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FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS
Honorary Secretary: Jeremy Montagu, 171 Iffley Road, Oxford OX4 1EL, U. K.
FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS

January, 1998

It's a bit frightening to think that I've written 90 of these things (well, nearly 90 – I missed one while on sabbatical)!

Anyway, as usual a happy new year to a minority of you (unless Barbara is harbouring a large number of renewals she's not yet sent me – she was – it's a majority of you – the card file is looking much healthier, with almost two-thirds already renewed). As I warned you last time, anyone who's not renewed by the time Eph does the envelopes for this Q will have to wait till the April one goes out.

Unless, that is, you can surf the net, because if you can, you will find us, probably including this Bulletin on our own brand new web site, thanks to Eph's generosity in sharing his internet site with us, at http://www nrinstruments demon.co.uk/fomrhi.html This is a temporary expedient, until FoMRHI gets its own site, or of course until Eph needs the space on his site for his own things, but it gets us off to a start. As it think I've told you, I'm not in a position to do it myself because of an idiocy on my computer, which makes the net inaccessible. For that reason I've suggested that Eph should discuss, in his Bulletin Supplement, what we should have on our site, and ask your opinion. So, over to him and you. You might just remember, though, that I'm not the only one who can't access the Web but can receive and send email, and that there is an even greater number who can't do that either, so fairly old-fashioned email, and really old-fashioned print is not yet out of date! By all means answer queries etc on the http:// but please repeat them to me by email (jeremy.montagu@music.oxford.ac.uk) for the Bull.

Which brings me to an email message from Bernard Taylor, who asked "Do you accept Comms by e-mail?", to which the answer is "Yes, please", either to me or to Eph (postmaster@nrinstruments.demon.co.uk). Paul Baker's Comm in this Q came to me that way, and so did Rod Jenkins's in the last Q. It's often the best way, and it's certainly the best way for notes for the Bulletin, like Bernard's and Marco Tiella's below, because I just feed them straight in.

Lost Member: Anyone know Thomas Murach who lived in Manosque in France? He doesn't live there now and his October Q came back. He renewed, I think at the RCM, but didn't then give me a new address.

Member's List: May I remind you that the next Annual List of Members comes with the next Q. If you have any changes, corrections, email addresses, and so forth, please let me have them before the First of April. If you're going to move house, do it now!!

Obituary: Bob Spencer, the famous lutenist and lute teacher has died. I should have noted this in the last Q, but I'd missed the notice of his death and Uta Henning pointed it out to me. He'd been a member since 1977, our second year.
Further to: Last Bull: Bernard Taylor: 'I agree that it is better to have an E M Exhibition every two years. Exhibiting every year is quite a drain on meagre profits and I noticed several regulars seemed to be missing. It would be a disaster if exhibitors were forced to decide for themselves which years to omit for then each year would merely have a random selection of exhibitors and so not be comprehensive. The organisers should decide on alternate years and then the quality and comprehensiveness would be enhanced.'

Kunsthistorisches Museum Conference: Uta Henning told me that it was very successful – all the more a pity that they didn’t give it earlier publicity.

Camwood: Paul Hailperin: ‘From the German-British Chamber of Industry & Commerce I have received information which I quote in part: “According to information we have obtained Camwood Ltd has gone to the process of a Company Voluntary arrangement according to Part I of the Insolvency Act 1986...” For all effective purposes this is probably the end of dealings with Camwood. Has anyone information on The Woodwind Quarterly? They also seem to have disappeared. In both cases it seems a pity, for the finances as well as for an interesting part of the scene.’

Museums: The Royal College of Music sends out a spasmodic newsletter (spasmodic because the last I’ve received was dated Summer 1997 and came too late for the last Q, in October). They’re working on a CD-ROM Catalogue, to which we look forward, also to a ‘Virtual Tour’, and as a result publication of the Addenda to the Wind Instrument Catalogue is delayed, though it should be with us quite soon now.

The Bate Collection has a Serpent Weekend on 10/11 July – anyone interested get in touch with them. Also an Introduction to the Viol on 2nd May, and a Bow-Rehairing Weekend 14/15 March.

Queries: Marco Tiella: ‘I wonder if you know existing Italian harpsichords of the 17th (1600-1700) in which are bracings, or traces of, in the shape of those in the C.Haward’s 1683 harpsichord.’ His email address is matiella@tin.it

John Summers (26 Kilmory Road, Lochgilphead PA31 8SZ, Argyll): ‘I intend making a set of Uillean pipes, either concert pitch (D) or a flattened set, which I am told would have a ‘sweeter’ sound. Can you advise me regarding work carried out on any woodwind instrument to obtain the optimum details of the taper bore? Can you advise me of other sources of information on Uillean pipes? I have the address of some makers but feel it not appropriate to ask them for their ‘secrets’.' He does not say why he then does think it appropriate to ask us! But if anyone can help him it would be a kindness.

Things available, etc: The last issue of the American Recorder (November 1997) has an interesting article on ‘Recorders and Angels: First Sightings in Catalan Art’ by Anthony Rowland-Jones; I’m not
wholly convinced that they’re all recorders, rather than six-hole duct flutes, but they could well be. Certainly worth a look, for anyone interested in its early history.

Marc Vogel has maintained the prices of his **CTS-5 Tuning devices** and points out that the strength of the pound makes them cheaper here than they were. His address Talgasse 2, D-79798 Jestetten. He also sells strings, plans for many keyboard instruments, etc.

The Department of Trade & Industry has produced a journal called **GIST** (Global Information on Science and Technology) which Dr Peter Carter, Head of International Technology Service, said contains all the latest news on S&T, ‘Since like all busy professionals, your members are flooded with too much to read, **GIST** gives them the gist of news in clear, accurate, short summaries’, he sent me a complimentary copy – if you write to him nicely he might send you one, too, subscription to **GIST** and its database is £47, a 50% reduction for the first year.

**Lira-Forum no.1** has appeared – see their note in the last Q – already with information about who is making Lira and similar instruments, etc. Their email address is pomlylra@magnet.at and postal Rennweg8, A-1030 Wien.

Ekkehart Stegmiller, who was a member way back, is again publishing books (including *Uta Henning’s Musica Maximiliana*) and selling books, music, recordings, and instruments. Address Mozartstr 1, D-89231 Neu-Ulm.

**Deadline for next Bull:** All Fools’ Day, please: Wednesday, April 1.

*That’s it* for now – I’ll have to hold this until I get the next batch of renewals from Barbara. I have, only one thing has come in, a pair of Comms from Jan Bouterse, who asks me to say: ‘In Comm 1539 I made a mistake: Nicolas Selhof played an important rôle in Dutch musical life in the 18th, not the 17th century as you can see, because he lived from 1680 to 1758.’ Like a number of others, he sent good wishes for 1998 – thank you all.

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The stability of each type of culture depends on an overwhelming majority being unquestioning followers of current fashion in what is considered good, with independent thinkers thought of as peculiar, laughable, stupid, irrelevant, or at worst, dangerous. In the Arts, an important function of critics is to reinforce, clarify and promote what the consensus of culture leaders deems as good taste. The critics are amongst those leaders, and those critics who can think beyond current fashions of thinking are no more prevalent than in the general populace, few indeed.

Jeremy is a good critic. Unless he is on one of his hobby horses (which is not the case here), he does a very good job of predicting how the normal reader will respond to the offering, as well as giving enough information about it for different kinds of readers to decide for themselves. This review is no exception.

I am not a normal reader. Being on one of my hobby horses, I applaud any attempt to produce performances which might be more historically accurate than the bland modern music-industry norm of no-risk professional accuracy in reproducing every note (and little more) of the musicologist’s edition of what the composer wrote with perfect intonation and no extraneous noises on an instrument in perfect balanced condition. This is aimed at the modern music consumer sitting relaxed in a reverie being swamped by music he or she is convinced is ‘pure’. Historically, musical performances were hardly ever so ‘pure’. Sharpened intonation could well have signalled heightened emotion to the listener. Improvisation was at the core of musicality (much of the surviving music was just improvisation recorded on paper). And the ‘bangs, clicks, thumps, twangs and crashes’ that offend Jeremy could well be what was expected in music making then. More tolerance of such violations of the standards of the modern music industry is likely to bring us closer to the real sounds of historical music. I admit that I am unusual in wanting this, rather than the usual performance which expurgates anything that a modern professional feels might harm his or her reputation as a professional.

Web site

When I was young and single, I worked as a physicist for Lever Bros. This gave me disposable income, so I helped a beginning instrument maker get started. He has since developed a worldwide reputation as one of the best. From this he has gained disposable income while I’ve lost almost all of mine. Three years ago, he returned (and topped) my generosity by giving me the present of a computer, a modem, and a subscription with an internet service provider. He also got me into an invitation-only e-mail club concerned with the technology of bowed instruments. It has mostly violin and bow makers in it, but there are also viol makers, museum people and academics. I and they have learned useful things from my being in it. I can invite others to join if they are interested.

The internet service provider offers free web space. My wife has been pestering me to create a web site for NRI because it might bring in some more business. I’m not convinced of that, but having our brochures more easily available could be a useful service. Armed with a faulty beginner’s-level translation programme and an HTML book, I was able to set up the site by Christmas. I put in over 100 A4 pages of our brochures, and it used up 1 megabyte of space. The free allocation of space is 5 megabytes, so there is plenty left. I asked Jeremy about using it for a FoMRHI site and he said ‘go ahead’. I was uncomfortable about any association between FoMRHI and any commercial organisation, but this donation of web space seems harmless enough since one goes directly to the FoMRHI site and there is no link to the NRI part of the site. If members still feel that this is inappropriate, let us consider this to be a temporary loan of web space until FoMRHI can get web space of a more appropriate type.

So now we have a web site! It is at:  
http://www.arinstruments.demon.co.uk/fomrhi.html

What is in it now is Jeremy’s statement of what our aims are, and details of how to become a member and buy back issues of our Q. Also in it is Charles Stroom’s Chronological Index which gives the table of contents for each Quarterly (numbered from 1 to 89 - complete). Charles says that the Permuted Index is unnecessary on the Internet because readers can easily do automatic searches
themselves with their computers (it is not now in a form suitable for inclusion, and he doubts whether getting it into that form is worth the effort).

I would be glad to put more in it, but want you the members to suggest what you would like to have in it. One idea I had was for members to write what they thought of FoMRHI (it could be signed or not) - its strengths and its weaknesses - so a prospective member could get an idea of what to expect. Another idea was to give our instructions for authors, inviting non-members to contribute (and hopefully to join eventually). Your suggestions are requested. If none of the 3 Honorary officers objects, and it is not too much work for me, whatever is suggested will be included.

Indexes

Some years ago, we printed and distributed to all members copies of the Chronological and Permuted Indexes of FoMRHI Quarterlies up to 52 (July, 1988) generated on his computer by Charles Stroom. Having 88 pages, it was printed and distributed just like an extra Quarterly. He has been sending Jeremy updated copies every year, and it now approaches 200 pages.

Jeremy, Barbara, myself and Mark Ellis (our printer) have been discussing getting an updated version to the membership. It is much too big to be bound like a normal Q. A durable binding process would be necessary, and that becomes remarkably expensive. Two possibilities of what we can afford to do are: 1) to send it to all members unbound, and you can go to your local stationers and get it bound yourselves, probably using the plastic comb method, and 2) we can produce the Chronological Index as a normal Q and the Permuted Index as two normal Q's. What do you think?

Some time ago, when there was some discussion of duplicating Comms from Q's that are out of print, some members indicated that they would prefer to delete some early Comms that they wrote. This is probably a good time to do something about it. If authors so wish, we can indicate that a Comm is withdrawn in the Chronological Index in the web site and in the distributed copies, and can delete all references to it in the distributed Permuted Index.

This is also an appropriate time to correct errors. They are inevitable. In the last set of Indexes distributed, I was credited with a Comm. I didn’t write! I would suggest that all authors of Comms in past Q’s should try to get to look at our web page and check whether their Comms are correctly listed in the Chronological Index, and report any errors to Jeremy or me.

Summer course in Latvia

Dates - July 23-31 1998
Place - Edole castle, near Kuldiga, Latvia.
Cost - Approx 200 GB Pounds / 350 US Dollars (includes food and accommodation).
Tutors - Stewart McCoy, lute; Margaret Westlake, recorder & viols; Ivars Cinkuss, voice and chorus; and Virve Kurkel, Dance.

The theme this year is the reformation, and the course will end with a Masque based on Burkhard Waldis’s play Parabell van verlorn Szohn (Parable of the prodigal son), first performed Riga 1527. Repertoire will however be late 16th century and taken mainly from the major courts and cities Baltic Sea area, including Riga, Koenigsberg and Copenhagen. There will be sacred music and secular music, in large ensembles and small ensembles of mixed voices and instruments, and dance in costume.

If interested, contact: Solvita Sejane, Riga Early Music Centre, Brivibas 85, Riga LV-1001, Latvia
Tel: +3712 275575 Fax: +3712 278060 e-mail: musbalt@com.latnet.lv . Details of the Riga Early Music Centre summer course at Edole Castle see http://www.icb.nl/musicweek/course.htm

The 1710 Michael Mietke single harpsichord in Hudiksvall, Sweden

Information collected by M. Griewisch and A. Kilström includes: a full scale drawing (3m x 1.20), a brief documentary report in English and German, and a set of photographs in both colour and black and white. Available from Matthias Griewisch, Industriestrasse 100, D-69245 Bammental, Germany. Phone and fax: +49 (0)6223 492 87.
This book is something of a curate's egg, good in parts and less good in others. Its main problem, though, is that it shows many symptoms of haste in compilation, or if not of haste then of carelessness, and I'd prefer to think that it was haste. One could cite many examples, but a few will suffice. There is text missing between pp 109 & 110, it may just be one line (p. 109 is a line shorter than 108) or it may be more. There are grammatical solecisms, in particular several appearances of 'a number of' are. There are numerous non-sequiturs, for example when discussing matters in England, we are told that there was a great change, and bang we're in New York with nothing more said about England. There are repetitions between authors, where both say the same thing, and more seriously there are contradictions between authors, when one says something happened and the other says it didn't. Several sections are, as one would expect, by the authors of the standard books on some instruments, and some of these show rather clumsy cuts and jumps, as though the editorial blue pencil had been too heavily used and without sufficient consideration. There are many loose ends, one example: 'and the apparently unsuccessful attempt to promote the playing of Beatrice Pettit', and that's all we're told - there's no end-note reference, no further comment, and we're never even told who was Beatrice Pettit. Another: 'brass instruments such as the firebird and superbone.' and no further information as to what are or were the firebird and why one bone was superior to another, and that in a book which purports to be a Companion to Brass Instruments! More information on the instruments and their use in jazz, and less space on a repeated panegyric of Miles Davis would be useful, too. Illogicalities abound, one example the use of [sic] to emphasise Berlioz's choice of a certain word, but the text is a translation, not that of Berlioz. There are occasional misuses of terms, for example that of Russian horn band, which was quite different from the Prussian horn band and the Torviseitsikko.

The initial note on nomenclature gives pitch terms on the American Standard system, and unless this is a joint publication, mainly aimed for the American market rather than the English, one does wonder why. It seems unnatural to have C₄ rather than c' for middle C in a book from a British university press. And since in many chapters of the book, and particularly in the glossary (which on the whole is excellent), the authors thread their way skillfully through the minefields of alto and tenor brass instruments by using terms such as 9-foot B flat, 7-foot E flat, and so on, it seems crazy on this initial table of pitches not also to indicate which C is 8-foot, which is 16-foot and so on. Unfortunately, Clifford Bevan's chapter on low brass is an exception, and just where we need the use of 'foot' the most its absence leaves one wondering which B♭ and which C etc. are meant; this is a chapter where differences between the book (the invaluable The Tuba Family) and the chapter are very apparent.

The first chapter, on lip-vibrated instruments (a horrid phrase - what's wrong with trumpets and horns?) of the ancient and non-western world, is as summary as one would expect, but adequate within the terms of this book. Any of us who are reading it with any advance knowledge will be conscious of omissions, but they will be different omissions for each of us! There is no way that Margaret Sarkissian could have included everyone's favourite instrument, and within the limits of thirteen pages she has done an excellent job. Arnold Myers on how brass instruments work is clear and not excessively technical. Bob Barclay on design technology and manufacture before 1800 is outstanding, as anyone
who knows his Art of the Trumpet-Maker would expect. All his descriptions are concise, crystal clear, and illuminating. In contrast, Keith Polk’s chapter on brass instruments in art music in the Middle Ages is diffuse, repetitive, confusing, and contradicts its own title by discussing their use in non-art music for the first four and half pages out of a total of thirteen and a half. It is clear that he is on the side of the mediaeval slide trumpet, unlike some recent authors, but he has a tendency, in this context and others, to say that there is evidence without telling us what it is. For example, when discussing the beginnings of the use of the upper part of the range, he mentions that there are imitative musical examples which illustrate this, but never tells us what they are. This chapter is certainly one that could have benefited from stronger editing had it not been for the need for haste on which we commented above.

Bruce Dickey on the cornett is excellent, as one would expect, as are Trevor Herbert on sackbut and Edward Tarr on the trumpet before 1800, though this last suffers from the need for compression. For example, while discussing differences between Handel’s and Bach’s trumpet parts he presumably had no space to consider the quite different characteristics of German and English trumpets, which were in great part the cause of such musical differences. Thomas Hiebert on the baroque and classical horn is the one who suffers from the missing text mentioned above, and he refers, with no further explanation to “a chromatic non-transposing mute” – how can a mute be chromatic? Unless, of course, he means chromatic in a colouristic, non-musical sense. Arnold Myers, on design and technology, has nice clear diagrams of valve types, a list of makers with some surprising omissions, and the very odd statement that on many valve tuning slides “the outer slides are soldered directly to the knuckles” – how then do they slide? I presume that it should be “inner” rather than “outer”.

Ralph Dudgen on keyed brass is one that suffers more conspicuously from contraction from a whole (and excellent) book (The Keyed Bugle) as does Clifford Bevan’s which follows it. Bevan says, in passing, that “the further the vibrating air in a tube needs to travel, the wider the bore needs to be in order to overcome the friction of the air against the tube’s walls”, which hardly explains why the 12-foot french horn has a rather different bore from the 12-foot F tuba. Perhaps, come to think of it, that’s why the french horn is so treacherous an instrument. Simon Wills has an interesting chapter on brass in the modern orchestra, but it is one to which several of the remarks above on haste apply. The same applies, curiously because he was one of the editors, to Trevor Herbert’s chapter on brass bands. Also I don’t think he allows sufficiently for the effect of demobilised military bands after Waterloo on the early nineteenth-century increase in amateur bands. Quite suddenly, over much of Europe, there was a considerable number of experienced wind-band musicians, and trumpeters and buglers, available, plus a large stock of army-surplus instruments (and I’d be surprised if none of the musicians managed to liberate their instruments as they went home). He does, of course, stress the importance of the Cyfarthfa Band, on which he has written excellent and important articles, but which, in this context of the amateur and artisan band, is almost irrelevant, as was its repertoire (some of it excellently recorded recently by the other editor, John Wallace) because it was far beyond the capabilities of the average band.

A group of the authors combines to produce the chapter on playing, learning, and teaching. One carelessness there is the citation of Altenburg by the title of Tarr’s translation, rather than the original, as of all other texts cited, and as is in the bibliography. Robert Evans on the post-classical horn conflates cause with effect on the growth of audiences and concert halls; it was the rise of a moneyed bourgeoisie as an effect of the Industrial Revolution that demanded the creation of the concert hall and thus of the larger orchestra and the consequent need for louder instruments. While discussing Britten’s Serenade he refers to the use of natural harmonics in the unac-
companied horn ‘Prologue’ and ‘Epilogue’ and the use of a flattened B♭ (14th harmonic) for the written A (13th harmonic) in recorded performances under the composer’s direction. In fact, the first recording used the A, and later performances used the B♭, I asked Dennis Brain why, and he told me ‘Ben preferred it’ – a piece of vernacular history. And it was not Dennis’s ‘move from a French to a German horn that ultimately tipped the balance in favour of the German-style instruments’ – Dennis was one of the last to change, he waited until his father, the greatest English narrow-bore player (his unsurpassed recording of the Brahms Trio, with Busch and Serkin, is available once again on CD), had died.

Roger Dean’s chapter on jazz suffers badly from the problems outlined at the beginning of this review, strong and sympathetic editing would have helped. John Wallace on solo and chamber music is excellent as is Simon Wills on avant-garde, with many very useful musical examples. As noted initially, the glossary on the whole is excellent, but there are some entries where a more careful reading would have led to slight modification, the most obvious example being that of the waterkey which is ‘used to release water caused by the evaporation [my emphasis] of a player’s breath’ rather than its condensation.

There is a long and extremely useful bibliography, citing many primary sources as well as secondary ones, and including many early tutors.

So, a number of chapters have been described above as ‘excellent’ and I am only sorry that I could not have said so of all of them. The approach is totally different from Anthony Baines’s book, and, with a generation between them, it is an approach that is needed today. Perhaps we may look forward to a revised edition – if so I should be very happy to produce the sort of list of detailed comments that I used to do in FoMRHI reviews, and I am sure that other reviewers would do so also, for this is an important book and one that is in everybody’s interest to help to make even better.

Cremona secret

I gave this title to an untitled Comm. submitted by Gheorghiu. Since he is not a member, I am free to comment on it in the same Q. For a couple of centuries, violin repairers and makers have been searching for physical characteristics that can be associated with the strong preference amongst musicians for Old Master violins. Gheorghiu is one of very many who have been able to convince themselves (and usually a few others) that they have found the answer. As is usual in these cases, his theory makes simplistic claims not supported either by the real physical theory of how the belly and back vibrate, or by any physical measurements.

It is very hard to take him seriously, and there is every reason to dismiss his claim as that of another crank. But there is just a faint possibility that the re-thickening procedure that he does can consistently improve a poor violin. Should we then accept his offer and send him a poor violin for him to transform? I say no. He is not telling us what he does nor how he does it. I feel that we have the right to ask for disclosure before considering taking him seriously. We don’t mind his getting rich improving violins while keeping the details of the method to himself, but we will only help him get famous for his ‘discovery’ if it really is a discovery, which requires that it works when any person competent in doing this sort of work does it.

Paul Baker’s Comm. in this Q

I don’t think that anyone will object to my replying in the same Q when what I have to say is that he is right and I was wrong in Comm. 1542. This is a perfect example of how one’s perceptions can be distorted by an attractive theory that is supposed to explain them. I thought about a sensible way that it could work and then thought that I remembered the cotton spinning around (that was probably a memory of Baker’s step 4) and I neglected to check what really happens in the playing situation. A great thing about publishing in FoMRHIQ is that if you say something stupid, you can be told about it and correct it while you can still remember what it’s all about. Thanks, Paul, for an excellent

VII Spécial is one of the very useful catalogues of private collections which Larigot publishes from time to time, this one of the collection of Alain Coulet. A fairly general woodwind collection, the speciality seems to be clarinets, with 26 of them from Amlingue to Wunderlich. There are also 9 saxophones, three of them by Sax. 3 flutes (one a Thomas Lot), 6 flageolts, 4 oboes, 7 bassoons, a bass sarrusophone, 2 tarogatos, five ophicleides, a serpent and a serpent Forveille, complete the collection. There are photographs of everything, most of them photographed back and front, and the bassoons photographed in separate joints as well as complete. In a supplement there is a xerox of the Evette & Schaeffer 1896 saxophone patent and the relevant pages from their 1907 catalogue.

N° 20 includes: A list of the wind instrument makers’ catalogues in the Bibliotheque Historique de la Ville de Paris, with their call numbers. A discography of recordings on ‘original’ instruments for clarinet and basset horn. An article by Beryl Kenyon de Pascual on a portrait of a girl with an early accordion, dated 1839, adapted from RIdIM Newsletter. A reprint of an article of 1913 on ‘La Musique Militaire à Travers les Âges’ by Colonel Malletterre. An article on ‘Musées et Collections’ by Arnold Myers, which originally appeared in T.U.B.A. with a nice photo of Arnold clutching an ophicleide in front of one of the brass cases in the Edinburgh museum. A ‘Conversation with Rod Cameron’ by Scott Hirsch, from Woodwind Quarterly. A short note by Bruno Kampmann on Goulding & D’Almaine’s Electrum key, with an analysis of the metal. A note from Alain de Gourdon, the present director of Lorée, with facsimiles of a number of interesting documents, catalogue pages, etc from Lorée’s archives. A note on the Musée Communal of La Couture-Boussey. A query on a saxhorn mouthpiece which unscrews at two points. Also on saxophone mutes, with information on one of these. And the results of the auctions in April and May in Vichy. Plus the usual advertisements of things for sale and wanted.

A good average issue, perhaps with a little more reprint than usual, but then if you don’t have access to RIdIM Newsletter, T.U.B.A., and Woodwind Quarterly, no harm in that. Larigot may not have the long scholarly articles of GSJ, JAMIS, Historic Brass, etc, but then they don’t have these useful catalogues of collections, nor the frequent reprints of makers’ catalogues and other useful information which make Larigot so valuable.
Review:

The Violin Explained - Components Mechanism and Sound
by James Beament, O. U. P., 1997

The blurb on the jacket fold is a fair reflection of what it is about:

There are more curious beliefs about the violin than any other instrument. Physicists have now discovered how the violin family produce their sound, but this knowledge is not accessible to players and makers. In this book Sir James Beament, a distinguished scientist with a lifelong involvement with musical instruments, explains, without assuming any scientific background, how strings vibrate, and the role played by the bow, bridge, and body in producing sound. These discoveries do not give a satisfactory picture of what we hear, and the core of the book explains how the sound relates to the sensations it produces. This leads to the conclusion that most of the myths about violins are groundless, but also that nothing can be achieved by applying modern technology to making or playing. Practical advice on strings, maintenance, purchase, and children's instruments completes the discussion.

There are other topics covered, including chapters on 'How Glue Works' and 'Varnish and Varnishing'. It is refreshing to find enough space given to the viola, 'cello and double bass to show their differences from the violin.

The explanations are superb. Beament is clearly an exceptionally fine teacher. He goes deeply into each topic, beyond the simplifications of the real situation than most elementary texts on acoustics offer. The stated assumption that no scientific background is required to understand it may be rather optimistic. Though mathematics is avoided, graphs and chemical diagrams of molecules are used, and above all, the reader needs to be able to think scientifically to follow his reasoning. Scientific understanding will not enhance musical understanding, and it is not necessary for pursuing the craft of instrument making (but as with any other aspect of our culture, it can inspire creativity in these areas). The only reason why a reader will devote the mental effort to learn the science of bowed instruments offered here is curiosity about how it all works, and that can only happen if he or she has the mental capacity and background to grasp the concepts. This makes the book useful to only a fraction of the musicians and instrument makers it is intended for. But for that fraction, this book is very strongly recommended indeed. It covers more of the relevant science, and explains it better, than any other I’ve ever read.

Besides explanations, Beament includes some historical background, opinions, advice, and many light-hearted comments. The historical background adds interest, but is not always accurate. For instance, Beament states that the earliest demonstration of a precise relationship between musical pitch and frequency of vibration was in the 19th century by Savart. This was done earlier by Mersenne, Bernoulli, Taylor and Euler.

Beament’s opinions include a healthy skepticism about myths in the violin world. This book has already attracted considerable notoriety from his statement that there is no demonstrable difference in sound between good modern instruments and good instruments made by the Italian Old Masters. In tests, this is statistically true. What has not been studied is finding and testing the few that can tell the difference, either because they have exceptional hearing, or because they know just what to listen for. I know a physicist-violinist who claims that he can almost always tell the difference from a quick pluck of the strings. This can make sense in the light of Beament’s comment that aural memory of transient sounds is much greater than for sustained sounds.

Rather more questionable is Beament’s opinion that when violinists report general differences in playing characteristics between good old and good new violins, it is largely a matter of suggestion. Playing characteristics involve feel, a sense that has very different limitations than
Beament is also skeptical about the need for playing in, and offers a rather weak model involving glue migration to explain it if it were true. I expect that the second edition will include my hypothesis of sound absorption associated with creep during settling in, hopefully supported (by then) by scientific measurements.

By being skeptical about the value of Chladni patterns in plate tuning, Beament can well be alienating the many violin makers who find the method very useful. These are mostly the makers most interested in scientific aspects of the violin. They are the ones who will most benefit from reading this book, and I hope that this slight by a scientist to their 'scientific' method will not dissuade them from reading it.

There are other tuning procedures that modern 'scientific' violin makers use that Beament does not mention. That includes tuning of the tailpiece and tuning of the flapping frequency of the pegbox-neck&fingerboard-neckblock arm to the main air resonance of the body. The former tuning appears to even out string responses and the latter tuning delights the player because it makes the instrument feel much more 'alive'. This usually leads to a report that it 'sounds better' though there may well be no difference in the vibrations processed via the ear.

In his opinions, Beament comes across as a very conservative traditionalist. To him, the instruments of the violin family, how they are played and how they are made have reached a level of perfection that is very unlikely to be 'improved' on. This was the prevalent opinion in the 17th, 18th and 19th centuries as well, and was as true then as now. In all of these centuries, the overwhelming majority of attempts at 'improvement' failed. But in spite of this, the violin in each century has been different from the previous one. Music styles and making styles change. The goal posts do move. I wish that Beament was not so discouraging to the young (in mind), intelligent, imaginative makers who want to explore beyond just what their craft teachers taught them, and want scientific understanding to help choose exploration paths.

This is a marvellous book for the breadth and depth of understanding of the science of bowed instruments it offers. If that is what you want, this is the book to get.
More on draw plate. Response to Peter Forester’s Comm. 1469

One of the figures in ‘L'Art du Facteur d'Orgues’ of Dom Bedos (1770) shows a device for thickening of wooden plates used in organ pipes.

Essentially the depicted tool represents a huge plane attached firmly to a bench. We can assume that it has an untoothed blade, set at around 45°. Notice the position of the plane which provides both room for working and stability for vibration-free cutting.

In principle Dom Bedos’ plane has much in common with that proposed by Mr. Forester. The conclusion made by him concerning the size of the filiere (fig. 49) is even more correct bearing in mind the size of Dom Bedos’ plane, as well as Mr. Forester’s ingenious supposition that the filiere might have been used by two workers.

I have used neither the plane of Dom Bedos nor that of Mr. Forester, but I have made a draw plate shown in the Encyclopedie in the fig. 38 and I use it for making purfling strips and violin-viola ribs.

The original might have been a sturdy tool, but the one I use is not suitable for cello ribs for it is too small. To make that draw plate is easy. The block of hardwood must be reduced to the shape close to that shown in the figure, but left robust in respect to all its parts. If not, vibration occurs if either the bladecarrying part or the base of the workpiece are too feeble and spring back and forth when pulling a piece of the wood. The same happen when the blade is not held firmly by the wedge, when it is too thin, or set at a wrong angle. In my draw plate the untoothed (!) blade is set at approximately 85°. The thicker the blade the better. It helps considerably to reduce vibration and avoid chatter marks on the work. Lastly, strips for purfling or veneers for ribs must be pre-planed and the tool clamped immovably in a bench.

Experimentation with different features of the draw plate results in a tool of a superior quality.
This text was presented to the session of RiDIM 1996. After having read Ephraim's Comm. 1509 I sent him the above to get his opinion. From this arose an interesting discussion via E-mail and I came to the conclusion that it would be of interest to the readers of the Quarterly.

Experiences gained in the search for musical iconographical sources in Trentino collected by the Cultural Service of the Province of Trent (Servizio dei beni culturali of Provincia di Trento).

As I was asked to search for musical iconographical sources in Trentino (collected by the Cultural Service of the Province of Trent) the work I did confirmed that even in peripheral regions the iconographical heritage can be enriched with interesting items for scholars of musical iconography. The search was easier than I had anticipated since the Cultural Service of the Province of Trent had already collected at that time more than 60,000 photographs of art objects, including almost 600 images of organological interest. Obviously, in the search for musical iconographical sources in Trentino, there were a lot of cases, in which I couldn't rationally choose whether a picture represent any instrument and which. F.i. is the psalter 'a testa di porco' (fig. 1) a dodecachordum-psalter, or the simplified reproduction of a psalter with more choirs than 12? Is the viella-like instrument in its rectangular shape (fig.2) a rare kind of viella or a simplified reproduction of a viella similar to the existing instrument of S.Caterina de' Vigri (TIELLA 1975, pi.XIII)?

Borderlines shift and blur, new categories are always coming into prominence, and the canons of classification are less clear than the practice. But is only to say that we may have some trouble in telling whether certain pictures (in common parlance) "represent a musical instrument", or in setting forth rules for deciding in every case whether a picture is a musical-instrument-picture (from GOODMAN)

In fact the work confirmed the author's opinion that musical iconographical sources have a high symbolic content but little technological interest. The lines of thought that joined my interest in the musical iconography with my inquiries into the problems of perception began to emerge just when I started studying the musical iconography as source of technological informations many years ago. My previous researches gave me the conviction that more profound study should be made of the processes of perception by which musical instruments in the iconographical sources are recognized by induction.

In the N.Goodman's book, "Languages of art", one can find a useful definition for a better understanding of the process of perception. N.Goodman, clearly explains why we recognize, in a picture, the contour of an object which we expect to have been depicted in it.

Therefore we are compelled in our habits to suppose which is the painters standard system. Realism is relative, determined by the system of representation standard for a given culture or person at a given time. Newer or older or alien systems are accounted artificial or unskilled. The very effectiveness that may attend judicious departure from a traditional system of representation sometimes inclined us at least temporarily to install the newer mode as standard. We then speak of an artist's having achieved a new degree of realism. Nevertheless, whether an object is "really fixed" or a picture is realistic depends at any time entirely upon what frame or mode is then standard. Realism is a matter not of any constant or absolute relationship between the system of representation employed in the picture and the standard system. The copy theory of representation is stopped at the start by inability to specify what has been to be copied (from GOODMAN).

There are some conditions as how to locate new images in an extant catalogue. Firstly, the object must be one of those we already know - it must pertain to a category of other similar images (defined by Goodman as "dense system"). Secondly, on this basis we are inclined to mentally fulfill the parts of the instrument lacking in the picture, making the borders of the sketch more and more graphically definite. Finally, if it is impossible for us to make the instrument image coincide with our precognition of it, we remain in doubt.
where the picture represents a variant of an already known instrument or a new instrument shape, not catalogued yet. Consequently few - or many - sub-types indefinitely enlarge the list of classified instrument kinds.

The way pictures and descriptions are thus classified into kinds, like most habitual ways of classifying, is far from sharp or stable, and resist codification. Exact and general conditions under which something is a soanso-picture or a soandso-description would be indeed hard to formulate. For instance Bellini’s luth is a luth-picture, and in English a luth is a luth description (from GOODMAN).

Thus, classification is how to choose predicates and how to ascertain whether they can be projected on art objects.

More accurate formulation takes some care. What a picture is said to represent may be denoted by the picture as a whole or by part of it. A “triangle” through occurrence in “triangle and drums” may denote given musical instruments and be a musical-instrument-description. If representation is a matter of choice and correctness a matter of information, realism is a matter of habit.

Pictures may function as representations within systems very different from the one we happen to consider normal ... As we saw earlier, among representational systems, "naturalism" is a matter of habit but habituation does not carry us across the boundary between description and representation. If seeing what properties a picture exemplifies or expresses is like applying an ungraduated meter, saying what the picture exemplifies is a matter of firing the right words from a syntactically unlimited and semantically dense language. Saying what a picture exemplifies is like measuring with no set tolerances.

The pictorial systems of exemplification are not nearly so standardized as most of our practical systems of sampling or gauging or measuring (from GOODMAN).

Nevertheless, cataloguing the musical iconographical sources is an important means for a better knowledge of the music history.

And if the point of the picture is not only successfully made but is is also well-taken, if the realignments it directly and indirectly effects are interesting and important, the picture - like a crucial experiment - makes a genuine contribution to the knowledge. The making off of new elements or classes, or of familiar ones by labels of new kinds or by new combinations of old labels, may provide new insight. But if the picture is recognized almost but not quite referring to the commonplace furniture of the everyday world, or if it calls for and yet resists assignment to a usual kind of picture, it may bring our neglected likenesses and differences, force unaccustomed associations, and in some measure remake our world. In sum effective representation and description require invention. They are creative; they inform each other; and they form, relate, distinguish objects. That nature imitate art is too timid a dictum. Nature is a product of art and discourse. Yet we do compare representation with respect to their realism or naturalism or fidelity. If resemblance is not the criterion, what is? The proposed measure of realism is the probability of confusing the representation with the represented? I recognize the images as signs for the objects and characteristics represented. Thoughts along these lines have led to the suggestion that the most realistic picture is the one that provides the greatest amount of pertinent information. But this hypothesis can be quickly and completely refused. Obviously, realistic and non realistic pictures may be equally informative; Informational yield is no test of realism. Just here, I think, lies the touchstone of realism: not in quantity of information but in how easily it issues.

And this depend upon how stereotyped the mode of representation is, upon how commonplace the labels and their use have become. Realism is relative, determined by the system of representation standard for a given culture or person at a given time. Newer or older or alien systems are accounted artificial or unskilled. Realism is a matter not of any constant or absolute relationship between the picture and its object but of a relationship between the system of representation employed in the picture and the standard system. Realistic representation, in brief, depends not upon imitation or illusion or information but upon inculcation. Almost any picture may represent almost anything; that is, given picture
and object there is usually a system of representation, a plan of correlation, under which the picture represent the object. How literal or realistic the picture is depends upon how standard the system is (from Goodman).

A catalogue of iconographical sources is a catalogue, images of art objects, to which some of us can—or want—to refer old musical instrument representations by induction.

What are the characteristics of the induction performed by human beings? Obviously we can take account of the evidence in subtle and sophisticated ways.... And on the whole we tend to persist in a choice so long as the evidence permits. But are we completely provided with a completely decisive preferential ordering among these hypothesis, or must we sometimes resort to chance? ... More to the immediate point of our present inquiry, though, is the disclosure of certain special features of the functioning of symbols not only in overt induction but also in such kindred processes of as category detection and pattern perception. Perhaps, indeed, these are earmarks of cognitive behaviour in general (from Goodman).

The surest way to put yourself in a position nearer to that of the painter is also to put yourself within the painter's standard system. Thus, I think that the only realistic pictures of musical instruments are a very few: those that painters drew according to the rules of the Western renaissance perspective (as far as I know, Ms Latin 7295 of BN Paris, Ms Ambrosiana reproduced by Rösci M. 1971). In fact none of the Trentino pictures can be qualified as realistic because the pictures are not correct under that system and also the information about the object that is obtained by reading the picture without knowing the system from which the painter worked according to.

This all adds up to open heresy. Descriptions are distinguished from depictions not through being more arbitrary but through belonging to "articulate" rather than to "dense" schemes; nothing here depends upon the internal structure of a symbol; for what describes in some systems may depict in others. Resemblance disappears as a criterion of representation, and structural similarity as a requirement upon notational or any other language (from Goodman).

Doubts in finding more exact placements of any new picture into extant cataloguing schemes do not matter: any picture is an unique, individual source of knowledges, despite of its apparent realistic features similar to other instrument types already classified.

Goodman's theory of symbols made me convinced of the impossibility to gain feasible knowledges in the field of musical instrument technology by means of induction. Pictures of musical instruments represent only themselves and not any object that we are induced to perceive as resembling them.

In the ancient texts issued for iconological purpose, usual musical instruments are indicated as graphic symbols, useful to enhance the significance of some moral qualities to be represented in the pictures—such as fortress, patience, endurance and so on—or concepts—such as grace, beauty and so on—("Violini debentur Christo & Angeli, Violetta Misericordiae, Clementiae, Amori Philotheae Jugenti, Cornetti conveniunt Mundo, & Philotea letenti. Tromboni Christo Judici & Justitiae. Theorinbi Liutti &c. pro simphonia hilaris in Actu primo post deceptam Philotheam, & in actu tertio in Choro Filiarum Jerusalem", Paulin, Philotea, Instructio pro Chorago, [7] [16..] (s.Tiella 1995, p.395).

Quoted bibliography:
Rocci M., Baschenis, Bettera s Co., Görlich, Milano 1971.
Goodman N., Languages of art, Hackett, N.Y., 51985, passim.
The inventory of the musical instruments of Michel Charles Le Cene (1743)

Le Cene was music publisher in Amsterdam, where he lived at the Boommarkt. After his death, an inventory was made of his books, music scores and musical instruments. The inventory and valuation of the instruments was made by Gerard Fredrik Witvogel and Pietro Locatelli, the Italian composer who lived in Amsterdam.

This is not to place to write about the importance of Le Cene for the music life in the Netherlands (and Europe); I will concentrate on the woodwind instruments.

All information can be found in the archives of the city of Amsterdam (Gemeentearchief), file number 5075, notarial archive No. 10226, act No. 539.

The prices are given in gulden (Dutch guilders) and stuivers (20 stuivers in one gulden).

No. 1 Twee zwarte 'fluyt dous' met ivoor van Bressan.
   Two black recorders, with ivory, by Bressan (6-0).

No. 2 Een bruine 'dito' van Bressan.
   A brown dito by Bressan (1-0).

No. 3 Een zwarte 'sang fluyt' met ivoor van Bressan.
   A black voice flute with ivory by Bressan (4-0).

No. 4 Een bruine altfluit van Bressan.
   A brown alto recorder by Bressan (2-0).

No. 5 Een bruine kwartfluit van Bressan.
   A brown (soprano) fourth flute by Bressan.

No. 6 Een zwarte ebbenhouten octaaffluit van Terton, met ivoor.
   A black sconcino recorder in ebony, with ivory, by Terton.

No. 7 Twee kwartfluiten met ivoor van Van Heerde.
   Two (soprano) fourth flutes with ivory by Van Heerde.
   The Nos. 5, 6 and 7 were valuated together for only 2-0.

No. 8 Twee octaaffluiten met ivoor van Van Heerde en 'rotting fluit'.
   Two sopranino recorders by Van Heerde and a walking stick recorder.

No. 9 Een bruine basfluit van Bressan.
   A brown bass recorder by Bressan.
   The Nos. 8 and 9 were valuated together for 3-0.

About the stringed instruments:

No. 11 A violine from Cremona by Amatis 1642, in a box (42-0).
No. 13 A violine by Hendrik Jacobs (26-0, together with a german violine and two violine cases).
No. 17 A viola da gamba by Henry Jaye 1632 (30-0).
No. 18 A viola da gamba by Joagim Tielke 1699 (20-0).
No. 19 A 'dito' with ivory by the same maker 1706 (20-0).
No. 20 A violoncello with 6 strings by Edeward Lewis, London 1704 (30-0).
No. 21 A big contrabas by Franck Boorelon, Antwerpen (Anviers) (20-0).
No. 22 A harpischord, with staartstuk, with 3 registers, by Heronimus Albrech Hanz (50-0).
All woodwind instruments in the list were recorders, and that is remarkable. Why were these twelve instruments in the inventory? Perhaps because the recorder was not in use anymore, and other instruments (oboes, traverso's) of Le cene were in use by other people? What was the position of the recorder in the year 1743 in Amsterdam?
I am not sure about the sizes of the recorders. The instruments of No. 1, *Fluyt dous* (from French Flûte douce) can be any size of recorder, but probably they were alto recorders in $f^\prime$. But then I do not understand No. 4, the *altfluit* by Bressan. The term 'altfluit' is not common in inventories or other Dutch literature. Maybe the instrument was what we call now as a tenor recorder in $c^\prime$. No. 3, the *sang fluyt* is a perfect translation of the English voice flute. The *kwartfluit* (Nos. 4 and 7) could have been a (soprano) fourth flute (a recorder in $b^\flat$), but no Dutch fourth flutes did survive, and I do not know about fourth flutes by Bressan. The *octaaffluit* (No. 6 and 8) was a sopranino recorder in $f^\sharp$, the *rottend fluyt* (No. 8) a walking stick recorder. Finally, the *basfluit* (No. 9) was a bass recorder.

About the prices: the black recorders (made in ebony) with ivory mounts (Nos. 1 and 3) had a higher value than the plain brown (stained boxwood?) and shorter instruments. It surprises me that the Nos. 5, 6 and 7 and also 8 and 9 were valued together. The prices were not very different from the prices of similar woodwind instruments in the Van Bolhuis auction, about twenty years later (see FoMRHI Comm. 1538).
Repairs to antique brass instruments

Traditional repairs to antique brass instruments are often the cause of further damage and corrosion over time. I have been looking for an alternative to brass patches for sealing leaks and cracks which may provide a possible intermediate option in between a full restoration and conservation as a non-playing instrument.

A shaped metal patch is quite rigid and I have seen many instruments that have split along the edge where the original metal is weaker than the patch. When the old patch is removed there is sometimes a sharp step where the metal has been thinned with abrasive and polished to remove excess soft solder, and very often on larger patches there is an air pocket in the centre where the fit is not exact, where any remaining flux or water has continued to corrode the brass.

A repair to an instrument that is not played every day need not be quite so strong as it would otherwise be. Nail varnish has been used as a cure for small leaks, and even plastic tape can be surprisingly permanent. I have had some fairly encouraging results with thin fibreglass patches which, if cut carefully, can be made no more obtrusive than metal ones, and can be applied with much less stress to the instrument. They are easy to remove with a sharp knife and with care they may be fitted inside an instrument where they cannot be seen, for example inside the bell throat of a french horn.

When I have suggested fibreglass repairs to other people, however, the reaction is usually that they would prefer a proper traditional repair in a metal that matches the instrument. Does anyone have any suggestions or experience in using other modern materials which may be suitable?
FoMRHI Comm. 1554

Maggie Lyndon-Jones

Renaissance woodwind in the Museo degli strumenti musicali, Rome

In March 1996 Nicholas Perry and I visited the Museo degli strumenti musicali, Rome, and were very privileged to be given permission by the curator Antonio Latanza to examine and photograph the instruments marked !! in Room 11. It was also possible to make a few basic measurements but we only had one morning to examine the instruments. A complete catalogue of the collection compiled by the previous curator Luisa Cervelli is available from the museum 'for cultural exchange only'. The museum technician Pietro Patacchiola was very helpful and patient.

The instruments seem to have been renumbered recently since the metal tag numbers attached to them by plastic thread do not correspond with numbers given by Tarr or Puglisi. Since returning, I have had access to the museum catalogue and have to report that there could be a problem with this method of labelling instruments. The overall lengths I measured for the mute cornetts were identical to those given in the catalogue, but not in the same order! Also the C.RAFI and M.RAFI flutes seem to have had their labels switched, and also tenor cornetts 0699 and 0700. For the sake of consistency I will give them the numbers they have in the catalogue rather than the numbers they actually had at the time of my visit.

With the exception of the recorder made by *.*, all the instruments listed in this Comm. were purchased by the museum 25 years ago from the Collection Marcello-Giusti of Giardino near Padua, originally collected by Benedetto Marcello (1686-1739).

Apart from tenor cornett 0699 all the !! cornetts and recorders seem to have been marked with the same tool, regardless of the arrangement or number of the marks.

Case 1

A SET OF CRUMHORNS BY IEORG WEIER

0690 Keyless soprano.
Marked  at top of body and  on bell.
Cap is missing.

0689 Keyless soprano.
Marked  at top of body and  on bell.

0688 Keyless alto.
Marked  at top of body and  on bell.

0687 One-keyed tenor.
Marked  at top of body and  on bell. Cap is missing. Woodworm. Keycover is engraved with the date 1524.

0686 Double-keyed extended tenor with sliders.
Marked  at top of body and  on bell. Woodworm. Keycover is engraved with the date 1524.
0685 One-keyed bass.
Marked ♫ at top of body and ♫ on bell. Cap is missing. Woodworm. Keycover is engraved with the date 1524.

0684 Double-keyed extended bass with sliders.
Marked ♫ at top of body and ♫ ♫ ♫ ♫ ♫ on bell. Woodworm. Keycover is engraved IOERG WEIER.

MUTE CORNETTS

0691 Marked HIE. S below bottom fingerhole. 642mm length overall.
Boxwood. Slight split in end.

0692 Marked HIE. S below bottom fingerhole. 640mm length overall.
Boxwood. No visible damage.

0693 Marked HIE. S below bottom fingerhole. 643mm length overall.
Boxwood. Mouthpiece off-centre. No visible damage.

0694 Marked HIE. S below bottom fingerhole. 639mm length overall.
Figured maple. No visible damage. [This instrument was labelled 0695 in March 96]

0695 Marked HIE. S below bottom fingerhole. 642mm length overall.
Boxwood. No visible damage. [This instrument was labelled 0694 in March 96]

CURVED CORNETTS

0696 Marked !! below bottom fingerhole, no mark on end. Left-handed. Round cross-section. 565mm length overall. Parchment covered. Faint tree-marks between fingerholes 3 & 4 same design as 0700.

0697 Marked !! below bottom fingerhole, no mark on end. Left-handed, round cross-section. 567mm length overall. Parchment covered. Faint tree-marks between fingerholes 3 & 4 same design as 0700.

0698 Marked !! below bottom fingerhole, no mark on end. Right-handed, round cross-section. 564mm length overall. Parchment covered. Very worn - decorations too faint to see.

TENOR CORNETTS

0699 Marked !! !! below bottom fingerhole, no mark on end.
!!
!! marks and tree marks are different to all the other marks in this collection. Round cross-section. 880mm length overall. Parchment covered. Slight woodworm (not recent). [This instrument was labelled 0700 in March 96]

0700 Marked !! !! below bottom fingerhole, no mark on end.
!!
Round cross-section. 887mm length overall. Parchment covered. Tree design is same as on 0701. Same profile as 0701. [This instrument was labelled 0699 in March 96]
0701 Marked !! !! below bottom fingerhole, no mark on end. !!
Round cross-section. 888mm length overall. Parchment covered. Damaged by woodworm around top binding. Tree design is same as on 0700. Same profile as 0700.

In Case 2 - 0706 an incomplete sordune

Case 5

FLUTES

0712 Bass. Marked M.RAFI/griffin in shield between mouthhole and end of instrument. 967mm length overall. Pear? Cracks in foot neatly repaired. [This instrument was labelled 0713 in March 96]

0713 Tenor. Marked C.'RAFI/shield between mouthhole and end of instrument, also shield between fingerholes 3 & 4. 678mm length overall, boxwood. Crack in foot neatly repaired. [This instrument was labelled 0712 in March 96]

0714 Tenor. No mark. 669mm length overall. Leather-covered boxwood? Small splits.

0715 Tenor. Faint A mark off-centre below mouthhole - probably A A originally. 660mm length overall. Leather-covered pear? Woodworm.

RECORDERS


0717 Tenor. Marked !! !! below windway and !! on foot. !! !
618mm length overall. Slight crack at foot. Plug made of coppiced wood. Part of consort with 0720, 0719 & 0718

0720 Basset. Marked !! !! below windway and !! on foot. !! !
944mm length overall. One key. Same knotty wood. Foot chipped, labium has brass edge. Brass band around bottom of wind cap held in place by 3 rivets - cap has split above rivets. Keywork original. Plug made of coppiced wood. Part of consort with 0717, 0719 & 0718

0719 Extended basset. Marked !! below windway & !! on foot. !! !
1118mm length overall including windcap. One key at front - touch is missing - and 2 at back. Original keywork. Keys are very narrow and overlap ingeniously. Foot chipped badly & also cracked. Barrel is badly split all over and very fragile. Plug made of coppiced wood. Part of consort with 0717, 0720 & 0718
0718 Bass. Marked !! below windway and !! !! on foot.

1378mm length overall including windcap, 1334mm without windcap (displayed separately). Oval hole in top of windcap and groove indicate that it was originally played with a crook which is now missing. Brass band missing from windcap. Barrel is much smaller than on 0719, slightly cracked. The foot is not damaged. Plug made of coppiced wood. Part of consort with 0717, 0720 & 0719

The recorders

All the recorders are made of orange-coloured wood with several small knots. They are not stained since the same colour was found under the fontanelles and where any damage had occurred. Pietro Patacchiola is of the opinion that the recorders are made of corniale. I believe this is the species Cornus mas or cornelian cherry, a member of the dogwood family. It is native to central and southern Italy and other Mediterranean countries, has hard durable orange wood which was traditionally used in ornamental turnery and for making daggers, hence the name 'dagwood'. It can grow to the height of about 10 metres with many small branches, and the wood therefore has many small knots. Dr Stradner of the Kunsthistorisches Museum tells me that in Austria it is known as Cornell or Dirndl, it is a traditional wood used to make fifes and the fruit can be made into Schnapps.

The cornetts

The curved and tenor cornetts are unusual in that they all have a round external cross-section. It is likely these are some of the oldest surviving cornetts, the faceted design being a later innovation. Tenor cornett 0699 has !! and tree marks identical to those found only on certain faceted cornetts in other European collections, so its rounded design may have been a one off commissioned to make up a set with tenors 0700 and 0701. There are two more cornetts in Room 13 which are of inferior craftsmanship.

The flutes

0714 & 0715 are the only known extant leather-covered flutes. It may have been applied (a) at the time of manufacture (b) by the original maker of the flute to disguise a repair, or (c) by a later repairer - not an option for instrument 0715 since he has marked the leather itself and not the wood: he wouldn't put his stamp on an instrument not made by him. There is a painting which includes a leather-covered flute in the Kunsthistorisches Museum, Vienna, by Paul de Vos (c1640) entitled 'Amor als Sieger' (KHM/GG 3554). K A is the stamp of Sigmund or Arazius Schnitzer (see Langwill). There are 3 other flutes with this mark in the Biblioteca Capitolare, Verona, and possibly also the bass flute in Vienna stamped with the date 1501 (GdM 88).

The crumhorns

The set of crumhorns is very interesting in that it contains the only known example of an instrument stamped ??? . This fact was not made available to Boydell who suggested that instruments stamped ? or ?? were made by two different makers, namely Joerg Weier senior and junior. However on the basis of this new evidence I suggest instead that there was only one maker, and that he used his stamp once on smaller instruments, once or twice on middle sized instruments and twice or three times on large instruments. He probably wouldn't have been able to refer to previous sets he had made since they would have
already gone out to other customers, so need not necessarily have been consistent. He certainly wasn’t consistent with the spelling of his name on the keycovers: on Rome 0684 it is WEIER, but on Vienna 679 it is WIER. Also, these crumhorns seem to be a set. It is extremely unlikely that the smaller sizes were made by one maker and the larger by his son. The fact that the largest instrument is stamped \(^\text{ff}\) and \(^\text{fff}\) makes this all the more unlikely. This theory about the number of marks applied to an instrument could possibly be true for other instruments.

Revised table of WEIER crumhorns based on p.167 of Boydell

<table>
<thead>
<tr>
<th>Size of crumhorn</th>
<th>(^\text{f}) mark</th>
<th>(^\text{ff}) mark</th>
<th>(^\text{fff}) mark</th>
</tr>
</thead>
<tbody>
<tr>
<td>Soprano</td>
<td>Rome 0690</td>
<td>Rome 0689</td>
<td></td>
</tr>
<tr>
<td>Alto</td>
<td>Merano 6848</td>
<td>Rome 0688</td>
<td></td>
</tr>
<tr>
<td>Tenor</td>
<td>Augsburg 3009</td>
<td>Augsburg 3010</td>
<td>Rome 0687</td>
</tr>
<tr>
<td></td>
<td>Brussels 2311</td>
<td>Augsburg 3011</td>
<td>Rome 0687</td>
</tr>
<tr>
<td></td>
<td>[Leipzig 1427, lost]</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Merano 6847</td>
<td>Rome 0687</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Vienna SAM 206</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Extended tenor</td>
<td>Rome 0686</td>
<td>Vienna 679</td>
<td></td>
</tr>
<tr>
<td>Bass</td>
<td>Brussels 2312</td>
<td>Merano 6848</td>
<td>Rome 0685</td>
</tr>
<tr>
<td></td>
<td>Rome 0685</td>
<td>Vienna 678</td>
<td></td>
</tr>
<tr>
<td>Extended bass</td>
<td>Augsburg 3008</td>
<td>Brussels 2313</td>
<td>Leipzig 1429</td>
</tr>
<tr>
<td></td>
<td>Merano 6849</td>
<td></td>
<td>Rome 0684</td>
</tr>
<tr>
<td></td>
<td>Rome 0684</td>
<td>= Rome 0684</td>
<td></td>
</tr>
<tr>
<td>Extended great bass</td>
<td>Prague 489E</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

This instrument is marked \(^\text{ff}\) on the body and \(^\text{fff}\) on the bell. All the other instruments in this table are marked identically on the body and on the bell.

See also

- **BOYDELL, B.** The Crumhorn and other Renaissance Windcap Instruments (Buren 1982)
- **CERVELLI, L.** La Galleria Armonica. Catalogo del Museo degli strumenti musicali di Roma (Rome 1994)
- **WATERHOUSE, W.** The New Langwill Index (London 1993)
Alternative Fingerings for long-foot Baroque Recorders

This article is not the result of an extensive scientific research. It deals with some observations I have made about recorder fingerings, and I hope that these observations will encourage other woodwind makers to do some research and to write about their results. I am interested in your experiences!

In Hotteterre-le Romain’s *Principes de la Flûte* (first edition 1707 in Paris) the same fingerings are given for the third register ($e^3$, $e^4$, $f^4$) of the alto recorder as we are used to play today on nearly all types of Baroque recorders. The $e^3$ must be played according to Hotteterre with Oh 1 2 . 4 5 (Oh = the thumbhole is half or partly covered), which is in fact an octave and a fifth above the a' (0 1 2 3 4 5). For the $e^3$ (Oh 1 2 . 4 5 6 7) the holes 6 and 7 must be covered as well, but for the $f^4$ hole 2 must be opened (Oh 1 . . 4 5).

We find the same fingerings (for $b^3$ and $c^4$ on the soprano recorder in c), not in a table but described by Paulus Matthijsz in the preface of *Der Fluyten Lusthof* by Jacob van Eyck (Amsterdam, 1644-1655). Gerbrant van Blankenburgh (Amsterdam, 1654) gives in his tutor for the *Handt-Fluyt* (= soprano recorder) for the $c^4$ also the fingering Oh 1 . . 4 5, but for the $b^3$ he uses Oh 1, simply overblowing the $b^2$ to the second register. This fingering for the $b^3$ is not new, we can find it in Agricola (*Musica Instrumentalis Deudsch*, Wittenberg 1528 and 1545) and in Canassi (Sylvestro Ganassi del Fontego: *Fontegara, la quale insenga di sonare di Flauto*, printed in Venezia 1535). However, Ganassi covers also the sixt hole for this note (Oh 1 . . . 6).

So far about the historical fingering tables; now about the instruments.

On some historical Dutch Baroque recorders, the notes of the third register ($e^3$, $e^4$ and $f^4$ on the alto instruments) were easy to play with Hotteterre-fingerings, but sounded much too flat. Well, I discovered that the main reason for this problem was that these recorders had long foot joints (107 mm or more on the alto recorder). On instruments with short feet (up to 105 mm, depending of the total sounding length of the instrument) the Hotteterre-fingerings could be used well.
I wondered why the woodwind makers made these long foot recorders. Some people were very convincing in telling me that the third register on these instruments was not used. In fact, there is a lot of woodwind music going up to d\textsuperscript{3} (only using the first and second register of an alto recorder), and sometimes not going below f\textsuperscript{1}. Actually, such music could be played not only on alto recorders in f\textsuperscript{1}, but also on traverso's in d\textsuperscript{1} and oboes in c\textsuperscript{1}, so this music could be sold easier. But what this was the real reason that some woodwind makers made recorders with different proportions (with long feet)?

I got a first indication that there was more going on in recorder fingerings during my investigations of the one key-bass recorders by Boekhout. Bass recorders were (and are) tricky instruments to make; one of the problems is that the 3th and/or the 4th fingerhole must be placed far from the acoustically ideal position, simply because our hands and fingers are so short. As a result it is not possible to overblow both the c\textsuperscript{1}-c\textsuperscript{2} and d\textsuperscript{1}-d\textsuperscript{2}-octave with common fingerings (0 1 2 3 to Oh 1 2 3 and 0 1 2 to Oh 1 2), even if the holes are drilled under a very oblique angle. The c\textsuperscript{2} will be too sharp, or the d\textsuperscript{2} too flat. One of the solutions is to find alternative fingerings and that is what Boekhout did. In fact he developed different systems, for instance covering the 6th hole for c\textsuperscript{2} (making the note flatter), or (the other system) the 7th hole for d\textsuperscript{1} (making this tone sharper). Finally Boekhout developed a bass recorder with a key on a large and low positioned hole 3, which instrument could be played as a 'common (= shorter) recorder'. I have written an article about the Boekhout-bass recorders for Tibia (a periodical of Moeck-Verlag in Celle, Germany), but on the moment (January '98) it is not yet published.

I got a second indication that there was more in the world of Baroque woodwinds than the fingering tables of Hotteterre when we we playing the instruments of Van Aardenberg, for the catalogues of the Haags Gemeentemuseum. Van Aardenberg, who learned the job of woodwind making in Haka's workshop, made instruments (mainly oboes and recorders, one short traverso in a\textsuperscript{1} survived; probably he made bassoons as well) in a very personal style, carefully finished in all details, but difficult to play with some notes rather much out of tune. Piet Dhont, playing one of the oboes in The Hague, discovered that on the Van Aardenberg-instrument the d\#-key had to be touched down (thus uncovering the d\#-hole!) for several notes, not only for the f\textsuperscript{1} and f\textsuperscript{2} (what in fact must be done on many other Baroque oboes) but even for the e\textsuperscript{1} and e\textsuperscript{2}, what is very unusual and remarkable, and for some other notes. As a result the player had to use (or could use) the small finger of the lower hand for many notes. With this information, I went back to the recorders and tried to use the little finger of the lower hand on some fingerings (thus closing the 7th hole on the foot joint).

The results:
- b\textsuperscript{1} (on soprano's) was possible with an easy and clear sound on long foot recorders with: 0h 1 . . . . 7, 0h 1 . . . . 6, or with 0h 1 . . . . 6 7, or even with 0h 1 . . . . 5 6 7.
- Hole 6 and/or hole 7 had always to be covered, otherwise b\textsuperscript{1} didn't speak. Interesting observation on the original Beukers-soprano recorder and my copy of a Van Aardenberg-soprano: all overblown notes could also be played with hole 0 completely covered!
- The pitch of this b\textsuperscript{1} was much more better than with Hotteterre-fingerings (0h 1 2 . 4 5).
- The alternative fingerings of b\textsuperscript{1} did not work on short foot recorders.
- The c\textsuperscript{4} was more problematical than the b\textsuperscript{3}; on one instrument (the W.Beukers-soprano recorder) this tone could be forced with the fingering 0h . . . . . 7, but the result was
too sharp; with a new head joint on the same instrument the c\textsuperscript{4} must be forced a bit more. With half-covering hole 1 the c\textsuperscript{4} came much easier, better in tune, but it needed some practice to play with this fingering.

Some measurements on the soprano recorder by W. Beukers (tuner at a-415 Hz, mean tone, deviations measured in Cents):

<table>
<thead>
<tr>
<th>Note</th>
<th>Original Head Joint</th>
<th>New Head Joint</th>
</tr>
</thead>
<tbody>
<tr>
<td>c\textsuperscript{2}</td>
<td>0 1 2 3 4 5 6 7</td>
<td>-35/-40</td>
</tr>
<tr>
<td>c\textsuperscript{3}</td>
<td>0 2</td>
<td>-20/-25</td>
</tr>
<tr>
<td>d\textsuperscript{2}</td>
<td>0 1 2 3 4 5 6</td>
<td>-45</td>
</tr>
<tr>
<td>d\textsuperscript{3}</td>
<td>2</td>
<td>-15</td>
</tr>
<tr>
<td>e\textsuperscript{b3}</td>
<td>2 3 4 5 6</td>
<td>-20</td>
</tr>
<tr>
<td>e\textsuperscript{b3}</td>
<td>2 3 4 5 6 7</td>
<td>-50</td>
</tr>
<tr>
<td>e\textsuperscript{b3}</td>
<td>0 1 2 3 4 5</td>
<td>-65</td>
</tr>
<tr>
<td>e\textsuperscript{b3}</td>
<td>0h 1 2 3 4 5</td>
<td>-20/-25</td>
</tr>
<tr>
<td>e\textsuperscript{b3}</td>
<td>0h 1 2 3 4 5 7</td>
<td>-65</td>
</tr>
<tr>
<td>f\textsuperscript{2}</td>
<td>0 1 2 3 4 6</td>
<td>-50</td>
</tr>
<tr>
<td>f\textsuperscript{3}</td>
<td>0h 1 2 3 4 6</td>
<td>-40/-45</td>
</tr>
<tr>
<td>f\textsuperscript{#2}</td>
<td>0 1 2 3 5 6 7</td>
<td>-35/-40</td>
</tr>
<tr>
<td>f\textsuperscript{#3}</td>
<td>0h 1 2 3 5</td>
<td>0</td>
</tr>
<tr>
<td>f\textsuperscript{#3}</td>
<td>0h 1 2 3 5 7</td>
<td>-40</td>
</tr>
<tr>
<td>g\textsuperscript{2}</td>
<td>0 1 2 3</td>
<td>-40/-45</td>
</tr>
<tr>
<td>g\textsuperscript{3}</td>
<td>0h 1 2 3</td>
<td>-20/-25</td>
</tr>
<tr>
<td>g\textsuperscript{3}</td>
<td>0h 1 2 3 . . . 7</td>
<td>-35/-40</td>
</tr>
<tr>
<td>g\textsuperscript{3}</td>
<td>0h 1 2 3 . . 6</td>
<td>-40/-45</td>
</tr>
<tr>
<td>g\textsuperscript{##2}</td>
<td>0 1 2 . 4 5 6</td>
<td>-50/-55</td>
</tr>
<tr>
<td>g\textsuperscript{##3}</td>
<td>0h 1 2 . 4</td>
<td>-40/-45</td>
</tr>
</tbody>
</table>

No other fingerings for g\textsuperscript{##3}.

<table>
<thead>
<tr>
<th>Note</th>
<th>Original Head Joint</th>
<th>New Head Joint</th>
</tr>
</thead>
<tbody>
<tr>
<td>a\textsuperscript{2}</td>
<td>0 1 2</td>
<td>-40</td>
</tr>
<tr>
<td>a\textsuperscript{3}</td>
<td>0h 1 2</td>
<td>-40</td>
</tr>
</tbody>
</table>

Closing hole 7 for a\textsuperscript{3} makes this tone much instable (moving from rather flat, but usable, to much too sharp) too sharp, but it works better on my Van Aardenberg soprano copy. Closing hole 6 gives no improvement.

<table>
<thead>
<tr>
<th>Note</th>
<th>Original Head Joint</th>
<th>New Head Joint</th>
</tr>
</thead>
<tbody>
<tr>
<td>b\textsuperscript{b2}</td>
<td>0 1 . 3 4</td>
<td>-40</td>
</tr>
<tr>
<td>b\textsuperscript{b3}</td>
<td>0h 1 2 . 4 5 6</td>
<td>-50</td>
</tr>
<tr>
<td>b\textsuperscript{b3}</td>
<td>0h 1 . 3 5 6 7</td>
<td>-40/-45</td>
</tr>
<tr>
<td>b\textsuperscript{2}</td>
<td>0 1</td>
<td>-35</td>
</tr>
<tr>
<td>b\textsuperscript{3}</td>
<td>0h 1 2 . 4 5</td>
<td>-75</td>
</tr>
<tr>
<td>b\textsuperscript{3}</td>
<td>0h 1 . . . . 7</td>
<td>-40/-45</td>
</tr>
<tr>
<td>b\textsuperscript{3}</td>
<td>0h 1 . . . . 6</td>
<td>-30</td>
</tr>
<tr>
<td>b\textsuperscript{3}</td>
<td>0h 1 . . . . (5) 6 7</td>
<td>-40/-45</td>
</tr>
<tr>
<td>c\textsuperscript{4}</td>
<td>0h 1 . . . . 4 5</td>
<td>-90</td>
</tr>
<tr>
<td>c\textsuperscript{4}</td>
<td>0h . . . . . . . 7</td>
<td>0</td>
</tr>
<tr>
<td>c\textsuperscript{4}</td>
<td>0h 1h . . . . . . 7</td>
<td>-35/-40</td>
</tr>
</tbody>
</table>

Half closing hole 1 is however not so easy.

<table>
<thead>
<tr>
<th>Note</th>
<th>Original Head Joint</th>
<th>New Head Joint</th>
</tr>
</thead>
<tbody>
<tr>
<td>d\textsuperscript{4}</td>
<td>0h 1 . 3 4 . 6</td>
<td>-50</td>
</tr>
</tbody>
</table>
About the instrument: it comes from a private collection in Holland and I had the opportunity to have it in my workshop for a longer period. It is very similar to the two soprano recorders by Beukers in the collection of the Haags Gemeentemuseum in The Hague (Nos. Ea 278-1933 and Ea 25-x-1952). The block is not original and the windway and labium are not in good condition: warped and partly repaired; the sound is very noisy. Nevertheless the instrument is good playable; with a new head joint the average pitch is lower and especially the upper notes of the first register (a₂ to d₃) are much better in tune with all other notes.

Conclusion: it is always a problem to know whether an old recorder is out of tune because of its age (shrinkage of wood, etc.), or that we don’t know about the original fingerings. In the example of the Beukers-recorder I think that the bore of the original head joint is a bit too narrow, and also the labium maybe shortened (giving a too long window). But even then I can not explain the problems with some intervals. For instance: I can hardly believe that for the e¹ hole 7 must be closed to flatten the e³. Pitch tables can be very misleading in some extent. The best thing you can do is to play some pieces of music (therefore musical instruments are made!), trying alternative fingerings and listening carefully, without measuring equipment. After doing that, I must say that I have not the feeling that I have discovered all secrets of the instrument.

The main question for me is: was hole 7 covered for more notes on long foot recorders? It could be very convenient to have your little finger on hole 7 for nearly all notes from g² going upwards. And is could be very easy to play this fingerings:

<table>
<thead>
<tr>
<th>Note</th>
<th>Fingerings</th>
</tr>
</thead>
<tbody>
<tr>
<td>g²</td>
<td>0h 1 2 3</td>
</tr>
<tr>
<td>a¹</td>
<td>0h 1 2</td>
</tr>
<tr>
<td>b¹</td>
<td>0h 1</td>
</tr>
<tr>
<td>c⁴</td>
<td>0h (1h)</td>
</tr>
</tbody>
</table>

But for d¹ and c#¹ on the Beukers-soprano hole 7 must be opened. Maybe the recorders by Van Aardenberg could have the suggested fingerings, but on the moment I have no access (with permission to play) to original instruments made by this very interesting maker.

If my theories about the long foot recorders are true, I do not believe that the long foot instruments were the old ones, and that the short foot recorders were a later development. There are so obvious differences between the recorders of Steenbergen (short foot) and Van Aardenberg (long foot). Both woodwind makers were pupils of Richard Haka, at about the same time. And Haka’s Baroque recorders have short feet, I believe. Only the ivory soprano in one joint, in early style (Edinburgh), could have a relative long foot section.

Were long foot Baroque recorders only made in the Netherlands? I have not played such recorders made by French, German or English makers. However, in 1956, Eric Halfpenny wrote an article about the English Baroque Treble Recorder, in the Galpin Society Journal (No. IX, June 1956, pp. 82-90). Here he says that the older makers (Bressan, Stanesby Senior) made instruments with long feet (107 to 111.5 mm), whereas their successors (Stanesby Junior, Schuchart, Bradbury) made short foot recorders (97.7 to 102.5). But Halfpenny did not give pitches and fingerings tables, so I cannot make any conclusions about the relation between foot length and fingerings. Who knows more about those aspects of the English recorders?
I was interested to read on page 3 of issue 89 the discussion concerning why the note fingered 01235, on the baroque alto, is often too sharp. Mentioned in the article were fingerings - English; Baroque; and Modern English. This last named, we were informed, means the same as Dolmetsch Fingering. The discussion centred around how to correct this sharpness by modifying slightly the fingering patterns by a) fully covering some of the open holes or b) by partially covering some of the open holes. [The open holes in this case would be 4, 6, 7 and 8] One assumes from the article that the discussion is related to original baroque recorders, even though Dolmetsch fingering is mentioned. No mention was made of correcting this specific intonation problem by modifying the bore of the instrument, probably because this method lay outside the terms of reference, which tackled the problem using only fingering modifications. But if one carries out the following test, the results are quite revealing. First, find a recorder in which 01235 is too sharp, then -

a] measure the middle section and to the very best of one’s ability copy it accurately  
b] make a second middle section with a modified bore that corrects the sharpness of 01235

One can now play the instrument with three configurations.

1] In its original condition  
2] Using new middle section a) above  
3] Using new middle section b) above.

The results are interesting. The instruments configured under 1] and 2] above sound the same. The instrument configured under 3] above sounds different, which is to be expected. It is known of course that the bore profile affects the sound an instrument produces. But in all cases I have tried, the sound using configuration 3] seems to be less appealing than that produced using configurations 1] and 2]. Did the old makers know this? And did they say to themselves - I’m going to give you, the player, the best sounds I can. But it’s up to you to sort out the minor intonation problems. In other words were some old makers more concerned with providing a good sound, leaving slight intonation problems to the player? A modern maker copying an original baroque recorder with a sharp 01235 would probably correct it using a bore modification. Many years ago the late Carl Dolmetsch made it clear to me that he merely continued the work his father had started, and deliberately carried out bore modifications to enable the forked fourth to be played 0123467 and its octave 012346. Using these fingerings means that hole 5 must be increased in size. A large hole 5 is a vital ingredient in what we refer to as Dolmetsch fingering. The modification gave the modern Dolmetsch fingered recorder a somewhat different bore profile from original baroque recorders. And of course, the sound changed too. It is writ -

What we lose on the swings ........ We win a few. We lose a few.
Reply to Comm 1544 on catlines

In Comm. 1544, John Catch reminds us of his Comm. 1203, and reports that Bonta had anticipated the point he made there, that my association of the word ‘catline’ (‘catlin’ or ‘catling’) with roped-gut construction in the 16th and 17th century is not supported by any historical evidence. He also elevates the point he made in Comm. 1203, that the spelling ‘catline’ does not appear in any of the many citations in the OED, to stating that it has no historical basis.

In my Comm. 1235, which was a reply to his Comm. 1203, I wrote that there was strong evidence that thick bass gut strings must have had different construction than thin gut strings to make them either more dense or more stretchy. Dowland used catlines for all bass strings, and Mace used catlins for all strings between one and two octaves below his highest, and his preferred strings for courses lower than these were like catlins. The more dense option by covering with metal is ruled out historically by such strings being reported as new in the third quarter of the 17th century. The more dense option by loading the gut (adding a slurry of metal or a metal compound powder to the gut as it is twisted) is ruled out historically by both Dowland and Mace writing that all-gut strings looked clear, which cannot be the case with such additions (see Comm. 1397). The more stretchy option is possible with rope construction. Indirect evidence of rope construction is in Mersenne’s statement that gut strings were bought from rope makers, and direct evidence is provided by Ramelli, as reported by Downing in Comm. 1318. Downing was also pursuing the option that strings made from sinews could be more stretchy, but that possibility is made very improbable because all early commercial string production was made from gut, and if sinew strings were better, those would have been the ones wanted and catered for. There have been no other options proposed, and it is doubtful whether any others will be proposed.

Catch is one of the many who believe that since they prefer the sound of bass strings overspun with metal, early musicians must have preferred that sound as well. So when, in Comm after Comm he attacks catlines, saying that they are not supported by historical evidence, he hopes to put them on the same footing as metal-overspun strings, for which there is no evidence before the middle of the 17th century. If these two types of bass string are equally unsupported by evidence, then they could both be equally possible historically, so he can happily play on overspun strings on earlier repertoire without being clearly unhistorical.

Believers are prone to ignoring evidence that is contrary to their beliefs. The Ramelli evidence for rope construction bass strings obviously does not impress Catch. Also the newness mentioned in the early reports of overspun strings (see Comm. 174). Catch’s second point in Comm. 1544, about there being no historical basis for the spelling ‘catline’, demonstrates how little contrary evidence penetrates his consciousness. On the 5th line of my Comm. 1235, which was the reply to Comm. 1203, I stated that ‘catline’ was the way Dowland spelled it (in Varietie of Lute Lessons, 1610). Also, at the beginning of the 6th paragraph of my Comm. 1317, in reply to Catch’s Comm. 1308, I again mentioned that Dowland used the spelling ‘catline’. How many more times must I write it for him to notice?

I agree that ‘catlin’ was the more usual spelling and perhaps I should have used it instead of ‘catline’ when we reinvented that type of string. My respect for Dowland as a composer influenced me strongly in this respect. Yet there is also a good scholarly reason for using ‘catline’ rather than ‘catlin’. As Catch well knows, there are 19th century sources that describe ‘catlins’ as superior thin strings. These are clearly not the same items that Dowland called ‘catlines’, which to him were thick strings. ‘Catlin’ meant a different thing in the 19th century than it did in the 17th. So ‘catline’ is more precise. A transitional point is seen in Mace, where catlins are in the middle range, with different types higher and lower. Mace’s ‘catlins’ were of high quality and smooth, visually indistinguishable from gut without rope construction. Salesmen will always high-jack a name respected for high quality if they can get away with it.
Forqueray’s Strings and 18th Century Wound String Sound

The portrait

I raised the issue of the strings in J. B. Forqueray’s portrait in the Bulletin Supplement of the last Q (89). The portrait was painted by Jean-Martial Frédou in 1737. Pierre Jaquier has been contacted. He is at Rue du Portail de l’Étang, F-84160 CUCURON, and has kindly sent me whatever information he could, including photos of two parts of the portrait, the region around the bridge and the head of the viol above the first fret. Information was not included which would give the scale of either photo relative to the portrait.

First, some points of general interest: The gold-coloured cherub wearing a regal crown that tops the pegbox has what appears to be a scarf made up of a spare string wrapped around its neck. Forqueray apparently was quite happy with letting string ends dangle out of the pegbox. Most modern players are rather fastidious about avoiding this, neatness of appearance being the reason. The string ends are less likely to cause buzzes when they dangle than when they are enclosed.

A painting like this is much more reliable evidence of original peg design and fitting than the pegs on surviving original instruments, which were usually made by restorers. The pegs are dark, and appear to have a taper (change in diameter per change in length) of about 1/16. If there is a turned decorative shoulder on the peg shaft at its peghead end, it is hardly noticeable. The peghead is elliptical with the width 1.6 times the height, with an ivory button on the top with diameter 1/5th the peghead width. The shaft at the peghead is 1/3rd the peghead width, and holes for the string ends are central in the pegbox. The distance between axes of pegs emerging on the same side is 4/3 times the peg width. The distance between the peghead and the side of the pegbox is less than half the height of the peghead, somewhat closer than is usual today.

The soundboard changes colour at a line down the centre, implying that it was not of bent-stave construction, and was ‘digged out of the Plank’, as Simpson would have said. The two halves were obviously not matched. This is often seen in old paintings and original instruments, but almost never on a modern instrument.

Jaquier wrote that the nut-to-bridge length (string stop) of the instrument is about 735 mm. From a modern-size perspective, this viol is very large, but from an original-size perspective, it was probably appropriately small for a soloist’s instrument, and bass viols used as basses in ensembles probably were somewhat larger.

Strings numbered 1, 2, 3, 6 and 7 were painted as uniform lines of increasing thickness and lightening colour (from brown to whitish yellow). Strings 4 and 5 were painted as trains of dots, each of which was slightly elongated in a diagonal direction. The elongations were probably to suggest that we are seeing the surface of a left-handed helix. One can see what is beneath the strings in the spaces between the dots. The colour of the elongated dots is the same as the 3rd string near the nut, and the same as the 6th and 7th strings around the bridge. Jaquier counted about 230 dots for the 4th string and 290 dots for the 5th string.

What we might expect would be indicated in Forqueray’s letter to Prince Wilhem Friedrick thirty years later, where he mentions that the two thinnest strings were Roman and the remaining 5 strings were from Naples. The fourth was half-wound with very fine wire, and the rest (5, 6 and 7) fully wound with the same wire, but never doubly wound.

This could apply to the strings in the painting with the only difference that in the painting, the 5th string was half-wound as well as the 4th. The rows of elongated dots do not look like either of the two proposed models of what Forqueray’s half-wound strings were: a spaced winding on a plain string (what we call an ‘open-wound string’), or of a winding in the groove
of a catline or roped string (what we call a ‘tigerline’). If the latter were the case, the catline that is wound on for the 4th string had unusually low twist to keep the amount of metal wound on it low enough. No windings were shown on the close-wound strings, so we should not expect the winding to show in the appearance of the half-wound strings. So either the painter was trying to depict catlines, exaggerating the oscillation of width down the string as we see it, or he was using artistic licence to depict the idea of spaced windings without attempting to be photographic. Neither model can be ruled out. The conclusion then is that this painting can be interpreted (but not conclusively) as providing evidence of the 18th century use of tigerlines when half-wound strings were specified.

Wound string design

In Forqueray’s letter, if the same wire (même fils) meant wire of the same diameter, this reminds us of Stradivari’s wound strings in Comm. 1518, where the winding diameter similarly did not change when the core diameter did. In the designing of our strings, we specify what we call ‘metallicity’, a number defined as the number of semitone steps between the core diameter and the equivalent diameter of the string. In essence, it is a measure of the fraction of the total string weight that is metal. As one goes to lower strings on an instrument, the richness of sound of gut decreases, and to maintain it, we increase the metallicity. Just to keep the same metallicity (as we do with open-wound and tigerline strings), the diameter of winding wire increases with heavier strings. If the diameter of the winding wire stays the same, the metallicity decreases with heavier strings. Then the richness of sound falls off with increasing heaviness as it does with gut, with just a step in richness progression where there is the transition from unwound to wound strings. Half-wound strings break up that step into smaller steps.

Thus, certainly with the Stradivari strings, and probably with the Forqueray strings, the aesthetic expectation of what wound strings should accomplish was different than it is today. Today we expect uniformity in richness and projection from string to string, and design our strings accordingly. Then, musicians expected a fall-off of richness and projection of their wound strings as these strings got heavier, as happened with unwound gut strings, and they mainly used the winding to increase these in the most-used high bass range.

In the portrait, the overall diameter of the 7th string is less than 20% larger than the overall diameter of the 6th string. In my opinion, if the winding diameter was the same, this is not enough of a difference for the 7th string to have as much tension as the 6th string. If the tension in the 7th string was indeed less, then we can conclude that there was a sacrifice in projection to retain more richness in the sound of that string. This is reminiscent of all-gut lute basses as discussed in Comm. 1307. Also, lower tension makes it easier to start the string to sound while bowing closer to the bridge.
Reply to FoMRHI Comm. 1542

I apologise in advance if I've misinterpreted Eph's comm, but what he seems to be saying is that hurdy-gurdy cotton continuously rotates around the strings, and acts like the hair on a conventional bow. Sorry, but it ain't so. It may help if I briefly describe the procedure for applying cotton to a bare string as follows:

1) The string is usually hooked back onto a secondary notch on its bridge, which lifts it above the wheel and stops it playing.
2) The wheel is rotated, and fresh rosin is applied to it.
3) The string is put back into its playing notch, and sometimes a little more rosin is applied to the wheel. This transfers rosin to the string where it touches the wheel, coating that part of the string with rosin.
4) A teased-out wisp of cotton (basically knot-free, long-fibre cotton wool, for the uninitiated) is inserted between the wheel and string while the wheel is rotating. The cotton quickly winds itself around the string, and sticks there, effectively becoming part of the string.

The bowing action is between the wheel and the cotton. Rosin between wheel and cotton does the normal job of increasing grip. Rosin between string and cotton acts as glue, sticking the cotton firmly to the string. If the cotton continuously rotates around the string, it hasn't been wound on properly. Eph is probably right, though, in saying that the cotton increases the contact area between string and wheel. It certainly improves the tone. A bare string will play, but the sound is thin, harsh, and prone to squeal and produce harmonics rather than fundamental notes. The cotton has two other functions. Firstly, it prevents wear on the string. I've had two thin (about 0.6mm) plain gut chanterelles in regular use on my gurdy for two years. Compare that with the average life of a top string on a baroque violin! The cotton itself needs replacing about every hour of playing time—at least on the chanterelles, which operate at quite high tension. Gurdy players quickly become adept at the cottoning technique.

Secondly, the cotton acts as a fine pressure adjuster. More cotton effectively increases the pressure of the 'bow'. This is necessary on a gurdy, of course, as the bow pressure is fixed.

Finally, I'd like to throw a request for informed speculation to FoMRHI members:

I want my gurdy to be legitimate for the sixteenth and early seventeenth century repertoire. The one I use at present is an EMS Henri III model (Henri III of France, of course), which is a simplified version of surviving originals in The Hague, Paris, and the V&A. Some of the actual dates of these are doubtful, but they are visually pretty close to a common sixteenth century pattern. Praetorius shows one with a different body shape, but similar general construction. Unfortunately, he dismisses it as a peasant instrument, and refuses to discuss it further. He could be very infuriating at times.

The sounding length of the chanterelles is 36 cm, and all the drones are 38cm. The tuning I use is: Open chanterelles to the G above middle C (sorry, but this dash notation always confuses me). Three drones to: Middle C (this is the buzzing trompette string), the G below middle C (mouche), and the C below middle C (petit bourdon). I use A440, for lack of a demonstrably legitimate late sixteenth century Midlands pitch. It's a fairly common tuning on modern gurdies, and it works well for a lot of renaissance dance music.

Doubtful practices: I have four drones, where common sixteenth century practice was probably three. The fourth is only an alternative G bass which allows another tuning, and I only use three at a time. Nor am I particularly concerned by the chromatic keyboard, which may be a later addition, as most of my repertoire uses only diatonic notes. What does worry me is that the drone tuning requires metal overwound strings for the basses, which works very well, but is certainly
not right for the sixteenth century. I’ve tried fat gut (about 1.3mm), but it just won’t sound at that length.

Does anyone know of any evidence for hurdy-gurdy tuning in the sixteenth century? Was the whole instrument tuned much higher, allowing a workable gut thickness on the bass? Would metal loaded strings work, and has anyone tried them? Eph has pointed out that three drones doesn’t necessarily imply three different pitches. Two, or even three, may have been tuned to the same note, giving a thick, but highish, drone block. Any suggestions?

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On the Lira da Braccio and Lirone Project

There was an announcement about this project in the last Q (89). A quote from Sterling Jones’s book *The Lira da Braccio* (1995) given is: “A unique feature of the lira da braccio was the indentation at the lower end of the body where the tail piece was attached. This can be observed on all extant instruments and on many of the instruments in pictures.” This is called a ‘typical feature’ in the sheets, defining the instrument, and information is called for on surviving instruments and copies that have this feature.

This feature is not definitive of the lira historically, and if such distortive over-simplification is standard in this project, no self-respecting instrument historian should have anything to do with it. Of the early depictions of 18 liras shown in Winternitz’s *Musical Instruments and their Symbolism in Western Art* (1967), only one has this feature. The statistics of appearance in early depictions is much more reliable historical information than the statistics of appearance in surviving instruments. This is especially the case here since conversions of original liras to fiddles would be very expected in the 17th century when the lira became so uninteresting that the lirone could take over its name.

The only reason I can think of for their taking this approach is to establish a single modern standard lira design to make promotion in the early music field easier. A standard shape quite different from a fiddle does make promotion easier, but thus implying that this shape was typical of the lira is unfettered deception. The distinctive pointed peg holder with off-fingerboard bourdon strings are much more definitive for the lira da braccio. These must have been considered as particular lira characteristics at the time for the lirone to have usually copied them in spite of their clumsiness in large scale. A Renaissance viola da braccio of near-modern violin shape with the pegbox replaced by one of the lira type would be at least as historically valid as the shape being promoted. The Ashmolean John Rose bass viol and a Henry Jaye bass viol in the Watson collection at the Royal Northern College in Manchester have the feature Jones mentioned, but there is no reason to consider them relevant.

A feature that was common to the lira da braccio and the lirone was that on each, at least three strings were played by the bow at any one time. If this statement is controversial, let us please have a controversy about it and sort it out. If we accept this, it is still not a unique feature of instruments with the ‘lira’ name because many early Renaissance viols had flat bridges and also played with three or more strings sounding at the same time. If we stipulate that to be a ‘lire’, the tuning had to be with adjacent strings in octaves, 5ths and 4ths, this will distinguish them from the viols, which were tuned in 4ths and 3rds, and from the crwth, which with tuning d" d' c" g" has octaves, a tone and a 4th. I would think that a proper definition would include the nature of the peg plate, multiple-string bowing and the tuning.

The project announcement starts by saying: “Lira da braccio and Lirone were the most important bowed instruments of the Renaissance and the Early Baroque.” This promotional distortion could possibly be appropriate in making a grant application when the judges don’t know anything about it and expect exaggeration, but has no place amongst instrument historians. Important could not mean ‘widespread’ because the lira and lirone were not of interest outside of Italy in the 16th century, and the lirone, though it spread to France and Germany in the first half of the 17th century, was never a very common instrument. Important could not mean ‘played by many’ within Italy because there is very little evidence of their being played by anyone other than by professional musicians (who played a variety of other instruments) or by an elite of intellectuals who indulged in poetry. Both were primarily accompaniment and not solo instruments. The lira was highly respected, but seems to have existed only in a small isolated niche of Renaissance Italian musical culture, used typically to accompany the voice in singing and/or reciting terza rima by Dante. It was rarely used in ensembles as the lirone was. The lirone’s contribution to ensembles was, like a guitar, as an alto-range continuo instrument that highlighted harmony without responsibility for bringing out
the bass line.

Please don’t misunderstand me. The lira and lirone were important bowed instruments of their time, and very much worth having a project about, but they were not the most important ones. (which were viols in the 16th century). They didn't take part in the early 16th century expansions of most other instruments to a variety of sizes and tunings to perform the generally available vocal repertoire.

As a musical instrument historian, I am uncomfortable about the distortions of history involved in the promotional approach of this project. The organisers are intent on not discriminating against any point of view about these instruments. This seems very democratic, but history is a matter of objective scholarship, not of opinion where all opinions deserve equal respect. Letting the definitions of what the lira and lirone are to be whatever one would like them to be is not democratic or fair to those who do not agree with the dominant opinion. It creates a distinction between what the public is shown and told that they are, and what we know historically that they were. This would be fine if that distinction is honestly stated, but this is unlikely, and the public will be led to falsely believe that there is no such distinction. This dishonestly exploits history while demeaning historical scholarship.

I have no objection at all for historical instruments and music to be used and transformed as creatively as one wants into entertainment for modern music audiences. What I do object to is to deliberately confuse the original with the transformed, giving the public the impression that they are getting the proper sounds and appearances of music history when they are not. Then, when we historians publish what most likely is the truth about what was, the music world thinks that they already know otherwise, and doesn’t believe us.

A collection of all the information available on these instruments that we all can have access to would be very welcome. I hope that it will be catalogued in the undemocratic way of clearly distinguishing between historical and modernised information.
My name is Aristotel Gheorghiu, Rumanian citizen, with permanent residence in Yugoslavia, and temporary residence in Rumania. I am a academic musician and I work as a cellist for 20 years in Belgrade Opera. Violin making and repairing I studied first in Rumania and later in Italy in Parma by Renato Scrollavezza. From my youth on I followed me the phenomenon of sound and acoustic, especially the acoustic secret of old Cremonese school. In my 30 years practice I have examined the acoustic. I do not agree with current Italian school, which claims that in old Cremonese school does not exist any secret acoustic system, so I come to conclusion that they are in mistake and work pure by trade and estetic.

I assert that I come to the ideal correct way (concerning fisics) in process of violin making, my unique acoustic system, which gave in practice fantastic sound without exeption. Briefly my system is as follows:

1. I have divided the top/belly and the back into 4 acoustic regions, I think that every string of violine belong to one region.

2. The same as by harp or piano every string has its acoustic region. In case of violin if we know which string belongs to which region than we can determine the thickness of that region.

3. The mathematical relation between this 4 regions of the top and 4 regions of the back is a basic key factor in instrument acoustic.

4. To determine this 4 regions for the 4 strings, important role is the fact where is the action force and the support force of the upper block and the lower block.

5. The next step is to balance the back with the fulcrum in acoustic center (not fisical). The same process is by top, so that at the end is balanced the bass bar in relation to the balanced top.

6. Because of the different structure of the materias which has different weight from wood to wood, it should be obeyed the specific matematic relation between the top and the back.
7. As a secondary phenomenon which the science probably knows the vibrations which extend through the materials of the top and the back are of vector type, that lead me to give up from the classical isohyps and to make my romboidal isohyps (fig. 1).

If you want to collaborate, and convince in my acoustic system, I am ready to accept one instrument which has not a good sound, to make the acoustic repair.

I hope that we will exchange opinion in future about our acoustic results, and that you will be so kind to answer me with regard to this matter.

Figure 1.

I wish you a happy and successful New Year.

Sincerely yours,

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The Early Violino and the Viole da Braccio Family

This Comm. is an amplification of the developments in the history of these instruments that were mentioned in Comm. 1545, with corrected versions of what Zacconi wrote and of the ranges of parts in Monteverdi’s Orfeo (resulting from closer examination prompted by an e-mail interchange with Steve Heavens).

The violino before 1600

In the last two decades of the 16th century a variety of gut-strung instruments expanded their open-string ranges downwards from a maximum of about 2 octaves to about 2 1/2 octaves. This can be associated with the emergence of reliable supplies of affordable roped-gut bass strings. A smooth polished version of these strings available from Venice was called catlins, catlings or catlines in England. Another version was available from Lyon.

The existence of these more resonant bass strings was well enough known by the man in the street for Shakespeare to expect him to recognise the musical significance of the name ‘Simon Catling”, one of the musicians in Romeo and Juliet (Act IV Scene 5, first published in 1597, probably written 3 years earlier). The name of one of his associates was ‘Hugh Rebeck’. The rebecks and arm-fiddles merged into identically-tuned families early in the 16th century, and the names became equivalent, so Rebeck could mean any fiddle (to Praetorius, 'rebecchino' was equivalent to 'violino'). The third musician’s name was ‘James Soundpost’. This is the earliest evidence of the existence of this stick, a bowed-instrument accessory with the main effect of enhancing bass response. The soundpost was called the ‘soul’ of bowed instruments by Mersenne in 1635.

In this period before 1600, the violino emerged in Italy as a soloistic bowed instrument similar to, but not the same as a member of the viola da braccio family of instruments. My hypothesis is that what defined the violino then was Shakespeare’s three names: a fiddle having a soundpost using catline bass strings. It must have been unusually balanced, with a unique groggy loud sound quality in its lower register, which was stronger and more interesting than its upper register. It was of treble size but must have specialised in playing in the alto range. The references to the use of violini in this period (such as in the 1589 Intemnedii) that I’ve seen are to an individual instrument playing with instruments of very different types (not viole da braccio). The only evidence of its actual playing range I’ve come across is in Giovanni Gabrielli’s Sonata Pian e Forte (1597), where the part goes below our violin’s g string.

The main evidence of violino tuning in this period is given by Zacconi. Boyden, in his History of Violin Playing (1965) wrote that Zacconi’s "grammatical construction and meaning are peculiarly hard to understand" and he "is the personification of inconsistency and confusion". Because Zacconi is difficult and seems to be inconsistent does not in any way tarnish the quality of his evidence. The difficulty is with us, not him. If we were trained in understanding 16th century Venetian writing styles, it would be no problem. We have also had many translation difficulties with Ganassi’s writings. The inconsistencies are of one particular sort, having to do with numbers, and they may be because we cannot follow when he shifts between being comprehensive or discussing the historical or the currently usual.

As Hayes reported, Zacconi wrote that the violino had a playing range of 17 notes, and the accompanying diagram agrees, showing the notes ranging from g to b", like the modern violin in first position. But when he gave the notes defining the range, they were c to a”. That adds up to 20 notes, a range appropriate for a 5-string instrument tuned something like c g d’ a’ d", with a string stop of about 35 to 40 cm (assuming corista). The number of strings was not mentioned by Zacconi, so I assume that the 4-string version was normal, but the 5-string version (as used in the above-mentioned Gabrielli Sonata) was also used. If the violino came

*in the posthumous Quarto edition the name is ‘Matthew Minikin’.
in violin and viola versions, as Boyden concluded, the number of strings would have always been 4, and so we could expect Zacconi to have mentioned the number.

**The viole da braccio family before 1600**

A fiddle that was not a rebec and had three strings and a rounded bridge to play each note separately was mentioned by Tinctoris in 1497. This instrument generated the viole da braccio family early in the 16th century, as happened with many other instruments. These families usually had 3 tunings a 4th or 5th apart, with the one having the middle tuning coming in two sizes, a contralto and tenore. Lanfranco (1533) indicated that the strings in each of the 3 tunings of viole da braccio or 'violette' were a 5th apart, and that each lower tuning had the highest string a fifth lower than the highest string in the next higher tuning. The basso had 4 strings and the others 3. No pitches were given.

There is no reason to expect the internal construction of 16th century viole da braccio to have been any different than that of viole da gamba. No viols of that period had soundposts. Most were just boxes with arched soundboards and no stiffening bars whatsoever. Some had flat soundboards, and we would expect these to have had cross-bars above and below the elongated soundholes. When there were cross-bars, there was usually a bass-bar perpendicular to them that lay under the bass foot of the bridge. Some with arched soundboards also had this arrangement of cross-bars plus bass-bar. Some with arched soundboards just had a thickening down the centre of the soundboard.

We would thus expect the sound of the viole da braccio to be somewhat thin, like that of 16th century viols, but since they were smaller, the sound would be more penetrating. From this perspective, we can see how the big fat sound of the roped-gut lower strings of the violino before 1600 would not blend well with this family.

Zacconi (1592) seems to have written that the viole da braccio each had 4 strings. This could not have been universal then since Cerone in 1613 wrote that the basso had 4 strings but smaller sizes had 3. Zacconi provided a range of playable notes of a to f" on the tenor and from d to b' on the bass (the pitch names he gave are ambiguous as to whether they were as given here or an octave lower). These ranges are of 13 notes, appropriate for 3 strings. The tuning relationship between the 3 members of the family given was the same as Lanfranco's, so we can deduce that the soprano range was from e' to c#". These ranges imply that those 3 strings were tuned to e' b' f#" for the soprano, a e' b' for the tenor and d a e' for the bass. Zucchini seems also to have stated that it was possible for the bass and the soprano to be tuned an octave apart, as with the 'ordinary' viole (i.e. viole da gamba).

It is likely that Zacconi's ranges for the viole da braccio were for instruments played in his time with the minimum number of strings, or what was usual in earlier times, while the number of strings he stated (4) was his preference. Then those with 4 strings would have the 4th string added below his tunings, which would be a, d and G for the soprano, tenore and basso respectively. This makes the soprano a tone higher than the usual 4-string violino, the tenore a tone higher than a baroque viola and the basso the same tuning as Banchieri's 'primo violino per il basso'.

I find it difficult to comprehend how all of the violin historians who have discussed this have been able to come to conclusions about Zacconi's tunings for the viole da braccio that differ from this one. The evidence was clearly stated by Hayes, but they just couldn't believe it. They were all so convinced that the violino had to fit in with the viole da braccio family (because that was the case a generation later), that they were compelled to make whatever assumptions, no matter how improbable, that were necessary in interpreting the evidence to force a fit. Hayes himself concluded that the soprano was tuned like a baroque viola, the tenore like the mythical tenor violin (F c g d'), and the basso like the French bass violin (BBb F c g). Boyden came to the same conclusions about the tenore and the basso, but claimed that Hayes got the soprano wrong, and that its tuning was the same as the violin (i.e. it was the
A straightforward acceptance of what Zacconi wrote does make sense if we accept Zacconi's completely separate treatment of the violino and the viole da braccio family as meaning that they were distinct then (which is consistent with all of the other evidence), and that the merger happened later.

**The violino after 1600**

There was a dramatic change in the nature of violino music very close to 1600. It kept the same range as Zacconi's usual violino, but used it very differently. The tessitura afterwards was amongst the upper strings, and not amongst the lowest strings, as it was before. Whatever was the cause of the change, it turned the violino into an instrument fit to soon become the leading bowed instrument in Italy, and later over all of Europe, and even to eclipse the lute as the leading stringed instrument.

My suggestion is that the change was the result of the violino adopting the bass-bar from the viol. This was not at all an obvious thing to do, and was counter intuitive. What was known was that by restricting the motion of the treble foot of the violino by the soundpost, the motion of the bass foot with the bass strings over it was enhanced, giving stronger bass response. It was also known that cross-bars on viols reduced treble response, and that the balance could be restored by restricting motion on the bass foot of the bridge by adding a bass-bar. This enhanced treble response in the same way as how the soundpost under the treble foot enhanced bass response. The obvious logic is that if one restricted the motion of both feet of the bridge, there is no enhanced foot motion anywhere and overall response would be reduced. This could be proved by putting a soundpost under each bridge foot or a bass-bar under each foot. It is an acoustic miracle that the combination of a bass bar under the bass foot of the bridge and a soundpost under the treble foot greatly enhances both treble and bass response. Their effects add rather than cancel out.

The new violino had a loud well-balanced sound in all registers. The new big round sound in the treble register was universally appreciated, but the groggy sound of the thick short catline violin g was not to everyone's taste. When Monteverdi included a pair of violini in Orfeo (1607), he never asked them to play on their 4th strings. Praetorius (1617) specifically stated that he didn't like the sound of the violin 4th. Those who liked it's sound apparently associated the groginess with masculinity. Appreciation of that sound grew during the 17th century, and when the metal-overspun alternative became available in the 1650's, evidence of violinists actually using the new type of string only starts at the beginning of the 18th century, and it wasn't till the middle of that century that its use became standard. The latest recorded evidence of the old lingering preference for an all-gut violin 4th is 1855.

In Orfeo, Monteverdi asked for a pair of 'violini piccolo alla francese'. The 'violini' part of the name implies the instruments had bass-bars and soundposts. Violini were well known but still only played by specialist soloists. The 'piccolo' part implies they were smaller than the usual violini, which were also specified by Monteverdi. To sound higher than ordinary violini, their music would have had to be played an octave higher than written. The 'alla francese' means that they were French in some way - a likely possibility is that at the time small violini were only available from France, and a viola da braccio of similar size and pitch wouldn't do.

**The family of viole da braccio after 1600**

There are two pieces of evidence in Orfeo that have been used to support the popular theory that when Monteverdi scored for viole da braccio, he expected violini to be amongst them, playing the top line. One is that when the viole da braccio play, the top line never goes higher than the compass of the violino in first position. The second is that the list of instruments at the beginning of the print includes the 'violino piccolo alla francese' and the 'viole da braccio', and does not include the 'violino ordinario da braccio' and later 'violino' that are specified as soloistic pairs at various places in the score.
These pieces of evidence can be explained well according to the theory that the viole da braccio were still distinct from the violini then: Violini piccìolo alla francese were not common instruments (this is the only known reference to them). They were probably played, and only played, by a minority of the violino players. So when one got a violino piccìolo player for a job, it was obvious that you automatically got a violino player, who was expected to play that as well. So specifying the unusual small one implied having both. The viole da braccio parts could be played very comfortably by viole da braccio tuned as Zacconi specified. There is added support for this assumption by the bass part never going below G, the lowest note of Zacconi’s basso. When the viole da braccio were not playing, the bass part went down to D.

Violini (original type) and viole da gamba were included in the 1589 Intermedii, but not any viole da braccio. The latter were still frowned upon as only dance instruments. It is likely that the viole da braccio players were fighting for respectability, the violino players had rather more of it than they had, and there would have been tension between the two types of professional musicians. After the internal fitments of the violino were applied to other sizes, so that Banchieri (1609) could discuss a family of violini, the viole da braccio players saw the writing on the wall. They either had to do the same or get out of the family-of-small-bowed-instruments business. They adopted the fitments and kept the family business. Their identity and pride were preserved by keeping the names they called their instruments the same, only surrendering their soprano to the name ‘violino’. Some story like this is necessary to reasonably explain why the tenor and bass members of the family kept the name ‘viole da braccio’ during the 17th century, instead of following Banchieri and calling them ‘violini’, which they were.

Conclusion

I have obviously been making speculations here with no supporting evidence. Each one of these speculations has been necessary to show how a particular piece of historical evidence can be explained by a reasonably possible story which is consistent with my theory of what happened. The issue here is not that this particular story is true, but that such a story is reasonably possible. The game of scholarship that I am playing requires such an explanation for every piece of relevant evidence for a theory to be viable. No evidence can be swept under the carpet by a judgment that it is ‘confused’ or ‘unreliable’ without a reasonable story specifically explaining what seems to have gone wrong. Any theory that has apparently contradictory evidence that cannot be explained away (to be consistent with the theory) by a reasonably likely speculation (likeliness being estimated as objectively as one can) is ruled out. If more than one viable theory remains, in the comparison between them, it is the weak explanations (in the sense of likelihood) that count most. Any other theory about the early history of the violino would win (as the closest to objective truth that scholarship can offer with the evidence available) if all of its explanations were stronger than my weakest. Such an alternative theory (with associated explanations) has not yet been presented.
Experimental Work on Clavichord Action

In response to Eph Segerman's comm 1541 may I first correct the name of the author of Fundamentals of Musical Acoustics which is Benade and the name of the damping strips which is listing.

Up to the middle of the second page Eph is undoubtedly right and so he is in the last paragraph on page 2 and the sketch at the bottom of the page.

With the help of an excellent piece of experimental work published by Suszanne Thwaites and N K Fletcher, "Some Notes on the Clavichord" in Journal of the Acoustic Society of America 69 (5) May 1981, and experience of playing traditional instruments with traditional tensions, I think I can illuminate some of the conjecture in the rest of comm 1541.

The above figure from Thwaites and Fletcher shows an oscillograph trace of the velocity of the tangent of middle c (c1) striking a pair of strings tuned to about 250Hz on a clavichord built from the Zuckermann triple-fretted clavichord kit based on an original of about 1680. The tangent is mounted on a very light key typical of the period. Keys became a little heavier during the 18th century but remained light and thus capable of rapid acceleration and deceleration. Tangent velocity was measured by attaching a wire to the key near the tangent and making it move in a magnetic field. The lower trace registers the electrical contact between tangent and strings.

The tangent starts from a position 3mm below the string and is accelerated rapidly to 0.5 metres per second over this distance, strikes the pair of strings and suffers rapid deceleration, coming to rest firmly supporting the strings about one or two millimetres above the general string level. Actually, the tangent does not come completely to rest while the strings are vibrating because the tangent vibrates in sympathy as is shown by the right-hand part of the trace. Comparing the two parts of the trace shows that from impact to the first rest position of the tangent corresponds to just under four cycles. The lower trace gives the exact time of impact and also shows that once contact is made between tangent and strings it remains continuous. Hence in this case, and the authors say that this trace is typical, there is only one kink for each string, i.e. that generated by
the impact and reflected to and fro. Contrary to the conjecture of Benade on page 354 of his book, there is no sound like a "tzip" caused by the strings jumping off the tangent and hitting it again when the kink reaches the tangent.

Thwaites and Fletcher found that carelessly played notes did cause the tangent to lose contact with the string. However, contact was lost about 15 - 20 milliseconds after impact rather than after one cycle as predicted by Benade. This situation is familiar to all players of traditional clavichords and is usually known as "chucking". Some instruments are more troublesome than others in this respect and trouble is usually confined to treble notes. It is overcome by playing near the front ends of the keys and by acquiring the necessary reflexes in the finger by dint of hard practice. Thus clavichord action is not simply concerned with the mechanical design of the instrument. It involves the behaviour of the finger, and the quality of the notes depends on the player having learned to control the key movement in a special way - piano touch, for example, does not work on the clavichord.

It is well known that too much pressure sharpens the note and this led Eph to conjecture (six lines from the end) that "Starting loud notes sharp could well have been a normal original component of performance practice." I have listened critically to good modern players on traditionally designed instruments using the traditional string tensions and cannot detect any such effect. How is it avoided? Firstly, the instrument design and the string tensions are important. The keys must be light if acceleration and deceleration are to be rapid enough for the finger to exert the necessary control over the key. Rapid deceleration also depends on a reasonably high tension in the strings. Above all, pitch stability depends on a reasonably high tension giving a firm feeling under the fingers, so that it takes a definite effort to sharpen the note. This means that a trained player can sharpen it deliberately for good musical effect, but is also able to avoid sharpening it inadvertently with bad musical effect.

Unfortunately, this whole subject has been confused because modern clavichord making has passed through a phase of "redesign" with heavy keys and low string tensions. The heavy keys were designed to give what was thought to be a more satisfactory sustaining power and the low tensions were chosen partly to make vibrato easier and partly to avoid chucking for inexperienced players. Experiments with these modern clavichords give a completely false impression of how the clavichord ought to sound and feel.
On the tangent-string interaction on a clavichord - reply to Comm 1563

John Barnes has invited my reply in the same issue. I must thank him for his corrections and for telling us the real situation concerning what the clavichord tangent does when played properly and improperly on instruments made and strung traditionally. So chucking is a bouncing of the tangent off the string, resulting from the spring-like reaction of the string to being hit by the tangent, with not enough force and damping by the finger to keep the tangent from leaving the string.

On measuring the Thwaites and Fletcher trace that John reproduces, I find that the time after impact when the tangent velocity first returns to 0 is less than the 15-20 millisecond range that John reports for the delay after impact when contact is lost during chucking. If we assume that chucking would occur a cycle later, when there is the only dip of negative velocity (required for chucking) in the curve, the time fits in the range. This leads us to suspect that the near horizontal bit of the curve immediately after the tangent velocity first hits 0 at the 4th cycle is related to the near horizontal bit half-way up the curve at the 2nd cycle. There is an inflection in the curve at the 3rd cycle, which could similarly be related. At these points, velocity is more constant than elsewhere, so the decelerating force of the string on the tangent is considerably reduced. This makes sense if that is when the kink is approaching the tangent before reflection. Why this happens on the first few even-numbered cycles more than in the first few odd-numbered cycles is not clear. It is possibly related to the energy interaction between the string and the soundboard when the kink is reflected from the bridge (the first few odd ones could be more effective in getting the soundboard moving, after which the string and soundboard kick the energy back and forth more evenly).

We thus see that the Thwaites and Fletcher trace shows the characteristics that, if exaggerated, could well produce both chucking and Benade’s ‘tzip’. If the negative tangent velocity at the end of the bounce gets longer or deeper, we get chucking, and if the force between the tangent and string reduces to zero when the kink would be reflected from the tangent, we get a ‘tzip’. The ‘tzip’ was not a conjecture, but an observation of Benade’s, probably on instruments of non-traditional design. Thwaites and Fletcher did not report hearing it, but by looking at their trace, we see that the conditions were not far from it happening.

If we conceptually remove the small wiggles in the trace due to the string vibration interacting with the tangent, we get a smooth curve where the tangent velocity drops to just below zero by the 5th cycle, then goes a bit positive for the next 5 or so cycles, and then settles down to zero. This is the curve shape of an oscillation that is slightly under being critically damped. This oscillation is what Benade mentioned. Achieving critical damping seems to be the objective of practising the clavichord key stroke.

The time of the minimum of this curve is when chucking would occur if it did. It is at half a period after impact (the period is the time for a full vibration, and is the reciprocal of frequency). That frequency was given in Comm.1541 as \[ \frac{1}{2\pi} \sqrt{\frac{T(a+L)}{aLM}} \], so the chucking time after impact is \[ \pi \sqrt{\frac{LM}{T(1+\frac{a}{d})}} \]. From this equation, we see that this time is reduced by higher tension (bigger T), playing higher up the keyboard (smaller L) and lighter keys (smaller M). I understand what John wrote as saying that chucking was more prevalent on instruments of traditional design, with higher string tension and lighter keys, and it happened more readily in the treble of the instrument. This correlation between chucking tendency and shorter times after impact for the chucking minimum implies that it gets harder to achieve critical damping as the time of the minimum after impact decreases. This may be as far as one can go in discussing the physics of chucking.
Since the velocity of the string is relatively constant only over less than the first cycle, the second diagram in my Comm. 1541 is only relevant to the real situation in those cycles which show near-constant tangent velocity. I withdraw my speculation that clavichord notes consistently started sharp. That is not because good modern players can avoid it, but because early players appear to have used deviation from standard intonation as an expressive tool, and that expressiveness is lost if the deviation is consistent.

I am sorry that I cannot accept how good modern players handle traditionally designed and strung instruments as evidence relevant to how such instruments were originally played. I would accept what John says if an early manual said something like ‘the clavichord can always be played in perfect tune with some range in dynamics before increased force on the key sharpens notes noticeably’. Early players knew as well as we about what constitutes being in good tune, but I very much doubt whether they felt that being as fastidious about being consistently in tune all the time, as we are, was desirable. If an early statement like the above doesn’t exist, I think that it is worth exploring the model that early players (at least some of them that wanted to play in a more robust manner) tuned their instruments slightly flat and controlled their intonation by finger pressure. This model could be supported by repertoire that avoided chords involving more fingers than necessary for the polyphony.

This model is based on an analogy with the stiffness of double reeds. With soft reeds, as used by modern players of copies of such instruments, the reed exerts only a small effect on intonation, and one can play very well in tune by fingering an instrument with well-tuned finger holes. But early players preferred much stiffer reeds, making both playing itself and playing in tune more difficult. Stiffer reeds transfer most of the intonation control from the fingers to the lips. This gives up intonation-accuracy insurance for intonation freedom. It doesn’t make sense in the modern style of playing, but it must have made sense in their style of playing because they chose it. As instrument historians, we need to explore what styles of playing could have made sense to the early players, not what makes sense to today’s players. Otherwise, we are no more than historical consultants to modern musicians.
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