottom C, tenor C, middle C, treble C and top C, but that is beside the point. Thus, when I wrote that the keyboard range of the early seventeenth century organ was C – c\textsuperscript{3} or d\textsuperscript{2}, I meant that the keyboard had a range of four or four and a bit octaves and included all keys, such as C#.

If this keyboard plays a rank of pipes whose lowest note is an open pipe with a body more or less eight foot long, then the rank is said to be at unison pitch. This convention has been used by organists and other musicians in many European countries (but not all) for a few centuries and applies even if the pipe is actually less than 7ft. As Dom Bédos writes, in 1778, ‘...A Trompette pipe may be only 7 pieds long, if its scale be narrow; but its pitch is said to be 8’t. Similarly the Cromorne, which is 4 pieds long, speaks at 8’ pitch, and is so designated.’ Interestingly, organ stops (what you look at, when seated at the keys, and push in and out in order to select appropriate sounds) in England were not described with any qualifying length notation until the nineteenth century, presumably since there was no need: the Open and Stopped Diapasons were always at the same pitch, the Principal always an octave higher than the Diapasons, etc.

I can see no reason to use I Oft (or 5ft) as a basis on which to calculate pitch, unless pipes are discovered which are indeed that length: extant pipes all seem to be slightly longer. My report that the unaltered Stanford on Avon pipes appeared to be at a pitch between one and two semitones (plus a fourth) higher than modern is surely presentation of findings rather than hypothesis? That this could have been the general (church) pitch level in England in the first half of the seventeenth century certainly is a hypothesis, and one that I would support. The Dallams were the most important organ builders in England from 1600 to c. 1665, providing instruments for most of the top musical establishments, and it would be surprising if their pitch pipes varied greatly. Bédos writes in a different country and century, but about actual practice: ‘There is liturgical pitch and there is concert pitch: the latter varies a half-step or more, according to the singers voices. Liturgical pitch is standard in France; ...the pitch pipe must be calibrated to this standard. Refer to an organ which is known to be exactly on liturgical pitch.’ A very unscientific test in our workshop showed that changes of wind pressure had less effect on pitch than fluctuations of temperature, so an organ tuned in winter to the same pitch pipe as another tuned to the same pipe in summer is likely to be closer to it than if a tuning fork had been used. Pitch pipes, always 1ft ‘c’, are still found in English organs. I would say that it is more likely that Dallam used a consistent pitch standard than less likely.
That there were pitch variations is clear, and there is the well known example of Christ Church, Oxford, whose organist recommended that the proposed new organ in New College should be half a tone lower than his, Robert Dallam suggesting that a quarter of a note would be sufficient.

At Wells Cathedral, Robert Taunton signed a contract on 3rd July, 1662 to build a "...fair well tuned useful and beautiful double Organ..." with "...Two open Diapasons of Metal the longest pipe of each Twelve foot and half... Two Principals of Metal, Six foot longe the longest pipe..." If anyone can use these measurements to establish a pitch level and get the Diapasons and Principals in tune I'll eat my hat. I would say that this is a standard 'transposing' organ, just like those built before the Civil War with a chromatic compass of C - d1 (see below). Is there any evidence that any church organs were built at 'choir' pitch before the arrival of Father Smith at the end of the 1660's? I can think of none. Dallam proposed a grand scheme for New College, Oxford involving two sets of stops at two pitches (based on 24ft and 16ft ranks), but ended up making an ordinary 'transposing' organ (evident from the surviving pipework), Renatus Harris obliging himself to 'Alter the pitch of New College Organ from Gamut in Desolrc to Gamut proper And to make ye Organ one Note lower...' in 1713. As Stephen Bicknell writes, 'activity in 1660 simply restarted where it had stopped in 1642'.

There is no indication of the compass at Wells, but when Renatus Harris took over the care of the instrument he was paid £10 (in 1681) for 'an hundred and two new pipes put into the Chaire organ.' Thomas Swarbrick presented a scheme for repairing the organ on June 8th, 1724, including "...three new sets of Keys, the old ones being so worne.' 'tis hardly possible to make use of them, and to make two new stopp'd Diapasons, which will consist of 102 pipes..." This implies in both cases a compass of fifty-one keys with the appearance of C - d1. It is possible that C# played AA, as in the chamber organs of the time, but I know of only one church organ with this compass (St. Mary's, Finedon) and this is much later (1717). The size of the pipes also indicates that this was a 'transposing' organ - they are just too big to be a very low pitched non-transposing instrument. We know that 'transposing' organs survived into the eighteenth century, but can it be possible that Wells still had one in the 1720's and later? It was conservative enough to introduce equal temperament only in 1893. Taunton's case appears to have survived until 1856, at which time it surely had a GG compass?

The pitches recorded by Dominic for choir (and transposing organs) vary between a = 440Hz and a tone above (plus a fourth)64. This would make Eph's 'consort pitch' of a fourth lower than this very low indeed, and I know of no domestic organs that are anywhere near this. In Comm 1127 Eph proposes that this pitch is about 2½ semitones below modern (around a = 375Hz) and that 'some organs were made at it'. I would be interested to know of any examples. In the next paragraph, he writes that 'After the Commonwealth... most new organs were made to a 10 foot FF specification'. My understanding is that they were made exactly as they had been earlier, i.e. to a 10ft CC specification. In the 1670's, when new organs were made at non-transposing pitches (organ C plays choir C), the keyboards had short octaves i.e. GG, C, AA, D, D#,... long octaves, i.e. chromatic from GG, with or without GG#, only appearing in the eighteenth century, except in exceptional instruments like St. Paul's Cathedral. Short octave instruments continued to be built into the second half of the eighteenth century, e.g. the England organ of 1764 at Gravesend.

The Winchester information has been much speculated on for a long time, and Eph's interpretation offers too good an opportunity for a response, I will quote slightly more than in previous Comms.:

The agreement of 27th July 1665 between the Dean and Chapter of Winchester Cathedral and Thomas Thamer:

"...The Great Organ to have in it nyne Stopps of pipes every Stopp containing one and fiftie pipes whose pitch is to be Gam ut in D sol re viz. In the front One open diapason of tynn the biggest pipe containing thirteene foot in length with his bignesse according to the Monycord..."
There have been at least three different attempts to explain this part of the Winchester Agreement in recent years. Dominic and Stephen Bicknell are united in assigning the thirteen foot pipe to an AA note (played, on a fifty-one note keyboard, by the C# key)\(^{39}\). Eph calls this pipe FF (nothing personal?) at (his) consort pitch\(^{40}\). Andrew Parker has it as DD\(^{41}\). I now propose that it is C.

As mentioned earlier, I have not come across any evidence that AA, played by C#, was in use in Church organs at this date, so I would view Dominic and Stephen's theory as doubtful, though possible.

Eph's hypothesis brings in 'church pitch of F'. My reading of his explanation (...it refers to which key on a ten foot organ corresponds with C....a tone higher than the ten foot organ...) results in the following relationships:

<table>
<thead>
<tr>
<th>10ft organ</th>
<th>c</th>
<th>c#</th>
<th>d</th>
<th>d#</th>
<th>e</th>
<th>f</th>
<th>f#</th>
<th>g</th>
<th>g#</th>
<th>a</th>
<th>a#</th>
<th>b</th>
</tr>
</thead>
<tbody>
<tr>
<td>Church pitch of F</td>
<td>a#</td>
<td>b</td>
<td>c</td>
<td>c#</td>
<td>d</td>
<td>d#</td>
<td>e</td>
<td>f</td>
<td>f#</td>
<td>g</td>
<td>g#</td>
<td>a</td>
</tr>
<tr>
<td>Choir pitch</td>
<td>f</td>
<td>f#</td>
<td>g</td>
<td>g#</td>
<td>a</td>
<td>a#</td>
<td>b</td>
<td>c</td>
<td>c#</td>
<td>d</td>
<td>d#</td>
<td>e</td>
</tr>
</tbody>
</table>

He also states that this 'church pitch of F' is about half a semitone higher than modern, and earlier puts choir pitch at about 2½ semitones above modern. This does not tally with the above table: an 'a' in 'church pitch of F' would be a 'g' at choir pitch. In the same paragraph, he says that this pitch (a tone higher than 10ft) was chosen 'because there was no more need for 10ft pitch and quire pitch was less used'. On the contrary, as I pointed out earlier, I can find no evidence that anything other than 10ft pitch was in use in churches at this date.

Andrew Parker interprets the Thamer contract as follows:

... This, by reference to the monochord, seems deliberately to imply a speaking length of 13' for bottom D, which, in relation to 16' C for a = 440Hz would give bottom D at 80.51Hz, or just over a quarter-tone above D sharp. This does make sense, for, when asked his recommendations about the Chair organ, possibly around 1691-2, Renatus Harris said that he proposed 'to make the pitch of the Organ halve a noat lower nd to make it Gamut proper'.

This implies that the Open Diapason is 16ft, something not found in English organs until the 19th century. Parker is also adrift in his interpretation of Harris's proposal, and makes no sense to me.

Disregarding the 'thirteene foot', this organ appears to be no different to the earlier transposing ones such as Magdalen, which also had fifty-one notes and had a pitch of 'Gamut in De Sol Re', according to Renatus Harris's 1690 agreement to alter it\(^{39}\). It is interesting to follow events at Winchester.

Renatus Harris made an agreement with the Dean on the 7th December, 1693 to put 'in ye present Organ case now standing in ye said Cathedral Church a usefull substantiall & great Organ consisting of the following Stopps viz One open Diapason of fine burnished mettall of fifty pipes in ye Front, a stopd Diapason all mettall except the first octave consisting of fifty pipes.....' He also undertook to repair the chair organ.

A compass of fifty notes at this time suggests a standard English short octave keyboard of GG, C, AA, D - c', and it would be surprising if Harris's new pipes were at a particularly low pitch, yet they are to fit into the existing case, which survived until 1825. Thirteen foot is a nice length for a GG pipe with its foot at a = 440Hz, and also for a ten foot C pipe with its foot. What is more surprising is that Harris only repairs the chair organ, and if the organ was a 'transposer', then it would have been left with a GG short octave '8ft' Great and a long octave '10ft' Chair organ.

Another factor against a thirteen foot speaking length is that it would surely have been larger than any other organ in the country, with the Wells instrument following at twelve and a half foot. The largest pipes in English organ case fronts remained at ten foot GG until the advent of the C compass in the nineteenth century, the earliest instrument with longer case pipes being at Birmingham Town Hall in 1834? Even the Exeter organ, with its twenty foot pipes, only has ten
foot pipes in the case, the 'extraordinary' ones being grouped around the pillars at the sides. The Renatus Harris' organ of 1710 in Salisbury Cathedral was the largest in the country when built: the opening for the largest pipe measured 12' 3" from the pipe tip up to where it disappears behind the carving, so 12½ - 13ft would be a reasonable total length including the foot, at 2' 2½". Another consideration is that front pipes are normally required to fill a particular space, and it is customary to make them overlength so that their tops are not visible below the upper carvings, the backs being cut out to give the correct tuning, e.g. at St Leonard, Shoreditch (built by Richard Bridge in 1756), the largest pipe has a body 12' 4½" (3770mm) long, but has a tuning length at the back of just over 11' (3370mm), giving a pitch of (8ft) GG.

A stumbling point with many writers appears to be the interpretation of the pitch 'Gamut in D'. Bunker Clark offers no explanation, but Stephen Bicknell proposes that 'when the Gamut key is pressed D sol re will sound'. I would say it is the opposite: when d is pressed, g sounds. This is the relationship between the 10ft organ pitch and choir pitch, and 10ft organs are known to be at a pitch of Gamut in D, e.g. Magdalen College, Oxford. However, I must point out inconsistencies. When the ten foot organs of the pitch 'Gamut in D' are converted to 'Gamut proper' at the turn of the century it is evident from pipe markings (both at Magdalen in 1690 and New College in 1713) that 'Gamut proper' is a tone lower than choir pitch. I can only explain this as being the pitch that Harris was normally using (he altered both organs), which was about a tone lower than (my) choir pitch. What is clear is that 'Gamut in D' means a 10ft 'transposing' organ.

On the arguments above, I can propose that the Winchester organ of 1665 was a normal 10ft instrument with a chromatic compass of C - d¹, the same pitch and key range as the early seventeenth century church organs.

It may be appropriate at this point to offer my explanation of 'Church pitch of f'. It is known that Smith's organ at St. Paul's was at this pitch, and it is also known that his normal pitch was 'high'. Dominic cites Ellis, giving three organs at 474.1Hz. This is precisely the 'choir' pitch of the Magdalen (Stanford on Avon) pipes, and I propose that 'Church pitch of f and 'Choir pitch' are the same.

Returning to Stanford on Avon, Eph quite rightly questions the apparent contradictions of pitch movement between 1631 and 1730's: going down a semitone, then back up one and yet ending up a tone lower. Unfortunately, a simple explanation is not possible (from me), but I will try.

This instrument was originally built as a Chair organ and had a chromatic compass of 51 notes, C - d¹, with the following stops:

<table>
<thead>
<tr>
<th>Stopped Diapason</th>
<th>(10')</th>
<th>which Harris agreed to change to: Stop Diapason* re-using those stops marked *</th>
</tr>
</thead>
<tbody>
<tr>
<td>Principal</td>
<td>(5')</td>
<td>Principal*</td>
</tr>
<tr>
<td>Principal</td>
<td>(5')</td>
<td>Flute</td>
</tr>
<tr>
<td>Recorder</td>
<td>(5')</td>
<td>Nason</td>
</tr>
<tr>
<td>Fifteenth</td>
<td>(2½')</td>
<td>Fifteenth*</td>
</tr>
</tbody>
</table>

One of the Principals was used for the front pipes, but half way up two pipes are duplicated (two f's and two D#'s). The two extras are slightly smaller in scale, and it is clear from the (surviving original) windchest that they are drawn from the other Principal. When Harris altered the stops in 1690 he merged the front pipes from both ranks (but using only one of the duplicated ranks). The drawing is a reduced copy of a full sized one showing all the pipe markings (and some other details) of a few of the front pipes.

Taking one pipe as an example, pipe no. 5 has an original Dallam mark of 'g#'. It is then marked 'g', 'DD#' (twice: once in Harris style, once in an eighteenth century style), and 'ee'. From examining the other pipes, I think it is possible that there was confusion about how to move pipes, merge two stops and transpose them, so that the person re-marking this pipe in 1690 thought that
by The fifty note compass implies a short octave GG - c1 keyboard, and Harris also agreed to make the organ one note lower and his 'DD#' supports this change, with the 'ec' mark implying a drop of one semitone. Added to the confusion of pipe marks, this pipe has evidently been cut down and patched: a filled scoop in the back shows that it could once have spoken approximately 'e' (at a = 440Hz); the filling in would make it around d#; an extension drops it down to d, and the scooping out of this extension brings it back up to d + 15 cents (about 148Hz). Each pipe has its own story, but certain trends can be detected:

1690 In order to change the pitch from Gamut in D to Gamut proper, the sequence is:

5ft C C# D D# E F F# G G# A A# B c etc.
8ft GG AA C D D# E F F# G

In order to lower the pitch by a semitone, the pipes are moved into the next hole up, and so need to be re-named: AA C D D# E F F# G G#

In fact, since the longest pipe cannot go anywhere else, it was kept as before, but lengthened. Instead of moving D to become AA, the old C# was used. Also, the old D# became the C instead of the old D.

A little higher, the f and f# are repeated, the second pair smaller in scale: they must be from the second Principal. If one of these pipes is inserted into the rank, the subsequent pipes can continue in their original places:

5ft Principal I d d# e f f# g g# a a#
(5ft Principal, combined) d d# e f f# f# g g# a
(8ft untransposed) A A# B c c# d d# e f
8ft transposed 1/2 tone down A# B c c# d d# e f f#

In the 1730's, the organ builder appears to have re-used the f pipe that was made redundant in 1690, keeping the higher pipes as before, but moving the pipes below down a semitone:

Before A# B c c# d d# e f f# g
(Originally called) d d# e f f# f# f## g g# a a#
After A A# B c c# d d# e f f# g

What is interesting about Harris's 1690 work is that if the Dallam organ of 1630 was at a pitch of about a = 475Hz (from the unaltered pipes as reported in Comm. 1261), then his Agreement ('the said Renatus Harris, his executors or assigns, shall and will alter the pitch of the said organs half a note lower than they now are; and the said organs, being now Gamut in De, Sol, Re, ...') and the evidence of the pipe marks results in a lowering of the pitch, firstly to about a = 425Hz (by making 5ft C into 8ft G), and then lowering it further to about a = 400Hz. Tansur describes this as 'old Consort Pitch' in 1746, if we can assume that 'Our new Consort Pitch', half a tone higher, is the usual eighteenth century level of about a = 425Hz. I might guess that this low pitch could also have been called 'Consort flute pitch'.

It will be noticed that Harris does not propose to change the pitch to 'Gamut proper', as he does at New College in 1713, and I had assumed that this was an oversight of the scribe. However, earlier in the contract he specifies the number of pipes that will be in the organ: 'Item. That the said Renatus Harris, his servants, workmen, or assigns, shall and will make to the great organ a new great twelfth of metal, a cedrine of metal, and a furniture of three ranks, and a cymbal of two ranks, and shall and will repair, well voice, and tune, in the great organ, the open diapason, principal of metal, stop-diapason of wood, fifteenth of metal: which great organ shall consist of five hundred sixty and one pipes: and make to the choir organ a new flute of metal and nason of metal, and repair, well voice, and tune in the choir organ the principal, stop-diapason, and fifteenth, which said choir organ shall consist of two hundred and fifty pipes.' This implies that the Great is
to remain a fifty-one note manual (C - d'), but the choir is to be a fifty note manual (GG - c', short 8\textsuperscript{v}). The evidence at Stanford supports the change to a GG compass, and apparently the pipes at Tewkesbury (the old Great organ) corroborate the maintenance of the fifty-one note state\textsuperscript{[13]}.

This is the only unequivocal evidence for two pitches/key compasses in one organ. However, there are other possibilities:

1. Winchester Cathedral, 1693, noted above, where Harris (again) provides a new Great of fifty notes, but only repairs the choir organ.
2. Canterbury Cathedral, 1684, where Smith also provides a new Great but repairs the choir (and lowers the pitch by half a note\textsuperscript{[14]}).

I said earlier that I thought it quite possible that a choir pitch level about the same as Magdalen College (about a = 475Hz) could have been in general use in England in the first half of the seventeenth century. I would go on to say that this pitch survived into the eighteenth century. John Harper has proposed that it may also have been at this level in the early sixteenth century\textsuperscript{[15]} from evidence in the Rites of Durham\textsuperscript{[16]}: the 'White Organ' on the south side of the choir, from before the Dissolution, remained in position until 1650 (James Smart heard it played in 1635 and 1636); Dallam built a new organ on the screen in 1621, and would surely have made it to the same pitch as the other, and that this pitch was one that Dallam would have been familiar with. The early sixteenth century documents only mention 10ft organs and C compasses, the same as when organ building activity revived at the beginning of the seventeenth century and Dallam was in the centre of activity.

I am certain that this complex subject will gradually become better understood and more familiar as more evidence turns up, original documents are re-examined, and the ramblings of organ builders are picked to pieces by scholars.

\textbf{Notes}

3. Ibid., p. 233
7. \textit{The Organs and Organists of Winchester Cathedral}, Betty Matthews, (1975)
8. BIOS Journal 9, 1985
9. FoMRHI Comm 1290, July, 1994
10. \textit{Winchester Cathedral Organs, One Thousand Years}, Andrew Parker, Friends of Winchester Cathedral, 1994
11. Reprinted by Hopkins and Rimbault in \textit{The Organ}, (1877) from Bloxam’s Register of St Mary Magdalen College (1857), where he mistranscribes ‘Gamut in De Sol Re’ as ‘Gamut in Do Sol Re’; See J. Harper, \textit{The Origin of the historic organ at Stanford on Avon} (The Organ Yearbook Vol. XXIII, 1992/3)
13. From a survey of the organ carried out by David Wickens, 1994.
14. \textit{Records of Father Smith}, S. W. Harvey, \textit{The Organ}, Vol. 1, p.97, 1921
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