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FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS
Hon. Sec.: J. Montagu, c/o Faculty of Music, St. Aldate’s
Oxford OX1 1DB, U. K.
We owe you all an apology for the confusion created over renewal notices in the last issue. I'm still not exactly sure what went wrong, but the likeliest scenario seems to be that somehow one of the disks containing a running list of renewals didn't get fed into a computer further down the line. Subsequently, a deluge of little pink slips poured through my letter box for weeks after the last quarterly, with much anxiousness expressed about lost cheques and many worries about not getting the next issue. We sympathize with those of you who have gone through the hassle of locating cancelled cheques and photo-copied money orders to prove you had paid and regret that you were inconvenienced in this way. Believe me when I say we've all suffered a bit on this mistake. To every member's credit this is a large, healthy fellowship, growing more each year, but it does take a tremendous effort to keep it all on track, and the time this takes is given voluntarily. Please keep in mind that someone on the other end of this journal has to stuff millions of envelopes for each issue, keep track of who has or has not paid, and deposit all the dosh. I suppose the one thing that is so heartening about this last problem is that many of you seemed genuinely concerned about losing the lines of communication that link us all together, FoMRHI. Again, thanks for your patience, we did eventually locate the missing disc and all seems to have ended well. Let us know, however, if there is any residual problem.

STOLEN INSTRUMENTS ALERT: I had a phone call from Simon Chadwick (0453-766883) about the theft of three instruments from the Cowle Collection in the Stroud Museum. It appears that an ivory four-keyed flute made by C. Simpson and two flageolets were stolen recently. One was by G. Peachey, Bishops Gate, London (boxwood) and the other was anonymous, in dark wood with ivory mounts and missing a mouthpiece. If you locate any of these please contact the museum Curator, Lionel Warlrund. The investigating officer's theory is that the theft was the work of a gang of ecstasy taking 'ravers' looking for a new source of entertainment. Hmmmm, could it be that flageolets are the next craze on the club scene?

LOST MEMBERS: We had Q 66 returned from the following addresses as no longer there: A Brown, Reykjavik; Welsh Lute Society, Abertridwr; and LACM Esteves Pereira, Mouquim, Portugal. Any ideas as to where they have gone?

ADDRESS CHANGES:
Nicholas Baldock, Feldstabe 7, W 5429 Katzenelnbogen, West Germany
Anders Emmersfors, Tallback, 66300 Skoghall, Sweden
Mel Stevens, 24 Oakroyd Close, Burgess Hill, West Sussex, RH15 OQN tel (0444-242492)
Huw Saunders, 110 Milton Grove, Stoke Newington, London, N16 8Q7
Peeter Talve, Jaruamaa, Janeda 2832, Estonia

BATE WEEKENDS: Our latest bowmaking weekend filled up very quickly. Next on the agenda is Gamelan on May 16th and 17th. In July we have Jane Clark's Couperin masterclass running 12-17th (the whole week is £60, £45 for Bate Friends and students, any single day is £15).

FOREIGN EXCHANGE: Charles Stroom writes that you don't have to send Dutch banknotes for his lists. He will accept notes in other currencies equivalent to DFI 5.

DEADLINE FOR NEXT Q: July 1st.

PSION II: M. Glover writes that he has developed a program that calculates frequencies from note names and vice versa. He claims it is handy for calculating how to set temperaments...
quoted in commas on a machine which reads in cents. For more information about the program and a photocopy of the listing contact him.

OUT OF PRINT BOOK: Denzil Wraight writes that *Kielinstrumente* by Hebert Henkel can be ordered from Breitkopf & Hartel (tel. 06128-6352) in Germany.

CODA: Finally, I'd like to say how much I've enjoyed holding down the main desk at FoMRHI central while Jeremy has been in Israel. From what I've heard about the weather there he didn't manage that tan, but in our frequent telephone conversations he always sounded relaxed and content. He will be back here around the 26th of April for those of you who wish to contact him. Meanwhile, I'm off to San Antonio, Texas to present a paper on *Authenticity* in Post-Modernist Musical Instruments at the American Musical Instrument Society's annual convention and stuff myself full of tacos and Tex-Mex music.

FoMRHI Comm 1074

Various Comments

Jeremy Montagu

I am writing from Jerusalem, where Q66 has just reached me; hence this rather grotty printer which has two faces, both unpleasant, and won't go down to the bottom of the page. But I shouldn't complain; I've been lent it free, for which I'm very grateful. I've been doing a lot of writing (improving the catalogue of my own collection, which I want to turn into a catalogue raisonné I don't think this printer will produce accents, and it certainly won't produce italics!), a history of instruments as illustrated by the Montagu Collection, which now numbers over 2,000 instruments), studying (learning to speak Hebrew so that I can talk to my grandchildren; they teach here from 8 am to 1 pm, which is quite a strain [imagine starting at 8.00 am in Oxford!], plus homework), and lecturing.

First, may I remind Bruce Haynes (Comm.1082) that Eph is one person. Neither he, nor you, should judge FoMRHI on the attitudes or behaviour of one member. Eph has said this, too. This applies to other members and other authors of Comms, too. We print what our members send us because, whether we agree with it or not, and sometimes whether we understand it or not (I'd be the first to admit that Felix Raudonikas is way over my head; I don't understand what he writes, but what has that got to do with it?), because we believe that our members are entitled to say what they wish to the membership as a whole. Other members can then tell them what they think of it! That, to my and I'm sure to Eph's mind, is what international scholarship is all about. Other journals send things to referees, and if the article matches the referees' personal prejudices, it gets printed; if not, not. That's not how we work. But equally, no member, not even the editor, can be compelled to put finger to key (not pen to paper; we draw the line at that) if he doesn't feel like it or is too busy at present with other projects.
Now to Jon Swayne (Comm.1077), but on metal, not wood. When I was a student, I bought a horn from Tony Tunstall which, when he bought it, had no top B natural (neither as the open 15th harmonic, nor on the 2nd valve; it was a single F horn by Carl Kruspe of Erfurt, made before they were taken over and went off to America). He had blown it halfway on, so that it was a dangerous note which would sometimes appear and sometimes not. I finished it off, and by the time I parted with it, for which I've been sorry ever since for it was a lovely instrument, it was a good note. Now why and how could that happen? My assumption has always been that we had to move some of the shit that always accumulates inside an instrument so that it did not interfere with a node or an antinode. Bearing in mind that any instrument gets washed out from time to time, and that this did not affect the presence of that note, this theory may well have been wrong. All that I know is that we turned a bad and unreliable note into a good one. That does not help Jon very much, but merely adds some evidence to the effect that a player can indeed affect an instrument by the way in which he plays it.

Now to John Bence (Comm.1078). Here I speak from ignorance, but surely it wasn't only Italians who used short octaves? Didn't we have them in England, too, and in Germany? One fairly obvious explanation is to span wider chords in the bass, sometimes useful in continuo. I've been fascinated for years by the obvious fugue subject produced by playing both 'black' and 'white' notes straight up the short octave.

Finally to Charles Foster (Comm.1081). Eph is the editor, not me; he types the title pages, not me. I make typing errors, too, but not those ones!

Enjoy yourselves. I'll be with you in July. Paul's obviously doing a great job, both with the Bate (which I hear from our boss, Professor Trowell, and from others) and with FoMRHI, and I'll leave April to him. Weather here, by the way, has been frightful in patches; the worst snowstorms for over a century; we would pick this winter to come here. But the last couple of days it's been as warm as an English summer (and they threaten more snow for the weekend!).
Comm. 1085

BOUWBRIEF ARTICLES

R.A. Chiverton

Here is the next instalment of Bouwbrief indexes, bringing us very nearly up to date. I had no requests for translations (if I dare call what I would produce that), probably because my offer was rather a grudging one - but if you feel something might be important, you could always ask. It might go into the FoMRHIQ, too.

DE BOUWBRIEF 1991 59

Clavichords - Tuning & Maintenance II

Where have all the Ganassi recorders gone? Koen Vermeij

Notating hurdy-gurdy (barrel organ) rolls Alec Loretto

(discusses making the slotted strips) Tom Meijer

Humidity & Musical Instruments Paul Wiegels

Materials & Tools XIX

A simple wind supply for a tuning box Koen Vermeij

(pipe organ making)

From spindler to wind generator W Krieger

For Discussion

Steam chests and thermometers Mauritz Stakenburg

Commentary on the commentary on the Von Bennigsen bow John Boersma

André Klassen

Books & Drawings

Book Reviews

The Organ and the Dividing Line between Church & State (a publication of the Union of Dutch Organists) John Boersma

GSJ 41, 1988; FoMRHIQ 59, April 1990

DE BOUWBRIEF 1991 60

Designing a Lute

Wind Supply for home organs I

Small conical (organ! pipes of wood

Materials & Tools XX

Making tailpieces with fine tuners

New glues stick well

For Discussion

Steam Chests

Books & Drawings

Observations on "The Hurdy-gurdy (viellel Book", by Herman Devit and Toon Moonen André Klassen

Book Review

Woodwind Instrument Construction, Stages of Development and Technologies (Holzinstrumentenbau, Entwicklungsstufen und Technologien) by Günther Dullat (Moeck Verlag) Arnold Riesthuis

Galpin Society Journal 42, 1989; FoMRHIQ July 1990

DE BOUWBRIEF 1991 61

Wind Instrument Keys - an overview

Wind supply for home organs II

Materials & Tools XXI

Suppliers of Harpsichord Kits

For Discussion

Practical Tips for violin makers using the Hutchins method of tuning

H. Kauffeld

Books & Drawings

GSJ 43 1990

18th Century Dutch Recorders in the Hague Public Museum, by Rob van Acht, Vincent van den Ende and Hans Schimmel (Moeck Verlag) Arnold Riesthuis

Book Review

The Recorder, a manual for purchase, maintenance, secondary tuning and small repairs, by Jan Bouterse

Geert van der Heide

Description of the Hess bureau-organ in the Hague Public Museum John Boersma
There was a problem of communication between Honorary Officers about renewals this year. I did not receive a list of 140 renewals and stupidly did not realize that I hadn't. We have always had a problem of many late renewals, and I just thought that, with the recession and all that, it was somewhat worse than usual. I decided to try an experimental departure from our past practice and sent the January Q to all 1991 members in the expectation that the vast majority intended to renew, thus saving effort and frustration. A slip was included with the Qs sent to members who apparently hadn't renewed. The first section was intended for the very few renewals that sometimes get missed off on the lists coming from Barbara via Jeremy. The only way we find out about these errors is when a member complains, and this section was intended to generate the complaints early so that there wouldn't be an interruption in the flow of Qs.

After the mailing, when a flood of complaints came pouring in, it was obvious that something was wrong. Luckily, it didn't delay the delivery of any Qs. But many members felt offended by the tone of what I wrote on the slip, besides being annoyed about our error. I must apologise to them for that tone as well as the error, particularly for my role in it. This was to neglect checking with Paul White (Jeremy's replacement while he is away) whether I've been sent all of the renewal lists; they are numbered, and I should have noticed the missing one.

A few members who had paid for '92 hadn't remembered that they had, and sent Barbara another cheque. She has decided just to tear the second cheque up. If this creates any accountancy problems for anyone, let us know, and we will do whatever you say to put it right.

Apologies again. As with other problems in the past, this one is very unlikely to happen again.
Those of us who are snappers up of unconsidered trifles sometimes have in our memories ideas or odd items of information which won't make up into a full comm but might interest others and may even trigger the recall of other information which may help build a fuller picture of something. I here disburden my own memory of some of these.

1. I have a Gerock five key clarinet which has a barrel stamped with the name "W LAST" (no quotes on the barrel, of course, no full stop after "W" - just a space). No Last or Wlast in Langwill (5th ed.). Hitherto unknown maker? Repairer? Merely a vendor?

2. I have seen the name "Hirsch" on a latish nineteenth century flageolet in cocus or something similar in a Cheltenham antique shop some years ago. Not in Langwill. Maker or Vendor?

3. There is a picture by Marzal de Saz (active 1393-1410, Valencian school), the Altarpiece of St George showing angels playing shawms, one of which has a bottle-shaped bell, and may have a cylindrical body:

4. A picture by Georges de la Tour (1593-1652) shows two presumably itinerant musicians having a fight (it's called "Rissa di Mendicanti", so their status may not be high). One has a vielle strung round him, and is armed with a knife and a cranked handle for the vielle. The other is using a one-key shawm to defend himself and has a key-less shawm through his belt. They are being watched by a woman in a state of shock and a violinist closely observed in his turn by a bagpipe player (it took me a long time to realise what his instrument was) whose instrument appears to have a long cylindrical chanter with large finger holes, and may have a drone with a bulbous end (only the end is visible behind his head).

5. A watercolour by Queen Victoria (Queen Victoria's Scrapbook - Marina Warner, 1979. Papermac. Sketch of Rosina, p.73) shows a theorboed English Guitar which from the pegs has 10 fretted strings and 6 basses.

6. In Vitré (near Rennes) there is a castle with a fireplace with a decorated overmantle showing (if memory serves me) a combination of lute and shawms. I believe it's 17th century and not a result of later maintenance.

7. There is a little museum in Clun which has a Kusder clarinet.

8. Can one fine-tune violins by f-hole adjustment?

9. In the Courtauld Collection there is a "Madonna standing with child and angels" by Quin ten Massys (1466-1530) with quite a nice representation of an earlyish lute (with small soundhole (1/4 the width of the belly at soundhole height and bridge about a soundhole width from the bottom of the belly).

10. There is a flute in the little town museum in the centre of Diss (I seem to remember that it is at the back of a sweetshop), a six key (all pewter plugs) by Hodson, 45 High Holborne, London. Tuning joint marked with rings and numbers to show effect of pulling out.
The C.H. Brackenbury Memorial Collection has been given to the University by the Government following its acceptance by the State (as a collection pre-eminent in its field) in lieu of inheritance tax. The Collection was assembled in the 1920s and 1930s, and has been on loan to the University since 1980. The many important items include an ivory recorder by Richard Haka c. 1680, several early oboes, a clarinet by Thomas Collier c. 1770, a Strobach bassett horn, a Haas trumpet c. 1730, a Hintz viol, a double bass by Leopold Midhalm 1753, an 18th century viola d’amore, an arch-lute by Rotunery 1699 and two British bagpipes c. 1800.

In the year, the Collection has also been given instruments by W. Russell Day, Anne Macaulay, Nadine Parks, Raymond Parks and Edward Planas. Several further items have been lent to the Collection, including instruments from the late Christopher Monk, lent by the Honorary Curator.

The Honorary Curator represented the University at the CINCM (The ICOM Committee on Musical Instrument Museums) meeting in Osaka, Kakugawa and Tokyo. He is a member of the Documentation and the International Directory Working Groups of CINCM.

Plans for re-housing the Collection have progressed, albeit slowly: Mr Brian Hartley, the museum designer appointed to plan the fitting out of the premises as a museum, presented preliminary designs, layouts, specifications and costings. His work was funded with the aid of the Scottish Museums Council.

The sackbut by Anton Schnitzer dated 1594 was lent to the National Art Collections Fund for the exhibition ‘Saved for Scotland’ held in the National Gallery of Scotland in August and September.

The Collection has been used for teaching purposes by University Staff, in particular for courses in the Faculty of Music on the History of Instruments, Ethnomusicology and Musical Acoustics. Several parties have made organised visits, and various scholars and instrument makers have visited to study particular instruments.

Several instruments have been photographed to provide illustrations for a textbook “Companion to Musical Instruments” which is being written by Dr D.M. Campbell, Dr C.A. Greated and the Honorary Curator.
An Inexpensive, Easily Made Counter-Bore That Works

The diagrams below pretty well explain it all. The spade bit can be ground along the sides to accommodate any desired bore diameter. If care is exercised in the alignment and drilling of holes, one pilot can be interchanged on a number of spade bits.

It is necessary to anneal the spade bit prior to enlarging the top hole and drilling the lower one. Then it is an easy matter to re-temper the bit.

Steel drill rod the same diam. as your pilot hole is used for the counter-bore pilot. It is unnecessary to either anneal or temper this part.

Only the holes in the pilot are tapped. The holes in the spade bit are of a diameter for the cap screw to snugly slide through.

Good Luck.

Kimber Rhoads
Of all the various types of double reeds, perhaps the most enigmatic were those that belonged to the dulcian family. Evidence for the construction of these reeds is extremely scarce. The few scraps of information attributable to sources within the late-sixteenth, seventeenth, and eighteenth centuries are, for the most part, vague, somewhat contradictory, and second hand in nature. In 1986, Beryl Kenyon de Pascual's important article on the bajón (Spanish term for dulcian) brought to light the discovery of a few surviving historic reeds associated with these instruments. This provided the first known physical evidence, from any period, for reed designs used on dulcians. Prior to this revelation, the only evidence for dulcian reeds had been iconographical, which, when lacking other, corroborative evidence, is somewhat limited as a source of information about reed design during earlier periods.

The earliest iconographical source for a dulcian reed is from a painting in a convent in Cuzco, Peru, by Bernardo Bitti, dating from the last decade of the sixteenth century (See figure 3.1). The most familiar illustrations of the dulcian and its reeds are found in Praetorius's *Syntagma Musicum* of 1619 and Mersenne's *Harmonie Universelle* of 1636 (See figure 3.2). Other sources include Collaert (around 1690) and Giovanni Benedetto Castiglione's (c. 1610-1665) *Adoration of the shepards*. These sources, it can be argued, were conscious attempts, within their respective media, to illustrate dulcians accurately. All show a reed that is broadly tipped.

Praetorius and Mersenne are those most concerned with placing the instruments in their proper musical context, rather than as elements of decoration. Praetorius, for instance, includes in each of his illustrations a scale that allows an instrument to be systematically sized and compared to the other instruments. Nonetheless both sources, Mersenne's woodcuts and the engraving of

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1Beryl Kenyon de Pascual, 'A Brief Survey of the Late Spanish Bajón', *GSJ* XXXIV (1984), pp. 72 - 79.
4Collaert's engraving *Encomium Musices* (Antwerp, c.1590), illustrated by Jan van der Strael does not show its (three-piece?) dulcian with a reed, however the bass shaw next to it has exactly the same type of crook and aperture, therefore it may be assumed to have shared a reed with the dulcian. See G. S. Fraenkel, *Decorative Music Title Pages*, (New York, 1968), pg. 39. Other sources in Fraenkel where the dulcian's reed is not visible include pages 89 and 90. Giovanni Benedetto Castiglione's *Adoration of the shepards* in the church of S. Luca, Genoa shows a missectioned, double flare dulcian, being played with a short, wide reed, on what appears to be a short large calibre crook. See *The New Grove Dictionary of Musical Instruments*, edited by Sadie, (Macmillan, London, 1984), pg. 181.
Figure 3.2 Engraving from Praetorius showing dulcian reeds: 1, 2, 3, 4, 5, 6, and 7.

Figure 3.2(below left) Woodcut of Mersenne's fagot reeds (A, B).

Figure 3.2(below A) Woodcut of Mersenne's bassoon reed (A).

FIG. 3.1 Outline of a jointed dulcian and a corrente-player as depicted in Bernardo Buon's painting (1590-95) La Asunción de la Virgen (Convento de la Merced, Cuzco, Peru).
Praetorius (Plate X), are restricted by the printing technology employed. As a result, neither is capable of the resolution needed to show the finer details of the reeds, which, being the smallest objects illustrated, suffer the most from this particular limitation. In addition to this technical problem, one is also faced with the ever-present difficulty of knowing how accurate illustrators were in representing the functional features of these reeds. This is further compounded by the possibility of an illustrator's conscious choice not to clutter up a composition with the inclusion of smaller features, even where space permitted, purely on aesthetic grounds. Taken together these factors can pose problems for the interpretation of any information gleaned from the illustrations.

For these reasons, it is difficult to trust the representation of a reed's scrape in these sources. Was this because the printing process was inadequate to show this particular feature clearly? Or simply, did the artist fail to understand the importance of this feature and omitted its details out of ignorance? Or, contrary to modern expectations, was a lack of scrape intentional? Could it be that bark covered the entire blade surface from tip to end, and in lacking a normal exterior scrape, was still capable of vibrating properly? If this is true, did it fall on the reed's internal gouge to somehow compensate for the lack of scrape? Within our normal sphere of experience, from what is commonly known about modern double reeds, these questions are not readily answerable.

Another equally disturbing feature of early illustrations is that the reeds lack any midpoint holding or adjustment banding of the type normally used on bassoon reeds of later periods. This banding is essential to the construction and tuning of modern double reeds, if neither printing technology, nor the illustrator were responsible for this absence important considerations are raised about the fundamental performance characteristics of the early double reed. The following line of questioning begs asking. How would the two, unbanded halves of the blades remain firmly in place or be prevented from shifting apart? Without this banding, how would an arch be formed in the reed's throat of the degree we see in the modern reed? Does the lack of tension produced by such arching produces indicate that reeds were not adjusted through banding in the manner that bassoon reeds are adjusted today? Because the arching of the blades is dependent on the arching achieved at the throat, can one assume that these reeds were less arched at both points than the modern bassoon reed? Should this suggest to us that the blades lacked the degree of tension associated with this arching, and therefore required a different embouchure from that used for 'high tension' modern reeds? If embouchure were affected in this way, would not this also effect sound quality, instrumental blend, dynamic range, and articulation?

Because corroborative written information is nonexistent, the conclusions one can draw from this illustrative evidence is clearly limited. Unfortunately, it is virtually the only information available on reeds from the period when the dulcian was the dominant bass for the double-reed family. However, accepting this inherent limitation, there are at least a few useful clues the illustrations may still yield. In this case the most trustworthy information suggests general aspects of formal design, rather than detail, with the safest inferences regarding the materials which may have been used in reed construction.
Returning now to the dulcian reeds illustrated in all four sixteenth and seventeenth century sources, we can note that they take the form of short, straight-sided isosceles triangles. These reeds appear to be relatively broader at both the tip and the throat than the modern bassoon reed, and are bound on the tube’s surface with fibre rather than wire.

Quite remarkably, these illustrated reeds share many formal characteristics with the twenty-one surviving bajón reeds now housed in the Museo del Pueblo Español in Madrid. It is noteworthy that these archetypal patterns (short, wide-tipped, flat-throated, deltoid shaped) appear to have survived with this residual Renaissance instrument in Spain, as it continued in use well into the nineteenth century. Taken together these two forms of evidence strongly suggest that this particular form of reed design is system-specific to both the dulcian and its Spanish counterpart, the bajón. If it is accepted that these instruments represents a little altered instrumental lineage that stretched from the middle of the sixteenth century until the beginning of the nineteenth, then it seems likely that the acoustical nature of the dulcian requires a broad, flattened throat and wide tip to produce the sound and performance characteristics considered desirable during this historical time-frame. It then follows that to perform, optimally, the repertoire written for these instruments during their history, this same type of reed must be used to tune and play reproduction dulcians.

In 1990 I had the opportunity to document the group of reeds (canas) mentioned by Beryl Kenyon de Pascual in her 1984 article. The reeds survive with four wind instruments. Three of these are dulcians: a small octave bajoncillo, signed SELM; an anonymous bass bajón (double curtal or Chorist-fagott); and an anonymous three-piece bajón , with late-eighteenth century bassoon-like features. The fourth instrument, an anonymous five-key bassoon, combines both

5See Pascual (1984), Plate XI for a good photograph of the three piece instrument. The three joints include wing, boot, and a long joint of normal length, ending in a flare, rather than tenon. This unusual feature apparently functions as the bell, and presumably limits the range of the instrument to low C. The instrument differs considerably from the three-piece dulcians in Vienna and Brussels, both of which seem in the transitional instruments from the seventeenth century. In this case the boot and wing joints share common characteristics with the stream-lining and keywork exhibited on late-eighteenth century bassoons. Beryl Kenyon de Pascual demonstrates that the bassoon made few inroads into Spain throughout most of the eighteenth and early nineteenth centuries, and did not seriously challenge the dulcian until the last few decades of the eighteenth. It seems likely that the bassoon and bajón were not in direct commercial competition until about that time. This is supported by a 1785 advertisement by Ferdinando Llop for sales of both one and three-piece bajón (p. 75), which raises some interesting possibilities as to why this instrument was made. Clearly, the instrument was not transitional, because the fully developed bassoon already existed. Why make an instrument like this, when the bassoon was, presumably by then, a technically superior instrument? There are many good reasons for retaining aspects of the Spanish instrument: a preference for its sound quality, resistance by local musicians to change; a lack of need for technological improvement to play the existing repertoire; or the existence of an unusual local pitch standard. If the bassoon could not adapt easily to suit these local musical requirements, then the logical alternative was for the local bajón maker to incorporate just enough of the useful design features of the bassoon into his newer instruments. Making new reamers is an expensive, laborious, and not always successful endeavor. It would have been simpler to incorporate as many of the existing bore and acoustical features of the bajón into the portable form of the multi-jointed bassoon. Mrs. Kenyon de Pascual presents one last clue as to why the bajón was eventually jointed. On page 75 she relates the details of a controversial audition in 1799, where two bajón players and a German, Matthias Weseley (possibly using a three-piece instrument), were in direct competition for a post as cathedral bajónista. Weseley was deemed to have an unfair advantage because his instrument could play more softly and was not a true bajón, being more bassoon-like.
French and German features: Porthaux/Prudent-like long joint and boxwood wing; Jehring-like bell; and odd, single-touch, F and Ab keys, both of which are unique in having a sharp angularity to their shape.

Of the thirty-three reeds currently in the museum's collection, eleven are easily identified (Reeds 1-11) as being mid-to-late nineteenth century bassoon reeds, and are assumed to have been used with the five-key bassoon. None seems small enough to have fitted the octave bajoncillo. The remaining reeds (Reeds 12 through 32) display designs and construction techniques that strongly resemble the evidence of Mersenne and Praetorius, and therefore are unique in comparison to the other surviving historic specimens of bassoon-type reeds in the world's private and public collections.

The Bajón Reeds

The following twenty-one reeds were originally sorted according to obvious visual similarities in scrape, materials (wrap, wire, cane) or other identifiable features of reedmaking technique. The sequential grouping found below was ordered initially to assist in determining whether a common hand was involved in construction and which, if any, of the reeds may have belonged to each of the collection's instruments. This sequence follows the preliminary documentation work carried out at the museum and remains in keeping with the museum's current catalogue. Subsequent to this, a closer investigation of the collected data indicated that some of the reeds originally thought different seem to bear a closer relationship to each other than was previously realized. Rather than reorder the sequence along these revised lines (thus both confusing the issues and any future investigations carried out in the museum), I have chosen instead to deal with the alternative possibilities for regrouping these reeds in the accompanying text.

Beginning with the more general features of this entire group of reeds, it appears that most, if not all, are hand gouged. The primary reason for this assumption rests squarely on the wide variation in tube and blade thicknesses, within the expanse of one side and between both halves of the reed. Further, most seem to have been hand shaped, this assertion is based on slight irregularities in symmetry and shape within each reed and amongst all the reeds. One has the sense that they have been made only with the aid of an eye guiding the hand, although in a highly skilled manner. Each reed has a (relatively) straight-sided, deltoid shape, in keeping with Mersenne and Praetorius. Almost all of the reeds have an extremely short, shallow scrape, ending considerably further from the first wire than on a modern bassoon reed. Unlike Mersenne or Praetorius, these

6The use of reeds from the third-quarter of the nineteenth century on an instrument built roughly seventy years prior to this, is indicative of the slow evolutionary process involved in reed development. See Paul White, 'Bassoon Reeds by Triébert and Massabo' FoMRH Quarterly No. 56, July 1989, pp. 27-40.
7This amounts to a cigarbox full of reeds tagged with the numbers I have listed below, along with photo copies of my measurements and drawings.
reeds are wire banded, as are bassoon reeds of later periods; however this feature differs markedly from later reeds in that each wire collar has a flattened, elliptical shape. This produces a broad flat arch in the reed’s blade area and a tube flattened into a similar shape, becoming round only at the base of the reed where the crook enters.

The group as a whole can be divided into two sizes. The shortest reeds (12-28) are remarkably similar to each other. Any disparity in size and shape is not beyond the expectations of reeds made to suit a particular bassoon, nor unreasonable considering that each reed appears hand shaped without use of a template. Most of these are around 55 mm long, with tip widths of 18-19 mm, and approximately 12.5-13.5 mm at the throat. Reeds 21, 26, 27, and 28 are of a similar style, but slightly longer at around 59 mm. Such a slight discrepancy is entirely acceptable when considering that any of the shorter reeds may have begun life at that length, but after considerable use were shortened, and retuned to extend their life span. The longer reeds (29-33) are much less homogeneous when compared amongst themselves, yet all are considerably longer and bulkier than the preceding shorter reeds, and considerably wider than the bassoon reeds (1-12). Lengths range from 62-66 mm, with tips as varied at 16, 17, or 20 mm, and 12.5-15.5 mm at the throat.

It is impossible to say with certainty which reed size can be associated with the one-piece bajón and which with the longer three-piece bajón. However, grouping the longer, bulkier reeds to the later three-piece instrument seems a viable option if we take into consideration a similar length for bassoon reeds used during the late-eighteenth and early-nineteenth centuries. Similarly, linking the shorter reeds to the single-piece, bass bajón is in keeping with the seventeenth century iconography commonly displaying short, wide reeds. While it is theoretically possible that any of the reeds may have been made for the bassoon, on the whole they appear disproportionately wide for use on that instrument. One, of course, cannot discount the possibility that the shorter reeds could have been used to raise the pitch of the three-piece instrument or that the longer reeds were employed to lower the pitch of the one-piece dulcian, but this seems the least likely of these speculations.

Eight of the smaller reeds (13-20) appear to be made by the same hand. Several of the reeds (smaller 21-25; larger 29-31 and 33) are distinct in being held together by four or five separate, single wires. This particular feature has not been observed on any other of the hundred-odd historic bassoon reeds documented so far; this unique aspect suggests that all may have been built by the same reedmaker (or a reedmaker imitating an eccentric reedmaker). In this case the overlap between both sizes suggests someone making reeds for two different instruments, such as the one and three piece bajóns. Perhaps the same player was playing both instruments together within his

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8 All of these reeds are assumed to be too large to fit the octave instrument. The most obvious indication of incompatibility being their inability to fit onto what must have been an extremely narrow crook aperture. Each of these reeds is suitable for use on crooks with more normal sized openings of between 4-6 mm.

9 In hindsight I realize now that the tips of the crooks belonging to these two instruments should have been measured and observed for signs of seating wear. This data would have been helpful in assessing which reeds might have belonged to each type of bajón.
own lifetime or moving sequentially from one to the next during his career. Of this subset (21-25) of smaller reeds, 23, 24, and 25 differ in not having a turkshead wrap.

Reeds 29-33 are markedly larger, with 31 and 32 narrower at the tip and throat, looking more bassoon-like than the other three. Some of this group may have been made for the five-key bassoon, possibly demonstrating a technological over-lap between the bassoon and three-piece bajón. However, because of their larger size and breadth, relative to the bassoon reeds, the three-piece bajón would seem a more likely candidate for most of these. Reeds 26, 27, 28 are loosely grouped in with 21-25 because they share similar shapes and sizes, although differ in other respects.

**Shorter Reeds**

An extended analysis of Reed 12 should prove the most efficient way to explore how bajón (and thus dulcian) reeds differ from what is known of other bassoon-type reeds, both modern and early. There are distinct advantages in proceeding with this particular reed, as many of its features are remarkably similar to those of other shorter reeds.

Fortuitously, Reed 12's holding wires were loose enough to allow its blades to be completely separated, thus affording a rare opportunity to examine how reeds were gouged in earlier periods. Its lack of a wrap allowed both the interior and exterior surface of the tube to be examined closely. Another fortunate coincidence is that this reed appears to be among the oldest in the Madrid collection and may well prove to be one of the oldest surviving double reeds in the world today.

One of the most striking aspects of this reed (and similarly reeds 23, 24, and 25) is the manner in which the bark on the rear portion of the blade has been whittled away, rather than scraped, in order to expose the softer under-structure of the cane. The cutting or scooping action that produced this result involved a short-stroked, inward and outward motion, differing considerably from the pushing or dragging motion inherent in a normal reed scrape. Visually, the texture (B) resembles the rippled surface seen on a prehistoric hand-axe or arrow-head. The effect has left a random mixture of (softer, inner pith) parenchyme and (harder, outer material) dermis on the rear portions of the blade. Normally one expects a gradual planing through this area, leaving the surface exposed in either one or another of the cane's stratum -- never, as is seen here, with interwoven strata. This technique has not been observed on any other historic bassoon-type reed. Initial impressions seem to indicate that this, rather crude looking scrape was the result of someone unskilled at reed making.

What makes one hesitate to assume this results from haphazard, amateur technique is the refined construction and distinctive quality of the crafting apparent on the reed's other features. The tip area consists of hard cane with a fine-grained pattern, all of which has been carefully

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10 Given the complete lack of written evidence about dulcian reeds from any period of history, perhaps a more suitable word might be 'pre-historic.'
cleaned and smoothly polished. With equal care and attention, the tube area has been scored half of its length with very fine, regularly spaced, carefully controlled cuts. These well-crafted features contradict amateurishness and suggest that the oddness of the scrape was intentional and may have served some special, as yet undiscovered, acoustical or articulatory purpose specific to this reed.

This and many of the other reeds have very thin, irregular gouge thicknesses averaging .6-.8 mm (1-1.25 mm is a more appropriate average for the modern bassoon reed). One half of the reed is of a different thickness to the other half, a factor which seems to indicate that the cane was gouged by hand. Examination of the underside of each half of the reed reveals some striking insights into the gouging process. Contrary to expectations the tool marks exhibit no sign of a long even gouge, nor any trace of a gradual tapering from the base to the tip of the reed. It is clear that the area directly under the blade has had extra gouging treatment. The resultant profile is of a long, stepped pattern, whereby the material under the blade area was thinned considerably more than the remainder of the reed's gouge under the throat and tube (See figure 3.4).

**Figure 3.4**

The transition between these areas is unexpectedly abrupt. This profile is borne out by thickness measurements taken from the exterior of the blade, demonstrating that the blade is of a relatively uniform thickness from tip to back (.4-.6 mm) and then suddenly becomes thicker at the point of the gouge's step. The interior texture exhibits deep, rough furrows running the entire length of the cane, with no sign of sanding or smoothing anywhere on the the under surface: a treatment common on the modern bassoon reed. This absence seems deliberate as the exterior surface has been smoothed and polished. Enough thickness coordinates have been measured to perceive a pattern, whereby the sides of each gouged blade are thicker than its center portion. The reverse is the case with modern (German) bassoon reed-making, which creates a raised spine in the center line of the blade.

The ferrous wires (single band, .6 mm gauge, rusted) are elliptical and keep the reed flattened at the throat. This flattened-ellipse shape continues through most of the length of the reed. Because the reed has completely lost the wrap that covers its tube, it is clear that the reed is triangular for almost its entire length, with the tube becoming cylindrical only at the absolute bottom where the crook enters. Residual indentations indicate that the base of the tube was formed round and held at one time with a holding wire of some sort.

Determining the age of any old reed can be difficult without verifiable, supportive documentation. Lacking a maker's stamp, inventory slip, or datable associated evidence one must turn to the colour and texture of the cane, the reed's shape and size, and the execution of its construction and design for hints of a possible age. Not knowing the conditions of storage or the
specimen's history before its arrival at the museum, it is difficult to know with absolute certainty if the reed is in fact very old or has prematurely aged in less than ideal conditions. However, when compared to the world's corpus of surviving bassoon reeds, the cane in this reed seems considerably older than any other observed, excepting reeds (23, 24, and 25) in this same collection. The style and materials used in the construction of this reed suggest that it may actually be an original reed dating from the seventeenth or eighteenth centuries, which has been continuously passed on with one of the collection's instruments long after it had become unusable. An alternative possibility is that it may have been made during a later period, but patterned on an older reed or in imitation of older construction methods passed down as part of a local bajón tradition. In either case it represents a distinctive type of double reed and one I find particularly pleasing to the eyes in its formal composition.

Reeds 13 - 20 are well crafted, and quite possibly built by a single professional player or reedmaker. On the whole all have a blade that is more spade-shaped than Reed 12. Features common to all include: a dark coral/rose, coloured wrap; a two-wire, double-twist, banding system; and the corners of the blades' tips clipped at a 45° angle. All are short, broad tipped, wide throated, and are not highly arched at either the blade or throat. The following should give an indication of how similar they are to each other in these respects (in mm: length/tip width/throat width): Reed 13, 54.2/18.7/13; Reed 14, 53.7/19/13.5; Reed 15, 55/18/12.5; Reed 16, 53/18/13.5; Reed 17, 55/19.2/13.5; Reed 18, 55/18/13; Reed 19, 55/18.5/11.5; Reed 20, 55/19.2/12. It is notable that the gouges on these reeds are considerably thicker than that of Reed 12, with thicknesses ranging between 1 and 1.5 mm. Again, discrepancies between blades suggest that these have all been hand gouged.

Reed 13 is remarkable for the expanse of dermis that entirely covers both blades, showing none of the softer parenchyme.

Reed 14 has residual crook impressions demonstrating insertion to only 3.5 mm. The first wire is missing, but has left an indentation that allowed measurements to be estimated. Again, the blade is comprised entirely of dermis.

Reed 15 is similar to the preceding two reeds.

Reeds 16 and 17 resemble each other more than the others in this subgroup, perhaps represent a secondary 'batch' by the same hand. They have thick, unrusted wires, with a tube covered in a waxed, darkish-pink string. A small design differential can be noted in the overall profile of the blade, exhibiting a slightly more exaggerated spade shape at its confluence with the tube. Reed 16 was originally about 20 mm wide before losing a slice from the side of its tip.

Reed 18 is missing its second wire. Its wrap, wire size, cane, and scrape characteristics are similar to Reeds 13-15. Like Reed 17, a crook seems to have been inserted to 3.5 mm. Portions of the edge of the tip are missing indicating that the blade was once much wider.

Reed 19 has residual crook impressions which again shows insertion to 3.5 mm, possibly relating it to Reeds 14, 17, and 18, both of which had the same markings. A small portion of the tip has exposed some parenchyme.
Reed 20 may have been marked with a maker's stamp. Unfortunately, the only trace is an undecipherable, odd 'y' shape, and therefore it is hard to tell if this is, in actual fact, a stamp, some form of scarring, or merely a blemish in the cane. The blade area is covered in dermis.

Reeds 21-28 are less neat and tidy in appearance than the preceding reeds. Several of these (21-25) have the multi-wire system mentioned above, usually four or five single wires, each wrapped once around the reed's throat. This unique feature suggests that all the reeds so fashioned were probably constructed by the same maker and any variations can be attributed to the materials available when different 'batches' were constructed.

Reeds 23, 24, and 25 have had their blades scraped in a very similar manner to Reed 12's blade, leading to speculation that all were made by the same maker. The blade in each appears uniformly gouged and scraped over the entire surface length to thicknesses averaging between .4 and .6 mm, again an abrupt step appears where the blade ends. The handmade gouges range between .7-.9 mm. As with Reed 12, these reeds also appear to be much older than the remainder of the reeds in the collection and the world. A quick comparison of measurements exhibits a slightly greater degree of variation amongst this subgroup (length/tip width/throat width): Reed 12, 54/17/12.3; Reed 23, 56/17/12.5; Reed 24, 57/17.7/13.5; and Reed 25, 55.2/18.2/14.1.

Reed 22 has five, separated, brass wires holding it together, similar to Reeds 26 and 27. The wires hang quite loose now, not unlike bangles, so it is difficult to tell how tightly they might once have been cinched. The impressions left on the cane suggest that they were grouped together three at the front and two at the back. These wires form very flat ovals, and although they have moved from their original positions, it is clear that they once molded the cane at the throat into a broadened, low-arched form. The tip has probably been shortened from its original working length and now appears as if it had once been crudely chopped with a blunt knife. The consequence is an uneven, ragged edge that probably would not have produced an acceptable response. Similar to Reed 12 it has been thinly gouged and with dermis covering virtually the entire expanse of the blade (see drawing). A greyish coloured, waxed-thread binding covers the tube.

Reed 23 appears to be very crudely made, indicating it might be from well before the nineteenth century. Once again, the whittled scrape observed on Reed 12 appears through much of the dermis area, with the scrape on one side of the reed being quite rough and unrefined. One blade was badly cracked and disintegrating to the extent that it could not be properly measured due to its fragile nature. The tip is unevenly cut similarly to reed 22. The tube is wrapped in waxed grey string, without a turban, but held on by one exterior wire. There are four separate ligature wires, all of a thin gauge.

With Reed 24 I managed to produce a very thin, high-pitched crow. This, of course, couldn't be properly soaked and therefore cannot be representative of the sound originally produced by the reed, but does at least demonstrate that this reed is capable of producing a sound, were it to be soaked. The whittle scrape described in the Reeds 12 and 23 is also seen on this reed, but in this case the reed appears to have had additional scraping done to the central area of the tip,
the length of the middle axis, and the extreme end of the blade where it makes the transition into bark material (See below). The cane along the sides where the halves of the tube meet is very thin and has started to shift out of registration. Its holding wires correspond to those on Reed 23.

Reed 25 has a large portion of one blade missing. Nevertheless, it is a very interesting specimen in that the whittle scrape appears only on one blade, while the opposing blade has had much attention paid to creating a smooth and highly polished surface. This juxtaposition suggests that the combination of these two scrape patterns may have been intentional and sequential, with one process following from the other as a counterbalance. This is in contradistinction to one of the tenants of one school of modern bassoon reed making, which holds that balance is of primary importance: that each side of a reed's blade should create a mirror image of the other and that both blades should also mirror each other.\(^\text{11}\) The ligature is made up of four separate wires, all now loosened, as is the case with Reeds 23 and 24.

Reed 26 has a single ligature made up of a thin-gauged, brass wire, twisted around three times. The shape of this reed is bassoon-like, having a markedly pinched waist at the throat. The single wire at the throat appears a conscious choice, as there is no impression left by a second ligature, however another holding wire was purposefully placed, rather precariously, at the absolute end of the tube. The tube is covered in a fine green thread, tightly wrapped without a turban. In hindsight it now seems probable that this reed is out of place in this grouping of bajón reeds and it may, along with the Massabo and Triebert reeds, have belonged with the five-key bassoon. Conversely, it may have been made for one of the bajon, but during a later period when the influence of the bassoon was more pervasive.

Reed 27 is wrapped in a thin, purple-coloured thread that ends in a turban. The two thin, brass wire ligatures are unusual in being triple-twisted at the front and quadruple-twisted at the rear. The scrape at the extreme end of the tips has exposed a straight swath of parenchyme, but apart from this small area, the remainder of the blade material is of fairly uniform dermis material.

Reed 28 afforded another exceptional opportunity to observe the underside of the cane for signs of internal profiling. In this instance, its broken state allowed the wires to be easily removed and replaced without damage to the reed. The inside revealed a very crudely gouged area, quite rough in places and without a hint of smoothing by abrasives. This is remarkably similar to what was observed on the underside of Reed 12. The blade has been scraped in an almost identical fashion to reed 27. The wrap has not survived and the wires are single twist.

**Longer Reeds**

The most obvious factor that sets Reeds 29-33 apart from the preceding reeds is that their greater length. If other features of reed design are compared (scrape, construction method, materials, and shape) these larger reeds, on the whole, appear more closely related to the previous shorter reeds than they do to the collection's eleven, nineteenth century, bassoon reeds by Massabo.

or Triébert. This relationship is further strengthened by the fact that, although longer, they retain the same proportionately wide throat as the smaller reeds, and therefore would probably not have been suitable for any eighteenth or nineteenth century bassoon needing to play in the upper-tenor range. Nonetheless, it is difficult to say for certain that one or two of these reeds were not used on the collection's five-keyed bassoon, or more remotely, that all of the longer reeds may have been compatible with both the bassoon and the three-piece bajón. One can envision a scenario where, just as a bajonista may once have played both one and three-piece bajón, in the late-eighteenth century, so too a bassoonist long after the last bajonista had retired, might have been called in to play the occasional late nineteenth bajón part.

Reed 29 exhibits obvious signs of radical shortening, resulting in a reed length of 62 mm. This assumption is based on two factors. From above, it can be seen that the tip of the blade has been cut at a slant (approximately 15°) where normally one would expect a cut perpendicular to the reed's length. This feature, along with the extreme thicknesses of the blades' tips (.5-.7 mm), would have made the reed unplayable in its present condition. Assuming that a tip thickness of .3-.4 mm is a normal requirement for acceptable articulation in early reeds, it then becomes possible to project a reasonable estimate for the reed's original length. The thickness coordinates for Reeds 30-33 indicate that thicknesses of .5-.7 lie approximately 5 mm behind the tips that have a thickness of around .4 mm. The incorporation of this figure into a projection of the reed's original length would extend it to around 66 mm.

By any standard of reed design, this reed is large, wide, and thinly gouged. The width of the tip at 20 mm is, along with reed 33, the widest of the group and it may have been even wider before being chopped off. One unusual feature of this reed is that the average thickness of the blade varies little from that of the relatively thin gouge (.7-1 mm) which is present throughout the remainder of the reed's body. A double ligature, composed of four separate wires (a fifth holding wire sits on the exterior of wrap), along with the thin gouge, suggests the possibility of this having been made by the same maker as reeds 12, 21, 22, 23, 24, 25, and 28.

Reed 30 has had its tip cut to a slightly crescent-shaped, convex curve. Compared with Reed 29, the gouge of the tube is fairly thick at 1.4 mm. The blades have been burnished to a smooth, hard finish. Whether intentional or not, one side of each blade is thicker, with the midpoint being slightly thinner, and the opposite side thinner still. It has been wrapped with a white waxed-thread of the kind used on Reeds 31 and 32. Like Reed 29 it has a separated wire, double-ligature system, but in this case, as in Reeds 31 and 33, there are five separate wires, grouped three in front and two at the back. Were it not for this wire pattern Reed 31 can be described in almost the same terms as reed 30, except that its gouge is considerably thinner at 1 mm. Although it is different in size and proportion, it shares a remarkably similar scrape with Reed 30. Both have the horizontal gradations (rather than the more typical U shape of early reeds) of strata often found in twentieth-century, French-system bassoon reeds.
Reed 32 corresponds closely to 31 and 30, except it is gouged considerably thicker at 1.6-1.75 mm. It also differs in having only two single wires, although they are cut, twisted and finished in a similar manner to the five wire systems of Reeds 30, 31, and 33. This seems the most bassoon like of all the larger reeds.

Reed 33 is similar to the previous three reeds, except for its thicker waxed wrap and five separated heavy-gauge wires that are silver coloured. The exterior silhouette of this and Reed 29 are remarkably similar, though the throat of this reed is narrower.

Summary and Conclusion

Because all thirty-three of the collection's reeds have been jumbled together without a record of when or how the reeds arrived, the most ticklish problem is to decipher which of the thirty-three reeds belonged to which of the three larger instruments.

I have proposed that the shorter reeds (12-28) belonging the one-piece bajón, an instrument that could date from as early as the seventeenth century or as late as the nineteenth. This association is based primarily on the similarities in size and shape that these reeds share with the dulcian reeds seen in sixteenth and seventeenth century iconography. The eleven bassoon reeds by Massabo and Triebert, it is fairly safe to say, were used with the collection's five-key bassoon. This leaves finding an instrument for the, less uniform group of longer reeds (29-33). The obvious choice is the three-piece bajón. For the sake of argument we will assume that this group probably was used on either of the jointed instruments, the bassoon or the three-piece bajón, both of which share similar design features and appear to date from the last quarter of the eighteenth century. As a sub-group the longer reeds are not as homogeneous as either the bassoon reeds (Reeds 1-11) or the shorter reeds (12-28). If examined individually the one common feature of the former group is that each seems to share design features with both of the two other reed groups, just as the three-piece bajón also shares similarities with both the bassoon and the one piece bajón. Generally speaking, the larger and smaller reeds share similarities in scrape, throat and tip widths, shapes, and ligatures, while on the other hand, the longer reeds also demonstrate similarities to the nineteenth century bassoon reeds in terms of length and, in some cases, a narrower tip width. Observing then that the longer reeds appear to fall into both camps, bajón and bassoon, we must question what factor or factors make it more likely they might be a part of one, rather than the other.

Of the collection's thirty-three reeds, the eleven bassoon reeds have not, as of yet, been addressed in any detail. Because these reeds are fairly typical examples of nineteenth century bassoon reeds and clearly intended for use with the five-keyed, late-eighteenth century bassoon, introducing them prior to this point would have detracted from the discussion focusing on bajón.

It is noteworthy that dulcian reeds developed for use on reproduction instruments, during the period from the nineteen sixties to the present, share roughly the same proportions as this group. This lends weight to the theory that short, wide throttled, and wide tipped reeds are system-specific to the dulcian.
reeds. However, they now provide an excellent opportunity for comparison, and can assist in associating the larger reeds.

The group of bassoon reeds were made professionally by either Massabo or Triëbert, both of whom were active as reedmakers in Paris during the 1860's. All eleven are fairly uniform in design and are very similar to the reeds described and illustrated by Étienne Ozi at the end of the eighteenth century. The following is a brief description of the major features of the reeds:

- lengths range from 61.5-66.5 mm;
- tip widths range from 15.6-18.5 mm; and
- the throat, measured at the first wire, is more tightly ranged from 9-11.5 mm, with a majority having 10 mm (other, more detailed, aspects of these reeds will be subject discussion later in the text).

It was stated above that the longer reeds (29-33) are statistically similar in lengths (61.5-66 mm) and tip widths (16-20 mm) of the bassoon reeds (compare above). It is understood among modern reedmakers that the longer a bassoon reed, the narrower it must become at the tip and, most especially, at the throat, if this is not the case articulation throughout the instrument is sluggish and the upper end of the tenor range becomes difficult to control. Generally speaking, bassoon reeds from the eighteenth and nineteenth centuries share similar lengths with the longer reed group, but tend to have much narrower throats and, somewhat, narrower tip widths. If we examine the eleven bassoon reeds and compare them to the longer reed group we see a similar pattern. However, these two groups differ significantly at the throat. The bassoon reeds' throat width which averages 10 mm is significantly under that of the longer reeds, which range from 11.2-15.5 mm and average well above 12 mm. If we include measurements taken for the height of the throat this discrepancy becomes even clearer. The height of the throat of bassoon reeds range between (front wire/second wire) 5-7.5 mm/5.5-7.9 mm, while Reeds 29-33 are 4-6 mm/4.5-6.5 mm, mostly on the lower side. The combination of these figures for height and width indicate that reeds in the longer group have very wide, flat throats, strikingly similar to the shorter reed group, and are markedly different from the rounded throats of the bassoon reeds. This flattening of the throat effects a lower tension on the reed's blade and as a direct result favours the lower tessitura of any instrument to which it is applied. Of the two jointed

13 Although it must be kept in mind that it is possible that the bassoon reeds may have been used on either of the bajone in the late-nineteenth century, it seems unlikely that this would have been an optimum solution for finding working reeds for these 'outdated' instruments. I realize, that comparing late-nineteenth century bassoon reeds with what may possibly be late-eighteenth century bajon reeds is not an ideal situation. On the other hand, the nineteenth century bassoon reeds are examples of reeds that were used on a late-eighteenth century bassoon, therefore have some validity as a usable reed pattern for this instrument.

14 It is noteworthy that these bassoon reeds were, at least somewhat, compatible with a bassoon designed and built in a period approximately seventy years before this. In actual fact the reeds differ very little from those in use in the late-eighteenth century. Clearly, prior to the twentieth century, the evolutionary change in the design of reeds was slow.

15 One obvious exception to this is the contra-bassoon reed, which must retain a wide blade to ensure (if not favour) bass response. Another function of the large throat opening is to allow the great volume of air needed to fill the instrument.

16 This is presumably due to the higher tessitura required of the bassoon during this period: often considerably above the upper limit g' normally associated with the dulcian. It also may be related to the bassoon's longer, more gradually sloping bore profile.
instruments, this type of reed would seem to be better suited to the wide bore and lower ranging three-piece bajón, rather than the narrow bored, wider ranging bassoon.

There are other more subtle differences between the group of larger reeds and the bassoon reeds. The gouge for the bassoon reeds is extremely thick, anywhere from 1.65-2.35 mm, whereas that for Reeds 29-33 at .7-.175 mm is considerably thinner. Another consideration is the thickness cross-section for the blades of the reeds. The bassoon reeds, when graphed, exhibit a sharper rise to the slope. For example, Reed 2 rises over 5 mm intervals: .35, .65, 1.15, 1.25, 1.35, to a gouge of 2 mm. In contrast Reed 31 rises in a relatively flat manner: .35, .57, .6, .67, .8, to a gouge of 1 mm. This demonstrates a marked difference in how the gouge and scraping profile interact in these two reed designs. The group of longer reeds have the flat, thick blade common to the shorter bajón reeds and similar to the gouge in figure 3.4, whereas the bassoon reeds exhibit the thickness profile commonly seen on bassoon reeds from the modern period. Closely related to the difference in gouge and scrape exhibited by both groups of reeds is the difference in approach to exposing stratum used by each group. The bassoon reeds' blades are usually extensively scraped well into the lower, softer strata of the cane, where the group of longer reeds use much harder cane material located closer to the exterior of the cane.

Although one can never be absolutely certain that the longer reeds were used on a bassoon, the weight of evidence seems to tip the scale in favour of these reeds having been used on a bajón of some sort.

Guide To The Measurements For The Bajón Reeds

<table>
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<tr>
<th>Measurement</th>
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<td>External Diameter</td>
<td>Provides the outer dimension of the reed</td>
</tr>
<tr>
<td>Internal Diameter</td>
<td>Provides the inner dimension of the reed</td>
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<tr>
<td>Length to Front Wire</td>
<td>Distance from the front wire to the tip</td>
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<td>Distance from the back wire to the tip</td>
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<td>Width at Back Wire</td>
<td>Width measurement at the back of the blade</td>
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<td>Width at Front Wire</td>
<td>Width measurement at the front of the blade</td>
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<tr>
<td>Thickness</td>
<td>Thickness measurement across the blade</td>
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<tr>
<td>Length of Blade</td>
<td>Total length of the blade</td>
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<tr>
<td>Tip Width</td>
<td>Width at the tip of the blade</td>
</tr>
<tr>
<td>Total Length</td>
<td>Overall length of the reed</td>
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</table>

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arch at front wire 5
arch at back wire 6

arch at front wire 5.5
arch at back wire 6.1

arch at front wire 5
arch at back wire 5.7

arch at front wire 4.7
arch at back wire 5.5
arch at front wire 4.5
arch at back wire 5

arch at front wire 4.5
arch at back wire 5

arch at front wire 3.5
arch at back wire 5.5

arch at front wire 4
arch at back wire 4.5

I- J
arch at front wire 4
arch at back wire 4.5

arch at front wire 5.5
arch at back wire 6.5
arch at front wire 5.8
arch at back wire 6.5
According to David Way, the sensitive hearing of the musician is maligned by the clumsy scientist who claims that the musician imagines what he hears. If a scientist makes such a claim (and I haven't noticed this), it is a personal one with no more authority than a layman. The musician and the scientist are reporting different aspects of reality, which I hope to show are complementary rather than conflicting.

Theory

Fundamental to any understanding of reality is ‘subjective reality’. This is the result of each person's individual experience in life, including how we understand and respond to the inputs our senses give us about what is going on outside and around us. The understanding is in terms of mental images or models of what and how things happen that the sensory input information is fitted into as best as one can. When the expectations of our models are violated by the input information, the models are either protected or modified. Protection is provided by ‘exception’ models that allow violations in particular circumstances (eg magic shows or unexpected intervention by outside factors). Another protective strategy is to ignore or avoid input information that can cause such trouble (and extreme of this is the type of mental disorder in which the individual gives up trying to reconcile input information from the senses with the mental models of subjective reality). A different kind of strategy is learning. Learning involves modification of old models or adding new ones to make better fits with old and new input information. Exploration induces new input information that can be part of a protective or learning strategy.

Studies have shown that a large fraction of communication between individuals is for the purpose of comparing each person's models of subjective reality with others to arrive at consensus. By this means large numbers of people can, with mutual support, share models in what might be called 'consensus reality'. Included in consensus reality is what can be called 'objective reality', which everyone (ignoring the mentally ill and a few philosophers) accepts as facts, being sure that everyone else (whether respected or not) would not disagree. Varying interpretations of the facts (if they are not considered facts themselves) are included in subjective and consensus reality, but not in objective reality.

Everyone who wants to convince someone else of something will cite what he claims and hopes will be considered facts in his arguments, trying to induce the learning response he wants. No one can be aware of all the facts in objective reality, and everyone learns to be skeptical when they are told about facts they didn't know about by people who want to convince them of something. Science is treated in a somewhat different way.

The objectives of science are to explore objective reality, adding facts to it. The training and traditions of science are to make observations and measurements in as unbiased a way as is possible, no matter how much emotional investment the scientist may have in a particular outcome. Non-scientists are aware of this, and as long as other scientists don't seriously question the observations and measurements (which is usually the case), these are added to the facts of objective reality without skepticism. A scientist usually reports his observations and measurements together with a theory (model) which generalises on them and suggests the significance of his results. When such a theory is presented, if it seems to violate a concept of consensus reality, scientists as well as non-scientists do not believe it. Non-scientists have an exception model, which is the stereotype of the 'mad scientist' to handle such situations. The other scientists try to imagine more believable alternative theories to fit the observations and measurements, and if they succeed, they design experiments where the new observations and measurements will choose between the theories. Eventually there is a winner amongst the theories (with believability in terms of previous consensus reality not being a factor). The winning theory becomes a new part of consensus reality amongst scientists, and then eventually becomes accepted as a new part of objective reality by all who become aware of it.

Practical

Acoustical research on musical instruments uses scientific equipment to add facts to objective reality, with the usual aim of associating these facts with the consensus reality models that have been
opinions of specialists, thus converting them into objective reality. When this aim has been to use measurements of sound to distinguish quality between a good professional instrument and an instrument judged to be of exceptional quality, this research has usually not been able to offer any clear associations of judged quality with the new facts generated by the equipment. Blind listening tests made by psycho-acousticians have also been inconclusive, experts being confused as well as lay-people.

What this means is that the specialists who set up the difference in quality are sensing things about these instruments that physical acousticians can’t recognise in their measurements and that psycho-acousticians can’t show in their tests. It does not mean that the specialists are just imagining these differences. These are part of subjective and consensus reality. A scientist can’t say that something in consensus reality doesn’t exist if he can’t observe and measure it. He can’t say that God or UFOs or communication from intelligences in other worlds in the universe don’t exist. All he can say is that he hasn’t observed and measured them. He doesn’t ever expect to observe or measure the first, very much doubts whether he ever will the second, but is optimistic about the third, sooner or later.

In making these just-mentioned predictions, a scientist cannot claim any more authority than any other informed intelligent person. Where the scientist’s opinion has authority that others can’t claim is in knowing what he can measure with the techniques he has available, how to do the measurements, and predicting how measurement techniques are likely to change in the near future. If special training (such as in mathematical analysis) is required to relate theories with the measurements, he can claim that authority as well.

The inconclusive findings of the physical acousticians and psycho-acousticians say positively that differences in sound alone between good professional instruments and what are deemed to be exceptionally good instruments are not obvious to the average layman, musician, instrument expert (maker, fixer or dealer) or acoustician. The musicians and instrument experts are the people who separate instruments into these categories. A sound-in-isolation experiment is completely contrary to their experience and training, where sound is not independently perceived, but integrated with the instrument’s look, feel, craftsmanship, reputation, and the player’s ease in getting what he wants out of it. These specialists are under considerable pressure to base their evaluations of total instrument quality on sound quality since the public, the ultimate customer, considers that sound is all that it is buying from the musician. But to these specialists, sound is just one factor in quality, and sometimes not even a major factor. This is obvious from the fact that most of the innovations in design (especially in wind instruments) and the use of accessories (especially strings) in orchestral musical instruments in the last century have been to make playing more comfortable, easier and safer, often to the detriment of sound quality.

The inconclusive findings do not mean that there aren’t any laymen, musicians or instrument experts who can tell the difference in a sound-only experiment. Such people most probably do exist, but scientist haven’t bothered to single them out. These exceptional hearers can’t tell what it is in what they are hearing which lets them tell the difference. Psycho-acoustic tests are available that can measure general hearing sensitivity, and new ones could be devised that might be relevant for this purpose. But the main generalisation to be made from studying these people is that they exist, and that is not a particularly interesting scientific objective.

Though super-sensitive hearing is not a particularly interesting issue scientifically, it is a very important issue in the consensus reality of the professional music world. The training of musicians involves not only developing playing technique and a ‘musical’ style of interpretation, but also the training to hear subtle differences. If one member of a performing group can hear a particular subtlety, all the others try hard to hear it as well, and rarely have the courage to say that they can’t, it isn’t there, or that it might not matter. In the highly competitive world of musical performance, reputation is all important, and it is a disaster if the word gets around that one’s ear is less than perfect. So appearing to hear that extra little bit that the most sensitive can becomes vitally important, whether one honestly can or not. There are some who can, many who convince themselves that they can, and some who make believe they can (and scientists are wise in keeping well out of it). And buying an instrument whose sound has that little bit extra (whether one can honestly hear it oneself or not) seems vital as well. So in the highly competitive world of instrument making, having a reputation for making such instruments is a tremendous advantage.
to read that the instrument maker can hear the difference (even if it took him 20 years to develop that ability) can only help.

One occasionally find pockets of different consensus reality. Here in Manchester not many years ago, the leader (principal violinist) of the BBC Philharmonic sold his old-master violin and played on a very moderately-priced new violin by a local maker. A large fraction of the violinists in the orchestra followed suite, buying from the same maker. He is now very comfortably retired in Scotland.

I am not saying that all good professional instruments sound the same. On the contrary, they all sound different, even two made in the same way by the same maker to the same design at the same time from the same batch of raw materials. Even the same instrument sounds different in different acoustic or humidity environments, or when played by different people or in different repertoires or for different purposes. (You must have heard the story of a violin dealer who asked his friend, who was an excellent player as well as a dealer, to play an instrument for a client; the friend quietly asked "do you want my 'selling' or 'buying' tone?").

This is all true for the acclaimed exceptional instruments as well as for the good professional ones. And one usually doesn't need super-sensitive hearing to notice the difference (at least with stringed instruments) when they are compared one after the other (but one needs exceptional aural memory to tell the difference if observations are separated by some time). In my experience with violins, the range of differentness amongst good professional instruments so overlaps that range amongst acclaimed exceptional instruments that those people who, by hearing alone, can tell the difference between the two classes of instruments must be focussing on much more subtle differences than the usual ones between instruments in the same class. (Dr. Colin Gough has suspected that the difference in sound between old-master violins and good modern ones is mostly in the low-frequency components of the transient sounds or noises produced by the instruments).

In summary, the relationship between the musician and his instrument is very complex, with sound being just one of various factors that it are generally difficult for him to separate. Because it is expected, comparisons of instrument quality are mainly expressed in terms of sound quality. When scientists look at the sound of these instruments in true isolation, they cannot recognise these reported differences. This does not mean that scientists can claim that what the musicians say is 'hogwash', as Way reports. Neither does this mean that what the scientists have done is 'hogwash' either, as Way implies. The situation is just more complicated than what the musicians are able to say, and what the scientists have so far assumed and have been able to measure.

There is a bit of space here, which I will use to make a comment on a completely different topic that relates to the Theory above. My disagreements with Haynes and Gwynn in other Comms in this Q involve models of pitch standards in the subjective realities of each of us. The other two seem to be happy with ignoring imput information (ie evidence) that violate their models (and they don't respect) as long as the information that they are more interested in is consistent with the models. This is commonplace in everyday life, but it is contrary to the discipline of scholarship.
Singing Lessons for Wood

When we talk of the "tone" of a musical instrument we usually mean "what it sounds like when it is played"; it is a value judgment. That is sensible; it is what we are concerned with. With a few unimportant exceptions (e.g. the Aeolian harp) no instrument has any "tone", in that sense, without a human player. The "tone" is dependent on the human being. With fiddles that is so painfully obvious that no-one I suppose would think that it needs rigorous proof. Watson Forbes (in "The Violin Makers") holds that "tone" is "90% player, 10% instrument" (not of course that you can really put figures to it - it is mostly, indeed he says nearly all, dependent on the player. No thinking string player would reverse his proportions). The inference is that the difference between an "old master" fiddle and a first-rate new one is at most rather marginal, and the evidence appears to support that inference. That is one reason why there is so much argument about the matter. Certainly I am not alone in wishing to see more conclusive evidence. Most of us will think that the importance of the player is also self-evidently true, although in a lesser degree, even for the less sensitive keyboard instruments.

That we are, inescapably, dependent on value judgments is a greater difficulty, for value judgments are basically personal, subject to variation according to person, time, and circumstances, and (in particular) much affected by preconceived ideas. There is often, within a given society at a given time, a general consensus of opinion, which may or may not be a recognition of an objective truth. Individual and consensus opinions can both be "conditioned" and often are (everyone is open to it) and yet we have to rely ultimately on value judgments. No physical methods of evaluating "tone" can be convincing, although they may instructive in some ways.

So we have the problem of extracting an objective truth from a mass of individual value judgments. Such problems are not impossible to solve. They are commonplace in fields as diverse as the licensing of new medicines and the evaluation of a videotape for training purposes. But they are complicated and take much trouble, time and (inevitably) money. It is crucially important for a valid result that every individual judgment shall be made without bias, conscious or subconscious, i.e. without those involved knowing which is which of two items being compared. Probably few readers are aware of what such testing requires, and that must be my excuse for this elementary exposition. Casual, random, uncontrolled tests cannot produce answers which will convince those who think carefully and dispassionately, just as they cut no ice with a Food and Drugs licensing body.
People of course talk about a violin as a "living" thing made of "living" wood and possessed of a "soul". There is no harm in these colourful metaphors as such, but some aficionados do seem to believe them literally. Anyone with some knowledge of what is involved in life and death, in nervous and mental activity, in learning and value judgment, finds that hard to understand. Such knowledge is of course "science". Scientists and engineers are disturbing, unromantic people who will start by verifying the supposed facts before getting drawn into theorising about them. Life would be simpler without them, and very different.

FoMRHI Comm 1092

P. Talve

Some more aspects about fiddle making.

I have spent lot of time inquiring the possibility of making fiddles with a flat soundboard and without any bar or soundpost inside.

I got an idea examining old Estonian folk instruments. The old makers have used little "soundbox" between the belly and the back. This box was used for a "kannel" (it looks like plucked psaltery) and for a "hiiu kannel" (it is bowed lyra). On the "kannel" soundbox stood under soundhole and it was glued between the belly and the back. On the "hiiu kannel" it stood under bridge and was glued to the place too. Both instruments had the boxes constructed from separate pieces.

I used a solid block from limetree. Outside dimensions were 45 x 45 mm. I hollowed out the middle part and let 4 mm for the walls. At first the walls had many holes, but further testing showed that they had no import. This "soundbox" must be 2 or 2,5 mm higher than the distance between the belly and the back. After assembling the soundboard have a little curve. The tension presses the soundboard to the normal level, otherwise the string tension will press soundboard lower from the normal level. I didn't glue this "soundbox" inside. It seems better when it is moveable. It will give possibility find the best place for it. On the drawing you may see the usual place of this "soundbox".

It needs sometime little trimming.

This "soundbox" have a double import. It protect soundboard against string tension and joint the belly and the back, so give more power to the sound.

But the main thing is the result. In my fiddles sound became sweet and light. It is not an aggressive sound, but comes easy to the fore from an ensemble.

There in one possible way to make a medieval fiddle. If somebody want more information about this, I am always ready to reply.

I would be very grateful for comments and criticism, too.
Jerome of Moravia's First Fiddle Tuning as an Individualized Modal Framework

Linda Marie Zaerr

Only two descriptions of fiddle tuning survive from before the fifteenth century: one by Jerome of Moravia, Paris, ca. 1280; and the other by Jean Vaillant(?), Paris, fourteenth century. Both treatises have been edited by Christopher Page. The tunings in the Jean Vaillant(?) treatise are probably designed for plucked stringed instruments (Remnant 67), especially since the tunings would be inappropriate for a flat-bridged instrument and inconvenient for a curved-bridged instrument.

Jerome of Moravia, however, presents three clearly articulated fiddle tunings, and I adopted the first of these tunings for my flat-bridged fiddle:

... ut scilicet prima corda faciat D; secunda Γ; tertia G in gravibus; quarta et quinta ambe unissone d constituant in acutis... Et talis viella, ut prius patuit, vim modorum omnium comprehendit.

(90)

(... the first string should make D; the second Γ; the third G — all among the graves, and the fourth and fifth strings should form two unisons at d among the acutae. ... Such a vielle as just described encompasses the material of all the modes.

[trans. Christopher Page])

The only question Jerome of Moravia leaves is how to tune Γ, or the very low G. Guido d'Arezzo describes Γ as the open string on the monochord (Micrologus iii). Richard Hoppin implies that Γ would represent the lowest note in "the normal range of male voices" (73), though he comments that this "obviously would not apply to a convent of nuns" (74). It seems reasonable that a male fiddler would tune his instrument with Γ as the lowest note he could sing comfortably. Thus the fiddle would be tuned to his vocal range. He could adjust when he played with other instrumentalists, but essentially the tuning would be personalized to accord with his range.

I infer that the same would apply to a female fiddler. I thus tuned Γ to the lowest note in my vocal range. It happens to be close to a modern D, but that is irrelevant since the tuning system is
relative rather than absolute, and thus more flexible. The approach is simple and straightforward. By this method there is no need to create elaborate restructurings of Jerome's ideas. His work can be read and followed exactly without having to pass it through the terminology of our century. The voice provides a fairly stable pitch reference which tailors the instrument to the individual.

Jerome states that this first tuning system renders all the church modes possible. Christopher Page points out that this is not strictly true if the player "restricts himself to the stopping positions which Jerome describes" ("Jerome" 96), but Jerome does not indicate that the fingerings he describes are exclusive. In fact, the instrument provides a clear and comprehensible system for understanding the church modes and playing comfortably within them, with the open intervals of fifths and fourths providing a framework for the pentachords and tetrachords. All the authentic modes thus begin on G (either of the two G strings), and all the plagal modes begin on D (any of the three D strings). Thus dorian mode would be played with the first and second fingers close together on all strings; phrygian mode would be played with the first finger against the nut on all strings; and so on. All the modes are possible, though Lydian is awkward on this particular fiddle.

The curved-bridged fiddle can also play all the modes and can begin anywhere on the instrument for any mode. The disadvantage, however, is the amount of thought required to remember where the tonal center is and to remember or work out the appropriate finger patterns for the tonal center and the given mode. The curved-bridged instrument is more flexible, but it also requires more conscious and active thought. The flat-bridged instrument is limited in that the tonal center is always G, but that limitation can be an advantage in freeing the player's mind to think of other matters, such as lyric or narrative text or improvisation.

This freedom to operate within clearly defined modes can be extremely useful. For example, in the Middle English romance *Sir Launfal* I find it works well to play in the dorian mode, which sounds ceremonious, when I speak of King Arthur's court, and in the phrygian mode, which sounds exotic and rather seductive, when I speak of the Dame Tryamour. This approach has medieval support. In his treatise *De Musica*, ca. 1100, John states that "different men are attracted by different modes," and he goes on to characterize the different modes using very personal adjectives:
Some are pleased by the slow and ceremonious peregrinations of the first, some are taken by the hoarse profundity of the second, some are delighted by the austere and almost haughty prancing of the third, some are attracted by the ingratiating sound of the fourth, some are stirred by the well-bred high spirits and the sudden fall to the final in the fifth, some are melted by the tearful voice of the sixth, some like to hear the spectacular leaps of the seventh, and some favor the staid and almost matronly strains of the eighth.

(133)

Since medieval theoreticians continually stress the importance of knowing the modes and being able to operate within them, since notated medieval music allows shifting between modes, and since Jerome's fiddle tuning system is designed for convenient modal playing, the choice of this system is very appropriate for a performer who wishes to be unencumbered by extra thought but still draw on the emotional variety offered by the modes. The benefits of reading Jerome's first tuning system in this way are freedom and convenience: the result is a fiddle which is tailored to the player's vocal range and which has complete modal flexibility around a stable tonal center.

Works Cited


TRAVELLING IN THE 16th C. and.... LUTE STRINGS.

During my peregrinations in the travelling literature of the Renaissance I stumbled on an interesting work written by a kind of "early ethnologist" born in France around 1517, Pierre BELON du Mans, also Petrus Belonius. Among his different works on "natural philosophy", we find the detailed description of his long trip to what we call today the Near East. His "Observations de plusieurs singularitez & choses memorables, trouuees en Grece, Asie, Iudée, Egypte, Arabie & autres pays estranges" were published at Paris in 1553 (re-edited in 1554, 1555, Paris and Antwerpen).

After his apothecary studies, Pierre Belon went to Germany to study botany. Then in France again in 1542, he went into the service of the Cardinal of Tournon for whom he had to fulfill various diplomatic assignments since Petrus Belonius mastered perfectly several languages. Then came his trip to Italy before being ambassy attaché of Aramon. (1)

It seems that Pierre Belon played the lute, or at least showed great interest for music and musical instruments. He devoted the chapter XLIX of his above mentioned work to lutes and other musical instruments seen in the "Turkey" of that time. Chapter XLVIII deals more precisely with lute strings. We can infer from one remark made in this text that he probably had to find strings for his own instrument in the country in question. That may have been the reason why he devoted a chapter to this subject giving us many details which may interest the modern researcher.

My idea to publish this small text is due to a suggestion of a lute maker who recognized the interest of some of this information dating from the first half of the 16th century. A small piece for the historical lute strings puzzle !

The original French version of the Paris edition of 1553 in reprint form:

**DES CORDES D'ARCS ET LUTS DE Turquie. Chapitre XLVIII.**

E four bien tard un homme portant une hotte viendra par les boutiques des bouchers, & prendra les tripes qu'on lui a gardées le jour & les porte à ceux qui en font de toutes fortes de cordes. Ils sauront singulièrement bien faire celles des arcs. Avec en asil grand nageur leurs arcs sont en corde de cordes de tripes.

Cordes d'arcs.

Cordes de luc.

Quant est aux cordes de Luc, ilz en font de toutes fortes, & bien fines, & des chanterelles qui montent bien aussi haut que les nostres, mais elle ne sont pas si argentines, d'autant qu'elles sont cordes de trois cordelles, lesquelles toutesfois ne vrayement n'en ferterie d'un luc de Venise, en deus autres de telles chanterelles on en trouve de toutes fortes & couleurs rouges, pers, verdes, jaunes, blanches, & en l'autre lieu en sa boutique, comme aussi des autres fortes de cordes du luc qu'on trouve par toute Turquie. Elles y sont plus frequentes qu'en Europe, dont se puis bien donner la raison : c'est que les Turcs ont de quatre fortes de gueremes & lutes, & de quelques plus seulement pour ou des vrayes ou des autres, & que n'adrent pas en France, ne en Italie, car

Quatre fortes de luts en Turquie

peu de gens de villages se mestent de jouer du luc, ou de guereme. Mais en Turquie plusieurs en seuent formen a leur mode.
Translation:

Ropes for the bows and strings for the lutes in the "Turkey"

"Late in the evening a man carrying a basket will visit the butchers' shops and take the guts kept for him the whole day; he will bring them to those who make all kinds of ropes from them. They [the "turkish" artisans] make especially well those for the bows. A great quantity of these latter is used since the bows are fitted with gut ropes.

Regarding the lute strings, they make different kinds, very thin ones and treble strings that can go as high as ours do; but they are not as silvery [in sound] as ours are, all the more so since they are made from three threads (2); nevertheless I could use them on a Venetian lute as no other ones were available. Such sorts of treble strings exist in various qualities and colours: red, blue-green, green, yellow, white; and you can find them easily in each haberdasher's shop together with all the other categories of lute strings in use all over Turkey. Lute strings are more common here than in Europe, a fact I can explain in the following way: the Turks have four kinds of guitars and lutes regularly played by a much greater number of people than in France or Italy where people able to play lute or guitar in the villages are rather rare. But in Turkey many people can play lute or guitar according to each fashion."

Notes:

1. For more details on this 16th. c. scholar, see DELAUNAY P., L'aventureuse existence de Pierre Belon du Mans, Paris, 1926.

2. I use here the term employed by E. Segerman in his communication "On the Number of Guts in a Gut String", FoMRHI-Comm. 325 (January 1981).

FoMRHI Comm 1095
John Barnes

Short Octaves

John Bence (Comm 1078) will find a concise history of the short octave in The New Grove. It was widely used during its period in all continental countries though not in Britain. It was common on organs, where it represented a considerable economy, since the omission of four low notes saved the provision of many big pipes which were expensive and took up a disproportionate amount of space.

I think the advantage for stringed keyboard instruments, where the saving was less significant, must have been that players preferred the convenience of having the same keyboard arrangement on their harpsichords, clavichords and virginals as on their organs.
Shelley's Guitar and 19th Century Stringing Practices

The survival of particular artifacts from the past can be greatly helped if they are especially respected because of association with a person continuously venerated from the time that he or she lived to the present. This is the case with the guitar once owned by Percy Bysshe Shelley, which was kept by Jane Williams and her family and given to the Bodleian Library (Oxford) before the end of the last century. According to the label, this instrument was made in 1816 (the final digit is indistinct and has plainly been altered) by Ferdinando Bottari in Pisa. It is the guitar featured in several poems Shelley wrote in 1822. From the evidence of his letters, it is clear that he bought it in Pisa in March-April 1822 for presentation to Jane Williams.

An unusual feature of this instrument is the design of the tuning pegs. They are made of brass with wooden buttons, similar in general principle to the pegs on the 1824 Lacote at the Paris Conservatoire and to modern metal-and-plastic ukelele pegs. The metal turning located above the peg head of the guitar uniquely has two diameters for winding the string on. The hole for threading the string into is on the thicker part (12-13 mm. in diameter), which is closest to the peg head. Above this is the thinner part (6-7 mm. in diameter). Presumably this arrangement is for efficiently winding up most of the slack in the string on the thicker part and then running the string up to the thinner part for more sensitive tuning. A disadvantage of having such pegs is their weight, affecting the balance of the instrument (the balance point on this rather heavy instrument is at the body-neck joint).

Of particular interest are the strings. On the instrument at the time of the examination were low-twist lightly-coloured gut strings with diameters 24, 29 and 26 thou (thousandths of an inch) respectively for the first three strings. The other three strings were made of tarnished silver or silver-plated metal (which most probably would be copper) wire windings on tightly packed fibres, which most probably are of silk. The overall diameters of the 4th, 5th and 6th strings are 35, 49 and 58 thou respectively. The windings (close-wound) number 7 in 40 thou, 7 in 60 thou and 5 in 80 thou respectively, measured along the length of each string.

A photograph of the instrument made c. 1898, shortly after it was acquired by the Bodleian, shows the third string clearly thicker than the fourth, and the second string broken, so the present two are clearly relatively modern replacements. The first is most probably in the same class because it is the most vulnerable to breaking (it was broken when examined), and it has the same colour, lighter than the old broken and spare strings kept in the guitar's case.

From the diameters of the wound strings and their windings, I estimate the string tensions of the 4th, 5th and 6th strings to be 10, 10 and 8 Kg respectively if the winding is silver, and 1 Kg less in each case if they are of silver-plated copper. For this calculation I assume a tuning of e',b,g,d,A,E at a'=430 Hz and estimate the core density to be 1.3 gm/cm$^3$. The string stop of the instrument is 65.4 cm.

Amongst the loose pieces of gut in the guitar case are three high-twist strands of diameter between 45 and 47 thou. They are the only strings that are clearly thicker than the fourth string and so are candidates for being the third string in the photograph. The tension of these strings as thirds would be between 9 and 10 Kg. The other loose pieces of gut kept with the instrument have diameters ranging from 21½ to 40 thou. There are many knots, and some of these strands of gut could have become associated with the instrument only as pieces tied to broken strings to make them long enough to mount on the instrument. Amongst the loose strings are candidates for being first and second strings at the tension level of those mentioned above. Others could well have been guitar strings that conform to a different tension fashion in the 19th century.

A general tension level of 9 or 10 Kg per string (with the 1st somewhat higher and perhaps the 6th lower for acoustic reasons) is apparent for this guitar. It is surprisingly high. Modern classical guitar string tensions are about 5 Kg (with the first at 6½ Kg). The only information on 19th century guitar string tension before now was from a statement by Flesch (1923), who discussed an old letter enclosing strings for replacement that was sent by Paganini to the Schott firm. It was shown to Heermann around 1890, and he measured them. From Flesch's report of Heermann's response, we can determine that the diameters were about 25 and 32½ thou (see The Strad, March 1988, p. 201). Heermann and Flesch assumed that they were violin second and third strings, but this does not fit into our history of violin stringing based on much other information, so it is likely that they were strings for Paganini's guitar. Assuming that they were second and third guitar strings, we deduced that the tension was about 4½ Kg per string.
This tension level was convincing because it agreed with the results of stringing experiments on surviving and reproduction 19th century guitars made by early music enthusiasts. The principle these musicians have followed has been to use the lowest tension that allows the instrument to be fully usable for the repertoire and their style of playing. This does not necessarily agree with the original uses and styles of playing. With violins at least, this has led to much lower string tensions than those indicated by the historical information.

It is possible that a fashion for particularly high string tensions on violins during the first half of the 19th century could have been followed by guitars as well. Spohr (1832) wrote "Generally speaking in order to obtain a rich and powerful tone, a violin should be furnished with the largest set of strings it will bear...". We find that his strings had about half again (ca 50%) greater tensions than were usual before and after this period. Of possible relevance here, there is evidence that Paganini did not partake in this fashion for high string tension on his violin. For further information on this period see The Strad, March 1988, p. 198.

It is also possible that the later drop in violin string tensions to 2/3 their level in the 1st half of the 19th century was also mirrored on guitars. Amongst the loose strings in Shelley's guitar case is one wound string with its ends clipped off. It has an overall diameter of 54 thou with 5 windings in 60 thou along its length. This calculates as a 7 Kg E string if the winding was solid silver (and about 1/3 Kg lower if it was silver-plated copper). The loose gut strings include candidates for the e', b and g strings at this tension level. One of these g's was used on the guitar after it was unused for some time since there are metal tarnish marks on the string spaced just like the frets. My guess is that Jane Williams and her family carefully preserved the original strings on this instrument, and on the rare occasions when someone wanted to play it, the original strings were removed and then afterwards replaced. Some of the more modern strings (especially those of plain gut) were added to the collection in the case for tying on to broken strings.

If the Paganini strings reported by Flesch were the first two of a guitar, the first would be at 7 Kg and the second at 7 Kg.

One problem encountered at Northern Renaissance Instruments in making reproduction strings for 19th century guitars has been that the fourth strings, made in the historically indicated way (metal on silk), at the 4 Kg tension we thought was appropriate, consistently broke too quickly to be practical. We used the thinnest wire that we dared for winding. The possibility of stronger silk being available then was considered, but the silk historian consulted knew of no evidence for this. Now that Shelley's guitar provides evidence for 7 and 10 Kg tension levels, we can increase the proportion of silk in the thicker string designs and make reproduction fourth strings that work. This practical point very strongly suggests that the stringing evidence provided by this guitar applies generally to all instruments of this type.

Shelley's guitar is of the largest size normally available in the 19th century. Its robust size, weight and decoration makes it appear to be a very masculine instrument. Yet Shelley gave it to Jane to play. His intention then, seems not to have been to give Jane a feminine means to express her own nature, but rather to give her a symbol of his masculine spirit to play on for his delight.

"Take
This slave of music for the sake
Of him who is the slave of thee;
And teach it all the harmony,
In which thou can'st, and only thou,
Make the delighted spirit glow,"
from "With a guitar. To Jane"

I would like to thank Lucy Blaxand of the Bodleian Library Conservation Section for bringing Shelley's guitar to my attention, and Dr. Bruce Barker-Benfield of the Department of Western Manuscripts for permission to examine the guitar and strings, and for being very helpful in various ways.

This guitar is on display until the 8th of August in the Exhibition Room at the Bodleian Library as part of the Library's exhibition in honour of the Bicentenary of Shelley's birth. Admission is free. Times are 9 to 4.30 Mondays to Fridays and 9 to 12.30 Saturdays. The first string to be seen is my replacement of the broken one. This was done before the above analysis, and so the diameter is as historically inaccurate as that of the second and third strings.
Sympathetic Strings

Physical and Musical Function

Sympathetic vibration occurs when one string that is not touched vibrates as a result of another string being played. This happens when the played string and the other string have common frequencies amongst their modes of vibration, and there is a means of transferring common-frequency energy from the played string to the other. A particularly effective route for such energy transfer is when both strings are on the same bridge. Sympathetic string vibration happens to some extent on almost all stringed instruments (without special sympathetic strings) because open strings not being played on can act as sympathetic strings.

When a string is plucked or struck, all of the vibrational energy is imparted to it at the very beginning of each note. The sound starts out strongly and then dies away. A sympathetic string absorbs most of its energy from the played string immediately after that beginning, reducing the energy delivered to the soundboard then. Later, after it is 'charged up', it adds to the energy delivered to the soundboard. Consequently, if a sympathetic string is involved, the sound dies away faster at the loud beginning of the note, but then dies away slower at the softer later part of the note. This effect has not usually been considered musically interesting, so even though the inventors of metal sympathetic strings (Edney and Gill in 1609) indicated that they could be used on lutes and other plucked instruments, this has generally not been the case. Nevertheless, this effect has been used to musical advantage by the early piano, which encouraged sympathetic vibrations on unplayed strings by damping them very poorly, thus better simulating the sound of the harpsichord (with a more rapid fall-off of sound at the beginning of each note and longer sustaining of the weak part of the note later on).

When a string is bowed, vibrating energy is continually added to the note by the bow, so there is no energy penalty to pay when a sympathetic string is 'charged up'. Thus a longer sustaining of the note after the bowing has stopped is the musically interesting contribution to the loudness of the sound made by sympathetic string vibration. To my ears, there is no difference in the quality of the sound, produced while steadily bowing a string, between whether the sympathetic strings on a viola d'amore are operational or whether they are damped. Consequently, I suppose that the main musical function of sympathetic strings depends on sounding whatever vibration energy they captured from the played note for a while after one stops playing the note. In legato playing this leads to a kind of echo effect with a residual sound from the previous note overlapping the beginning of a new note. The effect is much more noticeable in staccato playing, where the spaces between the bowed notes are filled with the softer ringing-on of the sympathetic strings. The sound is similar to that made by electronically adding artificial reverberation, i.e. a simulation of the sound expected from a much more resonant acoustical environment than is actually the case.

When the string tuning is largely in one chord, bowing one or more notes in that chord would favour sympathetic vibration of open strings. This seems to have been a factor in choosing such tunings on the lyra viol (see Comm 716) and the viola d'amore. The early instruments called 'viola d'amore' had metal strings (and no special sympathetic ones), and the 'd'amore' part of the name probably alludes largely to the increased sympathetic vibration due to metal strings ringing on longer than gut strings. This view is supported by the name remaining with the later types which had unbowed sympathetic metal strings as well as gut bowed strings.

In the rhetorical oratorical style of singing that was imitated by instruments (prevalent everywhere in Europe during the Renaissance and early baroque, and in the French style of the late baroque), there would be a small space between words, a greater space between verbal phrases (musical 'points', nowadays called 'motives') and even more space between sentences (musical 'strains', which correspond nowadays to musical phrases). On bowed instruments, these spaces are likely to have been created mainly by lifting the bow before giving full time value to the note at the end of each unit. It is in these spaces that the ringing on of sympathetic strings would be most noticeable.
Tunings and Characteristics of Sympathetic Strings

The baroque marine trumpet often had a multitude of sympathetic strings located inside the body. According to Prin (c.1742), they were all tuned to the same note as the single bowed string. With this tuning, all of the vibration modes of the bowed string (selected by finger damping at a node of each mode) are modes of the sympathetic strings.

Many lyra viols had sympathetic strings early in the 17th century (see Playford, 1661), and this instrument is clearly what Praetorius's English Viola Bastarda was. Both gave the tuning of the metal sympathetic strings as the same as the gut bowed strings. This was possible for the highest string because a type of iron that was available from about 1580 to 1620 that could tune as high as gut. For the lowest strings there were highly-twisted brass strings that were available then for orpharions and bandoras that could just about tune as low as catline gut strings. The tuning of this instrument was mostly in one chord, so bowed notes on that chord would excite open unbowed gut strings as well as the metal sympathetic strings. On later lyra viols the metal sympathetic strings were abandoned, and the effect by the unbowed gut strings was considered sufficient.

In Diderot's 'Encyclopedie' (1767-72), the sympathetic strings on the viola d'amore were described as tuned arbitrarily, with diatonic tuning or unison or octave tuning with the bowed strings mentioned as possibilities. Tuning all in unison with the bowed strings was not possible then because the special iron and highly-twisted brass were not available then (the Neapolitan mandolin used gut for the highest strings and metal-wound-on-gut lowest strings in an otherwise metal stringing). Thus Diderot's statement of unison or octave tuning with the bowed strings did not imply two different types of tuning, but rather some combination of unisons and octaves in a single tuning. Such a combination need not include any twisted metal strings at all, and would have included different sympathetic strings tuned to the same pitch (as on the marine trumpet). The order or sequence of tuning need not have followed that of the bowed strings either, being truly arbitrary.

The other possibility mentioned by Diderot, diatonic tuning, necessarily produces less sympathetic vibration for notes in the open-bowed-string tuning, a sacrifice made to get more sympathetic vibration on other notes in the diatonic scale. The choice between the two possibilities would presumably have been made according to how much modulation there was in the repertoire being played. A combination of the two types of tuning was also possible.

The baritone (baryton) had bowed gut strings and metal strings going over different bridges on the soundboard, and the metal strings could be plucked by the thumb of the left hand, as well as act as sympathetic strings. The diatonic tuning of the metal strings is appropriate for the plucking function as well as the weaker but more versatile sympathetic function.

The type of metal used seems also to have been arbitrary, with Leopold Mozart (1756) writing that those of the viola d'amore were of hardened iron, while Diderot mentioned that they were of brass. Brass has the advantage of being easier to tune accurately (because it is more elastic), but this choice has the disadvantage of breaking more often and taking more time to stabilise in pitch when replaced. If one wanted a large range of pitches to be present in the sympathetic strings, then a combination of iron and brass would be necessary (as Praetorius mentioned for the lyra viol). The fact that each later author only mentioned one type of metal implies that such a large range of pitches was not considered necessary then.

I know of no historical information on the diameters or tensions of sympathetic strings (Hardanger fiddle people may have some). From purely physical considerations, we would expect that the heavier the sympathetic strings are, the more energy they would store and then can deliver after the bowing stops. The factors that favour equal tension on bowed and plucked instruments do not apply here, so string tensions are free to vary, allowing strings at different pitches to have the same diameter.

In the modern style of bowing, where lifting or stopping the bow before the end of longer notes is a rare occurrence, the effect of the sympathetic strings is rarely heard. Since the sympathetic strings on viola d'amores are consequently largely cosmetic in function, their tuning and tensions are quite irrelevant, and very light sympathetic strings are usually used. I would expect that if the effect of the sympathetic strings was really wanted, much heavier strings would be used.
Writing a response to Bruce's Comm 981 could have five distinct objectives: 1. to prove that my model is superior to his according to the rules of scholarship, 2. to convince Bruce to abandon his model and adopt mine, 3. and 4. to convince the communities of music scholars and of early music performers that my model is the 'correct' one, or 5. to entertain the readership with the style of our debate.

The first should be obvious already to readers who appreciate that objective (not subjective) goodness of fit to the total body of evidence is the scholarly criterion, and have carefully read the Comms already presented by both of us. The second is extremely unlikely because Bruce obviously deeply believes in the truth of his model; he likes its attractive simplicity and the success of the French-designed baroque woodwinds when played with strings makes him feel that they were designed for this purpose rather than for wind-band use, preferring to think that the woodwinds cooperated rather than competed with the strings for pitch-standard preference. The third is also unlikely since the vast majority of music scholars are not sufficiently interested in the subject to bother looking closely enough to evaluate different competing models on offer, and will just consider the question as open until controversies are over. The fourth will suffer a similar fate since the early music performers have already invested in instruments at currently popular pitch standards, and so will not welcome any research that puts into historical question the choices they've made. The fifth depends on attractiveness of debating style, and here I feel at a disadvantage.

As a result of these evaluations, plus the fact that I like Bruce (so hurting him is painful to me as well), I've had difficulty in generating motivation to write this response. Now that it is clear that expressing my model without replying to Bruce is just as painful to him (and I can't keep quiet when there is something I want to say), so I must do it! I shall mix making another attempt at getting the message of scholarly choice across to the readership with discussing the points in Bruce's Comm (in the order they appear), showing how my model comfortably accommodates the evidence, as required by the scholarly rules. A table at the end may help comparisons.

Scholarship works with raw evidence that is accepted for what it is by all concerned, and hypotheses (or theories or models or interpretations or pictures or contentions or whatever one wants to call them) which are stories or descriptions of reality which would have generated the evidence if they were true. These hypotheses make both general and specific assumptions which, by their nature, go beyond the evidence. This hypothetical reality must generate all of the evidence in its sphere, and if any piece of evidence does not naturally result from that reality, specific assumptions need to be made as to how that particular piece could have become what it is.

Occam's Razor, the basic rule of scholarship, states that if there are two competing hypotheses that equally well explain the evidence, the simpler one is preferred. This is understood to mean that the hypothesis that makes the fewest and most likely assumptions is the simpler one. The question of likelihood, as the question of how well an hypothesis explains the evidence, are matters of judgement, and so they should be applied with restraint. We can consider each hypothesis as having a score, which is the sum of the number of assumptions, with each assumption weighted according to how unlikely it is. The choice is the hypothesis with the minimum sum. Thus, if everything else is equal, an hypothesis where all of the evidence fits naturally is preferred to one that has to explain away conflicting evidence, since the latter involves added assumptions. Similarly it is simpler to consider that a piece of evidence is true and representative of what it seems to be, rather than otherwise, even though we know quite well that otherwise is quite possible. If we could reject any evidence that we want because of this possibility, we can reject all of the evidence that contradicts our favourite hypothesis, and anything goes. Comparison between hypotheses can only be made if they relate to the same evidence, and that must be all of it that is available at the time. So if a piece of evidence seems to contradict one's hypothesis, one either modifies the hypothesis to remove the contradiction or one makes as reasonable an assumption as one can, explaining how that evidence could occur within the context of the hypothesis. The more likely that assumption seems to be, the less the fact that such an assumption has to be made counts against the hypothesis in the Occam's Razor comparison.
Pouring scorn on contrary evidence (e.g. calling it ’unsuitable’ or ’not proven convincingly’) is a debating technique that has no place in scholarship. Such dismissal of evidence is no better than ignoring it completely. Bruce’s model can’t even be taken seriously as an hypothesis unless he can dream up a reasonable story about what went wrong with Praetorius’s pitch-pipe diagrams, as well as other contrary evidence.

This covers the first point in Bruce’s Comm 891. He then states that “Silbermann is ambiguous and in disagreement with the weightier evidence of Quantz and Agricola”. In general, ambiguity in evidence only weakens it by restricting the range of hypotheses it can contradict. Ambiguity is unfortunate, but it does not reduce the power of the information in it that is not ambiguous. And if one piece of evidence appears to contradict another, it is the scholar’s job to do his best to find an interpretation of the situation (hypothesis) that removes the contradiction, and if he can’t, to try to imagine what went wrong with the evidence. Some of this is attempted by Bruce later in his Comm, and I will discuss these when I get to them. For now I will just state that the objective weight of an individual piece of evidence depends on its ability to discriminate between different hypotheses, and repeat that we cannot allow any unexplained disagreements between hypotheses and evidence.

Bruce next complains that I present “judgements between conflicting evidence as established fact”. I hope that this is not true. I present a model of what might be truth, and try to explain all of the evidence that I am aware of so that none is conflicting. Within my model, the only established facts are the evidence itself. Trying to accommodate all of the surviving information is not new (as Bruce suggests), but is the scholarly method. In Comm 1039, Bruce presented his frequencies for Chorton and Cammerton as established facts, with no hint that they were controversial. After my first pitch paper, Comm 442 (when I naively thought that all readers would be convinced), I’ve always wanted to make it clear that there was controversy. There is a saying about people in glass houses...

Next Bruce complains that I’ve based my work on an article by Mendel that is incomplete. I don’t claim to be aware of all the evidence that exists. I’m sure neither does Bruce; we just do the best we can with what we have. And if I become aware of evidence that I hadn’t previously known about that I can’t accommodate in my model, that model would be in serious trouble, and I will either modify it or go for another. I have been reading what Bruce has been writing, and he hasn’t yet offered any such evidence.

What Bruce is concerned about here is that Mendel poured scorn on his evidence, as he did on most other evidence. I agree with Bruce that Mendel has underestimated the significance of Bruce’s evidence. Bruce’s main hypothesis, that the usual pitch standard of surviving French-style baroque woodwind instruments was 1 3/4 semitones below modern (I shift it to 1 1/2 semitones because of wood shrinkage), is uncontested as far as I know, and so is the scholar’s choice. I even used it at a crucial point in another Comm in this Q to quantify ‘Consort flute pitch’ and to motivate its adoption. We part company when Bruce gets greedy and tries to squeeze the evidence to indicate that this was the pitch of Lully’s orchestra, and even that of J S Bach.

Bruce very aptly states “If one understands the context in which they were used, traversos and recorders are convincing historical pitch pipes”. This is crucial, and I would like to survey such contexts. If the context was of unaccompanied playing of solos, duets or larger ensembles of these woodwind instruments, the pitch standard needs only be just that which was convenient for that school of makers. If the context was the same but with continuo accompaniment, the wind instruments and the accompanying instrument would probably be acquired separately, and the latter would likely be of a type (like a harpsichord or theorbo) with more flexible pitch than the wind instruments, and one would acquire one of a size to be able to tune comfortably with those instruments. So in this context, the pitch standard required is again that at which the school of makers knows how to produce good instruments efficiently. If the context was that of an ensemble of mixed instruments with all of the others being more pitch flexible, all of the others would have needed to tune to the wind instruments (modern orchestras still tune to the oboe). But the woodwind pitch level should not be very far from the pitch at which the rest of the ensemble normally plays at without the wind instruments (because players get increasingly uncomfortable with how far away from their usual pitch that they have to tune to). This may have needed special pitch specification or modification of the woodwinds. Finally, if the context was playing with a similarly
pitch-inflexible instrument such as an organ, extraordinary measures would have needed to be taken to find or have made instruments to be able to play in tune together. These four contexts are in order of increasing trouble needed to form the ensemble.

My contention is that the French baroque woodwind instruments were developed for the first two of the above contexts, and that most of the surviving recorders and many traverses of these types were made and purchased for these contexts (i.e., for domestic music making, as implied by the tutors, and wind bands, not for professional orchestral playing). The pitch standard normally followed by this school of woodwind makers was independent of the standards followed by mixed ensembles dominated by strings (and often strongly influenced by organs) that writers tended to discuss and we know the music for today. Bruce's contention is that the pitch standard that the majority of surviving recorders and traverses conform to is the same as the most popular mixed-ensemble pitch standards in France and in Germany.

I am very comfortable with that aspect of my model that puts Bruce's pitch standard a semitone higher than the mixed-ensemble pitch in France where and when these instruments were developed. Wind instruments prefer to be sharper when they 'sing out'. They pushed 19th century orchestral pitch up a semitone (from ½ semitone below to ¼ semitone above modern) to balance against the strings which had become louder because of a 50% increase in string tensions. More recently, wind band pitch was similarly sharp to orchestral standard pitch. Bruce's pitch would be the expected wind band pitch in France then.

Now back to the specifics of Comm 891. I agree with Bruce's statement that there are problems with pitch-standard names which could vary in time and place in the actual pitches they refer to. To me, these problems are there to be solved, and not accepted as ambiguity, fuzzing up the situation. The next 7 pages of his Comm relate mainly to Praetorius. First, Bruce states that he is dissatisfied with my explanation (in the Appendix of Comm 683) of why organs of the pitch calculated from Praetorius's set of pitch pipes (which most organs were supposed to be tuned to) do not appear on Mendel's list of surviving German organs (where the original pitch is known with some confidence). My explanation was that such organs do exist, but they do not conform to the latter criterion. Let me expand on this: Concerning the German organs on his list, Mendel wrote (fn 80) that his table "cannot claim to be comprehensive or even representative" because "distressingly few organ descriptions and specifications include information about pitch. Fock (1974) is a rare exception, but this causes our table to be dominated by instruments built and rebuilt by Schnitger". In the 1680's Schnitger rebuilt organs of Praetorius's time (and built new ones) to a pitch of a semitone above modern. If this was their original pitch (as Bruce would have it), why should they need to be rebuilt? As I see it, Schnitger was working to the pitch wanted by many in Praetorius's time, and Praetorius unsuccessfully resisted (even in his involvement with building the Compenius organ). Schnitger's later organs were a semitone higher, at the normal early 18th century Chorton. The Catholic German organs at lower pitches that Praetorius mentioned are not on the list either (according to Bruce's picture as well as mine).

Next Bruce claims that "Essentially there is a conflict of about a semitone between (on one side) the apparent pitch of the organ pipes for which Praetorius provided dimensions and (on the other) the pitches of other instruments (including an organ) described and depicted by Praetorius. This conflict is unresolved." It is my impression that it has been resolved. There is no ambiguity in the former that would allow an interpretation of a semitone higher (that Bruce is promoting), while there is ambiguity in each instance of the latter (discussed by Thomas & Rhodes for the organ plate and myself in Early Music (May 1985) for recorders and the trombone) which do not clearly exclude either interpretation. Bruce can't just focus on the ambiguities of the latter, and ignore the unambiguous nature of the former, which forces a clear preference for the former.

After this is a table of pitches mentioned by Praetorius that no one argues with. Bruce notes that there is no mention (a blank) of the pitch a semitone below Praetorius's standard, and observes a similarity between this blank and blanks in the pitch lists one can make from the information given by Quantz and Agricola, and in Mendel's list of surviving German organs. There is no blank in Silbermann's list, and there wouldn't be one in the Quantz/Agricola list if we include Cammerton (the usual higher type) mentioned by Quantz. This higher Cammerton isn't included in the pitch list because no intervals relative to it were mentioned. An approach commonly used in promoting a product is to make minimum mention of the leading brand. We should be much more impressed by
continuity in the standards that were mentioned than continuity in those that were not.

Praetorius associated pitches both 2 and 3 semitones below his standard with Prague and Italy respectively, and both with Catholic churches in Germany. Bruce here distinguishes between his model, where the pitch of Rome and earlier 18th century Paris mentioned by Quantz and Agricola correspond with 3, while in my model it corresponds with 2 (Naples pitch corresponding with 3).

Next Bruce mentions the Muffat (1698) evidence, which is that the French (in Lully's musical circle) used a pitch a tone lower that German Comet-Ton, or even a minor third lower for operas or the theatre. We agree in associating German Comet pitch with Praetorius's Comet pitch (which was also his general standard) rather than the Comet pitch of 18th century German writers. (At this point I would like to express my gratitude to Bruce for this idea; I couldn't fit Muffat's evidence into my model until I saw it in one of his papers).

Then Bruce attempts to test my hypothesis that Praetorius's standard was close to (and somewhat below) modern, which makes the two French pitches reported by Muffat slightly more than 2 or 3 semitones below modern, against the observed pitch of the surviving French and French-derived woodwind instruments made at Muffat's time, which is slightly more than 1 semitone below modern. My explanation is that these immensely successful instruments were developed for an existing French wind-band standard, and that Muffat was not interested enough in such music-making to mention it because it did not involve violins. Let me expand on this.

The quality of these instruments was highly appreciated. Quantz mentioned that the majority of instrument makers could not scale their instruments to a different size and keep trueness at the new pitch. But the French orchestral instruments (excluding recorders) were adapted to play at other pitch standards. Quantz wrote about how such adaptations were made to go up a tone, to the very high Venetian pitch, and the intonation problems that resulted. In Italy this was done and tolerated because "the inhabitants do not have such good taste with regard to these instruments as they have for other things in music". Changing the pitch by only a semitone would obviously be more widely acceptable. When Quantz claimed that the French woodwind instruments influenced the change in Germany when "high Chorton was supplanted by Cammerton", the latter term either referred solely to the high or normal Cammerton (which required a semitone adaptation), or it was generic, including that Cammerton as well as "the so-called A-Cammerton" (which he preferred because it required no adaptation). He also mentioned adapting to lower pitch, so using these instruments to play a semitone lower in Lully's orchestra would seem likely as well. Muffat's experience with these instruments would most probably have been in this context, and it could not have seemed significant to him that they played sharper when on their own.

French woodwinds without adaptation would play at the lower French pitch (Muffat's opera pitch and Quantz's very low French chamber pitch) by transposing down a tone, as Quantz reported early 18th century oboes (most likely not French) in Rome did. The late 18th century Paris pitch fashion (about the same as Venice) probably involved transposing up a tone for added brilliance (this practice in Germany probably helped to drop Chorton to this pitch then). The French woodwinds at their natural pitch and without transposition seem to have induced the later 18th century Ton de Chapell in France (and thus called 'French pitch' in Germany) and Consort Pitch in England as well as A-Cammerton in Germany. Yet the pitch standard with these names never became the dominant orchestral pitch standard in each country. Violins are more brilliant a semitone higher. They are more so a semitone higher still, but first strings break much more often. Where traditional string aesthetics preferred warmth to brilliance, a semitone lower than the French woodwind pitch was usually preferred. Eighteenth century pitch-standard history can be seen as a dynamic interaction between various competing aesthetic preferences, with the high-quality French woodwinds playing an active role, but not dominating.

Bruce mentions that sometime in the second decade of the 18th century, there was a "radical innovation in the woodwinds in Germany, probably directly connected to the 'introduction' of A-Cammerton there". I would like to hear more about this.

An unsatisfactory aspect of Bruce's model is that the earlier 18th century Paris pitch mentioned by Quantz and Agricola corresponds with the French opera pitch mentioned by Muffat, and not with Muffat's more usual French pitch. This is obviously why he next tries to blur the distinction Muffat
used the terms 'French musical instruments' or 'French flutes', he meant instruments at Muffat's usual French pitch (I assume that they correspond to neither of Muffat's French pitches). Bruce also assumes that 'Opera flutes' (an alternative name Denner gave for 'French flutes') meant flutes at the French Opera pitch mentioned by Muffat. So, according to Bruce, 'French flute' is at either pitch. The solution, of course, is that Denner's 'Opera' was local, ie German Opera, not French Opera. Mattheson (1713) wrote that he did not distinguish between (German) Opera pitch and the class of Cammertons that ranged over a semitone. The bottom of that range, liefl- or A-Cammerton was the same pitch as the French woodwinds. So Bruce's German quotes equating French with Opera pitch clarify the specific situation rather than confirm Bruce's muddle. The Rousseau quote concerning French opera pitch is different, apparently Quantz's 'very low chamber pitch'.

Bruce's conclusion in favour of his model here depends on there not being "a significant number" of surviving French instruments at the lowest pitch (French opera pitch) at the actual pitch implied (about 3 semitones below modern) if the pitch of Praetorius's pitch pipes is to be believed. I would not expect there to be any surviving instruments for three reasons: 1. Opera instrumentalists were professionals. Professionals conform to changing fashions much more than amateurs, and will discard recently out-of-fashion instruments much more readily. As a result, as far as we know, almost all surviving instruments in collections were acquired for and played by amateurs. 2. The evidence for domestic use of this standard all concerns stringed instruments (Mersenne's viol and Carpentier's German guitar). 3. Most woodwinds playing at this pitch would be normal French ones transposing down a tone. So, I must say that I am very surprised that Bruce offers 3 flutes and one church organ (fn 14)! How can he explain these, as he must? I must explain the three 17th century French organs on Mendel's list pitched at 1 semitone below modern, which is outside my interpretation of Muffat. My supposition is that a wind-band tradition was particularly strong in the neighbourhoods of these churches, and the organ pitch was chosen so that these bands could be incorporated in the services.

Following this, Bruce introduces quotes from Praetorius and Walther (1732), indicating strain in voices and stringed instruments due to high pitch standards. In introducing the Walther quote, Bruce mischievously writes "almost surely describing a Chorion at $@+1 \text{Is}$ [his notation for a semitone above modern]". The statement seems to imply that there is some reasonably direct evidence for this, but I suspect that he is just expressing confidence in his general model and the evidence supporting it. Bruce is far from innocent of "sometimes presenting[ing] judgements between conflicting evidence as established fact". I hope that when I show how my model fits the evidence here and elsewhere, and don't continually remind the readership that it is just my model, the context and giving the reference for a fact will make that clear. If it does not, I can be accused of the same thing.

In these two quotes, Bruce tries to support his model by these two pitches (causing strain) being the same. I think that whether a sound is straining or not is more a question of aesthetics than pitch. Quantz wrote "the high pitch is much more penetrating than the low one, on the other hand, it is much less pleasing, moving, and majestic." The people who succeeded in establishing that pitch would have called it 'brilliant' and 'exciting', and the lower one 'dull' and 'boring'.

Next Bruce quotes Mendel's citing of a report by Fock of the lowering of the 1501 organ at Altenbruch bei Cuxhaven in 1649 to a pitch called 'Chormass' (which Fock gives, and happens to be just 2 semitones above the Praetorius standard as calculated from his pitch pipes). Fritzche (the rebuilder) wrote that this led to all 'Chormassige' instruments (including Zinken) being able to play in tune together. Bruce points to the apparent discrepancy between Zinken at this pitch and Praetorius's mentioning that Cornetten Thon and Cammerthon were equivalent. Bruce uses this to try to weaken our confidence in Praetorius. Cornetten Thon was a pitch standard for cornett-led wind instruments. Fritzche could have heard a normal cornett transposing up a tone or a smaller one tuned a tone higher in Praetorius's pitch. There is no conflict here, I assume that "chormassige instrumenten" meant instruments compatible with Chorton. Fritzche's pitch is a semitone higher than Bruce's pitch for Praetorius, and I invite him to explain this situation according to his model.

Bruce then tries to further malign Praetorius because of the obvious error in the graduation of the scale in Praetorius's Plate XXVII. This error creates ambiguity in measurements of the organ pipes on that Plate, but nowhere else. According to Bruce, this "error brings into question both
Praetorius's own control over the illustrations in his book and the Corrections made when they 'were sometimes taken'. A scholar has no right to be in the business if he doesn't take his source evidence very seriously. This is slander, not scholarship. If Bruce wishes, I would be happy to discuss the problems involved in making corrections on woodblocks if one discovers a mistake when most of it is already done.

The rest of Bruce's article concerns the pitch information provided by Quantz (1752), Agricola (1757) and Silbermann (1772). All tell about relative pitches and don't give us absolute pitch levels. Silbermann's evidence is perfectly straightforward with no ambiguity, but Bruce calls it "a can of worms". The reports by Quantz and Agricola have inconsistency and ambiguity, yet Bruce calls them "relatively complete and reliable". His assignments of quality of information seems more to be based on usefulness to his cause than on any intrinsic objective criterion such as the ability to discriminate between hypotheses. Mendel's similar judgmental approach to evidence was his main weakness as a scholar, so when he could no longer sustain his favourite model, instead of reshuffling the evidence and pursuing another model, he became cynical about the evidence and about the possibility of any model successfully explaining pitch history.

"If Comm 653 might have influenced Bruce's assumption that Quantz was referring to two Chorton levels, but I did not intend to imply it, as Bruce suggests. There certainly were different local pitches at different churches which could have been called Chorton, but when an author was writing for a general German audience. I am sure that there was one generally recognised Chorton that he referred to, unless this was during a transition from one generally recognised Chorton to another (this happened around 1770, well after Quantz wrote, when Schroter (1772) mentioned a high and low Chorton). Quantz was clearly referring to a singular Chorton when he wrote "The disagreeable Chorton prevailed in Germany for several centuries, as the old organs prove", the pitch he stated was a minor third higher than his favourite (singular) pitch, A-Cammerton.

The apparent disagreement about Venetian pitch between Quantz and Agricola needs to be resolved. When comparing pitches, the only specification of an interval between them that Quantz gave was in "the so-called German A-Cammerton, which is a minor third lower than the old Chorton." Less certainly quantitative was his statement that the former was "the mean between the French [previously called 'the very low French chamber pitch'] and the Venetian". Otherwise he was mainly concerned with highness and lowness. So when he wrote that "At the present time the Venetian pitch is the highest, it is almost the same as our old Chorton", the interval between them was not interesting enough for him to mention (it was not "equal to" as the pitches of Rome and Paris were 20 years previously). They were both disagreeably high for him, and the 'almost the same' could refer at least as much to the disagreeableness as to the pitch. Thus when we compare this statement with that of his friend Agricola. that "In Lombardy, and especially in Venice ...harpischords and other instruments ...[were] barely a half-tone lower than the ordinary Chor- oder Trumpetenton", it is clear that Agricola was trying to be carefully quantitative, while Quantz was not. Consequently, we can accept Agricola's evidence without considering that Quantz's particularly disagrees with it. It is likely that Agricola decided to cover the same ground again because Quantz had been too imprecise.

Quantz's Venetian pitch was a current pitch for instruments (as old Chorton was not). It was too high and "very low French chamber pitch" was similarly too low, while A-Cammerton (the mean between them) was just right. Does this not imply that "very low French chamber pitch" was also a currently used pitch? It would not then be the pitch of Paris 20 years previously, which both Quantz and Agricola implied had changed since then. In my model, this pitch is as much lower than A-Cammerton as A-Cammerton is lower than Venetian pitch. Being a tone below normal French woodwind pitch, that good transposition makes it advantageous for woodwinds, as Quantz mentioned. This handles all of Bruce's objections to his Table B (Mendel), and makes Bruce's logical exercises of Tables C to F unnecessary. Mendel often used the unlikelihood of any French pitch lower than 2 semitones below modern as an argument in discussions (though he listed one organ at this pitch). So he wouldn't have thought of this interpretation. He didn't live to see my Comm 442.

Bruce finally arrives at Table H, which I agree with except for the inclusion of Old Chorton I (which didn't become a generally recognised Chorton till c.1770), and the omission of the 'very low French chamber pitch' current Paris pitch and. of course, the higher or usual Cammerton (Bruce's style of salesmanship parallels that of Quantz and Agricola). Bruce claims that I include that Cammerton only on the authority of Silbermann. That is untrue. The authority is Kuhnau (1717), Walther (1732) and...
Adlung (1758), an writing that there were Cammertons both 2 and 3 semitones lower than Chorton. Quantz and Agricola would not have bothered to include the A in their discussions of A-Cammerton if there were no other varieties of Cammerton.

Fitting Silbermann's evidence into the Quantz/Agricola picture gives Bruce much trouble. He objects to doing it my way because Silbermann's French pitch is the same as Agricola's A-Cammerton, while Agricola wrote that former French pitch was a semitone lower than A-Cammerton. But why should we expect a pitch standard that Silbermann called 'French' in 1772 to be the same as a French pitch standard that stopped being so (implying that it had been supplanted) before Agricola published his work in 1757? On the contrary, we should expect it to be different, which is a clear argument against his placement. The fact that Silbermann's father built an organ at the former French pitch in a French city in 1716 is to be expected, and not an argument against my placement of Silbermann's pitches against those of Quantz/Agricola.

Mendel's table of French organ pitches has six 18th century organs at 2 semitones below modern with a gap between 1716 and 1773 where it is one semitone (including one made in 1732 and the other in 1759–68). This gap is consistent with Silbermann calling 'French pitch' one semitone below modern in 1772. In France, this pitch seems to have been called 'Ton de Chapelle', as the former French pitch a semitone lower was in its time. The old pitch persisted in the old churches, but then was called 'Ton de Chœur', which was literally true. Ton de Chapelle was associated with concerted instrumental music. In the final quarter of the century, the old pitch again became the fashion for new churches. The names did not change because the functions associated with them did not change (an alternative name for Ton de Chapelle was 'le diapason'). There were two other French pitch standards in the second half of the century, both largely instrumental: the very high one like Venice mentioned by Quantz, Laborde (1780) and Gervasoni (1800), and the very low one called 'Ton de Opera' mentioned by Rousseau (1763) which can be associated with Quantz's 'very low French chamber pitch' and even Muffat's Opera pitch. With all this complexity, it is understandable that the more precise Agricola did not follow Quantz in delving into explaining current French pitches. From a woodwind prospective, Ton de Chœur didn't use them, le diapason was at natural woodwind pitch, and the other two were transpositions up and down a tone from this (but Laborde mentioned that proportionately shorter bassoons were used for the high one in the Concert Spirituel).

Bruce's placement of Silbermann's pitches relative to Quantz/Agricola is unsatisfactory for more than just ignoring the change in French pitch. Comet-Ton was a recognized pitch standard, which automatically makes it independent of possibilities of individual cornets or trumpets being tuned otherwise (modern old pianos tuned a semitone flat, and all modern horns tuned a tone or a fifth flat, don't in any way put modern pitch into question). The change from Chorton being equal to the comet or trumpet standard to a semitone lower is testified to by Petri (1782) and even Fétis (1825), as well as Silbermann. That Chorton changed and not Comet-Ton is implied by a high and low Chorton reported by Schroter during the transition. (This change was probably influenced by allowing the use of French woodwinds transposing up a tone.) Thus Silbermann's Comet Thon should correspond with that of Quantz/Agricola. Bruce appreciates this and is not happy with his placement, and would have preferred a blank in the middle of Silbermann's pitches. He shouldn't for the clear evidence for the change in French pitch.

That's it! I think I've covered all of Bruce's points in Comm 891. There are not any others in Comm 1082 that are not already covered. It was a lot of work writing all of this down, and I'm still not sure that it will do anyone any good. I must congratulate all readers who have ploughed through this Comm so persistently that they get to reading this. I am sorry (really sorry) that what I've written here is not the kind of help that Bruce had in mind for his thesis. I do wish him and his thesis well.

The main point I've been trying to get across is that scholarly method involves hypotheses or models that provide reasonable explanation for all the evidence, and that is what I've been attempting to do. This is more important than simplicity of models, which is a deciding factor in scholarship only if competing hypotheses are equally successful in explaining all of the evidence. From the point of view of convincing people (scholars as well as non-scholars) to believe what one is saying, how simple and memorable a model is, as well as how expected it is, are essential factors for success. Bruce's model, of all major baroque pitch standards (where French woodwinds were used) being the
same as his French woodwind pitch, is simple, membrane- and because of the adoption of a -415 Hz
generic baroque pitch by the early-music movement, expected. It can’t help being popular.

Many, including Bruce, seem to think that in scholarship, competing models are judged as is done in
a courtroom. The judge or jury expects each side to present its preferred model of what happened
(and why), with supporting evidence (including testimony which is expected to be biased and could
well be false), and to try to discredit contrary evidence presented by the other side. Judgement is
made on the believability of both the models and the evidence, including relative amounts of
supporting evidence (the ‘weight’ of evidence).

Believability has nothing to do with scholarly choice. In making the scholarly choice, the evidence is
accepted no matter how unbelievable it is, and the choice is made according to how well the model
fits all of the evidence, no matter how unbelievable the model is. No one needs to believe the
model that is the scholarly choice, and disbelief motivates scholars to seek more evidence and/or to
try to imagine a more believable model that could then become the scholarly choice. If these
efforts are futile, by the time the scholars run out of ideas that could change the scholarly-choice
model, it is usually accepted by the scholarly community as believable. When Einstein published the
Theory of Relativity, it was the scholarly choice, but since it is so contrary to intuition (ie
unexpected), it took a generation for physicists to believe it. The Heisenberg Uncertainty Principle
of quantum mechanics was the scholarly choice when published, but Einstein couldn’t accept it, trying
for the rest of his life to disprove it.

I suspect that Bruce’s model will have a good run, but that mine will eventually be included in the
textbooks of music history (probably not in my lifetime). If Bruce hopes for his eventually to be in
the textbooks instead, he has a lot more explaining to do.

**SUMMARY OF PITCH STANDARD MODELS**

On the left are the numbers of semitones above or below modern for each pitch standard. Bruce
has been working to the nearest semitone, and his figure is on the left. To the right of it is my
estimate to the nearest half-semitone, an accuracy not always justified, but it avoids some kinds of
confusion. When accuracy is justified, the pitch usually is between the two.

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<th>German Pitch Standards (my model)</th>
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<tr>
<td><strong>Early 17th Century</strong></td>
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<th>Other Pitch Standards (my model)</th>
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<td><strong>France: Late 17 &amp; Early 18th C</strong></td>
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<th>Pitch Standard Comparisons (Bruce’s model)</th>
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<td>Praetorius</td>
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Further Comments on our Dispute

B. H.:

Eph has kindly sent me a copy of the new Comms on pitch included in this Q, with his warm and gracious references to my "mischievous" Comm 891. I’m delighted he’s taken the trouble to answer me, and appreciative that he has done so against his own inclinations; my faith in the FoMRHIQ is restored. Though I often disagree with them, Eph’s writings are of great use to me (and I’m sure other FoMRHI members interested in -- among other subjects -- pitch history).

Eph has rekindled a number of flashpoints in the pitch debate that I thought I had sufficiently "wetted" in my 1985 JAMIS article "Johann Sebastian Bach’s pitch standards: the woodwind perspective." It is frustratingly clear from a number of remarks in Eph’s Comm that he has not read that article, or if he did, it was in his sleep -- my later Comm 891, which he is supposed to be answering, was based on that article. This is like trying to carry on a dialogue with a deaf person. [An example, and one that surprised me, was to read that “What Bruce is concerned about here is that Mendel poured scorn on his evidence.” If Eph had read my article, he would have noticed that I myself, as an oboist, now discount historic pitch evidence from double reeds as being too subjective (p. 103).] The result is that Eph’s criticisms lack substance, and I have the impression he has not really spoken to my hypotheses. Still, as Mendel wrote, "A useful test of a hypothesis is to assume alternatives to it." So at the risk of boring everyone else but possibly Eph himself (and that’s not sure either), I look forward to replying to his Comms when I have had time to test and consider their implications as I now understand them. In the meantime, obviously, it would be interesting (and encouraging) if anyone else would care to join in.

E. S.:

Yes, I’ve read Bruce’s J.A.M.I.S. article. Yes, there are statements in it that aren’t in Comm 891 that I could argue with. The task I set myself in my Comm was to answer every point of dispute in Comm 891. I am not a masochist.

Could it be that both of us are “trying to carry on a dialogue with a deaf person”? I would like Bruce to make quite clear his views on what kinds of criticisms I could make that would have “substance”, and what I could write that would have "really spoken" to his hypotheses. It would be good if he could state what his rules of the game are. I tried to imagine what they were in the first full paragraph of the last page of my Comm. Bruce is absolutely correct in implying that I write with no authority when I try to guess how he is thinking. I agree with his Mendel quote about assuming alternative hypotheses, but how are we to compare the alternatives? What are the criteria? My position has been made clear to the point of boredom. What is Bruce’s?

I join Bruce in welcoming others into this dispute. If we are talking different languages, we will get nowhere without a translator.
Early 18th Century English Pitches, Especially ‘Consort flute pitch’ and ‘Church pitch of f’

Richard Platt (3 Stratton Place, Falmouth, Cornwall TR11 2ST) has been looking at organ contracts and found that two of them (for St. Dionis Back Church by Renatus Harris 1722, and for St. George’s Hannover Square by Gerard Smith 1724/5) used the term ‘Consort flute pitch’ in their specifications. Gerard Smith later crossed this out and replaced it with ‘Church pitch of f [as in] the Organ of St Paul London’. This paper follows from some points raised in our correspondence, and is an attempt to associate meanings and absolute pitch levels to these terms, adding to the picture of 18th century English pitch standards started in Comm 1063.

After sometime in the first half of the 17th century the term ‘consort’ mainly referred to a set of instruments of one type (such as viols or the violin family) accompanied by a ‘consortative’ instrument, such as the lute, virginals or organ, which played either all the parts or continuo. In early 18th century England, the term ‘flute’ referred to what we now call ‘recorder’ (as a class of instruments), or ‘treble recorder’ (as a specific instrument). The specific instrument was sometimes called ‘Common flute’, presumably to distinguish it from the ‘German flute’ (transverse flute), or called ‘Consort flute’, presumably to distinguish it from other soloistic recorders (such as the ‘Sixth or Voice flute’) that did not normally play in flute consorts. So ‘Consort flute pitch’ appears to be a distinctive pitch standard used by accompanied recorders (especially the treble recorder).

If most of the surviving English recorders from that time were made for this purpose, their predominant pitch standard of about 1½ semitones below modern (see Comm 1039 by Haynes), would be ‘Consort flute pitch’. (It is likely that wood shrinkage since they were made would have slightly raised the pitches of these recorders, so I assume that they originally were at rather closer to 1½ than 1 semitone below modern.) There presumably were other English recorders made at that time for concerted music at other pitch standards (such as at ‘Concert or Opera pitch’ a semitone lower, or the other popular orchestral pitch a semitone higher), but very few of these seem to have survived. These instruments would have been relatively rare at the time and mostly played by professionals (survival when fashionable use has ended has always strongly favoured instruments owned by amateurs).

It is unlikely that the choice of a church organ pitch standard was for the purpose of playing with recorders. But the name for the pitch standard could easily derive from chamber organs that were built for that purpose. A more likely reason for choosing that standard was the same as that for the growth in popularity of this pitch standard at that time in Germany (known as tief- or A-Kammerton there): namely to orchestrally exploit the highly successful French transverse flutes, oboes and bassoons. This was an unusually low pitch specification for a church organ at that time, and there could well have been factors (such as cost and voice ranges) which would argue against it, causing Gerard Smith’s change of mind.

‘Consort pitch’ was also a term used when there was no ambiguity as to what type of consort was being referred to. In the second and third quarters of the 17th century, the usual and most respected consort was the viol consort. So when Thomae Mace (1676) referred to ‘Consort pitch’ for his double-lute invention (p.205) and for his theorbo (p.217), it is clear that he was referring to his domestic pitch standard, and central in his music-making was an organ that mainly accompanied his set of viols. This pitch seems to be about 2½ semitones below modern (see Comm 1063). The small organ for Salisbury Cathedral that Thomas Harris agreed to build in 1668 had ‘Consort pitch’ in its specification, so it was probably at this pitch.

By the end of the 17th century, the viol consort was well out of fashion. Roger North (ante 1723) wrote about this decline. Talbot (c. 1694) apparently couldn’t find a treble viol to measure. Renatus Harris and Gerard Smith were being careful and specific in their ‘Consort flute pitch’ specification in the early 1720’s. Since this was the only organ pitch at the time that was talked about and included the word ‘consort’, there would be no ambiguity about the meaning if this was shortened to ‘Consort pitch’, as used in the specification for the St. Mary Redcliffe organ built by John Harris and Byfield in 1726. A comparison between the original and new consort pitches seems to be the issue when Tans’ur wrote in the first edition of his New Musical Grammar of 1746: “Our new Consort Pitch is more fitter for vocal performance than the old Consort Pitch, which is half a tone lower”. This fits nicely since the ‘Concert or Opera Pitch’ (that he mentioned in the
third edition in association with the pitch of Rome) corresponds with viol consort pitch, and so with
the 'old Consort Pitch', and this is indeed a semitone lower than the pitch of the surviving recorders
here assigned to 'Consort flute pitch', which became 'Consort pitch'.

If Williams (1964, p27-28), as reported by Mendel (1978/9, fn22), was right in his surmise that
Talbot's measurements (c. 1694) of organ pipes were of those of St. Paul's Cathedral (built 1695-1696
by Bernard Smith), it is difficult to imagine how a pitch of either ⅓ semitone below modern
(calculated for 2 pipes) or ⅔ semitones below modern (for the third pipe) could lead to the name
'Church pitch of f'. Gerard Smith, who wrote this comment, was a nephew of Bernard Smith who
made the St. Paul's organ, replacing one with the previously ubiquitous 10 ft CC specification. A 10
ft FF specification was adopted for a group of new organs built after the Interregnum, after which
only lower organ pitches were fashionable. There then seems to have been no consensus on a new
standard. It thus is reasonable to suppose that the old standard retained its authority for pitch
comparisons. According to this standard, the calculated pitch standards from Talbot's pipe
measurements would be 'Church pitch of e² or e'. If the guess by Williams was wrong, and St.
Paul's was at f of the old standard (⅔ semitone above modern), and that this organ was not
seriously retuned in the interim, this could make sense of the comment by Leffler (c. 1800, reported
by Pearce in 1911) that St. Paul's pitch was 'sharp'. Also, Gerard Smith could have changed to this
more conventional higher pitch while still having the French woodwinds in mind, since transposing a
tone to match 'Consort flute pitch' would sound well enough in most meantone tunings.

Leffler's comment would seem to be inconsistent with the note (dated 1835) associated with the
Foundling Hospital fork (measured at exactly ⅔ semitone below modern), which states "Ancient
Concert, whole tone higher; Abbey, half tone higher; Temple and St. Paul's organs exactly with this
pitch." But the organ at St. Paul's was lowered a semitone in 1802*, thus originally being at the
pitch that Abbey still was. The history of the pitch of St Paul's thus supports interpreting the 'f'
in the term 'Church pitch of f' as relative to the old ubiquitous standard.

The 'Ancient Concert' pitch mentioned would correspond with the highest of the 18th century organ
pitches on Mendel's list. This pitch could be used for concerts because transposition of a tone works
well (the fork pitch is particularly good for violin projection). I suspect though that this
information could have been garbled in the longer than a lifetime since it was contemporary, and
'higher' should be 'lower', Tans'ur's 'Concert pitch'.

Consequently, there is evidence suggesting the use in 18th century England of pitches at (at least)
every semitone between the lowest at 2⅔ semitones below modern and the highest at 1⅔ semitones
above modern. Following is a list of them like that for 17th century pitches in Comm 1063:

+1⅔ some organs on Mendel's list - ?'Ancient concert pitch'? (like German Chorton)
+⅓ 'Church pitch of f' - some organs such as Westminster Abbey and St. Paul's
0 some organs on Mendel's list - [modern pitch]
-⅓ 'Chappell pitch' - some organs on Mendel's list - for full projection of violins
-1⅔ 'Consort flute pitch' or 'Consort pitch' - recorders and French woodwinds (like A-Kammerton)
-2⅔ 'old Consort pitch' or 'Concert or Opera pitch' (Roman pitch)

* - This information is extracted from Dominic Gwynn's comprehensive collection of information on
early English organs (BIOS Journal 9, 1985). The references can be found there. Being a
newcomer to this field of organ research, I only became aware of this article after writing the first
draft of this Comm. I didn't find any evidence in it that would lead me to rethink my hypotheses,
but found that these references helped to illustrate, support and refine these hypotheses. Gwynn's
interpretations differ from mine in the following ways:

1. Gwynn is technically correct in calling the pitch of a 10-ft-CC specification a fourth higher than
one of 10 ft FF, rather than a fifth lower, as I do. I prefer the lower octave because the fifth
includes in it all of the easy transpositions the organist can make to other standards.
2. He assumes that the actual length of a 10-ft-specification pipe was longer (I presume by about half a foot) to give a pitch of about a semitone lower than that calculated by Mendel from exactly 10 feet. This would put 'quire pitch' at about 1/8 semitones above modern. He states that "it was to this pitch that most surviving organs were converted in the period after 1660, and from which they were subsequently converted to pitches used in the eighteenth century." The usual 18th century pitch of 3/4 of a semitone below modern was called either 'consort' or 'concert' pitch.

I would be most surprised if Gwynn has any evidence associating this latter pitch with the term 'Chappell Pitch'. This pitch should have been called something (Talbot seems to have called it 'Chappell Pitch', which may be equivalent to 'Common Church Tone'). I also doubt whether it was called 'concert pitch' until after Tansur's 3rd edition (1756) since Tansur gave no hint of ambiguity.

As for how much longer than 10 ft a 10-ft-specification pipe was, Gwynn is in a much better position than I (and most others) to tell. Let us see the evidence. If the pitch can be shown to be more than 1/2 semitone lower than that calculated by Mendel, then my explanation for why Bernard Smith used the term 'Church pitch of f' for the pitch of St. Paul's (which appears to have been 1/4 semitone above modern) becomes untenable. If Gwynn does not have clear evidence for such a pipe length, then his hypothesis becomes doubtful unless he can come up with an alternative reasonable explanation for Gerard Smith's term (I can't). I like 10 ft to be 10 ft because a 4th lower than quire pitch is old Consort (viol) pitch, good for viols playing with organs, and since viols were interchangeable with domestic voices in madrigals, good for congregational singing.

The evidence Gwynn offers in his Appendix B on the marked pipes of the Stanford-on-Avon organ (c 1630) seems not to properly support his conclusion. The longest pipe is likely to be more than 5 ft long (Gwynn does not report lengths), was originally marked C, and was marked G in the early 18th century modification. For that latter pitch name and its current pitch, its standard is 1/8 semitones above modern. But Gwynn reports a rise of a semitone in the organ's pitch in the 17th century, involving a second set of marks on pipes a semitone away from the first, with no change of pitch in the marking on the longest pipe. After such a pitch change, the longest pipe would become redundant, and be a dummy. It is quite likely that the organ was originally built a semitone lower than a proper 5-ft specification, and that this was later corrected to meet that standard. Gwynn offers no explanation for the early rise of a semitone in pitch. If it did not occur, this organ would provide the kind of evidence I am asking for.

Gwynn's Appendix A discusses Praetorius's pitch information and comes to an extraordinary conclusion. He states that Praetorius's "Cammerton/Chorton can be a' - c 430 Hz or c 460 Hz, depending on your point of reference", citing Thomas & Rhodes (1971) for the lower pitch and the Meyers note in Early Music (1984) for the higher one. Astonishingly, he ignores his own Comm 342 (1981), which removed doubts about the Thomas & Rhodes calculation, and makes this the clear choice. My Early Music note (1985) rebutted Meyers by citing Gwynn's Comm and by showing that all of the evidence offered by Meyers could be interpreted in favour of the lower pitch at least equally as well.

In the BIOS article, Gwynn then states his opinion that the higher pitch was more likely "if only because all North German organs (including the Companius organ at Frederiksborg) that have been reliably measured are at least a semitone higher than modern pitch." Mendel's list of these has no other organ made in Praetorius's time at such a high pitch. It has another, at less than a semitone above modern, and no other (except for two rebuilt by Schnitger in the 1680's) between 1512 and 1649 (I wonder why Mendel seems to assume that Schnitger didn't change the pitch). Praetorius fought in vain against a tendency towards higher pitches. The evidence supporting the above opinion is extremely weak (see also my Comm vs Haynes in this Q). The relevance here is that Praetorius stated that current English instrumental pitch was almost the same as his standard. Gwynn seems to equate that instrumental pitch with English 'consort pitch' (which one?), and with 'quire pitch' (in spite of the 460 Hz making the 10 ft pipe almost 11 ft long).

This is the same position on Praetorius's pitch standard as that of Bruce Haynes. If Dominic is still convinced of it, he is potentially Bruce's most valuable ally in his controversy with me (see another Comm by me in this Q). Perhaps now is the time for Dominic to join in.
COMMENTS ON EARLY 18TH CENTURY ORGAN PITCHES

The following are comments on Eph's Comm. Anyone really interested should have a look at my article (copies of the BIOS Journal are available from Positif press, 130, Southfield Road, Oxford OX4 1PA), which contains almost all I know about the subject. They can disagree with it if they like, but I don't think I have pushed the very fragmentary evidence further than it will go.

It is important to realise that the organ as a church instrument was separate in its development from secular organs in important respects, one of which was pitch. The development of the English organ was also very insular, partly because of the attacks on it 1550 to 1590 and 1644 to 1660, when no new church organs were made, and old ones were destroyed. This means that you cannot argue for church organs from chamber organ evidence, or other instruments, unless the connection is proven, and that foreign examples have little relevance unless proven. The final problem is that although the folklore of the English organ is highly developed, the archival and archaeological history is in its infancy.

1. This applies to church organs built up to about 1680, from perhaps as early as 1500. 5' front pipes and a C keyboard appear in the so-called Duddington contract of 1519 (the pitch of 'the keys'). The idea of transposing is first described in 1665, though it must have been practiced before. You had to transpose either a fourth down on the 5' or a fifth up on the 10' to get 'quire' pitch (the pitch of 'the pipes').

In organ terms it is more useful to think of 'the keys' being a fourth higher than 'the pipes', though as I explain, I think the sub-octave stop (the 10'), may have been intended to provide an extended compass. This high pitch seems to me to have important implications for the performance of early English organ music up to ca 1650, though no later.

From the musicological evidence it is certain that the lower pitch was used for accompanying voices, and presumably whatever other instruments were accompanying the voices. The solo pieces were played on 'the keys', and the ranges indicate that they were played untransposed. Only the largest organs had 10' stops. One or two references suggest that 5' was considered the unison (certainly in 1519, if they thought in that way at all). There are a number of continental chamber organs whose lowest rank was a 4'.

2. As I say, there is a reference to a longest pipe 'containing' 5', and the few surviving 5' fronts have pipes which are from 1\frac{1}{2}'' to 2\frac{1}{2}'' longer. This affects the 'minor third' theory for the performance of Tudor polyphonic music. There is more evidence to be collected from C17 front pipes.
Subsequent pitch movements also suggest a C17 pitch of between one and two semitones above A440. I am not prepared to be more precise than that, especially as there are one or two organs either side of those pitches, and a few organs by the same builder a semitone apart in pitch.

The situation at Stanford has undergone some re-interpretation, stimulated partly by its connection with the organ at Tewkesbury Abbey and originally Magdalen College Oxford (see John Harper in the forthcoming Organ Yearbook 23). It now appears to have undergone a fall in pitch of one semitone, probably in 1690, a fall of a fourth, perhaps also in 1690, and a rise of one semitone in 1730. The pitch of the speaking pipes is now about half a semitone below A440, which is as close as can be to C18 organ pitch, though the pipes which became dummies in 1730 have a pitch about a third of a semitone above A440 (towards 'quire' pitch). Since the marks show a fall and then a rise of a semitone, there is a discrepancy here. There is much patching and piecing at the tops of the pipes, and the pipe marks which show which pipe was used where when are not entirely easy to identify, which goes to show how difficult it is to be precise with so little evidence.

It seems likely that Talbot took "Chappell pitch" from Mersenne, along with about half of his organ notes. He mentions Bernard Smith (particularly St Paul's, first used 2/12/1697) and Renatus Harris, and used some information which can only have come from them. The pipe lengths may have, but there are stops not known in English organs in the list, and it is not known to which organ these lengths might have applied. I have not combed through the two drafts Talbot made for his organ entry, but it seems obvious that he was learning almost from scratch, and that he garnered facts from wherever he could get them. About English organ pitch, Talbot does not tell us much.

I associated 'consort pitch' (when applied to C18 organs) with the low French pitch, in this case about one to a half semitone below A440, because they appear at about the same time in organs connected with the Harris school, which had its antecedents in Brittany. Unfortunately the term and the pitch nowhere appear in the same organ. However, the low pitch is so widely used by all builders not directly related to Smith (which by 1750 means all 18th century builders), that it is reasonable to assume that consort pitch applies to that pitch where it is used. For instance, the Harris/Byfield organ at St Mary Redcliffe in Bristol was described in contemporary description as using 'consort' pitch; it is very unlikely that this was anything but A425 or thereabouts.

There is a tradition that Gerard Smith associated himself with the Harris school; his Ely cathedral organ copied a Harris case, he used a Harris contract as a model for the Hanover Square contract and Bernard left him a shilling in his will,
supposedly a sign of pique. But he also built an organ at Finedon in Northants which is a three tower version of Bernard's case at Trinity Cambridge, and we see him at St George's Hanover Square, St Paul's Bedford and Ely Cathedral using the high pitch associated with the Smith school.

It had not occurred to me that 'church pitch of f' might be the un-transposed version of the old 'organ' pitch (bottom pipe speaking F), but that would indeed seem to tie in with the circumstance of Smith apparently continuing its use. His organ at the Grote Kerk in Edam, while still a Dutch or rather, West Frisian, resident, was almost a tone high in pitch, so that the old pitch would have been familiar to him, and may therefore have given a fresh lease of life to the Common Church Tone. By 1700 this pitch might well have been a regular $+\frac{1}{2}$. It may have been higher earlier, or the evidence which gives a spread up to $+2$, may be misleading, or misinterpreted. We know from our most complete archaeological site, the organ at Stanford on Avon, how difficult it is to interpret the evidence.

One can easily imagine why organs had a pitch called consort or consort flute pitch, at a time when instruments were used increasingly in fashionable churches, but if the Harris school organs had a pitch compatible with these instruments, why did the Smith school persist in using higher pitches? The term 'church pitch of f', seems likely to refer to the old quire pitch (the pitch of 'the pipes'), but it does seem odd that it should be St Paul's that was so consciously old fashioned, and that what was deemed convenient for Harris organs was not for Smith. In any case, St Paul's seems, from recorded later pitch movements, to have been about half a semitone high originally.

3. I will say nothing about the C17 consort pitch. From the minute amount of organ evidence, I was aware of the possibility of at least one lower secular organ pitch. Praetorius' evidence was one suggestion. I cannot tie up this evidence of a low pitch (wherever it is precisely) with the small chamber organs traditionally connected with Bernard Smith, which are around one to two semitones above A440 where the pitch can be calculated at all.

I have a similar ambivalence about Praetorius' pitch. It is odd that German organ pitch in the 17th century never seems to descend to the A430 which my 1981 calculations suggested for Praetorius. I would prefer the accumulated evidence of North Germany's organs to abstruse calculations from a diagram. The only organ from Praetorius' list I know about which seems to have its original pitch is the Compenius organ at Frederiksborg, whose design and construction Praetorius may well himself have had something to do with. It has a high pitch (about $+1$). I know of no Dutch or North/Mid German church organ with a low pitch before the early 18th century, when contracts start to refer to temperaments and pitches, which would make the organs compatible with other instruments.
I do not equate this consort pitch with my 'quire pitch'. The latter had its origins in a time when voices and organ were used increasingly together or alternating, and came in the later C17 to be seen as the usual organ pitch (the Common Church Tone). Keyboards were supplied accordingly, exploiting an acquired taste for long compasses and pipes below C. This table gives my feelings about the way these organ pitches may relate to each other.

<table>
<thead>
<tr>
<th>Pitch Difference</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 1/2 to +1 1/2</td>
<td>C16 to C17 church organs ('quire' pitch)</td>
</tr>
<tr>
<td>+1</td>
<td>some Smith school organs and C17 chamber organs</td>
</tr>
<tr>
<td>0</td>
<td>one or two Smith organs</td>
</tr>
<tr>
<td>-1/2</td>
<td>C18 Harris school organs (consort pitch)</td>
</tr>
<tr>
<td>-1</td>
<td>C17 consort pitch</td>
</tr>
</tbody>
</table>

This is an objection that lies in common against all perforated pipes; the best that the makers of them can do is to tune them to some one key, as the hautboy to C, the German flute to D, and the flute-a-bec to F; and to effect this truly is a matter of no small difficulty .... For these reasons some are induced to think notwithstanding what we hear daily of a fine embouchure and a brilliant finger, terms so nonsensically applied, as they are, to the German flute, that the utmost degree of proficiency on any of these instruments is scarcely worth the trouble of attaining it.

Sir John Hawkins. 1776.