Quarterly No. 86, January 1997

FoMRHI Quarterly

BULLETIN 86
Bulletin Supplement
Membership List Supplement

COMMUNICATIONS

1487 Review: The Last Trumpet - A History of the English Slide Trumpet by A. Brownlow
J. Montagu 7

1488 Review: Larigot, ...VI Spécial, Septembre 1996
J. Montagu 8

1489 Pitch standards in northern Italy
E. Segerman 9

1490 The musical instrument - an oscillating system affected by high internal stresses
E. Weiss 13

1491 Leicester Early Music Festival 1997
J. Bence 15

1492 Violin research 15 years ago
E. Segerman 16

1493 On the calculations in Comm. 1480 by Lee
E. Segerman 17

1494 Mersenne's iron and Ruckers 6-foot pitch
E. Segerman 18

1495 Harpsichords and 'Buntpapier'
M. Puhringer 19

1496 Segerman, Godwin, Fludd, and the English cittern
P. Forrester 21

1497 Reply to Forrester's Comm. 1481
E. Segerman 22

1498 Response to Comm. 1482 and the Wensler G30 cithrinchen stringing
E. Segerman 25

1499 A provisional list of quinticlaves (alto ophicleides) in Europe and the U. S.
M. Jones, A. Myers & A. R. Rice 27

1500 Comment on Comm. 1486 (recorder patents)
D. Thomas 30

1501 Four baroque recorders at Sotheby's, November 1996
J. Bouterse 31

FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS
Honorary Secretary: Jeremy Montagu, 171 Iffley Road, Oxford OX4 1EL, U. K.
A Happy New Year to most of you — it looks like about two-thirds have already renewed, rather
better than some years, and our thanks to you for doing so. As I said in the last Bulletin, it helps
very much to get renewals in before this one goes out, and for those of you who read this at the
end of April or in early May, that’s why you haven’t seen it sooner. Any renewals that come in
after this was mailed will have waited to get it until the April Q goes out.

PRAISE FOR OUR PRINTER: Paul Hailperin commented, and I heartily agree with him, on
the excellent printing and the much improved quality of the paper of the last Quarterly — I hope
he reads what he prints, and in case he doesn’t I hope that Eph will point this out to him!

LOST MEMBERS: Istvan Magyar of Budapest joined us in time for the October Q and then
moved house — if anyone happens to know him and to have a new address, please let me know.

Tim Cranmore has moved (again) without letting us know. Any new address for him will be
gratefully received (as in the past!).

FOUND MEMBER: Marco Perini was never lost — it was just that a pedantic postman, seeing
the address wrongly written as Via Cadvilla didn’t think to try Via Caovilla. So Q 84 didn’t get
delivered, whereas a more intelligent chap did deliver Q 85!

IN MEMORIAM: Andrew Jowett, who has been a member since 1991, has died.

FURTHER TO: Bulletin Supplement, p.7, CD-RoMs: The dots to which Eph refers are, I
think, the pixels of his screen. I was allowed to look through the catalogue of the Institute of
Jewish Art in Jerusalem last week, and while one could enlarge things to a very considerable
extent, the limit was always the pixels of the screen, which produced a mosaic-like effect, rather
than the quality of the photo. And I think that such catalogues are valuable, even though they
may seldom be worth any of us buying them for ourselves. Despite my criticisms of Wallraf-
Richartz, I got a couple of new references from it, just as I got several from the IJA (their’s is
not yet published). These CD-RoM catalogues may be more useful for libraries than for us, but
bearing in mind the usual inaccuracies or vaguenesses in any catalogue which refers to musical
instruments (odd, isn’t it? They go to immense trouble to get many technical terms right in
archaeology or weapons, for instance, but seldom bother about the difference between a harp and
a lyre or a flute and an oboe), it is invaluable to be able to see what’s there. It is also a lot easier,
and cheaper, to go to the nearest art library than to Cologne, and even when one is intending to
go to Cologne next week, a preliminary canter through the CD-RoM can save a lot of time.

Comm. 1485: Steve Heavens writes (and also asks me to correct my mistyping of his email
address, for which see the Members’ List Supplement herewith — my apologies to him): ‘Alec
Loretto’s method of drawing a regular pentagon (Comm 1485) is not particularly accurate, as the
arc length is undersized by 1.7%. Can I suggest an alternative. Using his Figure 2, ignore F and
G. Join AC with a straight line and bisect it 3 times to give length AC/8. Set the compass at this
length and draw a circular arc, centre E, cutting CE at a point H. Draw a circular arc, centre D,
radius DH, cutting the circle between A and D at point J. The arc DJ is now 1/5 of the circumference, to an accuracy of 0.1%. Apologies for the lack of a diagram and mathematical explanation, but anyone sufficiently interested can easily test it themselves.

**QUERIES:** William Hendry asks whether anyone can produce help on rose making, especially those for harpsichords made from parchment etc – he’s more interested in those than the cast metal ones. He’d be very grateful for any references to printed details. All that I know are John Rawson’s Comm. 167 (Q 13, 1978) and Ian Theakston’s Comm. 234 and Paul Kemner’s Comm. 235 (both in Q 17, 1979). All are quite short, though useful, and unless there is something more detailed in print elsewhere, could anyone please write us a new Comm. on this?

Antonio Longo, who says that he has recently joined us though nothing has yet reached me (his address is Via degli Imbriani 35, 1-20158 Milan, +39-2-33220601), plays ‘ancient harp music in a chorus in Milan. I also reconstruct double and triple antique harps in order to be able to play this marvellous antique Celtic music.’ He would be very pleased to be in contact with other harp people, especially if they can help him with measurements etc of some harps he is trying to copy.

Gillian Alcock will be travelling to Europe to attend the 4th World Dulcimer Congress in Belarus. At the same time I will be continuing research into dulcimers (the hammered sort). I am compiling a Checklist of Dulcimers in Museum Collections. In the next FOMRH Bulletin, maybe you might alert members to the fact I am compiling this list and would welcome input. Which museums keep dulcimers and would they be able to tell me about them. I have a proforma information gathering sheet, which members might be generous enough to complete.

She also asks me, and I include this paragraph because it’s a good example of driving me up the wall – I produce a List of Members each year, in it there is a list of all the museums where we have a member or which are themselves members, almost all (certainly these two) with the names of a contact who should be able to help. Do please help yourselves by using the List of Members – otherwise I start to feel it’s not worth my while to spend the time on it, nor is it worth FoMRHI’s money to produce it! ‘The second issue is about the Brussels collection. I have seen the collection on public view, but what can you tell me about the unseen collection, the store? Is it enormous, how does one seek access, possibly the name of a helpful person to contact? There doesn’t appear to be a copy of Mahillon’s catalogue in Australia. Even though it is out of date, it would be interesting to know what dulcimers he included in his catalogue. Can you help in any way please? And I’ve thought of a third place which is of interest. Barcelona. I understand there is a catalogue of the collection there. It would be very helpful to know what their dulcimer collection is, to decide if I should go there while in Europe.’

Would Ignace de Keyser and Romá Escalas please be kind enough to respond to Gillian, whose address and email are in the List of Members?

Bjørn Eggen of Sundv. 41, 3115 Tinsberg, Norway (his email may be bjeggen@online.no – what I received marked that as ‘unverified’!) writes (I have altered his spellings etc): ‘I have just started building classic guitars after studying under Norwegian Luthier Leif Jorgen Johansen. Because of the high prices of soundboard spruce I have been looking for Norwegian spruce fit
for instrument making and especially guitar sound boards. In association with some local “wood-
freaks” in mid-Norway mountain area, we have located spruce fit for these purposes. They grow
at about 800 metres above ocean level and on the shadow sides of some valleys. These trees are
about 250-300 years old. The year-rings are at an even 1mm. During this search we have been
told that there was export of luthier woods to England during the Renaissance. This information
has been on a “from father to son” basis. The Ringve museum of historical instruments in Nor­
way has no knowledge of this, neither has the museum of Norwegian woods at Elverum. Do you
have any clues of where I can get any information on this subject? Are there any good instru­
ments still “alive”? Is there any information on luthiers of that period and where they got their
material? Any written sources at all?”

MATERIALS AVAILABLE: Martin Pühringer (slightly revised address in the Supplement
herewith) ’can offer soundboard wood for harpsichords and early pianos (slightly thicker). It is
quarter-sawn spruce with length 1.5 to 2.2 metres, width from ca. 6 cm upwards, thickness ca.
7 mm. The timber will be freshly sawn this winter and sufficiently dried (not kiln dried though!)
for shipping without any risk of damage. It will be available in spring 1997. The price is around
$850 - plus Austrian VAT (=20%). Orders would be welcome before Feb/March 1997 to have
some idea about the quantity required. The wood comes from the Bohemian Forest where I select
the trees myself and have them cut down and sawn to my specification. Since I shall be buying
several trees this winter I could easily have some more sawn for other makers if desired.’

Jan Strick (Rue Ernest Allard 38, B-1000 Brussels) writes: ‘We are actually working on a book
on the Flemish School of Violin Making in the 17th and 18th centuries. We would like to send
subscription forms to your members. For this reason may we ask you to send us the list of your
members and their addresses.’ I’m getting a bit chary of sending out our list for this sort of thing
– he doesn’t even say who ‘we’ are. So if you’re interested, write to him and ask him for a
subscription form, saying that I suggested it!

NEW JOURNALS: Two have come to hand. One, Le Messager, from Les Amis de la Musique,
Avenue Reine Astrid 73, B-4900 Spa, Belgium (who send out a useful list of new and second-
hand books on music and instruments from time to time) has some interesting material on French
violin and gamba makers, also on organs and some other instruments. It’s in the form of a 4-page
broadsheet newspaper (ie one large sheet of paper). There is also an Internet site: http://www.
tornado.be/~amis_musique/bienve.htm and an email address amis_musique@tornado.be

The other is the Royal College of Music Museum of Instruments Newsletter (they’re in the List
of Members under the name of Elizabeth Wells, the curator) with news of activities and acquisi­
tions – also saying that they will be closed until the end of April next to allow for work on new
show cases.

Neither give any indication of cost; the Messager I think is free to those interested; certainly their
Web site is.

EVENTS: The British Clavichord Society has a day on Haydn’s keyboard music at St John’s
College, Cambridge, on Sunday April 6, 11 am to 7 pm (approx). Derek Adlam, Susan
Alexander-Max, Richard Maunder, and Kenneth Mobbs are playing and speaking and there will
be time for discussion. Cost £25 (£32.50 non-members). Details etc from Judith Wardman, 26A Church Lane, London N8 7BU, t&fx 0181-341 4700; e-m 100603.2732@compuserve.com.

I told you last time about the Magnano Clavichord Conference. There’s also a series of concerts each weekend in August and early September (entry free) and an early keyboard course (also organology and singing), 14-24 August run by Bernard Brauchli. Same address, and email bbrauchli@worldcom.ch.

NEW BOOKS: One, which was light enough to take to Jerusalem, is reviewed herewith. The other, which looks magnificent but was too heavy to carry by air, and anyway looked more like serious work than I’d want to read on holiday, is Florence Gétrau’s magisterial *Aux origines du musée de la Musique – Les collections instrumentales de Conservatoire de Paris, 1793-1993.* I’ve only had time for a quick look at it – it seems fascinating and I’ll write more for next time.

CODA: That’s it. I’ll hold this open till tomorrow when I’ve done the Memb List Supplement.

DEADLINE FOR NEXT Q: All Fools’ Day looks fine – try to get in by the end of the previous week, and then I can spend Easter Monday (a holiday here) doing it. Remember that the new List of Members comes with it, so check that you’re up-to-date in that, and especially let me have any email addresses so that we’re all better in touch with each other – that was one of our initial aims in FoMRHI: rapid communication between colleagues, which was why to begin with we only allowed airmail abroad (until the post office started charging too much for it to be fair to insist on it). As you’ll see from the Memb List Supplement herewith, more and more of us are getting on to email. Sending the Q out by email would be excessively complicated because it would mean scanning in everything that comes on paper, and anyway I don’t think one really wants to read things of such length on screen. But maybe we should have a Web site? Any suggestions on this? And any offers to run it? (I’m a computer illiterate and haven’t the know-how – nor am I sure whether the University Computing Service would allow it).

Despite the frost, bulbs are beginning to come up in the garden, so there’s hope for the end of the winter!

Jeremy Montagu
Hon Sec FoMRHI
jeremy.montagu@music.ox.ac.uk
Folk Acoustics
In the Emil Weiss Comm. in this Q, the explanations of how the 3-day sound-blasting treatment works make no sense scientifically, but by mixing musician’s folk-lore about acoustics with acoustics generalities, it makes sense commercially. There is reason to expect it to work on newly strung stringed instruments (by accelerating creep), and perhaps a bit on old stringed instruments (temporarily by reducing moisture content, and maybe permanently by degrading some hemicellulose), but how can it improve wind instruments? Will it shake off dried saliva? I wonder whether his secrecy is to hide ignorance or is intended to enhance his image as an instrument magician. I also wonder how much he charges. He is not a member, so I am allowed to comment in the same Q.

Comm. 1486 is OK
Steve Heavens is mistaken in his claim (see Bull) that the method given of dividing the circle into ten in Comm. 1486 is not accurate. Let AE = r, the radius of the large circle, and AG = a, the chord length which, end to end, divides the circle into n parts. According to Pythagoras, a + r/2 = \sqrt{r^2 + (r/2)^2} = (r\sqrt{5})/2. Thus a = r(\sqrt{5} - 1)/2. The sine of half the angle subtended by the chord is a/(2r) = (\sqrt{5} - 1)/4 = sin (360° / 2n). Thus n = 180° / {arcsin[(\sqrt{5} - 1)/4]}. This is 10 accurate to the 8 digits on my calculator. To prove this analytically, we can use the expression: sin 5x = 5 sin x - 20 sin^3 x + 16 sin^5 x. If we substitute sin x = (\sqrt{5} - 1)/4, we get 1 absolutely, so 5x = 90°, and x = 18°. Then n = 180° / 18° = 10.

An editorial matter
I have a short Comm. in this Q correcting Lee’s mathematics in Comm. 1480. I would expect that he would have felt better about it if, instead of publishing his Comm., I had sent it back to him with suggestions for revision, as any proper editor of a journal should. I must apologise to him for this. If I had read it carefully and realised its shortcomings before assembling that Q, I would have done just that. But as usually happens four times a year, I get the bulk of the Comms. from Jeremy near the middle of the relevant month, and feel under considerable pressure to get that Q assembled and out to the printer as quickly as possible. So I only have a quick read through each Comm. to check whether it is incomplete, unreadable, libellous, dangerous (advocating bad practices) or irrelevant before assembly. Might I suggest, if a writer feels that his/her Comm. might benefit from my detailed editorial attention, that he/she sends it directly to me with a cover note requesting it.

Forrester Comm. in this Q
In this Comm. Peter criticises me for not consulting the primary source. Of course he is quite right. But from a purely practical point of view, who needs to bother when you have friends like Peter who are happy to sort you out when you go wrong? :- ) (This symbol, with variants like ; -) or 8 -) is widely used in e-mail to indicate a smile.) I’ll reply in the next Q.

Baschenis
Peter Forrester writes:
The Baschenis exhibition: I have two books on Baschenis which might be worth mentioning. They are both by Marco Rosci, and the second seems to be a rewrite of the first. Both also include material on his pupils and on Bartolomeo and Ventura Bettera. The second also has a section on the instruments by Giorgio Ferraris.
Evaristo Baschenis, Marco Rosci, Poligrafiche Bolis Bergamo, 1985. – 204 pages, numerous illustrations, sometimes five or six to a page, in both colour and black-and-white. Price in Milan two years ago, L.60,000.
The reproduction in both books is reasonable although not to be compared with seeing the paintings themselves. One thing that does emerge from seeing several pupils’ versions of the same painting side-by-side is the potential unreliability of a single uncorroborated image.

This is an admirably detailed study of the final incarnation, before the early music revival, of the natural trumpet, an incarnation which was in the main confined to the British Isles. It is a fascinating thought that players such as Bernard Brown, with whom I played many concerts (and who gave me the only trumpet lesson I ever had) and Ernest Hall, the famous BBC Symphony principal and professor at the Royal College of Music, were themselves pupils of the last professional natural trumpet players, Walter Morrow and John Solomon. Those two spent most of their careers on the valve trumpet, of course, Solomon, being the younger of the two, on the new four-foot-six B♭, Morrow on the six-foot F, but both began on the instrument which is the subject of this book, the English slide trumpet. It is only by one or two generations that we missed a continuous tradition of natural trumpet playing from Purcell’s trumpeter, John Shore, to the present day. For there is no doubt that the slide trumpet is a real natural trumpet, a wholly different instrument from the rubbish with as many holes as a colander which is only too often heard under that name today.

The slide trumpet does not try to modify the 11th and 13th harmonics (respectively halfway between F and F sharp and a very flat A) by venting the tube so that all the tone flies out through the holes like dust from a window. It does it by lengthening the tube slightly by extending its slide, but it remains an integral tube as it does so. One day, perhaps, more players will realise (some already do) that just as the Harpers, Snow, and Hyde were firmly in the tradition of Purcell, Handel, and the other great composers of the past, so could they be. The real problem, one suspects, is convincing the conductors, who seem only too often to be far less conscious or knowledgeable of the real nitty-gritty of instrumental authenticity than the players.

As and when this ever happens, most of the information that they will need is here. I say most because the one glaring omission is any real discussion of mouthpieces, other than a brief description and an illustration, misleadingly reduced in size, of Harper senior’s (the original print was deliberately full-size). Crispian Steele-Perkins, a major informant for this book, has proved beyond any doubt that using a mouthpiece of original diameter, depth, and shape makes an enormous difference to the sound and, against all expectation of modern players, that one has more control over tone, tuning, and range with the big original mouthpiece, with its hemispherical cup and sharp edge into the throat, than one ever can have with the small modern mouthpiece with its soft cup and gentle drift into the throat.

This book is a major feather in the cap for the Historical Brass Society. They are the most recent of the major organological societies, and here they are already producing their own monograph series, something that neither the Galpin Society, fifty years old this year, nor the American Musical Instrument Society have achieved. I could wish, mostly for their sake, that it was more a book and less a thesis. It has a number of those repetitions which are advisable to make in a thesis, to ensure that examiners take the point, but which are annoying in a book. It is grossly over-footnoted, again expected in a thesis, where every I must be dotted and every t crossed whether the sense be clear without or not. More seriously, perhaps, it does not follow up obvious lines of enquiry which are
important to the subject but perhaps marginal from the thesis point of view. One example is that of mouthpieces, noted above. Another is the sources of Neukomm’s music. He is quite forgotten today, but in the 1830s he was a major composer for the slide trumpet, which makes it all the more regrettable that in the musical section of the bibliography he is the only one without library references. There are some other oddities in this section. Neither Handel’s Messiah nor Samson appear, even though ‘The Trumpet Shall Sound’ and ‘Let the Bright Seraphim’ are acknowledged as the major war-horses of the nineteenth-century slide-trumpet virtuosi – the Seraphim does appear in a 1970 rehash by Brass Press! I can provide details of other such anomalies as I have caught on request and of points in the text where assumptions are rather too easily accepted, but I think it unnecessary to cite these here. One error that does need correction is the title of Harper junior’s Grand March for 4 Trumpets, reproduced in facsimile in figure 35 – Harper’s handwriting is perfectly clear and the title is The Dead March for 4 Trumpets.

Such small points, for example the cut between the bottom of page xxi and the top of page xxii, apart, this is an excellent book and will stand for many years as the prime source on a much-neglected instrument, one which we may hope will regain its rightful place not only for the performance of baroque trumpet parts, but for all music played in Britain between its invention in the 1790s and its demise almost exactly a century later. This, when it was newly-invented, is the trumpet on which Haydn’s London Symphonies were first played. A century later it was still the trumpet of choice for writers such as Ebenezer Prout and for the music of many of his contemporaries. It may not be the instrument for which Bach, Handel, and their contemporaries wrote, but its use in their music, since it is a real natural trumpet, would be far more ‘authentic’ than those horrid things with holes in their tubes.


This is volume 2 of the catalogue of Patrick Delile’s collection of trumpets and cornets. The first volume was III Spécial of November 1993, not as given here on p.3 IV Spécial, and covered the first 116 instruments in Delile’s collection. This volume runs from no. 117 to no.200. All the most interesting instruments are photographed, many of them from more than one angle. These Larigot special catalogues are invaluable for anyone who is working on wind instruments and who needs clear descriptions and illustrations of almost every possible variant of any instrument-type. We must be grateful not only to Larigot, but in particular to Bruno Kampmann who does much of the preparation of each volume and, I think, of the photography, and even more to the owners of the collections who are willing, as many owners of private collections are not, to reveal to all the extent of their collections.
Pitch Standards in Northern Italy

The 1994 issue of Ricercare (Vol VI) includes an article (in English) entitled ‘Pitch in northern Italy in the sixteenth and seventeenth centuries’ by Bruce Haynes. It contains a marvellous collection of evidence on the subject, and he should be heartily congratulated for collecting it. As would be expected, his interpretation of the evidence is forced to be consistent with his previous publications and he puts full trust in the pitches derived from modern blowing of surviving cornets. The most questionable as well as crucial part of the article is rushed (mostly confined to footnotes), so some of the relevant evidence that is important for evaluation of his interpretation is missing.

A very important contribution of this article is providing original names for two of the three main pitch standards discussed. These two were cornett pitches and were often referred to as such when organs were tuned to them. The lower was called *tuttopunto* and the higher *mezzo punto* (possibly originally meaning 'full-size cornett pitch level' and 'half-size cornett pitch level'). One source in the last quarter of the 16th century indicates that the usual pitch of organs in the region around Venice was *mezzo punto*. Another in this period gave the intervallic relationship between these two standards. That was Morsolino, who was consulted about changing the pitch of the Cremona organ, arguing that it should go up. This was in response to the original recommendations of the choirmaster and the organist, who wanted it to go down by about a semitone to be able to play with choirs and all sorts of instruments.

Morsolino wrote that the current Cremona organ pitch was *tutto punto*, and that all of the organs he knew that were used to play with the greatest performers were at *mezzo punto*, a tone higher than *tutto punto*. He recommended raising the pitch by a tone since *mezzo punto* played with the wind instruments, and though it was too high for chapel singers, all organists of merit transposed down when playing with singers. According to Diruta (1609, writing in Venice), the transposition would be down by a tone or a minor third (going around the circle of fifths either 2 steps in the flat direction or 3 steps in the sharp direction respectively), whichever is most convenient. Transposition by a tone would give *tutto punto* and transposition by a minor third would give the pitch standard that the original proposal for the Cremona organ was intended to achieve, usually called *tono corista*, meaning 'choir pitch'. Both were considered appropriate for church choirs and instruments, but *tono corista* rather more so, being the dominant standard for church choirs and stringed instruments throughout Italy and most of Europe at that time.

This is all quite straightforward and consistent with all of the evidence on pitch standards as I understand it. Venetian organ pitch (or *mezzo punto*) was somewhat less than a semitone above modern, *tutto punto* was a bit more than a semitone below modern, and *tono corista* was a bit more than a tone below modern. If all you want to know is what the positive contribution of this paper to our understanding of pitch standards is, you need to read no further. What follows here is a discussion of the evidence and a critique of what Bruce does with it.

The above picture is unacceptable to Bruce because of the following: A 1701 Prague source indicates that in (south) Germany and Bohemia there were two pitch standards a tone apart, the higher one called 'cornett pitch' and the lower one called 'choir pitch' or 'Chor-thon'. The latter was also used in Italy and France and went by the same name. With the German and Italian choir pitches the same, the above indicates that German cornett pitch would fall between the two Italian cornett pitches. That is not neat, especially if one wants a simple picture of cornett pitches so as to use the pitches of surviving cornets as evidence, and the cornets don’t tell us where they were made or played. Also, the German pitches are just what Praetorius wrote they were in this region 82 years earlier, and according to Bruce’s interpretation of Praetorius’s pitches, Cammerthon (which Praetorius said was the same as cornett pitch) was the same as the pitch of Venetian organs and wind instruments. Thus, to be consistent with his picture, the interval between *mezzo punto* and *tono corista* would have to be contracted from a minor third to a tone. This Bruce does by assuming that every time that Morsolino mentioned
the interval *un tuon* (a tone), he meant a semitone! He supports this assumption by interpreting multiple meanings as ambiguity that is resolved by discovering each author's individual and consistent usage.

The word *tuon* has multiple meanings in Italian as well as in English. It means 'manner' or 'pitch' as well as the diatonic interval of a major 2nd (which I call here 'a tone'). Which is meant is generally clear from the context. *Un tuon* could also mean a pitch difference in general, and when referring to organs, it could mean 'one pipe over' which is a semitone. The interval of a tone was usual, and if the writer was using it otherwise, he provided a context which clarified the meaning intended. Thus the Facchetti II 1626 proposal for the organ at Salò used *uno tono l'organo* with *unacana*, (one pipe), making it clear that a semitone was meant. Bruce offers another instance, that in a 1652 book by Barcotto, where we know that *tretuoni* meant a minor third (the interval between the organs in Rome and Padua), but it is not clear whether Barcotto expected his readership to know this. Bruce does not give us the context for us to see whether the book clarified whether organ pipe shifts were meant. He tries to establish an ambiguity where each author consistently used the term to mean a tone or a semitone without the need for context clarification. This does not make linguistic sense, since the readership of the different authors was essentially the same.

Bruce offers a proper quotation of another passage in Barcotto’s book, which states that the pitch of chamber organs in the Venice area was *un tuono* lower than that of church organs and was called *corristi*, a pitch appropriate for choirs and instruments. With both Bruce’s semitone and my tone interpretations of *un tuono*, *corristi* is a semitone higher than our respective *corista*s. Bruce considers that there was ambiguity in the pitch standard represented by the term *corista* since the term was also used to mean 'choral pitch' generally, but since many writers clearly used it for a particular pitch standard, I prefer to consider that *corristi* was synonymous with *tuttopunto*, and was different from *tono corista*. The interpretation of Barcotto’s *un tuono* as a tone is preferable to it being a semitone because the interval between two pitch standards, one considered to be appropriate for choirs and the other not, is never otherwise (as far as I remember) less than a tone.

The main argument Bruce uses to support his assumption that Morsolino meant semitones is that Morsolino, as he reports it, described the original proposal for lowering the pitch of the Cremona organ (by about a semitone) as a proposal for lowering it *un tuon*. Bruce does not quote the passage where this is mentioned, so we cannot see whether the context is the same as with the quoted passage. If there is no specific context clarification, there are still reasonable explanations for this discrepancy that are more likely than Bruce’s assumption that semitones was always meant. One is that the semitone of the original proposal was clear to the intended readers, so using the term in one of its other meanings (generally as an interval, which could be a semitone) would not be misunderstood. Another involves whether making the change to the Cremona organ involved shifting the pipes one or two holes. The cost of making new largest pipes for each rank is much greater if the shift were two holes rather than one. Morsolino could have considered that a shift of two holes was necessary for a proper job, and he may well have been right. No pitch change to the organ was actually made after the consultation exercise, and the reason given was that lowering the pitch would reduce its tone and liveliness of spirit. This would make sense if finance for a shift of only one hole was available and the semitone required was a rather wide one. Then much bending in of the tops of the pipes would be necessary to tune it to the desired pitch, and that could dull the sound noticeably.

The final point Bruce uses to support his semitone assumption is his claim that cornets of the time were tuned a semitone apart, not a whole tone, and that is consistent with the cornett pitches *mezzo punto* and *tuttopunto*, being a semitone apart. That is a surprisingly small interval. I am sure that one can play accurate scales on a cornett a semitone or more apart, using the same fingerings and just pitching by lip control. Sure, the scales feel different on the lips and have different sound quality, but no-one has any historical authority to say that one is right and the other wrong, or that early players couldn’t do both and transposed a semitone this way if it was appropriate. A tone seems to be a more likely pitch difference to make different sized cornets for. But Bruce writes with certainty that they were made a semitone apart, and
thinks that he proves it with a graph of the pitches of surviving 16th and 17th century cornetts.

The graph shows, separately for each century, the number of cornetts at each Hz. The number per Hz for the 55 cornetts from the 16th century is 6 for 465 Hz, 5 for 471 and 482, 4 for 476, 3 for 450 and 473, 2 for 437, 445, 463, 464, 466, 469 and 474, 1 for 434, 440, 452, 457, 458, 462, 468, 478, 480, 484, 488, 490, 493, 494 and 504, and 0 for each of the remaining Hz. The number per Hz for the 20 17th century cornetts is 3 for 472 Hz, 2 for 434, 450, 460, 464 and 487, 1 for 415, 440, 445, 447, 465, 471 and 490, and 0 for each of the remaining Hz values. The text says that there are 80 cornetts on the graph, so 5 cornetts included perhaps in an earlier draft are now missing.

A more meaningful way of representing this data is to group the pitches into ranges of Hz.

<table>
<thead>
<tr>
<th>Hz range</th>
<th>16th century cornetts</th>
<th>17th century cornetts</th>
<th>totals</th>
</tr>
</thead>
<tbody>
<tr>
<td>410-419</td>
<td>0</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>420-429</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>430-439</td>
<td>3</td>
<td>2</td>
<td>5</td>
</tr>
<tr>
<td>440-449</td>
<td>3</td>
<td>3</td>
<td>6</td>
</tr>
<tr>
<td>450-459</td>
<td>6</td>
<td>2</td>
<td>8</td>
</tr>
<tr>
<td>460-469</td>
<td>16</td>
<td>5</td>
<td>21</td>
</tr>
<tr>
<td>470-479</td>
<td>15</td>
<td>4</td>
<td>19</td>
</tr>
<tr>
<td>480-489</td>
<td>8</td>
<td>2</td>
<td>10</td>
</tr>
<tr>
<td>490-499</td>
<td>3</td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td>500-509</td>
<td>1</td>
<td>0</td>
<td>1</td>
</tr>
</tbody>
</table>

Anyone with experience in statistics would say that the most likely hypothesis for explaining this data is that there is a normal distribution centred on around 470 Hz in each case.

Bruce's interpretation is quite different. Working with the totals, he lays aside the 5 cornetts with pitches over 484 Hz, picks the group with a semitone range between 458 and 484 Hz centred on 470 Hz as representing mezzo punto, and picks the range 435 to 452 Hz averaging at 443 Hz, a semitone lower than 470 Hz, as representing tutto punto. The statistical case for the hypothesis of a normal distribution over the whole range is very much stronger than the case for Bruce's hypothesis of two distributions a semitone apart.

Bruce would have a much stronger case with a hypothesis that the data excludes two distributions a tone apart since there are no examples at a tone below 470 Hz, around 420 Hz. My response is twofold. Firstly, the statistics of survival of different forms of an early instrument has a strong relation with the statistics of use during its last period of being played, and hardly any relation with the statistics during the period when made. Cornetts were still used in the 18th century, and we have no reason to expect cornett sizes that had no use then to survive.

Secondly, the graph does not cover the pitches of cornetts with blown pitches a tone higher than 470 Hz, at about 530 Hz, (assuming a nominal pitch in A). That is because Bruce chose only to include what he considered to be 'normal' instruments in A. It is easier to change pitch with lips alone on a cornett than on a sackbut, and if the early pitching of sackbuts with the lips is a semitone lower than modern pitching, seems to be the case, then it is likely that early pitching of cornetts was rather more than a semitone below modern pitching. From this, we may expect that the cornetts in the graph were originally made for blowing in the range between corista and Praetorius's Cammerthon, centred on tutto punto, while a distribution of smaller ones were originally made to play in the range between Praetorius's Cammerthon and a tone higher, centred on mezzo punto. The main use of cornetts was to play with voices, so the larger size would have predominated. The smaller size would have played soloistically in high-pitch wind bands. Soloistic cornett playing ended in the 17th century when it was replaced by the trumpet, so after then the high-pitch ones became redundant.

An important piece of evidence not mentioned by Bruce is that Doni wrote (1640), as reported by Mendel, that organ pitches of Naples, Rome, Florence, Lombardy and Venice form a series ascending by semitones. Thus when Bruce cites the 1571 contract for the cathedral organ at
Ravenna to be tuned in *tono corista*, a tone lower than normal, this is evidence that the normal pitch in that city was that of Lombardy, and not evidence that *tono corista* was a tone below *mezzo punto*. There was apparently no significant difference between the pitch of Roman organs and *tono corista*.

Bruce generously and usefully offers a considerable amount of evidence that he has collected that is relevant to the topic but does not particularly support any argument. He does not feel, as I do, that it is necessary to interpret all of the evidence as far as one can. Thus, when he reports a 1609 contract for an organ in Reggio Emilia as being at 10ft pitch, a tone lower than cornett pitch, I am compelled to try to make sense of it.

English 10ft C pitch was about 4 1/2 semitones below modern, and 10ft F pitch about 2 1/2 semitones above modern, but the relevant Italian length of the foot is bound to have been different. It is likely that there was an organ-maker's foot that was widespread in Italy which avoided the wide variation in local measures, and it would be worthwhile to try to estimate what it was. Mendel cites Tagliavini as concluding that Italian contracts were for a pitch of F and the octaves below, and before about 1600, they were called 5ft, 10ft and 20ft, and later they were called 6ft, 12ft and 24ft. Comm. 1385 shows that Italian feet are mostly not much different from English ones, but some can be quite big (e.g. the Mantuan foot was 1.57 times as long as the English foot). If we assume that the cornett pitch of the contract is *mezzo punto*, 10ft pitch would have been at *tutto punto*, about a major third lower than English 10ft F. Thus the Italian organ maker's foot would then be about 33% longer than the English foot, or about 400 mm. But the 12ft pipe (for organs after about 1600) should be about a minor third lower yet, which brings us below Naples pitch, an extremely unlikely possibility. The simplest solution to the problem would be if the pitches and pipe lengths stayed the same after about 1600, but the organ makers switched to another locality for their supply of measuring tools, a locality where the foot was 20% shorter, a more typical length. We need more evidence to find what the real solution is.

Bruce and I agree on the early pitches of organs in Venice and Rome, but his *tutto punto* and *tono corista* are a semitone higher than mine. My story of the subsequent history of the pitch standards mentioned here is, of course, different from his. The pitch of string ensembles in most of Italy remained at *tono corista* till about 1800. Roman organs probably stayed at that pitch as well till then. By about 1780, organs in other regions settled at the pitch of Lombardy, close to modern, about half-way between *tutto punto* and *mezzo punto*, which is an easy transposition of a tone from *tono corista*. The other string ensembles followed the organs and played at the same pitch as similar groups in Germany (at *Cammerton*) and in England (at concert pitch).

Early music performers, if they are historically committed at all, are committed to the historically possible, beyond which they choose what to them is the most enjoyable and practical. There is rarely any serious commitment to the historically probable. A commitment to the current early music culture that they are part of is greatest of all. In that culture it is politically correct to accept certain pitch standards as good approximations to the most interesting historical ones. There has been a tremendous financial investment in having instruments made to play at those pitch standards, and any general change now would be completely impractical.

Bruce is committed to providing the historical justification for those choices. He can do this to the satisfaction of himself and most other early musicians because he doesn’t subscribe to the dictum of scholarship that every piece of relevant evidence must be explained according one’s concluding historical hypothesis, and explained more reasonably than any alternative hypothesis. In this study of northern Italian pitch standards, as far as I am aware, he has not flagrantly ignored important evidence that makes his conclusion impossible (as he did with Praetorius’s *pfeifflein* diagram), but by making his hypothesis consistent with his previous work, he has interpreted the crucial evidence in a way that, though not impossible, is much less probable historically than the straightforward alternative.
All musical instruments have got their "good" and "bad" days. This well-known physical phenomenon and apparently inexplicable behavioral fluctuations related to response, tuning and sound quality indicate the complex relationship between the internal stresses of an oscillating system and its internal damping.

From time immemorial, instrument makers showing rich experience and intuition in selecting materials and applying exact processing techniques have made progress in their field - not least in minimizing internal damping. For, it goes without saying, that only a superior musical instrument is the basis of a first-rate acoustic pattern.

Every musician is aware of the fact that proper continuous playing will positively influence the sound features of his instrument - of course, this process is always extremely time-consuming and, unfortunately, only leads to limited successful results. If the instrument is not played at all or needs to be repaired, abruptly, all the achievements will vanish.

Thanks to our sound optimization method (original development) it is now possible to emulate the "mechanico-acoustical process" of complete loss of internal damping. The method is applicable regardless of the age of the instrument or the frequency of playing it - however, it is imperative that the instrument should be free of design defects. This is true for all instruments; string, wind and percussion instruments alike allow to be enhanced in terms of sound quality. The resulting improvements can be checked by ear: direct response, exactly controllable dynamics, superior balance, no intonation problems, unique brilliance - and the typical instrument timbre will be retained.

For further information please contact: institute of applied acoustics (ifa) c/o none records, Bahnhofstr. 151, 63 263 Neu-Isenburg, Germany.

FON: +49 6102 - 72 37 20
FAX: +49 6102 - 72 37 21
email: ifa_by_emil@t-online.de

As of summer 1996
The musician and engineer Emil Weiss owns a patent on sound optimization.

By Achim Ritz

"The bass range is now full and soft, the sound warm and striking", praises a musician his instrument. "The trebles sound rich and beaming up to the highest ranges", describes a jazz musician the changes of his guitar and expresses his gratefulness for the "small miracle". The name of the miracle worker is Emil Weiss, and he owns a patent on a sound optimization method.

NEU-ISENBURG The American musician Leo Kottke is said to always put newly purchased twelve string guitars in front of a loudspeaker box in order to activate them, or, to put it more aptly, to make them oscillate so that they can develop a good sound. Mr. Kottke was on the right way with that simple experiment of sound optimization. One who further developed that method is a guy called Emil Weiss.

The jazz musician raised in Bratislava/Slovakia calls himself a passionate "against-the-grain thinker" and is a personality with a well-balanced mind. And, this is the very condition the instruments should be in that leave Emil's small workshop located in Neu-Isenburg (close to Frankfurt/Main) after a treatment of three days.

There are a number of tuba, saxophone and piano players who consider Emil Weiss to be a master of sound. In spite of the mostly positive echo of his clients, the professional mechanical engineer and jazz player remains a down-to-earth person. "I can't turn a plywood violin into a Stradivari", says he. But, what actually happens to the instruments?

According to Emil, it is not easy to understand the contents of his patent. Inventors likely to enjoy talking about their secrets or recipes are hard to come by, aren't they? The only information Emil is willing to reveal is as follows: Using mechanical devices attached to the musical instrument being treated, the instrument is made to oscillate. With this method the normal process of "removing the mechanico-acoustical dampening" of an instrument will be emulated in a technologically precise manner and within a short period of time; at the same time, a detailed measurement protocol of the process and its results will be prepared. The age of the instrument is of no consequence to the outcome of the treatment.

Emil Weiss gets the optimum performance of any transverse flute, harp, clarinet, or trumpet without changing the appearance or damaging the instrument in any way. Only the proper playing of, let's say, a guitar or a horn allows the instrument to "fully evolve its features". This adverse phenomenon is known to every musician and instrument maker. And precisely this phenomenon, which Emil Weiss also calls "collecting of experience by the instrument", is shortened by the man from Bratislava who founded an "Institute of Applied Acoustics".

Just as Leo Kottke, the guitarist, Emil Weiss, too, makes the cello or bass to oscillate in order to improve its sound performance. Exclusively by acoustic means without resorting to any chemical or thermic methods, improvements can be achieved that are open to objective tests. On the one hand, the typical character of the instrument will be retained, but on the other hand, the response will be easier, the brilliance improved and the sound more attractive. In short, after the treatment in Emil's laboratory, the instrument will be a more harmonious entity.

That's what any of Emil's client will confirm. All of them praise, among other things, the enlarged note decaying time and the optimum balance between the strings. "I even achieved the subcontra Bb2 that is not listed in my table of finger positions for bass tuba." One musician even sang "A Canticle to a De-iced Tuba".

For example, all the various materials a guitar is made of will never oscillate in the same way and thus hamper one another when the a note is
being generated According to Emil, this is true even in the case of a well-tuned guitar because of the high "exciting energy demand". Emil's sound optimization method could be compared to a group-dynamic process within a team that is supposed to work together smoothly.

Fir wood, palisander or plastic materials of an instrument are capable of generating a common good sound only if they are on the same wavelength, so to speak, i.e. if they show "team spirit". In this connection, a memory effect will occur. As soon as a "harmonious togetherness of the materials" is achieved, this state will last forever, and the instrument will always be balanced for the rest of its service life.

What you can hear with your own ears is often much more convincing than thousand words. And, therefore, Emil Weiss has been working together with "none - records, design, multimedia", a company based in Neu-Isenburg. In the sound studio of the company a CD is to be recorded that shows the sound of an instrument before and after undergoing sound optimization. In addition, the creation of a sound archive containing the improved sounds is intended.

Finally, music schools could benefit considerably by resorting to Emil's sound optimization method. For, beginners would have much more fun in playing optimized instruments than normal ones and thus the drop-out rate would probably decrease.

For further information please contact "none - records", fon:+49 6102 723720, fax:+49 6102 723721, or contact us via email ifa_by_emil@t-online.de

Leicester Early Music Festival 1997

Against all odds, the Leicester Early Music Festival has survived 7 years, and the 8th will take place at the end of May (May 18th - June 7th), with the main events over the Bank Holiday weekend (May 22nd-June 7th). From the initial concept of offering professional concerts to a wide audience in the Midlands, the event has expanded to include, in addition, workshops, competitions, Festival Dinner, musical ghost walk and much more. The basic philosophy is to generate new audiences as well as support established ones and to offer performing opportunities to new as well as proven groups. Next years performers will include Michala Petri, Florilegium, The Longslade Consort, Courtlye Musick as well as a few less well-known groups such as Kingfisher Chorale and Rostibolli. A performance competition established last year has expanded to include a Harp class, with substantial prizes. It is clear that the emphasis on making Early Music accessible to everyone increases potential audiences and fulfills a considerable need. It is, of course, very much in the interests of players and makers alike to promote professional concerts of Early Music and we hope FoMRHI members will be able to join us for some of the events.

For the 1996 Festival, 10,000 leaflets were distributed throughout the East Midlands, and nationally through National Early Music Association contacts. In addition, a series of advertisements were run both nationally, and locally. We had about 3,000 visitors to the Festival over the two weeks. There was press comment and local radio provided a 15 minute slot. Further, it attracted an audience far in excess of what might be expected from what is often considered to be of minority appeal only. Leicester Early Music Festival 1997 will be a high-profile event with wide exposure particularly to young professional families.

The Festival has now expanded to use several venues in Castle Heritage Park, a prime tourist attraction at the centre of historic Leicester Centre. The main site is the beautiful Church of St. Mary de Castro, which is available for displays of music, instruments and anything else associated with Early Music. We already know of two music shops, a bow maker and Morley (Harps) who will visit during the event.

Are there any FoMRHI members who would like to display material (free) or visit us? This is very much in all of our interests. I know that we are all busy (too busy), but without new audiences we have no sales and we ignore this simple equation at our peril. The cost is ZERO, except the time. If you can't spare the time, send us some literature which we will display.

Any members of FoMRHI who would like to take advantage of this offer should contact John Bence as soon as possible. Write to him at 126 Shanklin Drive, Leicester. LE2 3QB (0116-2707820)

FoMRHI Comm 1+9 | John Bence
Violin research 15 years ago

Peter Armitage has written the following letter:

Dear . . . ,

I was not aware of that ice research, but several groups were working then on violin research which used engineering methods recently developed to find the modes of vibratory motion of a complicated structure. The method, called 'finite element computer analysis' was developed for buildings and bridges. The structure was conceptually broken up into blocks (called elements) of simple geometrically-defined boundaries, each of which had known physical properties. The constraint equations (ensuring that the common boundaries of adjacent blocks moved together) were solved simultaneously with the block vibration equations to give the modes of vibration of the whole body. This is an approximation to the real situation, and to see how good it was, one could get the whole vibration spectrum of a violin by analysing the noise produced by knocking it with a hammer. If the knock happened at a node of a particular vibration, it wouldn't show up, so a few knocks at different places were necessary. The computer could show the distorted shape of the structure at maximum amplitude for each mode (with lines at boundaries between elements). The amplitude was generally exaggerated to show the shape distortion more clearly.

This research worked very well, so we now know what the low-frequency modes are. There were some surprises, such as a twisting mode, and one of the neck bending up and down at a frequency close to the air resonance of the body cavity. Makers later found that if they tuned the neck mode exactly to the air resonance, the players found that the instrument is easier and more satisfying to play, so most makers who are into using measurement of resonances to guide their work are now doing this tuning as well. It is called 'AO/BO mode matching'.

Forgive me if what I am adding below is something you are fully aware of, perhaps in your capacity as a physicist, but in case it isn't I will describe something that was demonstrated to me at Berkeley about fifteen years ago when I was visiting a vihologists' friend employed on the academic side of computing. He introduced me to a consultant engineer friend of his who had adapted a technique for demonstrating the tendency of vibrations of sea-ice sheets to develop destructive resonances when subjected to explosive drilling by oil prospectors. That technique involved collecting the responses of sensors at the intersections of regular, perpendicular grid lines crisscrossing the terrain, and feeding them into a computerized analysis program, which then displayed in skeletal 3D the ice's natural resonance patterns and amplitudes. As modified for violins, there was only one sensor, and the stimulus was applied successively at the intersection points on the belly, and probably back, by a small, padded, high-tech human delivering a measured blow. To me, the most surprising part of the display was the magnitude of distortion in rib shape, i.e. the extent of the change in a parallelogram or any rectangle joining the back and belly in the rib plane.

The purpose of this application was to compare the vibrational response of conventional violins with that of those made in materials such as fiberglass, aiming at a cheap, high-quality commercial instrument - an objective which had not by then been reached and probably still hasn't. However, if the engineer felt that it might ultimately be obtained if they could formulate a method to stimulate the differential flexibility of wood along and across the grain, if they succeeded, there would no longer be any need for handcrafted timber violins, and don't suppose they have yet succeeded. In any case, if we are to reproduce violin sounds of top quality by fully up-to-date technology in every sense, we are more likely to get there faster by the electronic route.
The finite element analysts helped makers in other ways by applying their methods to the tuning of plates and bridges, showing how taking wood off in different places affected the resonance characteristics. This work was not as influential as that of whole-instrument resonances because makers could do that research themselves with chisels on wood.

Another direction of research pursued then was to address the problem of the worsening shortage of good wood for soundboards by using modern materials that could mimic wood. There were several groups working on this. The best that they could do was a sandwich with a foam interior with skins of carbon fibre on the outside. As far as I know, all of this work was finally abandoned. Reasons were that the sound was not as good as hoped, even with mass production the cost would be high, and the musicians were resistant to non-traditional materials.

As for the final comment about reproducing violin sounds of top quality, electronics is the only way. It won't be long now before recording companies will be able to feed a score into a computer, specify the soloist, orchestra, conductor and concert hall, and artificially generate a recording that experts could not fault. The only reason for not doing it would be that it wouldn't sell. The recording industry is in deep trouble because it is the end of the road for the higher-and-higher-standards bandwagon that they and the critics sold to the public and the musicians, which induced record buyers to abandon old versions and buy new. Playing precision and fidelity of reproduction cannot be noticeably improved, so the punters are not buying new.

But reproducing violin sounds of top quality is really not the issue. On the most basic level, people will always want to play with noise-making toys, and a toy that offers more pleasure for oneself and admiration from others the more skill one gets in playing with it is bound to be a winner for many. Live music played on acoustic instruments will outlast electronic fashions.

Lee's mathematics in Comm. 1480 is not the way to do it, and there are some errors. His equation (3) wrongly assumes that the elastic modulus $E$ equals the total stress divided by the total strain. He later defines it properly as the slope of the stress strain curve. Stress $S = T/A$, and differentiating it and dividing by itself gives $dS/S = dT/T - dA/A$. It is usual to assume that $dA/A$ is vanishingly small and can be neglected. Thus the change of stress $dS = S(dT/T)$. Since the change of strain is $dL/L$, we can conclude that the slope $E = S(dT/T)/(dL/L)$.

The objective is to relate changes in length to changes in frequency. For this, one uses the Mersenne-Taylor formula, which is Lee's equation (1). It can be written as $S = 4f^2L^2p/g$, and differentiating it and dividing by itself gives $2(df/f + dL/L) = dS/S = dT/T$. Using the first of these to substitute for $S$ in the $E$ equation, the second to substitute for $dT/T$, and rearranging, gives $d(f/f)/(dL/L) = 1/(2K^2L^2) - 1$, where $K = 4p/gE$, as defined by Lee. If it were not for the -1 in this equation, it is consistent with Lee's final equation. The difference is irrelevant with respect to the final conclusion.

We are concerned with the region of the harpsichord that has harmonic scaling, so $fL$ is constant. Call that constant $B$. So $f^2L^2 = B^2$. Thus in the final equation above, the term $1/(2K^2L^2) - 1$ is constant. Call it $C$. So the relative change in frequency $df/f$, which is the change of pitch, is proportional to the relative change of length $dL/L$, over the whole region. That is, $df/f = C dL/L$.

Lee is concerned with absolute frequency so he can count beats, and absolute changes in length. So he wants $df/dL = C f/L$. Since $f = B/L$, then $df/dL = BC/L^2$, and so is proportional to the inverse square of length, not the inverse cube of length, as Lee concluded.
Mersenne’s iron and Ruckers 6-foot pitch

In Comm. 593 Richard Shann suggested that the ‘Chor-ton’ or ‘Chorista’ mentioned in the Duarte-Huyghens correspondence as the lowest Flemish standard in general use, and which 6-ft stringed keyboards were tuned in, corresponded with Praetorius’s preferred Chorthon. This was a tone lower than Praetorius’s Cammerthon, in which the 5-footers were tuned. This seems very likely to me. But when Mersenne’s value for the breaking stress for iron was used to calculate the breaking pitches on the highest octave of the 1581 Ruckers double virginal in N.Y., these pitches are a semitone lower than the Chorista pitch level, over 3 semitones lower than modern. One needs a semitone safety factor, so the calculated breaking pitch is a tone lower than the pitch assumed above, which is the lowest pitch that anyone is willing to defend.

I suggest that the problem is with the iron tensile strength measurement of Mersenne. Tensile strength varies with how hard the wire is drawn. The harder it is drawn, the stronger it gets. Hardness depends on how much the diameter has been reduced since the wire was last annealed. When wire is too hard, it breaks when forced to bend around a pin, and good harpsichord wire is as hard as one dares make it with this not happening. This involves planning the last anneal for the right diameter relative to the final intended diameter. A thicker wire has to distort more than a thin wire when going around the same pin radius, so it can’t be drawn as hard as the thin wire. Consequently thinner harpsichord wire is drawn harder and has greater tensile strength than thicker wire of the same material. Cary Karp discussed this in early Comms.

Mersenne’s measurements were all on wires 1/6 line or 0.380 mm diameter, which he probably drew himself. He was comparing different metals (pure gold, mixed gold, silver, iron, red copper and yellow copper), and a proper comparison would have optimised the hardness as well as keeping the diameter constant. He only mentioned the latter, so either he thought that, for the purposes of his comparison, hardness optimisation wouldn’t change the relationships enough to warrant the trouble, or he was unaware of the relationship between hardness and strength. So Mersenne’s tensile strengths are probably lower than they could be if he optimised hardness. Top-octave virginals iron strings would probably have been about 1/4 mm in diameter, which being thinner, would have been drawn harder than Mersenne’s 0.38 mm string, even if Mersenne’s string was drawn to optimal hardness.

The marking for the top of iron’s pitch range on the NRI string calculator is based on Mersenne’s measurement. For strings as thin as on a harpsichord or virginals top octave, this marking should be raised a tone.
Harpsichords and 'Buntpapier'

When I was browsing through the pages of Charles Mould’s new edition of 'BOALCH III' recently I happened to come across the name of Georg Christoph Stoy. The two-manual harpsichord described under this entry in Part II is said to bear the signature GEORG. CHRISTOP. STOY. C.P.S.C.M. AUGSP BEV. As the author points out it is not clear whether this refers to the maker, an owner or the decorator of this instrument. Without having seen the harpsichord (or a photograph) it appears to me that neither of this is in fact the case. The inscription can be identified as 'Georg Christoph Stoy / cum Privilegio Sacrae Caesaris Majestatis Augspurg BEV.'. BEV. might be a misreading of BAV., an abbreviation for Bavaria. Stoy was a fairly well known and successful manufacturer of various kinds of marbled and gilt papers commonly known in his days as 'Buntpapierer' in the city of Augsburg. He lived from 1670 – 1750. He was the holder of an imperial privilege for the production of Buntpapier issued in 1699 by Emperor Leopold I. (1640 – 1705) to the Augsburg oil-painter Mathias Fröhlich. In 1703 Stoy married Fröhlich’s widow and inherited the said privilege in 1709.

Thus the harpsichord’s inscription obviously refers to the paper which was used to decorate it rather than to a maker or owner. The maker of that instrument must therefore be considered as anonymous. A closer examination of these papers may reveal more information on the instrument’s history.

These decorative papers which can be found first in Japan (794-1184), Persia and other oriental countries (ca.1570) and Germany (Ravensburg ca. 1430) have been used as writing paper and for the decoration of various boxes, trunks, cases and other containers, in the bookbinding trade (e.g. as endpapers and covers) and in musical instruments such as clavichords, harpsichords and even early pianos.

There are basically three kinds of such papers:

- Marbled paper, also known as Turkish paper. The paint is applied to the paper by dipping it into a liquid where the paint swells and can be formed into various patterns with sticks, combs and other similar tools.
- Glue papers. Coloured glue is brushed on to the paper and the pattern is made by simple tools like combs, sticks, sponges and wooden or rubber stamps or rollers which are drawn over the paper to produce a kind of repeated ornament.
- Printed papers. Moistened paper or leather is printed from a wooden block or an engraved metal plate (copper or brass) similar to copperplate printing in a sturdy press. Patterns can be multi-coloured and are combined with silver and gold leaf (in fact the latter was some copper alloy with tin, zinc or lead as analysis has shown in this century). Varnish mixed with gold and bronze powders has also been used.

The most popular of the printed types is gilt paper or 'Brokatpapier' which is to be found in Augsburg for the first time around 1690. Leaf metal was printed to the prepared paper by means of heated stamps or engraved metal printing plates under high pressure. Commonly the paper was single-coloured. If there was only the structure of the printing effect to be achieved they printed without colour on metal-plated paper. One speciality of the Brokatpapier was the so-called 'Patronieren'. Various spots of different paints were applied to the paper by means of a template ('Patrone') before printing with leaf metal. The outline of the spots did only rarely coincide with the plate’s pattern. Sometimes they used colour-stripes, a repeating rhombus or some other geometrical form. Only very few papers survive which have been patroniert carefully and almost with the precision of hand-coloured copperplates where the whole process is different and much easier since the colouring follows the printing and not vice versa. Another variant was 'drap d’or' or 'drap d’ argent'. Here the paper was hand painted after the 'Patronieren'.

Around 1700 Augsburg, which was already very famous for its calico-printers, copper-printers and 'Patronierer', became a center for the production and distribution of Brokatpapier. The connection with these crafts was of high importance in the development of the Buntpapier production. These papers were normally traded within fairly large regions. Papers from Augsburg were offered for instance at the famous Leipzig fair at the
beginning of the 18th century. The different patterns of the papers were kept unchanged over a long period which makes the dating more difficult. Moreover, there have been found examples of papers that bear different signatures, sometimes more than two. Often new owners of a paper making business had the signatures of the printing plates changed. Probably due to ease of application such signatures appear hardly ever on other than printed papers. It would appear that the harpsichord's paper is such a printed type. Since Stoy did not get that imperial privilege before 1709 he would not have used that signature before. Therefore the decoration or redecoration of the harpsichord must have been carried out after that year. Comparison with Stoy's catalogue ('Muster Charten') which he published several times during his life may present more details on the date of the harpsichord. Is it possible that this instrument started life as a South German or Alsatian harpsichord of the 18th century which was later rebuilt in France? This is of course pure speculation. Nevertheless this case might be a good example of how the decoration can be of great help in the dating of an unsigned and undated instrument. Further investigators are referred to the collection Olga Hirsch in the British Museum London or the Deutsches Tapetenmuseum in Kassel which are amongst the richest and most extensive collections that represent all types and periods of 'Buntpapiere'. Stoy would certainly be well represented there.

The Bachhaus Eisenach has an anonymous undated single manual harpsichord (no. I 77*) which also carries a signed paper on the rim above the soundboard. The inscription reads 'Cum priv. S. C. (? )' and 'Ausp. del C. Stoy). On account of this it has been possible to date the harpsichord at ca. 1715. It is believed to be of South Thuringia. A very similar paper, possibly also of Augsburg origin, has been used in a clavichord by J. C. Speisegger, Schaffhausen 1725 in the Museum of Musical Instruments in the Grassimuseum Leipzig, Nr. 3072 (see Herbert Hayde, Historische Musikinstrumente im Bachhaus Eisenach, 1976).
In the Bulletin Supplement for October 1996, Eph Segerman presents an addendum to our great little English cittern controversy. Unfortunately he appears to have fallen into a well-known trap - not consulting the primary source.

Fludd's knowledge of instruments would seem to have been theoretical rather than practical and he was also hampered by having his printer and engraver abroad. There are some obvious mistakes - Italian tablature inverted, music examples garbled - but also some useful information - (incorrectly) colour-coded frets on the cittern - and possibilities worth investigating - single or double courses on the orpharion, and what seem to be extra frets in the enharmonically problematical positions on the bandora, etc. He wrote in Latin and this introduced the difficulty that materials, etc., not known to the Romans, had to be rendered by their nearest equivalent, so that copper or bronze can mean copper, bronze or brass, iron can mean iron or steel.

In 1973 Joscelyn Godwin published articles on Fludd in the journals of both the Galpin Society and the Lute Society. Fludd, in his section on the orpharion and pandora, describes the strings "aerea" - bronze, presumably brass. Godwin seems to have mistakenly transferred this description, "brass", to the cittern, in his G.S.J. article. Fludd does not say this. He uses the word "metallicis" - metal. Cassell's Latin Dictionary regards the word as rare, although used by Pliny -"It is more usual to specify a particular metal, eg. ferrum, aes."

Fludd does however describe the strings more fully in his introduction "De Barbito" - "On the Lute", (translated in Godwin's L.S.J. article) " Orpharion & Pandora, quorum soni procedunta chordis cupreis & ferreis. Cistrona, quae 4 tantum chordas duplicatas habet, easque cupreas & ferreas," - literally, copper and iron, for which we should read brass and iron. His bandora, orpharion and cittern are all strung with a mixture of brass and iron strings, which is usual at this period.

Eph seems to have used Godwin's mistranslation to arrive at the (non-existent) all-brass stringing on which he has unfortunately built his hypothesis.
Peter opens this Comm. calling my Comm. 1468 (that it was a reply to) ‘discursive and
dogmatic’. I will accept that criticism. It applies to Peter’s contributions to this debate equally
as well. I cannot compromise on my insistence that a statement of what was, that cannot
reasonably and objectively explain all of the relevant evidence, can be no more than a working
hypothesis. Only when it can explain all adequately can it have the status of theory, to compete
with other theories for being closest to truth. Peter cannot compromise on his insistence on
having the freedom to come to a strong conclusion about what was without having to take
account of evidence that he feels must be wrong (such as the high octave for the Praetorius
zitterlein), even though he cannot make a reasonable case explaining how it went wrong.

The great value to me of a debate like this is that it forces me to think carefully, and shows up
inconsistencies in my approach. I now realise and admit that I have been biased by
expectations of Meuller’s wire having a breaking pitch fitting in with Robinson and English
pitches, and so have not taken what Praetorius wrote as seriously as it deserved. Praetorius’s
stated (Crookes translation with my bracketed insertions) in Chapter 4 just before the tuning
tables that: ‘All through this work [except where otherwise indicated], instruments and voices
are classified according to chamber-pitch, and not according to [the preferred] choir-pitch.’
When I first read this many decades ago, I was confused by not realising that the bracketed
insertions were implied, and couldn’t imagine how the statement could possibly be consistently
true. So I have felt free to ignore it whenever it seemed appropriate. The suspicion that it
could not be fully trusted has persisted even though I can’t remember ever finding an instance
where it has been demonstrably violated. This was mistaken. My interpretation of the pitch of
the zitterlein violated it, and the reason why I couldn’t find the zitterlein in the tuning tables last
time I looked (when writing Comm. 1468) probably was that my subconscious mind tried to
avoid my having to face the inconsistency.

The tuning table shows the highest zitterlein string was g”, so it must have been g” in
Cammerthon since there is no contrary evidence. This makes the breaking pitch of Meuler’s
wire three semitones higher than that of gut. The tuning table shows that the highest orpharion
string was g’ or a’. Since the a’ is in Cammerthon, this also indicates that the breaking pitch of
Meuler’s wire was three semitones higher than that of gut. Learning to trust our sources of
historical information is a very slow difficult process, but it is the only way forward towards
truth in historical research.

This conclusion is inconsistent with Praetorius’s statement that English pitch for wind
instruments [and other instruments used in light music, such as the orpharion and zitterlein]
was about the same as Cammerthon, combined with Robinson’s statement in his cittern tutor to
‘set up your Trebles as hie as you dare venter’. The difference is three semitones. When
evidence from two sources conflict, respect for the sources implies that one must search for a
reasonable explanation for how they both can still be true, and then only consider that one
source is mistaken when that search fails. This is a far cry from Peter’s approach, which
happily assumes that a source is mistaken when the only evidence (which is not real evidence)
that he has against it is that it disagrees with his intuitive judgment.

My suggestion for resolving the conflict between the two sources is that the strength of
Meuler’s wire was not consistent, and at least some batches distributed in England were
inferior to those available in Brunswick. If production of the wire were easy, other wire
makers would have discovered how to do it as well. When competing wire makers make the
same product, a batch of inferior quality would not be sold because it would endanger
reputation and future share of the market. Meuler, having a monopoly, would sell everything
he made. All of it was stronger than normal iron. We would then expect that there would be
wider variation in the strength of Meuler’s wire on the market than of other types of wire.

Angled frets were only a necessity for the bandora to get the 7-course open-string range of 2
octaves and a tone with ordinary iron top strings. They were never necessary for the orpharion, but they were a fashion which made the lowest strings sound better. Donald Gill's famous quote calls angled frets a fashion. The Palmer orpharion could well have originally had an open-string pitch range of 3 octaves (covering the bandora range as well) with an archlute extension neck, and was probably 'restored' to Praetorius specifications in recent centuries.

I agree with Peter that the names Chor Laute and Chor Zitter are Praetorius's indications that these instruments normally played in his preferred Chorthon, but consider Peter's assumption that other depicted instruments, like the orpharion or zitterlein, without the 'Chor' label, were intended for the same pitch but Praetorius neglected to say so, is quite outrageous. Sure, Praetorius made some mistakes, but they are very rare, and unless we have evidence showing that there is a mistake because of an inconsistency, historical scholarship is much better served if we accept what he wrote was true. Peter's is the discipline of an artist, not that of a scholar.

Praetorius was writing for a readership that never saw many of the instruments he discussed, and it was a good idea to reiterate the pitch standard for the tuning of the orpharion because many of his readers might find it hard to believe that a lute-tuned instrument of that size could be tuned so much higher than gut.

I agree with Peter that in the period when tripled courses were used (Mersenne is the latest source I am aware of), a tripled second course was rare (if it happened at all). With only a tone difference with the first course in tuning, a type of metal adequate for one course should usually be adequate for the other as well. The only evidence I know of for a difference of metal for the two courses is Playford's introduction to the cittern part of his book for cittern and guittern, with only the tuning (clearly not reentrant) in the introduction to the guittern part. I will not be tempted to play Peter's game and suggest that Playford was confusing the two instruments. The second course does have more zing in the tone if it is brass. That is why Peter uses it. Iron seconds have adequate tone quality, fret in tune, stay in tune and last much longer. So the lack of evidence for triple stringing on the second course is indicative of the rarity of Peter's stringing practice, not of my suggestion being a 'non-starter'.

There are so many similarities between the above introduction to the cittern part of Playford's book and the introduction to Robinson's cittern book that there can be little doubt that Robinson's book was the model. Similarly, Robinson's cittern introduction was an abridgement and adaptation of his lute introduction. If a tutorial point is well expressed in one tutor, it would be foolish to express it differently if it applied in an adaptation for a different but similar instrument. Such similarities in adaptations are only a problem if it is done incompetently. Such incompetence did occur in a Vreedman gittern publication, where bits of the original cittern model were not properly changed to apply to the gittern. I have found no evidence of such incompetence in Robinson's adaptation of his lute tutor for cittern, so Peter's assumption of an error ('set up your trebles as hie as you dare' was carried over to the cittern where it didn't apply) is without any support whatsoever.

Peter needs that statement to be in error to allow the highest strings to be of Meuler's type and used as the high octaves (with a twisted brass low octave) of a third-course triplet. He claims support for this course's stringing from Robinson's tuning instructions, where the strings of the 1st, 2nd and 4th courses are described as being 'of one sound', while those of the third course are 'in one tune'. Peter claims that 'one sound' probably means unison and 'one tune' octaves. That is highly unlikely is shown by what Robinson wrote in the equivalent place in the lute tutor: 'Now you shall learne to tune your Lute, and for a generall rule, first set up the Treble[s], so high as you dare venter for breaking, setting them both in one tune or sound called an unison:'. If Peter responds by claiming that 'sound' can only mean 'unison', and 'tune' could mean either 'unison' or 'octaves', he is saying that Robinson use of 'tune' in the cittern book was being deliberately ambiguous. I can't imagine a reason for Robinson being deliberately ambiguous about this if octaves were the usual practice, as Peter claims.

I think I remember seeing a paper, probably by Luis Gripp, collecting many cittern stringings from depictions as well as musical sources and, if I remember correctly, there was a variety of
arrangements of doubled and triple courses. Until I locate that paper I will withdraw my claim that there are other examples of a tripled first course. Apologies for mixing up Toppel Cythar with Renovata Cythara which had normal early French stringing with tripled 3rd and 4th courses. The Mersenne quote Peter gives mentions 'always at the unison' with the possibility of one, not two, octave strings.

I am a staunch supporter of Peter's hypotheses that the comb on a cittern served as a guide and support for a rod which was the actual tail-fixing, and that a unison pair of strings often was one wire that went from one peg, around the tail fixing and back to the other peg. He does not dispute my claim that if a triplet course included a single-wire pair plus an individually fixed string, it is more probable that one can continue playing if there was a string breakage than if the course was only a single-wire pair. I do not dispute his claim that replacing a single-wire pair is quicker than replacing a single string, if the latter involves the time of making a loop. If one made loops on the strings beforehand, the single string would be faster. A pre-looped string might be mounted on a comb-rod tail fixing by threading the free end around the rod and then putting it through the loop (as is done on viols), but this may be inadvisable since the sharp curvature of the loop around the string can lead to weakening at that point.

A properly made loop should not tighten around an end-pin or rod. The free end and the rest of the string should be twisted around a hook or other form about twice the diameter of the end fixing. To do it on a rod in place in a comb, make the loop around the rod plus another rod under it, and when finished, pull the other rod out. Initial twisting should have the free end and the rest of the string approach the twisting point at equal angles (around 45 degrees) from the final string direction (making it look like a twisted string) for some length, and then one twists the free end around the rest of the string (like an overspun string) for a few turns, leaving the free end sticking out for at least 3 mm perpendicular to the string.

Since the bridge on a cittern is on or near a cross-bar under the belly, the belly would only collapse if so did the bar. If the bar withstood the pressure of the bridge from the string tension for 16 years, it would not suddenly collapse from that pressure without the added pressure of something outside leaning on it. If the customer claims resolutely that there was no such added pressure, I can't imagine that Peter would be so naive as to believe him.

The surviving Virchi instrument I know of is a very fancy one made for a nobleman who probably want an instrument of normal size, big enough to sound well when played with a few simple chords to accompany a song. The Virchi cittern book was written for serious players with a lot of technique, and some chord stretches strongly imply a rather smaller size. I am sure that quite a few Virchi citterns were made of appropriate size for the music in that book, amongst which could have been some that came to England and made small citterns respectable there. None of these have survived, which is not surprising since no English citterns have survived either. It is only Jim Tyler's speculation, ignoring hand stretch, that claims the book music was for playing on the surviving instrument.

The book states that it uses a new tuning instead of the usual hexachord tuning. All authors from Lanfranco to Praetorius and Mersenne imply that the hexachord tuning was a (if not the) standard 6-course Italian tuning. The surviving written music for that tuning is just one small source with a very unusual semi-diatonic fretting pattern. It was this tuning that I referred to that had the open-string range of a sixth.

The English small cittern, for which the surviving English repertoire was apparently written, played at only two pitch standards, the light-music standard where it played with violins and recorders and the serious-music standard (later called Consort pitch) where it played with viols and flutes. Robinson's tuning instructions were for solo playing and they were presumably for the higher standard. This would surely not have stopped a cittern player playing in the lower standard in an 'ideal' Consort from breaking out into a solo piece.

I would be more impressed by Leycester's knowledge about the c.1600 cittern if his inventory included an old cittern.
Donald Gill claims that since Praetorius’s English zitterlein and Wensler G30’s cithrinchen ‘had such closely similar string sizes that tuning in the same octave was a reasonable assumption’. Assuming that the premise is true (see below for the evidence), the assumption would be very reasonable if there was no other evidence that contradicted it. But there is such evidence, which is the tunings (with the octaves specified) reported in Praetorius and Wensler G30. Then Donald’s assumption is not reasonable any more unless he can show how the Praetorius octave is reasonably likely to be in error. If there was such an error, it couldn’t be one of carelessness since the high octave is specified both in the text and in the tuning tables. I don’t need to argue for the high octave because Praetorius specified it. It is up to Donald (or Peter F.) to argue how Praetorius got it wrong (not how much nicer things are if he were wrong). Donald wants cittern history to remain as simple as it was when he first discovered and enjoyed it, and Praetorius’s octave is not seen on any other cittern. Just as unique were Meuller’s strings and the strength demands of the orpharion’s first course.

What Donald has informed us about the Wensler G30 cithrinchen ms is: the tuning for 5 courses, the material, gauges and construction (either plain or twisted) for 6 courses, and that the music is for 5 courses. The 6th course specification cannot be consigned to the ‘mystery’ bin and be ignored. A 6-course cithrinchen must have been played at the time, and possibly was the direct ancestor to the English guitar. In Comm. 1468, from the relative heaviness of the twisted strings (see below), I suggested that they should be single. That strengthens the relationship with the later English guitar, which apparently had single overspun basses. The scribe would have had 9 strings, probably on 10 pegs. This was never a problem with the Spanish guitar. The names ‘octaven’ and ‘conter octave’ now appear to signify tuning at the low octave, like ‘bourdon’ on the Spanish guitar. The scribe could easily have had only 5-course repertoire available to copy, with the 6th course used in improvised music such as chordal vocal accompaniments. I don’t see where there is any problem in making a reasonable interpretation of this evidence. I wonder what kinds of other ideas Donald is hoping people would come up with.

The objective information that can be calculated from the Wensler and Praetorius stringing evidence (using relative and ignoring absolute pitch indications) follows:

<table>
<thead>
<tr>
<th>course name</th>
<th>nominal pitch</th>
<th>gauge equivalent</th>
<th>number of semitone steps relative to highest string in tuning pitch</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wensler G30 cithrinchen</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 quinten</td>
<td>a'</td>
<td>9</td>
<td>steel</td>
</tr>
<tr>
<td>2 quarten</td>
<td>e'</td>
<td>7</td>
<td>brass</td>
</tr>
<tr>
<td>3 tertien</td>
<td>e'</td>
<td>5</td>
<td>brass</td>
</tr>
<tr>
<td>4 secundem</td>
<td>a</td>
<td>4</td>
<td>twisted brass</td>
</tr>
<tr>
<td>5 octaven</td>
<td>f</td>
<td>2</td>
<td>twisted brass</td>
</tr>
<tr>
<td>6 conter octave</td>
<td>-</td>
<td>1</td>
<td>twisted brass</td>
</tr>
<tr>
<td>Praetorius zitterlein</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 quint</td>
<td>g''</td>
<td>11</td>
<td>steel</td>
</tr>
<tr>
<td>2 quart</td>
<td>d''</td>
<td>8</td>
<td>steel (or brass)</td>
</tr>
<tr>
<td>3 tertz</td>
<td>a'(or bb')</td>
<td>5</td>
<td>brass</td>
</tr>
<tr>
<td>4 secund</td>
<td>f''</td>
<td>10</td>
<td>steel (or brass)</td>
</tr>
</tbody>
</table>
The gauge diameters are taken from Bakeman (GSJ, 1974), a mathematical approximation to measurements. In this gauge system, the diameters double for a decrease of 7 in gauge number. Then adjacent gauge numbers differ by 1.71 semitone steps in diameter.

The brass equivalent diameter is the diameter of a brass wire that has the same weight per unit length as the string in question. For steel strings, the brass equivalent diameter is the square root of the ratio of densities times the steel diameter. From our measurements on twisted brass strings, the low-twist equivalent diameter is 1.4 times the diameter of each wire it is made from. For medium and high-twist strings, the equivalent diameter is that of low twist strings plus a correction which is 0.03 and 0.08 mm respectively. In the calculations it is assumed that the twist on the cithrinchen 4th was low, medium on the 5th, and high on the 6th. If we call the diameter in this column the ED for the string, then the entry for that string in the next column = log(ED for that string / ED for the highest string) x 12/log 2, and represents the number of semitones difference in pitch between that string and the highest one if the tensions were the same. The next (last) column shows that difference in pitch between the same strings given by the tuning. A comparison between these last two columns gives an indication of comparative tensions.

Praetorius indicated that the strings were of steel and brass, so the highest would be steel, the lowest brass, and from this information, we can’t be sure of the other two, so both are included in the table.

From the semitone steps in diameter difference between the various twisted strings and the highest cithrinchen strings, we can estimate the nominal tuning of the 6th course. The 4.2 diameter steps between the 4th and 5th course, compared to the 4 steps difference in tuning, suggests that the twisted strings were in equal tension. Then the 2.8 steps between the 5th and 6th course suggests that the 6th was likely to be three semitones lower than the 5th. Its nominal pitch would thus be d. This makes a lot of sense since it continues the alternation between major and minor thirds characteristic of this tuning (with the exception of the fourth on top). The English guitar tuning also has alternating major and minor thirds plus a fourth, covering the same range of a twelfth, but the fourth is in the middle, rather than on top. Any history of the English guitar would have to include the Wensler G30 ms 6-string tuning as an important precursor.

The above apparent accuracy with which the pitch of the 6th course was determined is spurious. There were 1.71 semitone steps between each gauge number and the next, and no evidence that half-gauges were available. If there was an ideal diameter that was just right and the gauge used was less than 0.86 semitones different, that gauge was the closest one available. So the figures in the next to last column of the table should be viewed as having a little less than a semitone of expected error either way. Except for the jump in tension between the untwisted and twisted strings, the conclusions about the tension profile of this cithrinchen stringing that I offered in Comm. 1468 must be withdrawn in the light of the more accurate calculation here (they were arrived at using the string calculator, which is not accurate enough for these purposes).

A very possible reason why Praetorius’s zitterlein (at the high octave) had unusually high string tensions for its size could be that Meuller didn’t produce his strong wire in gauges any thinner than 11.
A PROVISIONAL LIST OF QUINTICLAVES (ALTO OPHICLEIDES) IN EUROPE
AND THE UNITED STATES

Mark Jones, Arnold Myers and Albert R. Rice

The quinticlave or alto ophicleide was a keyed brass instrument initially made by Jean
Hilaire Asté (known as "Halary" or "Halari") in 1817. According to Mahillon (Brussels catalog,
vol. 1, p. 302) the instrument was made in E♭ or F with 8 keys and by 1822 carried from 9 to 12
keys. Extant examples are rare and were made during the 1820s through the last third of the
nineteenth century. The majority of instruments found were made by makers in France followed
by makers in Belgium, the United States, the Netherlands, Italy, and England. A few instruments
were made with a narrow bore and most examples carry nine keys. The following list was
compiled in order to document the makers and characteristics of this little-known brass
instrument and to locate examples. Twenty-two instruments are listed alphabetically by maker's
name followed by ten anonymous examples with a brief description and location in an
abbreviated form following that given by William Waterhouse in The New Langwill Index. A
and collections are also listed with the last name of the maker of the quinticlave in order to
indicate the number of examples held in each.

Museum curators and collectors with quinticlaves in their collections which are not listed
are requested to write or E-mail Albert R. Rice (6114 Corbin Ave., Tarzana CA 91356-1011,
al_rice@cuemail.claremont.edu) or Arnold Myers (30 Morningside Park, Edinburgh EH10 5HB,
Scotland, a.myers@ed.ac.uk).

Agliati, G., Turin, fl. 19th century (Agliati was probably a dealer since this is the only known
example of an instrument with his name), 9 key, CH-Basel, Bernoulli 30.
Courtois neveu aïné, Paris, fl. 1803-62, formerly played in the Boston Brass Band, ex Pillsbury
Couturier, Jacques, Lyons, fl. 1812-36, E♭, B-Bruxelles, 1253.
[Darche, Paris, fl. 1830-65, 9 key, E♭, D-Leipzig 7602 destroyed during World War II]
Embich, Ludwig & Co., Amsterdam, fl. 1820-44, 9 key, E♭, NL-Den Haag, 1952x0156
Graves Samuel & Co., Winchester, fl. 1830-50, 9 key, E♭, US-NH-Winchester (Conant Public
Library).
Guichard, A. G., Paris, fl. 1827-45, 9 key, E♭, signed vertically on bell, US-CA-Claremont, B3
Klappman frères, Paris, fl. mid 19th century (Klappman frères were probably dealers since this is
the only known example of an instrument with their name), 9 key, US-DC-Washington-S,
205858.
Mahillon, Charles, Brussels, fl. 1836-1945, 9 key, Eb, B-Bruxelles, 1252.
Printemps, Jacques, Lille, fl. 1820-47, 9 key, narrow bore, F-Paris, 652.
Salf, Toulon, fl. 19th century (Salf was probably a dealer since this is the only known example of an instrument with his name), 9 key, CH-Basel, Bernoulli 186.
Sax, Charles, Brussels, fl. 1815-52, 9 key, Eb, B-Antwerpen, 67.1.54.
Van Engelen, F., Lierre, fl. 1813-50, 9 key, Eb, B-Bruxelles, 2744.
Anonymous, 9 key, Eb, B-Antwerpen, 3764.
Anon., 9 key, Eb, B-Antwerpen, 63.24.1.
Anon., 10 key, possibly in F, ivory mouthpiece, narrow bore, formerly played in the Marlborough Yeomanry Band, UK-Reading-University, 51/302.
Anon., 10 key, Eb, NL-Den Haag, 1952x0092.
Anon., 11 key, Eb, UK-Edgware, 547.
Anon., 9-key, Eb, possibly German (bell engraved with maple leaves and acorns), US-CA-Claremont, B182.
Anon., narrow bore, 10-key, Eb, US-NH-Concord, Giles collection.
European collections: (17)
B-Antwerpen: Sax, Anon., Anon.
B-Bruxelles: Courturier, Mahillon, Van Engelen.
CH-Basel: Agliati, Salf.
D-Stuttgart: Anon.
F-Paris: Printemps.
NL-Den Haag: Embach, Anon.
UK-Edinburgh: Gautrot.
UK-Edgware: Kohler, Anon.
UK-Reading: Anon.
United States collections: (15)
US-CA-Claremont: Guichard, Anon.
US-MI-Dearborn: Graves, Courtois neveu aîné, Anon.
US-NH-Giles: Anon.
US-SD-Vermillion: Sax.
BIBLIOGRAPHY

[Baines, Anthony]. *Antique musical instruments of historical interest* (Edgware, n.d.)
Heyde, Herbert. *Hornen und Zinken* (Leipzig, 1982).
Comment on Comm. I486 (Recorder Patents)

In his account of some of the more interesting recorder patents Alec Loretto described at some length the virtues of the bell key, but he rather glossed over the fact that the bell key as we know it today was never in fact patented. According to Daniel Waitzman, in a comprehensive survey of the bell key (Recorder and Music Magazine, September 1968), it was invented independently by a number of workers (including Carl Dolmetach), and the first one was apparently made in 1953 by John J.F. Juritz, a physics lecturer and bassoonist in Cape Town.

The 1958 Dolmetach patent No 852165, described by Alec Loretto and illustrated in fig. 3, was a device which was intended to avoid the mechanical problem of fitting a key over the end hole (and perhaps the mousetrap-like appearance of such a key at the end of the elegant treble recorder). Unsurprisingly the design was not fully satisfactory because the new side-mounted 'end' hole was smaller than the normal end hole and it effectively shortened the playing length of the instrument. (But why was the foot joint not made longer to enable these two faults to be corrected?)

To this day the unpatented end-mounted bell key has failed, despite its very real benefits, to gain popular support among makers and players. Incidentally, in describing the strange effects produced by the instrument with the bell key closed, it might have been helpful if reference had been made to Comm. I430 in which the acoustics of the bell key phenomenon were explained.
A week before the auction of musical instruments at Sotheby's, London, I heard that there was one Dutch recorder on sale. Well, I asked and got the permission to see this instrument, a voice flute by W. Beukers, from Amsterdam. Shortly before my departure however I realized that I had seen this instrument before, and so I, after my arrival at Sotheby's, I asked to see the three other recorders, and to take some measurements as well. These three instruments proved to be very interesting and as they maybe went from one private collection to another, and maybe therefore becoming not accessible in future, I will give here some information (all measurements in millimeters).

Alto recorder by Stanesby Junior.
A boxwood instrument in three joints, stained dark brown, bought somewhere on a flea-market for £15, as Mr. Graham Wells from Sotheby's told me.

The stamps were all very sharp:

STANESBY in a slight curve, the stamp on the foot was visible on the front if
JUNIOR the 7th fingerhole was turned to the right.

The recorder was in a not too bad condition. One crack in the socket of the foot joint, and a windway which was very dusty. The upper half of the head joint was warped rather ovaly. The other parts of the recorder were only slightly ovaly in cross section.

The radial face of the wood (showing the stripes) was visible on the front (where all fingerholes are) of all joints. Some people say that this was always done by baroque woodwind makers, but I have seen some well made historical instruments showing the tangential face (the ‘flames’) of the wood on front of the joints (for instance the middle joint of the J. Denner tenor recorder, see below). One of the advantages of the radial face on the front is that there is not so much chance that cracks will occur in the labium and windway; cracks will come in the first place where the tangential face of the wood is visible. A disadvantage, also on this the Stanesby-Jr. recorder is that the wood shrinks vertically much more strongly than horizontally, causing an ovality on cross section which can result in a narrower windway. For instance, the dimensions of the wood of the head joint at the window was now 31.6 (vert.) x 32.2 (hor.).

I did not remove the block, so I can not describe the shape of the windway, how it slopes or is curved in length-direction, and so on.

The window is: 13.0 (wide) x 4.7/4.8 (long); at the players end the windway is about 17.0 (left to right, or East to West); that is very wide, I have not seen windways of baroque alto recorders with such dimensions. Much more normal is 14 at the players end, and about 11.5 to 12 at the windows end. But on the Stanesby-Jr. recorder it looks all quite normal and undamaged. The windway is slightly curved in E-W direction, on top (the roof) a little more than on the bottom (the block). The labium corner is nicely on line with the block surface, with the same curvature. The chamfers at the window’s end looked to me in good condition, sharp and apart from some dust rather clean and -important!- not too big.

About the middle joint: it surprised me that all fingerholes were so very round, as if they were drilled directly to the final dimensions, and later only undercut but not enlarged on top. Is that typical for recorders by Stanesby Junior?

The sound of the instrument was not free, a bit muffled and noisy, caused by the dirt in the windway. But the undertone was one of good quality: stable which good harmonics in the lower register, and easy speaking in the top notes. I suppose that after cleaning this recorder must sound very nicely. And further: all octaves in tune, and the instrument playing at
a- 415 Hz! I give here the results of playing, deviations in cents, tuner set at a-415 Hz.

<table>
<thead>
<tr>
<th>Note</th>
<th>Cent</th>
<th>Note</th>
<th>Cent</th>
</tr>
</thead>
<tbody>
<tr>
<td>f1- 0</td>
<td>1 2 3 4 5 6 7</td>
<td>+/- 0</td>
<td>f2- 0</td>
</tr>
<tr>
<td>g1- 0</td>
<td>1 2 3 4 5 6</td>
<td>-10</td>
<td>g2- 2</td>
</tr>
<tr>
<td>a1- 0</td>
<td>1 2 3 4 5</td>
<td>-5</td>
<td>a2- 0h</td>
</tr>
<tr>
<td>b2- 0</td>
<td>1 2 3 4 6 7</td>
<td>+/- 0</td>
<td>b2- 0h</td>
</tr>
<tr>
<td>b1- 0</td>
<td>1 2 3 5 6</td>
<td>+20</td>
<td>b2- 0h</td>
</tr>
<tr>
<td>c1- 0</td>
<td>1 2 3 5 6 7</td>
<td>+/- 0</td>
<td>c3- oh</td>
</tr>
<tr>
<td>d2- 0</td>
<td>1 2</td>
<td>-5</td>
<td>d3- oh</td>
</tr>
<tr>
<td>e2</td>
<td>-5</td>
<td>+/- 0</td>
<td>e3- oh</td>
</tr>
<tr>
<td>f#2- 0</td>
<td>-10</td>
<td>f3 not measured, but good interval to e3</td>
<td></td>
</tr>
<tr>
<td>g#2- 2 3 4 5 6</td>
<td>-10</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

h = tone hole only partly covered

What surprised me was the flat f#2, with normal fingerings, but perhaps it makes a good mean tone major third a2-f#2. And, as on more historical recorders, it is uncertain how to play the b2. With Oh 1 2 3 5 this tone is very sharp, it is just the opposite of a good mean tone major third g2-b2. But closing hole 6 makes the tone much too flat, and closing hole 6 only partly is not so easy. My question: how was the b2 played in baroque times? Nevertheless, I have seldom seen a historical recorder still so well in tune.

The instrument was played more or less intensively a long time ago, but surely not so recently.

Some dimensions (L = length, SL = sounding length, W = width, Ø = dimension of bore or of the turned parts, the dutch term is 'diameter').

Head joint: L 192.3; SL 132.5; windway: L 59.8 (W 17.0 to 13.0); window (W x L): 13.0 x 4.7/4.8; Labium with window: L 29.5, Wmax. 17.0; Socket: L 29.7, Ømax. 24.9

Ø bore at socket end: 19.2. Towards the block the bore is slightly wider, maybe up to 19.6. Where the block is visible at the beak of the recorder, the Ø of the bore is > 20.0. Middle joint: L 27.4 (upper tenon) + 203.5/204 (= SL; this joint is slightly curved) + 15.2 (lower tenon).

Ø bore 18.8/19.5 (upper end) to 14.4/14.6 (lower end). At the upper tenon the bore is warped ovaly; from this point towards just before fingerhole no. 4 very wide, narrowing from 19.5 to about 17.0; after hole 4 there the bore is narrowing more strongly, to about 14.5. Foot: L 101.5; socket: L 15.9, Ømax. 20.2 ; Ø bore14.3 near the socket to 12.0 after about 60 mm; after that point the bore is almost cylindrical to the end, where the bore is 11.8.

On the same auction, there were 2 recorders by J.Denner (Jacob Denner, the son on Johann Christoph), an alto in f1 and a tenor in c1. Both recorders from one private collection in Germany, and I suppose that these instruments were recorded by Ph.T. Young, in his book '4900 Historical Woodwind Instruments' (1993, Bingham, London) as alto recorder no. 12 and tenor recorder nr.6 (because Young mentions a horn repair ring on the foot).

The stamps on both recorders are not very sharp: I.DENNER in a scroll, a fir tree below with on the left an 'I' and on the right the 'D'.

The tenor recorder was for me much more interesting than the alto. The sound was better, the pitch (again a-415 Hz) convenient for modern baroque practice, and there are not so
many 'real' tenor-recorders (that means: not made as low pitched voice flutes, without key and with narrower bores).

The tenor recorder, in boxwood, was a much played instrument, with several damages. A horn ring was turned to repair a crack at the socket of the foot, in fact there is still a crack over the whole length of the foot. On the middle joint, Denner put the tangential face of the wood on the front, on the head joint however he preferred to put the radial face on the front.

The windway was at the players end rather worn, with a big step (2.8 mm) on that end. The whole block was fitted very loose, so I could easily remove it, just by gently pushing it, without any hitting. I could see that the windway was in length direction sloping downwards (as on many modern Moeck-Rottenburgh factory recorders), and not upward (as on many historical baroque instruments). But the head joint was also very curved, the short side of the curve being on top of the instrument (where we find the labium and windway), so it may be that originally the windway was more directed parallel with the axis of the head joint.

In E-W direction the windway was almost flat, and so was the labium corner. The top-chamfer was clean and sharp, the block-chamfer was in not so good condition, rather rounded at the edges. But the sound of the recorder was all right: full and sonorous in the lower register, a little difficult in the top notes, maybe because of a worn thumb hole.

There is a nail groove on it, with such an angle that it was suggesting to me that the recorder was played with the left hand down, and the right hand up.

Compared with the Stanesby-Jr. alto recorder, the Denner tenor gave me not so much a feeling of historical sound (not enough resistance, I suppose), although the sound was easier, cleaner and less noisy.

The tuning table, tuner set at a-415 Hz, deviations in cents.

<p>| | | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>c1</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6 7</td>
</tr>
<tr>
<td>c3</td>
<td>0h1</td>
<td>-</td>
<td>+</td>
<td>5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>d1</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>e1</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>f1</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>f#1</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>5</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td>g1</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td></td>
<td></td>
</tr>
<tr>
<td>a1</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>b1</td>
<td>0 1</td>
<td>-</td>
<td>+</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>c2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>d2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>e2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>f2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>f#2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>g2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>a2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>b2</td>
<td>0 1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
</tbody>
</table>

Well, the forked fingered tones give on this recorder again some questions. The g#2 is very flat to a2, but I have seen identical problems on other long recorders.

The octave interval a1-a2 on the instrument is also a bit flat, but not so much. Of all finger-holes, 1 and 4 were drilled slightly upwards, hole 3 slightly downwards. Making a copy, I would suggest to drill hole 3 a little lower on the joint, and slightly bigger, if you fingers are long enough to cover the hole.

About the f#2: here the same problem as with the b2 on the Stanesby-Jr. alto; who knows the answer how to play this tone in tune?
Some dimensions of the J. Denner-tenor recorder.
Head joint: SL 170 (front) and 174 (back side); L 242/246. Socket: L 34.3; Ømax. 30.0. Window: 14.6 x 5.2, block: L 72.7 (somewhat longer than the windway), W 14.2 (windows end) to 17.3 (players end). Labium: L 31.0, Wmax 17.8.
Bore of the headjoint: 23.1 just under labium, 22.9 going through.
Middle joint: L 32.4 (upper tenon) + 278.5/279 (SL) + 17.7 (lower tenon). The bore tapers from 22.0/22.3 at the upper end slowly to about the 6th fingerhole (Ø about 19), and then strongly to 15.7/16.0 at the lower tenon.
Foot joint, with brass key: L 150.2; socket L 18.3; Ø 22.3. The bore of the foot is almost cylindrical: after the socket 15.6, then narrowing to 15.0 just after the key hole and so over most of the length of the foot, and at the lower end again widening to 15.7.

The alto recorder of J. Denner. Again a Denner recorder in -I suppose- unstained boxwood. The turning is fine, and with a typical Denner-profile. Compared with the Stanesby recorder, the dimensions (lengths of the joints and place of the fingerholes) are almost identical, but the bore is much narrower and so the pitch is sharper, about 10 cents.
The sound of the instrument is a little noisy, the lower tones are stable (accept well an increase of breath pressure), the top notes are somewhat difficult, maybe again because the thumbhole was very worn, in the same way as on the tenor recorder.
Although the recorder was well (but not luxuriously) made, I was not so impressed by the quality when playing the instrument. I can't exactly say why, but there was something missing. The Stanesby recorder had that 'extra' what made me curious.

I have not removed the block, so I have only a few measurements of the head bore. Ø 18.6 goes through, but at the window I expect a diameter of about 19.5. A head joint of an alto recorder by the same maker, in Hohenzollern Castle in Sigmaringen (South Germany) has about the same dimensions, and there the bore of the head joint is conical, from 20.0 at the beak opening, 19.4 at the window, 19.0 at the end of the candle flame, and then about cylindrical (18.7/18.8) to the end. Again, the head joint of the alto recorder at Sotheby's is clearly ovaly in cross section in the upper section. There is an obvious contraction of the bore at the upper tenon of the middle joint; further I have seen no problems on this instrument. The bore of the middle joint shows some 'waves' (or reamer-ends), after hole 6 the bore narrows strongly. The bore of the foot is strongly 'X-shaped', the narrowest point in the middle of the joint. This looks much more like the bore of a foot joint by Johann Wilhelm Oberlender than by Denner himself (according to the observations by Thomas Lerch), but the stamps are obvious by Denner.

Some dimensions:
Head joint: L 193.7; SL 132.3; socket L 30.2 (very long!), Ømax. 25.1. Window: 12.3 x 4.6/4.8 (that is a long window); Labium: L 26.7, Wmax. 15.7. Windway: W 13.8 at players end to 12.3 at the window. In E-W direction the windway is very slightly curved, almost flat also. Bore of head joint: see above
Middle joint: L 29.6 (upper tenon) + 203.5 (SL) + 14.9 (lower tenon). Bore: 17.7 at upper end to 12.7 at lower end.
Some bore dimensions (Ø - L): 17.6- 2/58; 17.2- 81/85; 16.8- 88-100; 16.4- 110/115; 16.0-150/155; 15.8- 158/165; 15.4- 173/178; 15.0- 191/200; 14.6- 212/219; 14.0- 232/hole6, 13.6- 235/237; 12.8- 245/- through
Foot: L 104.0; bore: 12.7 at end of socket and then straight conical to 10.7 at 65 mm, then regularly widening to 12.5 at the lower end.

Some pitches (tuner at a-415 Hz, deviations in cents):
- f1: +15; f2: +15; f3: +5/10; g1: +5; g2: +30; a1: +5/+10; a2: +10/+15; b\textsuperscript{b1}: +/- 0 (hole 7 closed) and +25 (hole 7 open); b\textsuperscript{b2}: -15 (hole 6 closed) or +/- 0 (hole 6 partly covered); b1: +20 (hole 7 closed); b2: +35; c2: +10; c3: +10; d2: +/-0; d3: -5; e2: +25; e3: +/-0/15.

The e2 and g2 are too sharp surely because of the tenon contraction. I was surprised by the relatively flat d2 and d3; b2 is again sharp (fingered 0h 1 2 3 .5).

Conclusions

It is always a great moment to play historical instruments, and I know, in many musea it is not allowed to do so. And the curators are right, playing too much can damage the instruments. Therefore I didn't play long, only for checking the pitch. And I know that's not the right way: the instrument must be warm, and after some minutes of playing you must take your measurements. Well, that is again impossible, it is allowed only in some private collections where the instruments are used to be played. I wrote about the problem of knocking out the blocks: I do it alone if it can be done without any force, only by gently pushing. I always give information to the curators of the collections where I have been: measurements, descriptions and photo's, so people after me have not to take the same risks. But (there is always a but): I am always missing some information on measurements taken by other people, and even on measurements I have taken years ago myself. So, whatever you do, your observations and measurements are never perfect. But I try to make the best of it and make a good documentation.

There is nothing that can replace your own experience of measuring and playing historical instruments; there is nothing that give more inspiration to have an original instrument in your own workshop. Barthold Kuyken, the traverso-player wrote: there is growing a new generation of woodwind makers who have never or hardly played original instruments. That's a dangerous development, and I agree with him. Also players do not know the sound of historical instruments. They ask for louder instruments, for oboes and recorders with not-historical fingerings. What is the end of that?

Finally: I have taken full measurements of the Stanesby-Jr. alto recorder and the Denner tenor recorder. Who wants to have these measurements (including some colour photo's of details of the instruments) can buy these. Send me for each instrument £ 15 or hfl 45 (forty five dutch guilders), either by money order or by post giro. For both instruments together: £ 25 or hfl 75 (there are no grants for me available now, so I have to ask some money for the drawings).

My giro-account and address: 2218523, for Mr. M.C.J. Bouterse, Sandenburg 69, 2402 RJ Alphen a/d Rijn, Netherlands.

About the W.Beukers-voice flute: this is a problematical and damaged (windway and labium) instrument, and I will discuss this only Dutch voice flute in my dissertation.

ps: last information: the Stanesby-recorder was sold for £ 22.000, the other instruments were not sold at the auction.
1996 FoMRHI List of Members — 3rd Supplement as at 13 January 1997

* in left hand margin = change of address or other change

* Philippe Bolton, +33-4-9061-8611; fx +33-4-9061-9782.
* Tadeusz Czechak, Zelazna 76A m63, 00-894 Warszawa, Poland; +48-22-654-02-52 (med str instrns; M, P).
* Winfried Eggenweber, Neubaugasse 21/3, A-1070 Wien, Austria; 93-21-54 (ww, esp recrdr; M. rep).
* S W Ettlinger, 7 Long Beech, Ashford, Kent TN23 4XU, UK.
* Dietrich Hakelberg, Hartkirchweg 69, D-79111 Freiburg i.Br., Germany; +49-761-443789.
* John J Haluska jr, 31 Cedar Avenue, Apartment 36, West End, NJ 00740, USA; (908) 571-1033; jhaluska@notes.cc.bellcore.com (hpschd, vrgnls, clavchd, gtar, sitar).
* Steve Heavens, 106202.3036@compuserve.com.
* Uta Henning, Reichertshalde 42, D-71642 Ludwigsburg, Germany; t/fa 07141-257498.
* Lewis Jones, 18 Mare Street, Hackney, London E8 4RT, UK; t/fa 0181-533 6404.
* Peter Andreas Kjeldsberg, Ringve Museum, Pb 3064 Lade, N-7002 Trondheim, Norway; +47-7-914515.
* Darja Koter, Pokrajinski Muzej, Muzejski Trg 1, 2250 Ptuj, Slovenia.
* David Lasocki, Music Library, Sycamore 0009, Indiana University, Bloomington, IN 47405, USA; lasocki@ucs.indiana.edu (recrdr, trav; P, res, W).
* Tom Lerch, Gruenheider Weg 100c, D-12589 Berlin, Germany; 030/6486794 or /25481147.
* Renke Lody, D-25761 (the rest as before).
* Donald Mackinnon, see: Mimi Waitzman.
* Peter O’Donnell, odonnell@blue.weeg.uiowa.edu or psod@aol.com.
* Joseph O’Kelly, boxes@globalnet.co.uk.
* Guillermo Peñaalver, c/San Roque 10, portal A, 2ª dcha, E-41001 Sevilla, Spain (recrdr, trav; P).
* Martin Pühringer, Marktmühle, Stelzen 4, 5, & 8, A-4170 Haslach, Austria; t/fa +43-7289-72242.
* Huw Saunders, 0171-503 5824; 101332.1472@compuserve.com (hpschd, embossed papers; M).
* Wiebke Schneider, Sprinkstraat 2, NL-6269 AP Margraten, Netherlands.
* Jon Swayne, t/fa 01458-850911; js@swayne.demon.co.uk.
* Gijs van Ulsen, 026-3341434.
* Henri Vanherle, Oude Gentstraat 4, B-8760 Meulebeke, Belgium; 051/48.91.76.
* Mimi Waitzman & Donald Mackinnon, 85 Cholmley Gardens, Fortune Green Road, London NW6 1UN, UK; 0171-431 5507.
* Charles Wells, t/fa 0302-846492.
* John Weston, Mowbray Lodge, Marshbrook, Church Stretton, Salop SY6 6QE, UK; 01694-781288.
* Lou Zeekaf, Clotildestraat 49, NL-6132 GG Sittard, Netherlands; 046-4-517246.

Museum:
Ptuj

Pokrajinski (Darja Koter)

Please send any corrections for the next main List in April