BULLETIN 111

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The next issue, Quarterly 112, will appear in May 2009. Please send in Comms and announcements to the address below, to arrive by May 1st.

Fellowship of Makers and Researchers of Historical Instruments


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Well, so far so good. Once again we have more than enough material for another Quarterly, so that again some material has been held back, for the May issue. In deciding what to print and what to hold back, I have been swayed by the considerations that, (1) at least something by each author who submitted multiple papers should appear, (2) the range of subject matter should be as broad as possible, so there will be something of interest for everyone, (3) responses to previous Comms, including those printed a long time ago, should appear straight away and (4) no paper should be held back for more than one issue.

Happily, although our revival kicked off with a monograph on lutes, Comms on woodwind and reeds are now starting to arrive, as hoped, and there are some announcements about important developments in online resources. The last page is a form you can cut out and send off if you would like to donate to the fund to raise money for a proper home for the Kessler collection of viols.

Members' announcements are always welcome – if using a computer, please send these as plain text emails, rather than attachments.

A number of people have asked when subscription forms will be sent out. In case you hadn't heard, there are still funds from when you, dear reader, paid your last subscription seven years ago, so you have been credited with a full year of Qs before we start to ask for any money. Subscription forms are planned to go out with the May issue, so henceforth our subscription periods are likely to run with the UK tax year, rather than the calendar year!

We hope by that time to have a website up and running, where you will be able to read or download antediluvian Qs, buy more recent issues, or pay your dues online.

Keep those Comms coming!

**Where are they now?** Over six years our address database has got a bit out of date. Does anyone know the whereabouts of Peter Koval, Robert Greensitt, Martin J Turner and Peter O'Donnell of Iowa City. They paid their subscriptions along with everyone else, and are entitled to receive FaMRHIQ; recent correspondence has been returned by the Post Office. Many thanks to all of you who have given information so far, including news of those who have gone to mix their music with that of the angels.

**Email addresses, please!** If you haven't received any emails from us this year, that means we don't have your email address. It makes communication so much easier if we have it. We promise not to send out any spam, or pass it on to anyone else. Please send a brief message to Lutessoc@aol.com, and we can add you to our list.

**STANDING CALL FOR PAPERS**

The Fellowship of Makers and Researchers of Historical Instruments welcomes papers on all aspects the history and making of historical musical instruments. Communications or ‘Comms’ as they are called, appeared unedited (please don't be libellous or insulting to other contributors!), so please send them EXACTLY as you wish them to appear – in 12 point type, on A4 paper with a 25mm or 1 inch border all round, or to put it another way, if you are using non-European paper sizes, then the text area must be 160 x 246 mm (or at least no wider or longer than this). Our printers usually make a reasonably good job of scanning photos.
You can send contributions EITHER on paper, OR as a Word-compatible or PDF attachment. If you really do not have access to a word processor of any kind, we may be able to retype typed or handwritten submissions.

NOTE OUR NEW ADDRESS:

FoMRHI  
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United Kingdom

and the email address for Comms sent as attachments (and other email correspondence) is Lutesoc@aol.com

Non-members will be given a year's free subscription if they send in a Communication to the Quarterly.

If you ever sent in a paper (in the last 6 years) for the Quarterly, and it never appeared, please re-send it, to the new address.

There are plans to scan back issues of the Quarterly and make them downloadable from a website, to be set up; in the meantime you can obtain back issues for the princely sum of £3 per issue, including postage; send a cheque payable to FoMRHI, at the above address, or write with your credit card details.

If your interests have changed, and you don't now want to be a member of FoMRHI, please let us know, to save postage costs.

MEMBERS' ANNOUNCEMENTS

Classical handmade guitars and one steel string. At cost price. Spruce fronts, indian rosewood backs and sides, ebony fingerboards, oiled finish. Made with great enjoyment by me. Call Mike, on 01702 556892 (Southend area).

Jan Bouterse's dissertation about Dutch woodwind instruments and their makers, 1660-1760, has been translated and published in English language at: http://home.hetnet.nl/~mcjbouterse/inhoud&samenvatting.htm; also http://www.kvnm.nl/current/03Catalogus/BN_9.htm, with English summary. He is on the point of finishing a (thick) manual on making woodwind instruments, incorporating his experience gained from research into historical instruments, though this is currently in Dutch only.

Contact: Jan Bouterse, Sandenburg 69, 2402 RJ Alphen a/d Rijn, Netherlands tel. + 31 172 445957 e-mail: mcjbouterse@hetnet.nl
Announcement

The Viola da Gamba Society’s *Thematic Index of Music for Viols* has now been added in its entirety to the web-site (www.vdgs.org.uk) and comprises all additions and corrections to November 2008. In future it is intended to update the work annually. Some of the files are large and will be split into smaller units at the next up-date. Copies on CD-ROM can still be made for those who would prefer this format.

Volume II of *The Viola da Gamba Society Journal*, edited by Peter Holman, is now available on-line. Contents are:

1. ‘The Nomenclature of the Viol in Italy’ – Bettina Hoffmann, translated by Richard Carter and John Steedman;
3. ‘Laurence Sterne the Musician’ – Peter Holman;
4. ‘Laurence Sterne, Charles Frederick Abel and the Viol’ – Claire Berget

Reviews: *The Viola da Gamba Society Index of Manuscripts*, vol. 2 (David Pinto);
Mary Cyr, *Essays on the Performance of Baroque Music* (Susanne Heinrich);
William Lawes, The Harp Consorts (PRB Viol Consort Series, no. 62) (John Cunningham)

The editor of Volume III (to be published at the end of 2009) will be Richard Carter, Bahngasse 11, A-3420 Kritzendorf, Austria (e-mail: johanna.richard@utnet.at). A theme will be viol music in tablature, although papers on other subjects are also invited; please contact him or the General Editor, Andrew Ashbee (e-mail: aa0060962@blueyonder.co.uk). A style sheet has now been compiled, and is available on the Society’s website.

Andrew Ashbee
Jan Steenbergen and his oboes

Who was Jan Steenbergen and when and where was he working as a maker of woodwind instruments? There are no more than two of three documents which give some information about him. From a document in the Amsterdam municipal archive we learn that the 26-year-old flute-maker Jan Steenbergen, born in Heerde, was betrothed in 1702, with his father Jan Steenbergen in attendance. Two years earlier, an advertisement was published in the Amsterdamse Courant, which reads as follows (translated): 'Jan Steenbergen, master flute-maker, presently living in Kerkstraat in Amsterdam opposite the Amstel Church*, makes and sells several types of wind instruments for a fair price, money back if not satisfactory; the aforementioned J. Steenbergen worked for 8 years in succession for the widely renowned master Richard Haka, and since then as a master himself.' Steenbergen probably placed his advertisement shortly after embarking on an independent career at the age of 24. This would mean that he learned his trade from Richard Haka between 1692 and 1700, about the same time that Coenraad Rijkel and Abraham van Aardenberg were serving their apprenticeships. The date of Steenberg's death is not known. According to Waterhouse (New Langwill Index, 1993, p. 384) he died in 1752, but there might be a mix-up with the death-date of another maker, Engelbert Terton, documentary proof of whose death in 1752 exists.

In a prospectus of the German company Moeck's copies of Steenbergen instruments, published around 1990, the year of Steenbergen's death is given - without reference to any source - as 1724. This date is however inconsistent with the fact that four years later (in 1728) the burial certificate of Steenbergen's wife Margrita (Margreta) van der Heine does not state whether she was a widow when she died.

*: Frans Bruggen lives nowadays next to the Amstel Church (Amstelkerk), which means that two recorders by Steenbergen returned very close to the place where they were made.

Some of the boxwood instruments by Steenbergen were never stained and on these instruments we can see the very clear maker's mark: I: STEENBERGEN*(nota bene the dot above the I and the full stop after the last N) in a scroll, with a fleur de lis below.

Recorders and oboes

Eight or nine recorders and eleven oboes by Steenbergen did survive, but some of them are not complete. We do not know about traversos made by Steenbergen, but there is a record of a chalumeau in the catalogue of the sale of Michiel van Bolhuis' estate in 1764 in Groningen (see Comm. 1538 in FoMRHI-Q No. 89 from 1997).

Steenbergen's alto recorders (see Comm. 1569 in FoMRHI-Q No. 91) are interesting because they vary rather much in the design of the bore, window/windway and fingerholes. The alto in the Bruggen-collection has a large window and a relative wide bore in the foot and lower section of the middle joint. The ivory alto, now in the National Music Museum (formerly the Shrine to Music Museum) of the University of South Dakota (USA), has a much smaller (shorter and narrower) window and also a narrower bore in the lower section of the instrument. These differences have an effect on the playing characteristics of the recorders: the Bruggen alto with a stronger sound, the alto in Vermillion much more modest. Both altos have a pitch of about a1=410 Hz, thus slightly lower than a=415 Hz. There is a loose middle joint of an alto recorder with the traces of the Steenbergen stamp in the Gemeentemuseum in Den Haag, which is longer and has a wider bore than the Bruggen-alto. I expect that this instrument had a (much) lower pitch. It is important to know about the characteristics of these recorders when examining the oboes by the same maker (it is easier - or more accurate - to assess the pitches of recorders).

We do not know which of the recorders of Steenbergen is the oldest. The differences between these instruments may be the result of a development in the designs, or it is all about the adaptations a skilled maker could apply, listening to the wishes of the buyers of the instruments.
The oboes by Steenbergen

Eleven oboes by Steenbergen could be traced and examined for my research. In the list below I have used the numbers as they are published in my dissertation (Bouterse 2005). All oboes are designed as descant oboes, with the c1 as fundamental.

10- oboe in c1, ebony, silver rings, silver keys; Gemeentemuseum Den Haag, The Hague, Netherlands. Inventory number: Ea 7-1952.
11- oboe in c1, unstained European boxwood, silver keys; Gemeentemuseum Den Haag, The Hague, Netherlands. Inventory number: Ea 3-x-1952.
12- oboe in c1, ebony, ivory rings, silver keys; private collection, Tokyo, Japan.
13- oboe in c1, ebony, ivory rings, silver keys. Bell probably not original; Instrumentenmuseum, Brussels, Belgium. Inventory number: 968.
14- oboe in c1, unstained European boxwood, brass keys; Instrumentenmuseum, Brussels, Belgium. Inventory number: 2611.
15- oboe in c1, ebony, silver keys, bell missing; Musikinstrumentenmuseum, Berlin, Germany. Inventory number: 2940.
16- oboe in c1, European boxwood, brass keys; bell missing; Musikinstrumentenmuseum, Berlin, Germany. Inventory number: 2949.
17- oboe in c1, unstained European boxwood, brass keys; collection Han de Vries, Amsterdam, Netherlands.
18- oboe in c1, brown-stained European boxwood with ivory bell-rim ring, brass keys; top joint unstamped (maker perhaps Van Aardenberg); private collection (not Han de Vries), Amsterdam, Netherlands.
19- oboe in c1, unidentified tropical wood, silver keys; top joint by J.G. Ludewieg; Musikwissenschaftliches Seminar, Universität Göttingen, Germany. Inventory number: 636.
20- oboe in c1, brown-stained European boxwood, brass keys; National Music Museum, Vermillion SD, U.S.A. Inventory number: 6089.

The history of the oboes

The top joint of oboe no. 18 is unstamped, but the turned profiles and the rather steep conical bore of this joint point to Abraham van Aardenberg as its maker. It is also likely that Van Aardenberg added the ivory bell-rim and that he maybe also stained the wood of the oboe.

The top joint of oboe no. 18, turned (in the style of the oboes) by Abraham van Aardenberg.

The top joint of oboe no. 19 is made of a different wood than the rest of the instrument. It is stamped 'J.G. Ludewieg', who perhaps made it to replace a damaged instrument, recycling the finial as a component of the ivory finial bead on Steenbergen's original top joint. In turning a finial cup, Steenbergen follows a tradition to which we owe Haka’s and Rijkel’s oboes (but only on their shorter descant oboes, though) and which climaxes in the oboes of Hendrik and Fredrik Richters. In The New Langwill Index Waterhouse states that a woodwind maker called Gregorius Ludewieg was active in Leipzig between 1770 and 1783; one of his creations was an Inventionstrompete. The name I.C. Ludovic also crops up: there is an oboe of his in Linz. According to Heinzl, G. and I.C. Ludovic are identical, but he does not go into details. The museum of musical instruments in Brussels once owned three Steenbergen oboes. Two of them (inv. nos. 968 and 2911) are still in the collection and were described for this investigation.
the third, no. 967, disappeared without a trace around 1980. The instrument was made of dark-stained European boxwood with ivory rings and three brass keys; its total length was 581 mm. Mahillon described this last oboe on p. 250 of his catalogue of the Brussels museum, giving its length (including the staple) as 62 cm and its pitch as exactly a semitone under normal.

Steenbergen's instruments in the Berlin Musikinstrumentenmuseum were formerly in César Snoeck's collection. Two other oboes (nos. 10 and 12) were owned by the Dutch conductor (Concertgeouworkest) Willem Mengelberg. The whereabouts of a third oboe by Steenbergen in the Mengelberg sale catalogue (1952) are unknown; the possibility that it is the Steenbergen oboe no. 18 cannot be ruled out. Incidentally, the oboes in that catalogue are listed as schalmeien (shawms) and Steenbergen's name is wrongly spelled as Sternbergen. The oboes no. 20 was formerly in a Dutch private collection.

Characteristics of the Steenbergen oboes

Jan Steenbergen's oboes can be divided into two groups. The first consists of nos. 10, 13, 15 and 19, traditionally designed in what I call the 'Haka/Denner style', with the striking feature of a middle bead complex just above the lower (round) key ring. Oboes no. 13 and no. 19 each has shallow finial cup. The second group consists of oboes no. 11, 12, 14, 16, 17, 18 and 20, which are homogeneously designed with the eye-catching characteristic of a bead complex quite high up on the flare of the bell. In this second group there is no middle bead complex above the lower key ring, nor are there any scribe lines on the middle bead complex; the finial face is turned slightly convex. Most of the turning is neatly executed, Steenbergen evidently preferring subtle details to bold patterns; the key rings, for example, tend to be rather low.

Oboe no. 13 by Steenbergen, part of the middle joint with the keys and the rather low key rings. The arrow points to the 'middle bead complex', characteristic for many oboes by R. Haka, J.C. Denner and the first (and maybe oldest) group of oboes by Steenbergen.
The boxwood of the oboes in the second group is of the best quality I have ever seen: with very narrow and regular growth rings and straight grain. The joints of these oboes are also hardly warped (in length as well in cross section) and between the instruments the bore dimensions are only very slightly different. This can be seen as an indication that the boxwood was very well seasoned and treated by Steenbergen.

The boxwood oboe no. 16. Steenbergen often put a fleur de lis on the face of the finial (where the very fine quality of the wood can be assessed).

Oboe no. 16: there is no middle bead complex just above the lower key ring on the oboes of the second group.
undercutting with wider grooves and irregular spots around the hole
(Steenbergen-no. 12, hole 2)

Oboe no. 17, view into the bore of the bell, with the deep grooves alongside the resonance holes.

Oboe no. 11, with the bead complex rather high on the flare of the bell (arrow), also characteristic for the oboes of the second group.

A striking characteristic of several oboes in group 2 (but as far as I know not on no. 20) is to be seen in the bore round the finger-holes. In perspective the bores of these instruments look quite disorderly, the most extreme example being oboe no. 12. These adjustments cannot be dismissed as carelessness; they must have been intentionally executed by Steenbergen, using special tools. As yet, however, we do not know whether his intention was to benefit the instrument's timbre and speaking ability, or to improve the intonation. Similar lateral lines are encountered on the occasional instrument by Richard Haka, on or around the sidewalls of the undercutting of certain finger or resonance holes, but it is only in Steenbergen's second group of oboes that the phenomenon is so noticeable. Incidentally, there are no such undercuts on his oboe no. 20. The vanished Steenbergen oboe inv. no. 967 in the Brussels museum of musical instruments was made, according to Mahillon, of black-stained boxwood with silver keys and large ivory mounts. However, this information does not suffice to designate the oboe to the first or second group.
Table 1: Measurements of the Steenbergen’s oboes

<table>
<thead>
<tr>
<th>instrument</th>
<th>SL sections and total I + II + III = total L</th>
<th>L hole 1-6*</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>group 1</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>no. 10:</td>
<td>212.1 + 213.8 + 146.6 = 572.5</td>
<td>193.6</td>
</tr>
<tr>
<td>no. 13:</td>
<td>217.0 + 216.5 + 145.6 = 579.1</td>
<td>190.4</td>
</tr>
<tr>
<td>no. 15:</td>
<td>212.0 + 215.5 + 145.6 = 575</td>
<td>189.8</td>
</tr>
<tr>
<td>no. 19:</td>
<td>215.6** + 216.7 + 145.6 = 577.9</td>
<td>190.1</td>
</tr>
<tr>
<td><strong>group 2</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>no. 11:</td>
<td>212.3 + 210.1 + 149.3 = 571.7</td>
<td>192.1</td>
</tr>
<tr>
<td>no. 12:</td>
<td>212.5 + 210.5 + 149.0 = 572.0</td>
<td>192.1</td>
</tr>
<tr>
<td>no. 14:</td>
<td>212.0 + 210.0 + 142.1 = 564.1</td>
<td>191.6</td>
</tr>
<tr>
<td>no. 16:</td>
<td>213.0 + 210.0 (bell missing)</td>
<td>193.1</td>
</tr>
<tr>
<td>no. 17:</td>
<td>212.0 + 211 + 147.8 = 570.8</td>
<td>192.9</td>
</tr>
<tr>
<td>no. 18:</td>
<td>209** + 209.0 + 148.0 = 566.0</td>
<td>190.2</td>
</tr>
<tr>
<td>no. 20:</td>
<td>212 + 210 + 150 = 572</td>
<td>not measured</td>
</tr>
</tbody>
</table>

*: the distance between holes 1 and 6 is measured at the outside of the instrument; the internal distance might be some millimeters longer; L = length, SL= sounding length; **: this joint is not made by Steenbergen; ***: bell is lost; total length of the instrument according to Sachs (Sachs 1922, p. 274; NB: Sachs’ measurement accurate to 0.5 cm).

Table 2: Some bore measurements of the Steenbergen’s oboes

<table>
<thead>
<tr>
<th>instrument</th>
<th>bore upper joint top-min-bottom</th>
<th>bore middle joint min - max</th>
<th>bore bell min - max - lip</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>group 1</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>no. 10:</td>
<td>8.8 - 6.6 - 10.8</td>
<td>12.0 - 16.6</td>
<td>20.0 - 47.2 - 43.0</td>
</tr>
<tr>
<td>no. 13:</td>
<td>8.8 - 6.4 - 11.7</td>
<td>11.5 - 17.0</td>
<td>17.3 - 49.5 - 45.6**</td>
</tr>
<tr>
<td>no. 15:</td>
<td>8.2 - 6.4 - 11.6</td>
<td>11.4 - 16.7</td>
<td>***</td>
</tr>
<tr>
<td>no. 19:</td>
<td>8.2 - 6.0 - 11.0**</td>
<td>11.7 - 17.5</td>
<td>19.8 - 48 - 41.6</td>
</tr>
<tr>
<td><strong>group 2</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>no. 11:</td>
<td>8.6 - 6.1 - 11.7</td>
<td>11.7 - 17.8</td>
<td>19.1 - 48.5 - 42.0</td>
</tr>
<tr>
<td>no. 12:</td>
<td>8.9 - 6.3 - 12.0</td>
<td>12.0 - 17.5</td>
<td>19.8 - 43.8 - 38.3</td>
</tr>
<tr>
<td>no. 14:</td>
<td>8.8 - 5.8 - 11.7</td>
<td>11.8 - 17.5</td>
<td>20.5 - nm - 39.0</td>
</tr>
<tr>
<td>no. 16:</td>
<td>8.6 - 6.2 - 11.7</td>
<td>11.7 - 17.1</td>
<td>***</td>
</tr>
<tr>
<td>no. 17:</td>
<td>8.9 - 6.0 - 11.7</td>
<td>11.8 - 17.2</td>
<td>19.8 - nm - 39.9</td>
</tr>
<tr>
<td>no. 18:</td>
<td>8.4 - 6.1 - 11.6</td>
<td>11.8 - 17.4</td>
<td>19.8 - 44 - 40.2</td>
</tr>
</tbody>
</table>

min: minimum diameter max: maximum diameter
**: this joint is not made by Steenbergen;
***: bell is lost
From the data in table 1 we see that the longer oboes (such as no. 13) need not necessarily have the longest distance between holes 1 and 6. This is the reason why no statements can be made about differences in tuning by referring to the lengths of the joints. The oboes in the second group appear to be more homogeneous with regard to joint-lengths: the original top joints differ by only 1 mm, the centre joints by 2 mm at the most, the bells by 1.5 mm. An interesting exception is no. 14, whose bell is visibly shorter than that of other oboes. Steenbergen evidently had trouble centering the turned (lower) and reamed (upper) sections of the bores with regard to one another (probably due to a 'bend' in the bore while he was working). Hole 1 in all Steenbergen oboes is drilled noticeably upwards, hole 4 only slightly so. Holes 3 and 6 are often drilled slightly downward. Hole 2 is interesting: on oboes nos. 10 and 15 it is drilled at a downward angle and on the other instruments upwards. The key and tuning holes are all drilled straight.

Eight oboes were measured in detail. On the whole, the bores of the centre and top joints follow the same regularly curved and slightly parabolic pattern; only no. 12's bore has an almost straight-conical profile in the centre and top joints. At the bottom of the top and centre joints of oboe no. 10 the bore follows an almost flat course for a stretch of ca. 60 mm, but this could be due to tenon contraction, a phenomenon rarely observed in the other instruments. In oboe no. 12 the passage is very short and the transition to the counter bore fairly abrupt; in all the other instruments the passage is slightly longer and the transition to the staple and main bore more gradual.

The top and centre bores always connect smoothly - with the exception of oboe no. 10. The top-joint bores of oboes 13 and 15 are almost identical (the counter bores of these instruments, both of which belong to the second group, differ slightly here), but correspond fairly closely (including the counter bore) with oboe no. 11 from the second group. There is a very gradual transition from the bore of the top joints to the passage, where the diameter of the bore is most narrow. Oboe no. 12 (see the graph below) has a much more sharp transition from upper joints bore to the passage.

The bore of the top joints of oboes no. 18 and 19 - these sections were not made by Steenbergen - deserve special mention. In no. 18 the bore of the unstamped upper joint runs steeply conical between the reamer entrance at the bottom of the top joint to the top joint of L143.
Then comes a very long passage (ca. 60 mm), followed by the counter bore. The pronounced conicity of the steep section of the bore is typical of the top joints of oboes by Van Aardenberg, which however have wider bores and a much shorter passage. In comparison with Van Aardenberg's in the Gemeentemuseum Den Haag (Inv. No. Ea 438-1933), the same reamer was inserted some 40 mm less deeply into the top joint of Steenbergen's instrument. It looks very much as if Van Aardenberg himself made a new top section for the existing Steenbergen oboe (also because of the characteristic pointed middle bead in the turnery. He used the his own reamer(s) to do so, not copying the top-joint bore of any of his own oboes but arriving at a kind of compromise with the bores of Steenbergen oboes as we know them. The finger-holes are also less deeply undercut than those on the original Van Aardenberg oboes. The bore of the top joint stamped by Ludewieg on oboe no. 19 differs only slightly from the bores in authentic Steenbergen top joints. The counter bore is very steep and short, though. The passage is again unusually long; for an approximately 40 mm stretch its diameter is less than 6.2 mm.

Graphs of the bores of three oboes (nos. 12, 18 and 19) by Steenbergen

The centre joints of the Steenbergen oboes differ more from one another than the top joints of the same instruments. The bore of no. 10's centre joint has a marked resemblance to that of no. 18, though, and generally speaking to those of no. 11 too. Such differences as there are apply less to the general line of the bore than to deviations within short tracts, probably caused by the widening finger-hole undercuttings, which influence the width of the bore. Comparing the centre joints of oboes 13 and 15, we see that the bore diagrams follow almost the same course, but with a considerable 'length-lag'. Steenbergen inserted his reamer some 50 mm further into the centre joint of his oboe no. 13! He may well have used the same reamer for the centre joints of nos. 12 and 16. The lowest 50 mm of no. 16's centre joints do show a 'wave', indicating that Steenbergen used a different reamer for that section. As is the case with the centre and top joints, there is not much difference between the bell bores either. The absolute values are fairly consistent, and Steenbergen invariably turned the bell-lip in the same manner: with a hemispherical inner edge and a sharp bend (i.e. not a gradual transition) towards the inner convexity of the bell. No. 14's bell is somewhat shorter than those on Steenbergen's other oboes, but here we have an unusual situation: the maker seems to have had trouble centering the wood when turning this bell and had to do some extra cutting to save it.
Graphs of the bores of three oboes (nos. 10, 11 and 12) by Steenbergen

1- Van Aardenberg-no. 13
2- Beuker-no. 1
3- Terton-no. 10
5- Boekhout-no. 18
6- H Richten-no. 8
8- Steenbergen-no. 11

Bell-lip shapes of some Dutch oboes

Summing up: it is not clear why there is such little variation into Steenbergen's top-joint bores and so much more in the centre joints. Nor is it certain whether Steenbergen used one or more reamers for each section of his oboes. Finally, a few parallels can be drawn between his oboe and recorder bores: the foot-joint bores of two of his alto recorders (nos. 5 and 7 in Bouterse 2005) are the only ones which could have been made with a reamer used by Steenbergen for the centre joints of some of his oboes.

Conclusion

Why this article? I want to indicate that for scientific research it is always interesting to investigate several instruments by the same maker. If you see only one or two of his instruments, you will surely miss some details which are characteristic for the maker. But you will also confronted with some new questions, such as about the typical fingerhole undercuttings on the Steenbergen oboes. If you had only seen the oboe no. 14 in Brussels, you might think that Steenbergen was a sloppy maker, not able to make a good inner shape of an oboe bell. And you didn't realize that this bell was some mm shorter than those of the other oboes by the same maker.

Another interesting question for the research is how the oboes of Steenbergen are related to those of other woodwind makers. Abraham van Aardenberg made recorders and oboes in a very personal and recognizable style. Coenraad Rijkel was probably more conservative, his instruments not unlike those of his uncle and teacher Richard Haka. I believe that Steenbergen...
was more influenced by the instruments of other makers, perhaps by those of Bressan and Stanesby. The alto recorder by Steenbergen in the Brüggen collection could easily be an English instrument, the ivory alto in Vermillion is the only Dutch recorder with double holes for f# and g#. I know, it is difficult to prove such theories; but it is always good to look further and not to stop your research after one instrument, or after the instruments of only one maker. I believe that the oboes of Steenbergen are very well made, instruments on which a wide variety of baroque music can be played.

No investigation is complete without a playing session. And here is a problem. Only two of the oboes (nos. 11 and 12) could be played for a short time, and then there is always the problem of finding the right combination of staple and reed. The results are always subjective but some useful information can be obtained by comparing with other instruments. Oboe no. 11 was played by Piet Dhont for the catalogue of Dutch double reed instruments of the Gemeentemuseum (p. 217). He played the oboe at a pitch of about 10 to 20 cents below a=415 Hz. In Piet Dhont's opinion Steenbergen's instrument sounds less reserved, more open, more direct and more robust than the fairly intimate-sounding oboe no. 10 by Terton in the same collection. On the whole, Steenbergen's oboe no. 11 may be said to play well, with a sound whose character is less pronounced than the Terton oboe and therefore perhaps suited to a somewhat wider range of baroque music. Steenbergen's oboe no. 12 was played only briefly; it proved possible to make it sound at, or slightly below, a=415 Hz.

Several contemporary makers (Andreas Glatt, Peter van de Poel among them) have chosen Steenbergen oboes for their point of departure in making copies. This is an indication of the fine quality of the original oboes and also of the wide range of music for which they are suitable.

Bibliography
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* This is the English translation of my thesis, with descriptions, measurements and photos of the instruments; see for a summary, table of contents and a review by Jeremy Montagu: http://home.hetnet.nl/~mcjbouterse/inhoud&samenvatting.htm. The photos and drawings in this article are taken from the thesis.

Measurements of Steenbergen oboe no. 12, ex-Mengelberg collection, nowadays private collection Japan. Instrument in ebony, with ivory mounts and silver keys.

Upper Joint: L 237.5; L without tenon: 212.5; tenon: L 25.0; baluster/finial: L 103.5; Øext at lower shoulder: 21.2;
finger-holes (L from shoulder of tenon to centre of hole; ØWxL; Øext-max):
hole 1- 74.3; 3.0 x 3.2; 18.8 drilled upwards
hole 2- 38.6; 3.4 x 3.6; 19.8 slightly up
hole 3l 7.5; 2.7 x 2.9; 20.9 slightly down
hole 3- 7.3; 2.7 x 2.9; 20.9 slightly down
bore (Ø, Lmin/max, from lower end):
11.9/12.0- 0; 11.2- 15/ 36; 11.0- 41/ 42
9.6- 88/ 89  8.8- 113/114  8.4- 119
7.6- 141/142 7.4- 149/152 7.0- 176/178
6.4- 199  6.3- 200/ and through
counter bore (from upper end):
8.8/8.9- 0  8.5- 7  8.0- 16  7.0- 26  6.4- 37
6.3- through

middle joint: L 236.8, L without tenon: 210.5; tenon: L 26.3; socket: L 26.0, Ø-max 17.5
finger-holes (L from upper end to centre of hole; ØWxL; Øextmax):
hole 4l-  48.7;  3.8 x 3.8;  21.4 drilled slightly upwards
hole 4r-  48.8;  3.7 x 4.0;  21.4 slightly up
hole 5-  85.4;  4.8 x 5.0;  22.5
hole 6-  117.8;  4.6 x 4.7;  23.9 downwards
key holes, L from lower shoulder:
d#-l-  47.8;  ca 5.4;  25.8
d#-r-  47.5;  ca 5.5;  25.8
c-hole-  16.0;  ca 6.9 x 6.8;  26.8
All holes with irregular undercutting grooves, visible looking through the bore in the bore around
the holes.
bore (Ø, Lmin/max, from lower end):
17.5- 0  17.0- 11/ 15  16.6- 28/ 30  16.0- 49/ 56  15.4- 80/ 86
15.0- 101/108 14.4- 125/130 14.0- 141/146 13.4- 165/167 13.0- 176/182
12.4- 199/200 12.2- 203/205 12.0- 211/ and through

bell: L 149.0; socket: L 26.3, Ømax 24.4;
resonance holes at L 51.0 and 50.7 from upper end, Ø 5.0 x 4.9 and 5.0 x 4.8, Øext 28.2;
bore (Ø, Lmin/max, from lower end):
38.2 x 38.3- 0 (lip)  43.6 x 43.8 - 8  40.0- 20  36.0- 28  28.0- 45
22.0-  69  21.0-  80  20.5-  89  20.0- 111/118  19.8- 115/-
Observations on Glue and an Early Lute Construction Technique.

Dincer Dalkilic is an oud maker working in the Turkish tradition. His efficient method for constructing an oud bowl is interesting (1). Heat bent ribs and inter rib purfling are assembled on an open mold of the bulkhead or ‘toast rack’ type. After coating each rib joint surface and purfling strip with hide glue and working little by little from one end of the joint to the other – the purfling strip is fed into the joint using a heated iron to remelt the glue. The surfaces of the joint are, at the same time, brought together and held in position with glued pieces of paper - each piece of paper being set in place by touching it with the heated iron. After completion of the bowl assembly, the mold is removed, the internal rib joints reinforced with glued paper strips in the usual manner and the pieces of paper, covering the whole of the exterior of the bowl, removed by wetting and scraping.

This method of construction is practically identical to that outlined by Henri Arnault de Zwolle in the 15th C (2).

Thomas Mace, writing in the 17th C, also describes a similar procedure, using pieces of paper scorched in place with a hot iron, for gluing a soundboard to a lute (3).

Many oud makers prefer to use hide glue in the construction of oud bowls but may not employ glued paper strips to hold the rib joints together using instead steel pins, positioned along the edge of each rib and driven into the mold, to provide a clamping force on the joint.

This raises a question about whether or not the paper pieces might shrink and apply a clamping force to the joint as they are scorched in place.

Fibre orientation in both machine and hand made paper is random. However, if the paper is stretched during manufacture it may suffer from ‘dried in’ strain and develop ‘grain’. This occurs mainly, but not exclusively, in machine made papers where the paper passes in a continuous strip through a series of rollers as it dries, being constrained in the longitudinal direction but not laterally. If paper with ‘grain’ is moistened it will shrink in the direction of the ‘grain’ when re-dried.

In order to investigate this question two wooden test pieces were placed close together (but left free to move laterally) with a small gap between set with feeler gauges. Paper pieces, first saturated in water, were coated with hide glue, laid across the joint (with the grain direction running across the joint) and then scorched in place with a hot iron. The gap between the test pieces was then re-measured with feeler gauges. No closure of the gap in the joint was detected over the course of several tests indicating that by the time the glue on the paper had hardened by scorching most, if not all, of the shrinkage of the paper had already taken place. It is concluded, therefore, that the paper strips do not have the advantage of providing any significant clamping force.

Glues

Glue used for lute making was either hide or fish glue or a combination of both – glues applied hot and reversible in their application. Fish glue is made from the swim bladders of fish (isinglass) or from fish skin and other scraps. The latter, is a relatively weak, cold setting liquid glue commonly used for gluing paper but generally unsuitable for instrument construction.
Pliny the Elder in his "Naturalis Historiae" of 76 AD mentions both kinds of glue in Book 28, Chapter 71 and Book 32, Chapter 24. However, Pliny was uncertain about the origin of fish glue (Ichthyocolla) stating that "some say it came from the skin of a fish while others say that it came from the belly of a fish".

Cennino d’Andrea Cennini in his "Il Libro dell’ Arte" published in 14th C Florence describes the use of both types of glue - that made from goat skins being good for making lutes and fish glue, made from various kinds of fish, being good for mending lutes - although why one glue should be better than the other for the specified purpose is not made clear. As both glues were said to be available in sheet or leaf form, the fish glue referred to was probably isinglass.

Thomas Mace is more specific and recommends for lute repairs, "...some of the clearest and best made glue, together with isinglass (both which mixed together make the best glue)". However, Mace does not give the proportion of (hide?) glue to isinglass or explain why the mixture of glues was beneficial.

I singlass (4) is a clear fish glue made from the swim bladders of fish - the finest coming from Russian sturgeon although the swim bladders of other fish species such as Cod or Hake or freshwater fish like the Grayling were also used historically (see Comm. 1182 by Remy Gug). I singlass is supplied dried, in sheet or leaf form.

I singlass is a pure form of gelatin but with a fibrous structure making it stronger and harder than hide glue. It also has superior bonding properties that may be the reason for its preferred past use in instrument repair.

Reference to other early technologies may provide more information about the properties of isinglass glue.

The superior strength of isinglass glue is evident in its past use by the early Asian bow makers. Their small but powerful reflex bows were of composite construction - layers of wood, horn and sinew held together with glue. The bowyers, according to the climate in which a bow was to be used, adjusted the proportions of glue and other materials of a bow. In cold damp environments, bows were made with a greater proportion of horn, a moderate amount of sinew and glue, and a small portion of wood. The anonymous author of a 16th C treatise on the Arab bow states that "Others have recommended that in countries of excessive heat the sinew should be saturated with glue made from the best parchment, which is characteristically moist and, therefore, suitable for hot regions but not for those which are cold and humid". This implies that isinglass was preferred for bow making and could suggest that bow makers mixed the two types of glue in order to optimize glue properties for a given environment.

Hungarian bowmaker Csaba Grozer, who makes replicas of early Asian bows, claims that isinglass is the best glue in the world for making this type of bow as it is very strong, does not become brittle with time and has an extended "open time" allowing the bowyer to saturate the layers of sinew filament in good time before the glue sets.

One formula for a "Diamond Cement" appearing in the "Cyclopedia of Useful Arts", London & New York, 1854, is made by dissolving isinglass in dilute spirits of wine or common gin. The glue is used hot and alcohol added from time to time to make up for that lost due to evaporation. Charles Holzapffel in his "Turning and Mechanical Manipulation" Volume 1, 1843 gives the same formula for "Diamond Cement" saying that it is often used for attaching ivory to ivory or
ivory to wood. He further confirms that common gin acts as a preservative, extending the shelf life of the glue, by stating “When isinglass is dissolved in water alone, it soon decomposes”. (5)

Joseph Moxon in his book “Mechanick Exercises or the Doctrine of Handy-Works”, London 1703 also mentions the practice of adding alcohol to hide glue. “Some joiners will when their glew is too thick, put small beer into it, thinking it strengthens it: I have tried it, and could never find it so, but think that it makes the glew weaker, especially if the small beer chance to be new, and its yeast not well settled from it, or so stale, that it be either draggy, or whit mingled with the settlings of the cask”. (6)

Another strong glue reported in 19th C references is made by “infusing common glue in small pieces with isinglass in spirits of wine”.

There is no evidence to confirm that hide or isinglass glues used either separately or combined and dissolved in alcohol, were ever used for instrument making although an isinglass/alcohol glue would likely have been suitable for making ivory lutes.

Notes
Henri Arnault de Zwolle describes the construction of an open bulkhead style mold and the method for gluing and assembling the ribs, each rib being joined to its neighbour “with a long piece of paper coated with ordinary glue and with a rather warm iron”. No mention is made of pre coating the rib joints with glue, however, recent trials where glued pieces of paper were applied over closely fitting ‘dry’ joints and then scorched in place with a hot iron confirmed that melted glue from the paper does not seep into a joint by capillary action. “Ordinary” glue is assumed to be hide glue which was used for the tests.
(3) Thomas Mace “Musick’s Monument, 1676.
The size of the little pieces of paper used by Mace were “... so big as a pence or two pence...”. The English silver pence and two pence coins of Mace’s time measured between 12 mm and 15 mm in their original minted condition but were frequently subject to fraudulent ‘clipping’ which reduced their diameters somewhat. However, a piece of paper measuring between half and three quarters of an inch square is probably the size meant by Mace.
(4) Not to be confused with waterglass (sodium or potassium silicate) that is sometimes referred to as isinglass.
The sturgeon is now a threatened species (quoted by CITES) due to destruction of spawning grounds and over fishing. Isinglass from sturgeon is still available on the market but costs several hundred dollars per kilogram.
(5) Brief trials undertaken to compare the relative strengths of hide glue made with water and hide glue made with brandy (40 proof spirit) confirmed that the alcohol acted as a preservative in prolonging the ‘shelf life’ and strength of hide glue but that there was no measurable increase in strength due to the alcohol content. I singlass glue was not tested.
(6) Small beer in Moxon’s time was a weak beer – probably less than 2% alcohol in content.
Philippe Bolton

Thomas Stanesby Junior's "True Concert Flute"

In 1732 the recorder was probably already on the decline and being progressively replaced by its rival, the German or traverse flute. Since the end of the 17th century the solo or concert recorder was principally the alto (or treble) in f, whose use seems to have been particularly widespread in England. When the recorder was used together with other instruments it was common practice to transpose its part into a key that suited its range. This seems to have been first done around 1710 by two renowned musicians, Woodcock and Bell, but they did not succeed in having the recorder generally accepted for playing together with other instruments.

For this reason the London maker Thomas Stanesby Junior suggested using a tenor recorder, which he called the "True Concert Flute" in a pamphlet he had printed in 1732, in which he states that not having any notes below f is a drawback for the recorder and that the transpositions usually carried out for this instrument induce a "very great error".

Instead he proposes making a recorder in c that could play music at the written pitch, and thus be used in concert without having to transpose anything. It could also have access to the repertoire of the oboe, the flute, and the violin within its compass.

The tenor recorder itself was of course no novelty, since it had already existed in the Renaissance, but with a completely different musical function. Tenor recorders were made during the baroque period by the Hotteterres in France, Denner in Germany, Bressan, and Stanesby himself in London. But the idea of using the instrument in this way seems to have been completely new.

To give more emphasis to his argument, Thomas Stanesby built a tenor recorder with a completely different look from the traditional recorder shape. It was made in four parts, with a separate joint for each hand, and a foot resembling that of the flute.

This new recorder had a definitely solo character. Its wide bore gave it a beautiful tone quality reminiscent of that of the baroque flute. Its foot was bored with a double hole giving an easy c#. Very few recorders had double holes during the baroque period.

There is at least one original of this magnificent boxwood recorder in the Musée de la Musique, in Paris (N° E.980.2.86). There are black & white & colour pictures on the museum website: http://mediatheque.cite-musique.fr. The recorder is now somewhat bent, as often occurs with boxwood over the years, but this does not seem to have affected its sound quality.

Around 1735 the French musician Lewis Merci, established in London, published some solos for the new instrument, but this was insufficient to change the course of events. The growing popularity of the transverse flute unfortunately prevented Stanesby's invention from taking, and his initiative ended up as a failure.

However we still have this wonderful instrument, which could have saved the recorder from oblivion for a long time.

In the Encyclopaedia Perthensis Or Universal Dictionary of the Arts, Sciences, Literature, &c, published in Edinburgh en 1816, can be read the following statement: "The English flutes made by the younger Stanesby come the nearest of any to perfection."

The Musée de la Musique has published the measurements of this unusual recorder. It is a long instrument, not really for small hands. The right hand stretch is fairly big.

There are a few tuning problems. Some of the octaves are wide, especially g, and the medium notes from b to eb are all a little low. These problems have to be solved by makers in order to make an acceptable copy. If equipped with a bell key this recorder can even have a complete chromatic range of two and a half octaves or more.

Stanesby's idea of using a tenor recorder for playing the repertoire of the flute seems in a way more logical than the modern practice of using a voice flute for this, because the fork fingerings, and
strong and weak notes are in the same places on your instruments. Moreover it is easier to play in "flat" keys with a recorder in c than with one in d.

Here is the text of Stanesby's pamphlet, in which the term "Flute" is used for the recorder:

A new System of the Flute a' Bec
or Common English Flute

WHEREIN is propos'd to render that Instrument Universally useful in Concert, without the trouble of Transposing the Musick for it. Humbly Dedicated to all those Gentlemen who like the Instrument.

By THO: STANESBY JUN'.

WHEREAS, it may be supposed that the Projectors of the F. Flute, commonly call'd the Concert Flute, never intended if for an Instrument to be us'd Universally in Concert, by making it go no lower than F. and as they made their Basses in F. also never intended other Basses to be us'd with them, than those of their own kind, by which a progression of one sort of Tone only is Effected; But we find now in all great Performances of Musick, that a mixture of Tones by different sorts of Instruments is best approv'd, and the Flute only is excluded, being incapable of Concerting with the rest, having a defficiency of three usefull Notes at the bottom; and though 'tis an easy Instrument, and the Compass extensive, yet for that defficiency can't Universally be us'd.

Many great Masters, and delightfull Performers on this Instrument have endeavoured to make it acceptable in Concert by Composing the Flute part in one Key, when the other Instrumental parts were in another, by which means Flutes of all sizes became Concert Flutes when us'd there, which induces a very great error: for when the size of the Flute is chang'd, tho' the Performer is told by the Tone of the Flute that the lowest note speaks B, or C, or D, yet he still calls it F, and so every note is call'd F, in its turn, tho' at the same time it is insensibly to the Performer Transpos'd to its proper Note by help of the Flute. This is the cause of a great deal of trouble in Transposing Musick, writing out Parts, find-
ing out things proper for Transposing, different sizes of Flutes, and many other inconveniencys, thro' which the Flute is exclam'd & rejected, and has lost much of your Esteem.

THEREFORE, in order to reinstate this Instrument to your favours, and also to encourage the Art and Mystery of making good and perfect Instruments, I propose to produce the Flute to an equal pitch and Compass with the Hoboy, or Traverse Flute, so that any Musick written for thos Instruments, or for the Violin in their compass, shall be play'd by the Flute in Concert a unison to them, without the trouble of Transposing or writing out Parts for the Flute, otherwise than what fits those Instruments: which will so facilitate the use of the Hoboy, English Flute, and Traverse Flute, that whosoever can use one, can use all, and one general Scale of the plain Notes will serve all.

THIS is to be done by making a Scale of the Notes on the Flute, from C below to D in alt, the same as for the Hoboy, and then by using a Flute whose lowest Note is C. that Flute will perform in the same unison with the Hoboy, or Traverse Flute and must appear to be the true Concert Flute, and being the same pitch with all other Concert Treble Instruments, may be as universally us'd as any of them, and will be the addition of another Tone to the Harmony wherein it is us'd, and thus becomes universally useful in Concert.

NOTWITHSTANDING the great use this will be of, in bringing the Flute upon an equality with other Treble Instruments, and as universal in Concert as the best of them, yet I don't expect it will be universally received at its first appearance, because many Gentlemen who have taken much pains to attain the handling of the F. Flute, may be unwilling to learn any new Scale, but I shall leave those Gentlemen to their choice, only beg they'll be so favourable as not to condemn this till they prove the other better, for tho' they have call'd the lowest Note of the Flute F, all the days of their lives, yet if they please, it may as easily be call'd C.

THUS having shew'd you in few words the usefulness of it, I here present you with a full and perfect Scale of all the notes on the C Flute, which I more especially recommend to all those who shall learn that Instrument, and being done in the common way, wants no Explana -tion.
A Scale in C. for the Flute A'lec. or Common English Flute.
Oil paintings of musical instruments—should we trust the Old Masters?

David Hockney carried out research at the very end of the 1990’s into certain painting techniques used by many of the ‘Old Masters’. I have no idea if this is widely known by instrument makers and researchers. It culminated in a lavish book published in 2001 entitled *Secret Knowledge- Rediscovering the lost techniques of the Old Masters*. I was first made aware of his research through a fascinating TV program that he made, which fortunately I videoed and have watched many times. I have heard some hearsay that it caused a bit of a stir among art historians. I do not know whether this stir was of the “We-knew-about-this-all-the-time” or “Hockney-is-talking-a-load-of rubbish” variety. But I do know that anyone who has ever been involved in interpreting the evidence of musical instruments painted by the Old Masters should read this book.

An enormous advance in realistic painting took place around 1430-1440. This has long been noted by art historians but it has never has been properly explained. Hockney gives a very clear case that from this period onwards many of the Old Masters were using convex mirrors to project the image onto the canvas.

If you already find yourself sceptical about this I really encourage you to read his book as the evidence is compelling. It certainly has changed the way I view paintings from this era. Now it becomes clear why so many old Flemish portrait paintings measure about one foot square. This is the optimum size for a projected image using a concave mirror. Most of us will be familiar with early portraits where the background is very dark, yet there is bright light on one side of the subjects face. And the irises of the subject are contracted. The subject would have been sitting in full sunshine and the artist would be painting in a darkened area using the image projected by a concave mirror.

The next big advance came around 1510 when there had been significant advances in glass making which allowed lens maker to supply artists with lenses that they could use for this purpose.

Long before I knew anything about Hockney’s research I enlarged to life size the Albrecht Dürer woodcut of a bagpiper and mounted it on plywood- it stands about 5 foot high. I usually take it to The London Early Music Exhibition to jolly up my stall.

The original woodcut is post card sized and yet when I had it enlarged all the proportions of the bagpiper still remained absolutely correct. I often mused that it seemed to have a ‘photographic look’ and marvelled at Dürer’s skill to draw such a small figure with perfect proportions. He made that print in 1514, which is only four years after lenses started to be used by artists.

A lens reverses the image it projects. A clue that Dürer used a lens for this print is that the image is ‘back to front’. The piper is ‘left handed’. And, yes, it is quite possible that he actually was a ‘left handed’ piper. But you try looking at any paintings of musicians and see how many of them suddenly become left handed after 1510!

Working in conjunction with an optical scientist Hockney gives a detailed analysis of some of the inevitable distortions that appear when an artist tries to fit several projections together into a complete painting. He shows various ‘clues’ one can look for.

Once one can spot these ‘clues’ and can be confident that the artist has painted using this technique one can have much more confidence in the dimensions of a musical instrument in a painting.

*Secret Knowledge- Rediscovering the lost techniques of the Old Masters.* David Hockney (Thames & Hudson) 2001 ISMN 0-500-23785-9
A Method of Fixing Loose Soundbars

When restoring antique harpsichords and clavichords, it is quite common to find that one or more soundbars have become detached from the underside of the soundboard at one end or the other. The probable cause is the exposure of the soundboard to wide variations in relative humidity, causing it alternately to shrink and expand. Since the grain of the soundbars is usually at an angle to the grain of the soundboard, and movement of the wood occurs mostly across the grain with almost no dimensional change along the line of the fibres, these variations in RH create stresses in the glue joint, which after many years of such stresses may fail. If the instrument is to be restored to playing condition, loose soundbars present at least two problems:

1. they may produce a buzzing sound on some notes; and
2. they reduce the stiffness of the soundboard, altering its acoustic response and reducing the force with which it can resist the downward pressure of the strings exerted through the bridge.

This Comm describes a method, devised with the help of Miles Hellon, for re-fixing such partially detached soundbars without the need to remove the soundboard from the instrument. It can be used only where there is access to the underside of the soundboard through an opening such as the window or 'mousehole' usually found in the belly-rail of a clavichord.

The object of our attention at the time was a large eighteenth-century clavichord. A preliminary examination of the underside of the soundboard through the window in the belly-rail, using a mirror, showed that two of the three soundbars passing under the bridge were loose. The first step was to establish the exact positions of the two loose sound-bars. A low-voltage fluorescent lamp was inserted into the soundbox through the window in the belly-rail. With the room in complete darkness, sufficient light penetrated the spruce soundboard to show, as shadows, the precise outlines of the three soundbars. The positions of the two loose ones were marked on the top surface of the board using low-tack masking tape.

With the lamp withdrawn and light restored to the room, each loose bar was then dealt with in turn. Two holes were made in the soundboard on either side of the bar about 15 mm in from the detached end, using an ordinary steel sewing pin (diameter 0.65 mm). Steel music wires 0.27 mm in diameter were passed through each of these; an improvised wire hook, inserted through the window, was then used to locate the ends of the wires inside the soundbox and feed them out through the window. A small plate (approx. 25 × 15 mm) was prepared, made of phenolic resin from an old printed-circuit board (since this resists the adhesion of animal glue). Two small holes were drilled in this plate, slightly further apart than the width of the soundbar. Each wire was then passed through one of the holes in the plate, and stop-pieces of brass were attached to the ends of the wires to stop them pulling through. The method of attachment needs to be capable of resisting considerable force; after experiments, the best method seemed to
be to drill tiny holes in each of the stop-pieces and to pass the wire through the hole, round 360° in a loop and through the hole again, pulling it tight and then making the attachment completely secure by a blob of electrical multicore solder (see Fig. 1).

Fig. 1: Steel wires passed through the plate and secured with stop-pieces of brass and solder

The wires were then pulled up through the soundboard and the plate was manoeuvred into place under the soundbar. An oak block was prepared with two holes drilled through laterally to receive two ordinary 4 mm diameter wrestpins, inserted in opposite directions; the two wires were secured to these wrestpins. A packing piece of softwood, the precise width of the soundbar, was placed directly below the oak block: it was then possible to bring the loose end of the soundbar back into firm contact with the underside of the soundboard by tightening the wrestpins (see Fig. 2).

Fig. 2: Wrestpins, inserted in an oak block, used to draw up the plate and close the (hidden) joint between soundbar and soundboard

Before gluing up, the joint was observed through the belly-rail window, using a mirror, to check that all was well: in particular, that the plate was not skewed to one side but sitting squarely under the soundbar. The wrestpins were then loosened so that the joint opened up. The softwood packing piece was removed, and a third pinhole was made directly above the soundbar. Using a hypodermic syringe, first hot water (to clean the joint and reactivate any remaining traces of old glue) and then glue were injected through this hole into the joint, whereupon the softwood piece was replaced and the wrestpins were tightened closing up the joint.
Twenty-four hours later the wires were cut, the oak and softwood blocks were removed, and the plate and two short ends of wire were extracted from the soundbox. The joint was then re-examined using the lamp and mirrors to check that adhesion was complete. The pinholes in the top surface were (almost) closed up with a drop of water on each.

This method produces a sound repair, which is very obvious when viewed through the window in the belly rail, but almost undetectable from above.
Medieval and Renaissance Multiple Tempo Standards

Tempo is usually considered to be an aspect of performance that is unrelated to instrument design and acoustics. This is not strictly true since speed of response is an important factor for musicians when they need to play fast. But in general, specialists in historical instruments should be interested in how they were used originally, not only in how they are used today.

All musical cultures, including historical ones in Western Europe, have *tempi giusti* (default tempo standards) for most types of music performed. This is simply performing in the expected way, which has always been a dominant part of what musicians do. This does not imply that musicians have been inhibited by these standards when they wanted to do something different with tempo.

There is no medieval evidence on actual tempos of instrumental playing, but there is unambiguous evidence on tempos of vocal music. In the Renaissance, there is excellent evidence on both. Unfortunately we have not yet fully understood the evidence, and this study is an attempt to make it clearer.

Let us start with a footnote in Willi Apel's, *The Notation of Polyphonic Music*:

> The proper choice of modern equivalents for the mensural notes is, of course, closely bound up with the consideration of the tempo for the piece in question. Our basic principle in this matter is, to choose the reduction in such a way that the modern quarter-note [crotchet] becomes the beat in moderately slow tempo, somewhere in the vicinity of M.M. 60. As will be seen later ..., the mensural notes signified not only relative values but had, in a given period, fairly constant absolute durations as well, a fact which makes it possible to choose a uniform scale of reduction for practically all the pieces of any one period, or, at least, of any one type. It must be noticed, however, that, in turning from one period to another, the 'absolute' value of any given note changed considerably, namely from short to much longer durations. This appears particularly in the case of the B [breve] which about 1225, designated the shortest value of music (brevis, short), while, in the sixteenth century, it was the longest value in practical use. As a matter of fact, the 'moderate beat' was represented successively by the L [long] (1200-1250), the B [breve] (1250-1300), the S [semibreve] (1300-1450), the M [minim] (1450-1600) and finally the Sm [semiminim], i.e., the quarter-note [crotchet] (1600-present), so that reductions in the ratios of 1:16, 1:8, 1:4, 1:2, and 1:1 appear appropriate for the periods just named.

The objective in this paper is to refine the above summary of the history of note value tempos by relying as much as possible on objective interpretations of evidence (avoiding judgements of unacceptable speed not backed by evidence), and trying to understand how the changes might have occurred. In my two 1996 articles on tempo in *Early Music*, I postulated slower medieval tempos than those given by Apel. A closer look at the medieval evidence finds slower and faster versions of the usual tempo standard. This leads to accepting Apel's tempos as faster versions possibly chosen then. It also leads to considering each change as a switch of recognition from the usual to the slower tempo standard, thus allowing reading the new shorter note values that composers were introducing without actually having to sing faster. The sequence of changes will be inferred from quite explicit tempo evidence from the final period and from the mid-fourteenth century, plus other evidence about changes and lack of changes.

**Fourteenth and thirteenth century evidence**

Mid-fourteenth century Italian evidence was given by Johannes Vetulus, who wrote that the breve

2 Mensural notes are notes which indicate their length in time relative to other such notes.
in tempus perfectum medium lasted for the time of an uncia. He made it clear that there were 480 uncias in an hora, of which there were 24 in a day. In modern time units, that makes an uncia last for an eighth of a minute. He also mentioned that this 'universal time unit [uncia] contains in itself 54 athomi [the smallest indivisible units of time], 27 particulariter vocis [the smallest units of vocal time], 9 minims of major prolation and the tempus (breve) of the novenaria divisione [the mensuration with perfect tempus and major prolation].

In this mensuration, breves lasted for 7.5 seconds (M.M. 8), athomi for 0.14 seconds (M.M. 432), particulariter vocis for 0.28 seconds (M.M. 216) and major-prolation minims for 0.83 seconds (M.M. 72). Musicologists have not been able to accept such a slow tempo for this period. Sachs (p.188) wrote that to make 'a satisfying enunciation of the text', the minim should be one-third as long as that of Vetulus, so he 'must have been mistaken'. Gullo made his suggestions as to what Vetulus's mistakes might have been, without offering any evidence which would suggest that any mistake had been made.

Vetulus wrote that 'From this uncia the musician derives tempus rectum and perfectum, not the maius [major] or the minus [minor] tempus, but the mediocritir [mean]' . What were these different versions of tempus perfectum? A likely answer to this question was given earlier in the century by the Frenchman Jacobus of Liege, who wrote just before the Ars Nova took hold:

'In order to understand better the old musicians [of the late thirteenth century] and their rules, one should notice that a double or triple mensuration of the longa, brevis and semibrevis exists — that is, a quick ("cita"), a slow ("morosa") and a medium ("media"). This is pointed out also by the moderns. One of them says this: one can sing in three different ways — 'tractim,' [slowly] 'velociter,' [rapidly] and 'medie' [mean]; however, the notation remains the same in each case. Another, in limiting himself to tempus perfectum, says: tempus perfectum may be of three kinds, namely 'minimum,' 'medium' and 'maius'.

What Jacobus tells us here is that in addition to the standard tempos, there were slow and fast versions of the mensurations both in his own period and in the previous 'Franconian' period. At about the same time, Philippe de Vitry, another Frenchman, wrote that tempus perfectum can be minimum, medium and majus, while tempus imperfectum may be minimum or majus. A possible explanation of why there were only two speeds for tempus imperfectum would be if the fastest of its three speed choices was ruled out because the minims in it would have to go faster than the particulariter vocis limit. This would be the case if the minim in minor prolation was 4/3 faster than in major prolation in France as it was in Italy and (using Vetulus's figures) if the fastest speed was three times that of the medium one.

A difference of a factor of three in tempo without a difference in the notation is not as outlandish an idea for that time as it would be now. That is just what happened between the 'Franconian' and 'Ars Nova' periods. Jacobus of Liege wrote 'in modern writing the third part of the brevis perfecta... and the brevis perfecta is worth as much as the longa

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A mensuration is a particular combination of tempus and prolation; tempus is the number of semibreves in a breve, and was called 'perfect' if it is three and 'imperfect' if it is two; prolation is the number of minims in a semibreve, and was called 'major' if it is three and 'minor' if it is two.

Salvatore Gullo, Das Tempo in der Musik des XIII. und XIV. Jahrhunderts (Berne, 1964), pp 69-76.


Jean de Muris, Ars nova musicae (1319) reported that 'according to one account, there are two sorts of time'. It is likely that the one of the three ignored here was the slowest, trans. in Oliver Strunk, Source Readings -Antiquity and the Middle Ages (Norton, 1965), p 173.

A brevis perfecta was a third the length of a long, while a brevis imperfecta was a half the length of a long.
was tormently. This apparently applied to each of the three standard speeds. A clue as to how much faster the fast mensuration was than the medium was is in the next quote from Jacobus: 'Here one must know that when the ancients say that tempus perfectum cannot be divided into more than three semibreves, they presuppose the quick mensuration ('cita mensuratio') ... the semibrevis is to Franco the same as the minima ... is to moderns.' If the 'modern' minim was three times faster in quick mensuration than in mean mensuration, then according to the numbers given by Vetulus, it would just reach the particulariter vocis limit (resulting in just the tempo that Sachs wanted). That would then be the case for the slowest semibreves of the 'ancients', and so was truly indivisible.

Jacobus stated that the quick mensuration was customary practice amongst the 'ancients', particularly in the motets. In the hockets they used a very quick one ('citissima') where the breve did not last longer than the minim of the 'moderns'. He was trying to show that the innovations of the Ars Nova were unnecessary. What did they really accomplish? Amongst these innovations was the shift of the slow mensuration to become the new medium one and the old medium one to become the new fast one. The old fast one became redundant because the new shorter independent note values, the minim and the semiminim (crotchet), allowed notation of the fastest notes in the medium mensuration.

Before we go on to other historical periods, it may be worthwhile to speculate on how Vetulus arrived at his time of the athomi. We now know that there is no indivisible smallest unit of time (except for the relationship between time and energy on the atomic scale according to Heisenberg's uncertainty principle). So what was the fastest repeating thing he knew of that he could measure? I suggest that it was the fastest that notes could be played on an instrument such as the harp or psaltery\(^1\). It was 7.2 notes per second. That is two thirds of the baroque fastest speed on bowed and wind instruments of 10.8 notes per second reported by Quantz\(^2\).

Let us now look to what happened before the Franconian period. Apel\(^3\) reported that a study of thirteenth century organa and motets concluded that there was a similar shift by a factor of three in the duration of each note value in the middle of the century, when the semibreve and breve respectively replaced the breve and the long. Though there is no information on multiple tempo standards early in the thirteenth century, one would expect a situation similar to that which pertained later. The transition to the Franconian involved the introduction of a new fastest note value, the semibreve (in several species including a minimum one, which became the minim), which expanded the palette of note values available to composers, and was compensated for in real tempos by a shift towards the slower amongst the current tempo standards.

**Sixteenth and seventeenth century evidence**

This is all I can say about tempo standards up to and including the fourteenth century. The tempo situation in the fifteenth century will be approached by first looking at the transition between the fifteenth and the sixteenth century. Apel\(^4\) wrote:

'During and after the Josquin period\(^5\) the sign \(\Phi\) was universally adopted as a time signature, to the almost complete exclusion of the signs of integer valor, \(C\) and \(O\)\(^6\). We now find under this sign exactly the same note values, \(\breve{B}\) [breve] to \(\breve{F}\) [fusa, i.e. quaver], which formerly were used under the sign \(C\). ... No attempt is made here to explain why the sign \(\Phi\) was so universally adopted throughout the sixteenth century without any apparent significance and meaning.'

\(^1\) A late twelfth century account of especially fast playing in Ireland on a harp or psaltery is in Typographia Hibernica by Gerald of Wales; discussed in Christopher Page, Voices & Instruments of the Middle Ages (Dent, 1987), p.228-30.


\(^3\) W. Apel ibid p.342.

\(^4\) W. Apel ibid p.192.

\(^5\) Late fifteenth and early sixteenth centuries.

\(^6\) \(C\) is the mensuration that has imperfect tempus and minor prolation and \(\breve{C}\) (C stroke or cut-time C) is C diminished by half.

\(^7\) \(O\) is the mensuration that has perfect tempus and minor prolation and \(\breve{O}\) (O stroke or cut-time O) is O diminished by half.
The replacement of C from before Josquin by afterwards, with no change in note values, can be explained by a rationalisation of the notation system that apparently occurred around the middle of the century that doubled the length of all note values in minor prolation, so that semibreves of all of the four basic mensurations would have the same length. Then what was previously called C became $f$, a diminution of the new slower mensuration for minor prolation. The term integer valor, which means 'normal time values', applied to this rationalised set of mensurations then, with 'normal' meaning theoretically appropriate rather than common practice.

The late fifteenth century composers also apparently tried to eliminate mensuration speeds that were not notated. That did not last long. As documented by Schroeder, many musicians around the turn of the century performed $f$ slower than twice as fast as $o$, which diminution should indicate. The shorter notes in $f$ go faster than the same notes in $o$, and it seems that singers objected to such speed. Early in the sixteenth century, a new slow mensuration (by a factor of two) appeared with the introduction of the new note values of the semiquaver and the demisemiquaver. It was mainly associated with instrumental music, and $f$ was the most common time signature, with four crotchets to the semibreve bar. It had the same tempo as integer valor C, but that representation of the tempo was never really fashionable. It was meaningful only to theorists trying to interpret current practice in terms of the writings of earlier theorists. An augmented version of the standard $f$ usually used by singers was more meaningful to musicians at the time.

The concept of tactus is central in any discussion of tempo in the sixteenth century. It usually was an invariable time unit expressed by the hand moving first down and then up (or the other way around). The tactus was the time for both motions to occur. Neusidler wrote that it was the time between strikings of the (Nuremberg) tower bell signalling the hour, or the time it takes to 'nice and gently' add up money saying: 'eins, zwey, drey, vier'. Enough information about the Nuremberg Town Hall tower clock mechanism in the sixteenth century survives to indicate that the time between bell rings was over two and less than six seconds. The money-adding time seems to indicate that it was longer than two seconds. A similar counting exercise, to give the tempo of four crotchets in a semibreve tactus, was prescribed in the next century by Simpson: 'One, Two, Three, Four' pronounced 'as you would (leisurely) read them'. Simpson also wrote that 'Some speak of recourse to the motion of a lively pulse for the measure of crotchets'. The range for the normal pulse is M.M. 60-80, so the tactus lasted for four pulse beats or about 3 to 4 seconds.

Gerle was an instrumental composer, as Neusidler was. He wrote that the tactus included one semibreve in the mensurations $f$ and $o$, three semibreves in $f^3$ and three minims in $i$. He added that the tactus recognised by the singers corresponded with the breve, and that if a song went too fast, both singers and instrumentalists augmented their note values. The normal instrumental mensuration speed was called 'semibreve time' or alla semibreve, and when it was augmented, it would become alla minima. When the singer's breve time or alla breve (the original diminished integer valor of the late fifteenth century) was augmented, it just became alla semibreve.

By the middle of the century, C started to be used for alla semibreve as well as $f$. From then on to late in the seventeenth century, the two signs were used either as equivalents or with $f$ somewhat faster than C. When they differed, they tended to be near the extremes of the pulse range. In seventeenth century France, alla semibreve $f$ was abandoned, with only alla semibreve C remaining. There were signs for diminution to faster tempos well-used by composers then. There

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19 To augment is to play slower by notionally replacing the note values by the next larger ones; to diminish is to play faster by replacing them by the next smaller ones; some early writers used these words the other way around.
20 Hans Neusidler, Ein neugestordent kunstlich Lautenbuch (Nuremberg, 1536); quoted in Sachs ibid p.203.
21 E. Segerman ibid. 1 p.235.
22 Christopher Simpson, A Compendium of Practical Musick (London, 1667).
is no evidence of a tradition of diminution that was not notated\textsuperscript{24}, but there can be little doubt that musicians were free to do it if they had any reason to do so.

Apel's 'Facsimile 2' on p. 7 is apparently an early example of \textit{alla semibreve} tempo. It is the 'Branle commun' from Attaingnant's \textit{Quatorze gaillardes} (Paris, 1530), and it contains demisemiquaver ornamental notes in groups of 4. According to Quantz, the fastest speed at which a competent musician can be expected to play was 8 notes per pulse beat. With 8 demisemiquavers per crotchet, at this fastest speed, the branle would have been played at crotchet = pulse, in agreement with Simpson. If a player found that it was too fast, he could augment to \textit{alla minima}.

The fifteenth century

From the above analysis, we would expect that the later fifteenth century tempo of \textit{integer valor} C would similarly be with the semiminim (crotchet) having a pulse tempo. It now remains to link the fourteenth century tempo information discussed previously (slow, medium and fast mensurations with a pulse tempo for the minim in the medium version of tempus perfectum with major prolation) with the late fifteenth century information (with a pulse tempo for the crotchet in \textit{integer valor} C, and for the minim in the ubiquitous $\ddot{c}$).

There is some information on absolute tempos from the fifteenth century. Michaelle Savanarola\textsuperscript{25} gave evidence that probably dates from near the middle of the century, which stated that a physician can remember the fastest end of the range of the normal pulse (c. M.M. 80) by learning the beat of the \textit{quattuorvaria divisione}\textsuperscript{26}, and the slowest end of the range (c. M.M. 60) by learning the beat of the \textit{senaria imperfecta divisione}. The problem is then to find which note value in each \textit{divisione} can give the appropriate ratio of about 4:3 in speed (M.M.) or 3:4 in note duration. There were two semibreves per breve in both \textit{divisiones}, and the number of minims per semibreve was two in \textit{quattuorvaria} and three in \textit{senaria imperfecta}. Apel's idea that the length of the breve was constant in all \textit{divisiones} doesn't work here since the semibreve ratio would be one and the minim ratio is the wrong way around. As mentioned before in the context of a century earlier, the minim in minor prolation was 4/3 faster than in major prolation, which works fine here for the beat being a minim, but a century is a long time in the history of an aspect of culture subject to changes of fashion.

In an early fifteenth century Italian dance manual, Domenico de Piacenza\textsuperscript{27} stated that the bassadanza was the slowest, the quadernaria was faster with 5/6 the \textit{misura} (tactus) as the bassadanza, the saltarello faster yet with 2/3 the \textit{misura} of the bassadanza, and the piva was the fastest with half the \textit{misura} of the bassadanza. The mensuration of the bassadanza and saltarello was \textit{senaria imperfecta} and of the quadernaria and piva it was \textit{quattuorvaria}. The latter pair appear to have been related to the former pair by sesquialtera (3:2) proportion in note length.

How the Domenico information might relate to that of Savanarola is not immediately obvious. Savanarola was clear that there was only one speed for each mensuration, while Domenico had two. If we consider either of Dominico's pairs to be at the standard tempi for their mensurations, the duration ratio of \textit{quattuorvaria} to \textit{senaria imperfecta} would be much closer to Savanarola's ratio if the beats were semibreves than if it were minims. But there were two versions of each mensuration. It would make sense if Domenico's saltarello and piva tempi were in a fast mensuration of those of Vetulus's period, while the popular songs that Savanarola's musicians presumably sang were at a slower mensuration with pulse tempos for minims. If, in the slower mensuration, the piva \textit{misura}

\textsuperscript{24} Fritz Rothschild, in his book \textit{The Lost Tradition in Music} (London, 1953), when writing about the seventeenth century 'Old Tradition' that Bach mostly followed, postulated that in semibreve time, C also had a diminished fast movement when there were only sparse ornamental semiquavers in the music. Amongst the large amount of evidence from writings of the time that he presented, none from the seventeenth century supports this conclusion.


\textsuperscript{26} \textit{Divisione} is Italian for mensuration; \textit{quattuorvaria} has minor prolation and \textit{senaria imperfecta} has major prolation.

\textsuperscript{27} Domenico de Piacenza, \textit{De la arte di Ballare el danzare} (1416); discussed in Mabel Dolmetsch, \textit{Dances of Spain and Italy} (London, 1954), pp.1-8.
was augmented by a factor of two and the saltarello was augmented by a factor of three, the duration of minim beats becomes Savanarola's 3:4. Then, Savanarola's mensurations were the same as Vetulus's, but 11% faster. In this interpretation, the tempos of semibreves in the bassadanza, quadernaria, saltarello and piva in M.M. were about 40, 50, 60 and 80 respectively. These tempi are two thirds of those estimated by Sachs, which is reassuring since modern estimates tend to be fast.

The Domenico evidence seems to indicate that in a musical environment dominated by factors of three, as was the situation up to the middle of the fifteenth century, non-notated differences in mensuration speeds could be by a factor of 3/2 when the usual three was too extreme. Afterwards, when binary mensuration dominated, a factor of two was usual. In the transitional period early in the fifteenth century, it is suggested that they were independent, with ternary mensurations following the earlier practice and the binary mensurations the later one.

The role of the pulse

Curt Sachs believed that the basic tempo in all periods gravitated to what he called a 'motor unit', the normal leisurely stride of man, which he assumed was M.M. 60-80 per step. I agree with the gravitation principle and the tempo, but base it on the pulse, not on the stride. In a recent paper, Bettencourt found that the average walking speed in a city depended on its population: about 1 m/s if the population is 10,000 and 1.5 m/s if it is a million, a 50% difference. The stride is very much dependent on cultural factors. The pulse depends only on the physiological state of an individual. It has recently been realised that hearing is the earliest sense of the outside world that a growing fetus develops in the mother's womb. Thus all humans have a strong imprinting of the fetus's extensive pre-natal auditory experience of the mother's beating heart. If a recording of a beating heart is played in a nursery of newborn babies, there is less crying. When a person is resting, there is an alternation between a longer and shorter beat, which could have been why ternary rhythms were considered most 'natural' in the middle ages. There is one pulse beat for every pair of heart beats. The fetus hears the pulse as the repeating time of the sounds of the beating heart. This is the body's natural rhythm beat, so it is why I expect that at any time, the speed of one note value in the most popular mensuration corresponds with the pulse. All of the hard evidence on tempos conforms with this principle.

When one is excited, the pulse rate is faster, and so when a composer wanted to insert a passage in a more exciting tempo, it would be somewhat faster. Such an alternative was a binary mensuration in ternary-dominated medieval music and a ternary mensuration in binary-dominated Renaissance and baroque music. There is Italian medieval evidence that the usual tempo ratio was 4:3, and I suspect that this was true for the French as well. In the later period, the the most used ternary rhythm was with 3 minims in a tactus in semibreve time, when the ratio was 3:2.

Conclusion

Attention is here drawn to the evidence that in the late thirteenth and fourteenth centuries, there were slow, medium and fast tempo standards with no notation indicating which. This suggests that musicians had a choice in performance. The medium standard seems to have been the usual one. Some repertories such as motets were more susceptible than others to be performed at the fast standard. There was a note-value augmentation between these two periods that involved the introduction of new shorter note values. Composers added these either to be able to notate in the medium standard fast notes only heard at the fast standard, or just to expand the palette of note values to use. The note-value augmentation could well have resulted from most musicians resisting singing shorter notes than they were used to by augmenting the music. This would have forced the old medium standard to become the fast standard in the new set of standards for those compositions. It is presumed that a similar process was involved in the previous note-value

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28 C. Sachs *ibid*, p 204
29 C. Sachs *ibid*, pp 32-4
31 Doubling the tempo certainly increases momentum, but it does not resonate with the pulse rate of normal excitement.
augmentation in the middle of the thirteenth century.

There seems to have been a note-value augmentation of mensurations with minor prolation in the middle of the fifteenth century, well after when the note value of the quaver (\textit{fusa}) was introduced. The quavers were used sparingly (not densely enough to have to be counted), and most musicians stayed with the old medium mensuration (the new fast one), which then became the ubiquitous cut-time mensurations. What this note-value augmentation accomplished was to make the semibreve last the same amount of time in all of the four basic mensurations when the tempo has not been altered by a canon (written instruction on how it should be altered), a proportion or a stroke (cut time). This made beating time easier when all tempos were being specified.

New note values were introduced around 1500, associated with an augmentation (by a factor of two) of the note values of the usual tempo standard (with cut-time signatures) used by singers since the middle of the fifteenth century. Both mensuration speeds used the same cut-time signatures, so the difference was not notated, suggesting that there was some free choice. The new standard, called \textit{alla semibreve}, was probably started by singers augmenting away the short notes that composers increasingly offered them, but it was eagerly adopted by instrumentalists to provide time for elaborate divisions on popular song tunes and for learners to think through what experienced players did automatically. The singers' old standard was called \textit{alla breve}. The names indicated the note value of the \textit{tactus}. If there was no indication otherwise, the evidence indicates that the \textit{tactus} length was usually four pulse beats.

This study follows the evidence wherever it leads. The main reason why it was not done ages ago is that it is very difficult to imagine how many of the slow tempos could have meaningfully listened to. It might be helpful to consider that the primary intended listener to the particularly slow music was not any human, but God. As God is infinite, inhabiting all space above the earth, humans have always felt that communication with a Deity is enhanced by being in a place that reaches high, such as a mountain or a cathedral. Similarly, God is eternal, so stretching the time of a message (with sustained conviction) could have been considered an advantage for its successful transmission. Since God is omniscient, He knows the words, so understandability to human listeners is not a necessary requirement. The requirement that must be met, though, is singers with large strong lungs.

\textbf{Tables}

The following table is a summary of the note values with a pulse tempo in non-notated multiple-tempo standards that I know of evidence for. The note values for up to 1450 are in ternary mensurations, and those for after 1450 are in binary mensurations.

\begin{table}[h]
\centering
\begin{tabular}{|c|c|c|c|c|c|}
\hline
\textbf{Tempo Species} & \textbf{1200-1250} & \textbf{1250-1300} & \textbf{1300-1450} & \textbf{early 15\textsuperscript{th} c. 1450-1600} & \textbf{1500-}\textit{alla breve alla semibreve} \\
\hline
slow & (minim) \textit{breve} & \textit{semibreve} & \textit{semiminim} & \textit{semiminim} & \textit{fusa} \textsuperscript{31} \\
medium & \textit{breve} & \textit{semibreve} & \textit{minim} & \textit{minim} & \textit{semiminim} \\
fast & \textit{long} \textsuperscript{32} & \textit{semibreve} & \textit{semiminim} & \textit{fusa} \textsuperscript{31} & \textit{semifusa} (\textit{semiquaver}) \\
very fast & & & & & \textit{and demisemiquaver} \\
\hline
\multicolumn{5}{|l|}{new note value} \\
\hline
\textit{semibreve} & \textit{minim} & \textit{fusa} \textsuperscript{31} & \textit{semifusa} (\textit{semiquaver}) & \textit{and demisemiquaver} \\
(\textit{perf.}, \textit{imperf.} and \textit{minimum}) & (\textit{crotchet}) & & & \\
\hline
\end{tabular}
\caption{Medieval and Renaissance Note Values Corresponding with the Pulse}
\end{table}

\textsuperscript{32} if the usual was too fast (Gerle) \\
\textsuperscript{31} in hockets (Jacobus of Liege)
As an idle comparison with the late baroque, the following table gives the Italian names for tempos with the note values that corresponded with the pulse, as reported by Quantz.

Quantz's Tempos with Note Values Corresponding with the Pulse

<table>
<thead>
<tr>
<th>Tempo species</th>
<th>semibreve</th>
<th>minim</th>
<th>crotchet</th>
<th>quaver</th>
<th>semiquaver</th>
</tr>
</thead>
<tbody>
<tr>
<td>Common time</td>
<td>allegro</td>
<td>assai</td>
<td>allegretto</td>
<td>adagio cantabile</td>
<td>adagio assai</td>
</tr>
<tr>
<td>Alla breve</td>
<td>allegro</td>
<td>allegretto</td>
<td>adagio cantabile</td>
<td>adagio assai</td>
<td></td>
</tr>
</tbody>
</table>

Some twentieth century problems in interpreting the tempo evidence

Since the twentieth century revival of early music, the music has usually been performed at least twice as fast as the above analysis would indicate was usual at the time. This was driven by musicologists expecting faster tempi because their training to make sense of the music involved playing it at modern faster tempos. They made judgements about whether a proposed absolute tempo was too slow when there was no supporting early evidence of a slow limit, and have been astonished when a composer indicated a repeat at twice the speed. Some of the most influential early examples of these interpretation problems follow:

A. Dolmetsch wrote, 'Mersenne, in his “Harmonie Universelle”, published 1636, first gives the time value of a Minim as that of a beat of the heart.' Mersenne did liken the beats of the heart with the alternating hand movements of beating the measure (tactus), but he never stated equality, saying that musicians 'make a measure last more or less as they wish'. He did equate the second with the pulse (which takes as long as two heart beats), and at various points mentioned two, three and four seconds as possible durations of a measure. Dolmetsch then quoted Simpson “Some speak of having recourse to the motion of a lively pulse for the measure of Crotchets; or to the little Minutes of a steddy going Watch for Quavers”. He dismissed the pulse evidence because of its variability, but embraced the watch evidence. A typical English watch at that time sounded 300 times per minute, from which he deduced M.M. 75 for the minim. But such watches had verge escapements, and the sounds alternated between an escapement and a recoil in each oscillation of the balance wheel, which resulted in very different-sounding ticks and tocks. Thus there is an ambiguity as to whether 'little Minutes' meant sounds undifferentiated between ticks and tocks, or balance wheel oscillations (the time between ticks). A lively pulse implies M.M. 70-80, so the pulse and watch evidence can only be consistent if 'little Minutes' meant balance wheel oscillations. Thus the M.M. 75 applied to crotchets. Dolmetsch's conclusions on tempo in the seventeenth century were double those of Apel in the first quote above, and that of the evidence. They have been followed by most interpretations of Renaissance and early baroque music since, with alla breve music usually performed at least as fast as alla breve tempos, and alla breve music at least as fast as alla longa (semibreve pulse) tempos.

Sachs often came to the right conclusions, but his reasoning was at times confused, leading to inconsistencies. Simpson's completely unambiguous evidence of equating the pulse with the crotchet, was surprisingly not mentioned. But Sachs quoted Simpson's leisurely counting of the four crotchets in a measure, and wrote 'Such counting yields about M.M. 64', which appears to agree with the pulse evidence. But then he wrote that Simpson's instruction, to count 'one 'two'

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34 J. J. Quantz, *ibid*; but C. P. E. Bach wrote in *Essay*, p.413, that at Berlin 'adagio is far slower and allegro far faster than is customary elsewhere'.
37 M. Mersenne, *ibid*, Prop XVII Cor I, Prop XVIII Cor III & IV.
38 A. Dolmetsch, *ibid*, p.31.
39 Apel was inconsistent in the tempo estimates he offered in different places in his book.
40 For more evidence, see E. Segerman, *ibid* I (fn. 3 above).
with the hand down and 'three' 'four' with the hand up, implied that two of these syllables formed a motor unit, implying that it was a minim. This apparent inconsistency could be due to a confusion between the heartbeat and the pulse, since he included this error in the ending of his previous paragraph with 'The metronomical value of both these motor units, the stride and the heartbeat lies between M.M. 60 and M.M. 80.' A similar confusion appears when he misreported\textsuperscript{42} that Mersenne 'equated the tactus beat with a pulse beat' and because that varied, he preferred a second 'for the semibreve or motor unit, which amounts to M.M. 60 for the quarter note [crotchet]'! Also, in his discussion of tactus\textsuperscript{43}, he associated its length with two, rather than four motor units. His confusion could have been associated with his stride motor unit involving only one step of the two-step repeat, and his pulse motor unit involving the repeat time of two heartbeats. He probably also intuitively preferred the motor unit to correspond with a minim, while his reason said it was with a crotchet.

Following his discussion of Simpson's counting, Sachs calculated the average tempo of a crotchet from Praetorius's statement\textsuperscript{44} that, at a good moderate speed, 160 breves should be played in a quarter of an hour. The result was M.M. 85, and he correctly concluded that the motor unit corresponded with the crotchet at that time. A useful late baroque reference given by Sachs\textsuperscript{45} was 'in A new musical grammar of 1746 [p. 44], William Tans'ur, like Mersenne, gave the quarter note one second, that is, M.M. 60.' A crotchet-pulse default tempo giusto was still alive and well in mid-eighteenth century England, and most probably elsewhere.

**Modern performance**

In the history of music performance, there have been many revivals of music from earlier periods, and in each case, the revived music was modified to conform with current fashions in performance style. They may have been started by historians, but when they succeeded, they were led by musicians. The early music movement is no exception. Sachs\textsuperscript{46} quoted Beethoven (1826) complaining that 'we can hardly have any tempi ordinari any more, now we must follow our free inspiration'. This is still assumed in current performance style, at least theoretically. The primary responsibility of musicians of any time is to seek audience approval. Those who would like reassurance that the tempos they are using were possible historically will find support in this study. Those who would like to believe that the tempos they are using reproduce historical norms will be disappointed. Possible, yes, but typical, no. The assumption supported here, is that musicians always felt free to augment or diminish tempos if the music was too fast or too slow for their purposes. Such purposes, and how they are pursued, change with time. By using modern tempos, early musicians are only following historical precedent.

\textsuperscript{42} C. Sachs, ibid, p.273.
\textsuperscript{43} C. Sachs, ibid, p.219.
\textsuperscript{44} Michael Praetorius, Syntagma muskum iii (Wolfenbüttel, 1619), p 88.
\textsuperscript{45} C. Sachs, ibid, p.314.
\textsuperscript{46} C. Sachs, ibid, p.326.
The Earliest Soundposts

The most obvious function of a soundpost is to inhibit the motion of the treble foot of the bridge. The string vibration rocks the bridge, and with movement of the treble foot inhibited, the rocking of the bridge pumps the soundboard's vibration only with the other bridge foot. If the back at the other end of the soundpost doesn't vibrate much at any of the relevant frequencies, it makes no acoustic difference if the soundpost is integral with the treble foot of the bridge, which goes through a hole in the soundboard. The main practical accomplishment of having such a soundpost is that the strings on the bass side of the bridge have greater influence in pumping the soundboard because of a greater distance from the fulcrum of the rocking motion – equivalent to a higher bridge for those strings. It is much simpler than the post-Renaissance tradition of fitting a separate soundpost.

This arrangement of integral soundpost and bridge foot survives in contemporary southeastern European folk rebecs, such as the Greek lira and Bulgarian gedulka. Going back in history, it is evident on surviving 18th century Welsh crwths. Going back further, we have it on some German viols in the 1530s. In Comm. 840 on p. 54 of Q 49, I reproduced the following two woodblock depictions of such viols (passed on to me by Michael Morrow) from treble and bass part books in the Basel University copy of Christian Egenolf, Reutterlein (Frankfurt am Meyn.1536).

In that Comm, I guessed that the black rectangles under the bridge foot were heavy metal plates that would perform the function of a bass bar. That was silly. Such a plate wouldn't do that. There is another illustration of a set of such viols in a wall painting in the Knights Hall in Goldegg Castle in the Pongau region, that is also dated 1536. The bridge-foot situation is most clearly shown on the large viol on the right of the picture.

There is no earlier evidence for the use of a soundpost. For several decades, the Italians had been enhancing bass response by making their viol bodies much deeper, and the innovation of using a soundpost for this purpose seems to have been an alternative that did not achieve wide acceptance. But the idea was now around, and could be applied elsewhere.

I've suggested that a bass-enhancing device such as a soundpost was the reason why the smaller French fiddles, as discussed by Jambe de Fer in 1556, had four strings while those elsewhere, also tuned in fifths, had only three. The depictions of 16th century French fiddles I've seen don't show soundboard slots like those above, so either there were less obvious holes, or soundposts were built into the bodies when they were made. The soundholes were far from the bridge, so the soundpost could not have been inserted and adjusted afterwards, as is the modern way.
The historical basis of the modern early-music pitch standard of \( a' = 415 \) Hz

The frequency of Praetorius's Cammerthon pitch standard\(^1\) has been the subject of some controversy recently. It was an essential factor in the adoption of \( a' = 415 \) Hz for the modern early-music baroque pitch standard. At the end of Praetorius\(^2\)'s book about instruments, after the Index, and before the list of errata, is a 2-page addition entitled only 'NB'.\(^3\) It includes a diagram giving dimensions for making a chromatic octave of square wooden and round metal pitch pipes. The stated intention was to define his primary pitch standard, which he called \textit{recht en Chormass} or \textit{recht en Thon}, for organ makers and singers to tune to.\(^4\) This appears to have been a more precise version for organ tuning of the pitch standard he generally called \textit{Cammerthon} (or the usual or \textit{rech te Chorthon} in his area of Germany).

In the 19th and 20th centuries, various scholars have used the dimensions specified to find the frequency of that standard either by measuring it from pipes made, or by calculating it directly from the physics of the air vibration in an open cylindrical organ pipe with a mouth opening. The earliest determination of Praetorius's pitch from the pitch-pipe diagram was \( a' = 423 \) Hz (0.7 semitones below modern) by A. J. Ellis\(^5\) in 1880. Early in the 20th century, A. J. Hipkins\(^6\) mistakenly assumed that the pitch standard represented by the pitch pipes was the Chorthon of Catholic churches (a tone lower than his Cammerthon) that Praetorius preferred to his own, and so Hipkins assumed that Praetorius's Cammerthon was a tone higher than Ellis's determination, i.e. \( a' = 475 \) Hz.

The apparent origin of the modern early-music pitch standard of \( a' = 415 \) Hz is in Bessaraboff's famous 1941 book\(^7\). His suggestion was that, for practical purposes, we should approximate the original pitches with the closest pitches to whole semitone steps from modern \( a' = 440 \) Hz. Thus, accepting Hipkins's erroneous conclusions, Bessaraboff assigned Ellis's 423 Hz for Praetorius's Chorthon to \( a' = 415 \) Hz and his Cammerthon to \( a' = 466 \) Hz. He claimed that this Chorthon pitch 'is the tonality of the musical system of the classical period, which lasted from about 1600 until 1810-20'. We now know that this is a gross distortion and oversimplification\(^8\), but the grain of truth here is that the pitch of Praetorius's pitch pipes continually remained as the usual standard for violin-led string ensembles in north and much of south Germany, Scandinavia and England though the period stated. In other regions in Europe, till late in the 18th century, the Chorthon pitch a tone lower than the pitch-pipe pitch was more often followed.

One problem with Bessaraboff's proposal (beside being based on a misreading of Praetorius by Hipkins) is that it was based on Ellis's determination of the pitch-pipe pitch. If he made the proposal later, when better determinations of the pitch (see below) indicated that it was up to 10 Hz higher, his pitches would have been 440 Hz for Chorthon and 494 Hz for Cammerthon. This highlights the other problem with his proposal, which is that the important pitch standards of the

\( ^2 \) M. Praetorius, \textit{Syntagma Musicum} II (De Organography) (Wolffenhüttel 1619 & 1620).
\( ^3 \) M. Praetorius, ibid pp. 231-2, translated in S. Heavens, 'Praetorius's pitchpipe \textit{Pfefflin zur Chormass}', \textit{FoMRHI Quarterly} 78 (Jan. 1995), Comm. 1328, p. 60.
\( ^4 \) Clear evidence that these were the same as Cammerthon is given in S. Heavens & E. Segerman, 'Praetorius's Brass Instruments and Cammerthorn', \textit{FoMRHI Quarterly} 78 (Jan. 1995), Comm. 1327, pp. 56-7.
\( ^7 \) N. Bessaraboff, \textit{Ancient European Musical Instruments} (Harvard Univ. Press, Boston, 1941), p. 378.
time fall near the middle of his semitone range, as a small pipe such as this one is grossly amplified.

One great attraction of Bessaraboff's proposal to early musicians is that it blurs the picture enough that they can justify the use the same instruments for all baroque and classical music, including copies of the superior later-baroque French woodwinds which played at c. 410 Hz, about a semitone lower than Praetorius's pitch. Another is, at 415 Hz, the rate of breakage of violin gut 1st strings is lower than the rate that most 19th and early 20th violinists, who used such strings, had to tolerate. The steel 1sts that modern violinists had grown up with lasted much longer, and a pitch a semitone lower appeared to be a reasonable practical compromise to use gut strings to demonstrate their commitment to historical accuracy.

Bunjes⁹ built a set of reproduction pipes with the resultant pitch being $a' = 430$ Hz, and Bormann¹⁰ did the same with the resultant pitch being $a' = 427$ Hz. Thomas & Rhodes¹¹ calculated the pitch using the method of Ingerslev & Frobenius¹², with the resultant pitch being $a' = 426$ Hz. D. Gwynn¹³ surveyed previous determinations and added his own corrections to that of Bunjes, which he considered most reliable, with the resulting pitch being $a' = 433$ Hz.

Organ historians try to follow an organ's pitch history by studying its records of repairs and alterations, and on each of the pipes, studying the nominal pitch names written on them, the styles of that writing, the signs of pitch alteration and the final pitches. When the pitch of an organ is changed, pipes can be shifted to be activated by different keyboard keys and their lengths can be shortened by trimming (or cutting scoops), or lengthened by adding an extension. Smaller changes can be made by widening or narrowing the tops of pipes. When a pipe was shifted to be activated by a new key, the new nominal pitch was sometimes marked. The trimming of pipe lengths can rarely be detected, nominal pitches on pipes are often missing and records of an organ's repairs and alterations are notoriously incomplete. Occasionally, original decoration on some pipes or the space inside an original organ case can put limits on some original pipe lengths. The original pitch of an old organ is usually estimated from the pitches of pipes with the earliest pitch-name markings that show the least evidence of alteration.

Some experts on German organs made in the 17th and 18th centuries make the generalisation that their original pitches tended to be at about a semitone above modern throughout that period. There is no question that this was the case late in the 17th century, but we are concerned with the situation on Praetorius's time, early in that century. One very highly regarded organ, in mostly original condition, is the 1616 Compenius organ in Frederiksborg. Its very unusual all-wooden piping resists the tinkering with pitch that metal pipes have always been subjected to, and it fits neatly into an original case so original pipe lengths couldn't have been longer. It appears to have been made originally at a pitch of about a semitone above modern, and Praetorius was consulted on its design. These experts are very impressed by the sound of this organ and its association with Praetorius, and so they are very skeptical about Praetorius's pitch-pipe evidence, which implies that his pitch standard was about a semitone lower than the pitch of this organ.

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⁹ P. G. Bunjes, *The Praetorius Organ* (Concordia, St Louis, 1966), Chap. XIV, pp. 772-866

¹⁰ K. Bormann, *Die gotische Orgel zu Halberstadt* (Merseburger, Berlin, 1966)


Praetorius wrote that most of the organs in his time were tuned to his pitch (Cammerthon or 'proper' Chorthon), but that there also were many at a tone higher and lower, and 'not a few' a semitone higher. He mounted a spirited argument against the tendency in his time to raise the currently fashionable pitch to a semitone higher. A likely scenario is that he lost the battle against the higher pitch for the Compenius organ, but hoped (vainly, it turned out) to win the war with the arguments in his book. That organ is the only one amongst the about three dozen organs he esteemed (listing their stop dispositions) that have survived well enough for modern researchers to be able to estimate what their original pitches were. The vast majority of his esteemed organs could easily have been at the pitch he specified. There are three other German organs that Praetorius could have known when writing the book that have had their original pitches estimated. We have no idea about what he thought of them. Two had the pitch of a semitone above modern, and one was approximately at modern pitch. Since the pitch of the first two remained in fashion later in the century, the probability of their survival would be greater than others. In conclusion, there can be no statistical case made from the pitches of the few early 17th century German organs estimated that the most prevalent pitch was different from what Praetorius claimed.

There is also written evidence indicating that the most popular organ pitch level early in the 18th century was a tone higher than Praetorius's pitch, and that it dropped by a semitone late in that century. This change in pitch recognition is not reflected in the general conclusions of the organ specialists. In my analysis (that accepts all of the written evidence), the fashion of German organ pitch changed as follows: Early in the 17th century (Praetorius's time) it was a semitone lower than the constant level assumed by the organ experts, it was at that level (a semitone higher than in Praetorius's time) later in that century (when Schnitger was the major maker), it went up another semitone around 1700 (to follow the pitch of the ancient organs), and it dropped a semitone about two-thirds into the 18th century. We would expect these organs to be at the organ experts' pitch levels by late in the 18th century. I would be very surprised if the organ experts can tell the difference between the pipes remaining where they were during all of the 18th century (which they claim) and their being shifted a semitone at the beginning of the century (with the longest pipes unused) and back again later in the century.

The two competing theories are that Praetorius's pitch was as deduced from his pitch-pipe diagram, and that his pitch was about a semitone higher, as usually found in the surviving German baroque organs. The subjective choice that is usually taken is to decide which evidence one trusts more. A more objective choice between them should depend on the relative probabilities of how well the pitch-pipe evidence can be explained assuming the higher pitch theory, and of how well the surviving organ evidence can be explained by the lower pitch-pipe theory. It was shown above that there is no statistical case for inconsistency between the surviving organ evidence and Praetorius's lower pitch-pipe pitch.

The organ specialists have not attempted to explain how the pitch-pipe evidence could be consistent with their higher-pitch theory, but a harpsichord specialist who supports that theory has attempted this. He noted that Praetorius had neither specified the wind pressure nor the mouth dimensions of his pitch pipes, and he proposed that these could have been high enough to get a pitch a semitone higher. As a model, he picked a late 16th century Innsbruck organ with pipes having extraordinarily large mouth dimensions, which has been restored with an extraordinarily high wind pressure of 90 mm water column. Assuming room temperature, these parameters and Praetorius's dimensions, he got a good part of the way towards pushing the pitch up a semitone on a

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14 M. Praetorius, ibid p. 103.
15 M. Praetorius, ibid p. 15.
16 J. Koster, 'Praetorius's Pfeifflin zur Chormass', presented at the Conference 'Pitch and Transposition, 16th-18th Century' organised by Internationale Musikprojekte, Hochschule für Künste, Bremen (October 1999).
test pipe he made.

To support his theory that the mouth dimensions were larger than expected, he also presented the mouth dimensions and diameters of 19 pipes (marked with the same nominal pitch as one of the pitch pipes) from surviving German organs roughly contemporary with Praetorius (their lengths have most probably been altered, so that is not relevant evidence). I calculated the averages of the pipe diameters and the mouth dimensions. Assuming Praetorius's pipe length, a wind pressure of 75 mm water column (considered to be the maximum that could be expected by a specialist on early German organs, who happens to advocate the higher-pitch theory for Praetorius's pitch) and the annual average temperature of 10 degrees Celsius in Praetorius's region in Germany (churches were not heated), I calculated the pitch of a pipe with the average mouth dimensions and Praetorius's diameter. The method of Ingerslev & Frobenius was used, with a slight correction for the average mismatch between their test pipes and their theoretical calculation. The result was $a' = 437, 436, 435$ and $434$ Hz for the temperament being equal, sixth comma, fifth comma and fourth comma meantone respectively. If I use the average diameter of the pipes instead of Praetorius's diameter the results are 2 Hz higher. If I assume a wind pressure of 55 mm water column (like on the Compenius organ) instead of 75 mm, the results are 3 to 4 Hz lower. The uncertainty in the calculation method is about ± 6 Hz. This calculation supports the previous studies on Praetorius's pitch.

The semitone-higher theory for Praetorius's pitch has been an article of faith amongst wind-instrument specialists since Anthony Baines suggested it in his famous book on woodwind instruments. He wrote that "Recorders at Verona identical in shape and in size with those in Praetorius's scale drawings at 'chamber pitch', sound a good semitone above modern pitch; say about $a' = 470". His criteria for being 'identical' must have been rather fuzzy since I (and others) have found that there is a systematic error in the sounding lengths of the recorders in Praetorius's drawing, so that as depicted, the pitch standard varies, with the smallest ones at a standard about a semitone lower than the largest ones. I suspect that his large recorders were in his collection but could not be used in his ensembles at Cammerthon pitch.

We have reason to expect that a large fraction of the surviving wood instruments would sound about a semitone above modern because they were made in Venice, where they were played with organs, and that was the pitch standard of Venetian organs. Woodwind instruments made there were used extensively throughout Europe, and the woodwind specialists interpret this as suggesting that this pitch standard was largely universal (including the German regions Praetorius knew). This could well have been true for most bands of Venetian woodwinds, but the expectation of these specialists that this carried over to the pitch standards of string bands does not have any supporting evidence, and is unlikely because wind bands and string bands rarely played together (the difference in pitch standards probably was a factor). Praetorius's insistence that both types of instruments should be played at the same standard was very unusual for his time. A minority of surviving instruments (mostly transverse flutes and mute cornetts) were made at lower pitch standards, apparently for playing with stringed or keyboard instruments at lower standards.

The assumptions that became popular initially in the early music movement soon became traditions. It is not in the interest of anyone involved with the movement to change them now. All I am attempting to do here is to get the historical record straight.

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18 The Compenius organ at Frederiksborg castle has a wind pressure of 55 mm water column according to the 'Compenius' entry by H. Klotz in The New Grove Dictionary of Musical Instruments I (Macmillan, 1984), p. 449.
Notes on the Symphony

Images of the symphony (otherwise known as the organistrum) first appear in the 12th century. It was bowed by a rosined wheel turned by a crank and one or more strings were usually stopped by a mechanism hidden in a box that was operated by apparently equally-spaced protruding knobs. During the 13th century it became fashionable to hide the soundboard and bowing wheel as well. Small symphonies tended to be enclosed in a rectangular box, about a third or half a metre long with the crank on one end, a protuberance on the opposite end, and equally spaced keys along the whole of one edge. We shall call these 'box symphonies'.

The spacing of multiple features such as frets, knobs and keys in depictions can often be unreliable since they were usually executed without the actual instrument present to view, and the visual memory could easily be of the feature's multiplicity without detailing how the spacing between them varied, resulting in equal spacing as the simplest possibility. So we can see equally-spaced keys in an 18th century drawing of a hurdy gurdy when we have good reason to expect that the key spacings would decrease as they get closer to the wheel. Nevertheless we must take seriously the presence of keys over the full length of one edge of the box symphony, since this is a feature that is most unlikely to be falsely edited from indistinct memory. Accepting that this feature is historically accurate leads to the conclusion that there must be an offset between the key position and the string stopping, since a number of the keys must be on the wrong side of the wheel. This makes it more likely that on the earlier symphonies the equal spacing of the knobs, as seen in all depictions, is also historically accurate. It thus seems that the early symphony makers were trying to do the same thing as the organ makers did, to have the spread between knobs or keys to be controlled by musical interval alone. At that time, the developers of the organ and the symphony were in the same religious musical establishments, so this may be expected.

A 13th century source stated that the symphony had three strings with the two unstopped drone strings an octave apart and the melody string tuned a fourth or fifth below the high drone. There were 8 tangents that stopped the melody string, providing an octave of 9 notes of a diatonic scale from c with both b flat and b natural. When the stopped string was a fifth below the high drone (being the mode final), the possible modes that could be played in were mixolydian and dorian (or hypomixolydian), and if it were a fourth below, the possible modes were lydian and hypolydian. The simplest way of changing from the former to the latter would be to tune the melody string up a tone. Tuning it up another semitone would facilitate playing the phrygian and hypophrygian modes, but since this was not mentioned, we can presume that it was less common.

The above was concerned with the original large (about 1.5 metres long) instrument that rested on the laps of two people with the soundboard facing away from the players. The strings were reported to be thick. One player turned the rosined wheel with a crank, while the other stopped each note by operating the knobs that protruded from the top of a box that enclosed (and hid) the stopping mechanism. To have a second player to turn the wheel was like having a second person to work the organ bellows.

An 18th century copy of a drawing from another 13th century source (since lost) shows a 3-string symphony with the 8-stop mechanism exposed. It appears that all of the strings were

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4 M. Gerbert, *Du Cantù et Musica Sacra* II (St. Blasien, 1774), Plate XXXII, p. 16.
stopped by turning the knob on its shaft, which h
projected from the shaft towards the pegbox and pressed up against the strings when the knob was
turned. Since all of the strings were stopped, this is not the same mechanism as that of the
instrument discussed above, but an alternative. It could have easily been converted to operate in the
other way by adding blobs to the ends of the protuberances under the melody string. The knobs are
identified with the pitches associated with them, and they are the same as in the first source
discussed.

The drawing shows letters on the soundboard on both sides of the strings, which apparently
indicated string tunings. On one side is m d G , and opposite these on the other side is d D d.
Bachmann\(^5\) suggested that 'm' could mean 'melodia', and that the d and G after it indicated the
pitches of the other two strings. He did not interpret the letters on the other side. He concluded
that, with the C pitch given for the melody string on the neck, the tuning was G d c, similar to
Jerome's 3rd tuning. This wouldn't work with the stopping of all of the strings simultaneously, so
he suggested that the depicted mechanism was mistaken, and only one string was stopped. When
there is no relevant evidence that suggests that such a mistake is likely, an interpretation that avoids
assuming a mistake would be preferred:

I suggest that the three letters on one side were associated with the three on the opposite
side, so the left pair indicated that the melody pitch (m) was assumed to be d in both. Then the
middle and right pairs indicated that the other strings were either d and D or G and d. This leads to
two tunings, both with one other string in unison with the 'melody' string, and the third string an
octave lower in one tuning and a fifth lower in the other. In the first tuning D d d, all strings would
be playing the melody, imitating an octave mixture on the organ. In the second tuning d G d, the
playing would be in strict parallel organum. The apparent inconsistency between the C on the
stopping scale and the d as the 'melody' pitch is easily explained by each of them intended to be
pitches relative to others and not relative to each other. The C was relative to the other stopped
pitches and it had to be given a pitch name that allowed the others to be normal notes without
sharps or flats other than a flat for b. The d was a pitch normally used with g's and other d's for
fiddle strings, as in Jerome's tunings.

The depictions of these large symphonies usually show 6 to 8 knobs. An exception is a
sculpture on the Portico de la Gloria in Santiago de Compostela, which had 12.\(^6\) The distance
between the tuning-peg end of the neck and the knob closest to the bridge on this instrument was
still a bit more than half of the distance to the bridge, as in the others. This implies that the range
was an octave like the others, but the stopping was chromatic.

Assuming that the equal spacings of the knobs was realistic, there must have been offsets
allowing stopping at a distance from the knobs. The hiding of the stopping mechanism in a closed

\(^5\) W. Bachmann, op. cit., p. 111.
\(^6\) W. Bachmann, op. cit., Plate 80; the knob man seem here to be deactivating one knob while activating another.
box implies that making it mysterious was preferred to letting it be a visual distraction, and offsets could provide such a distraction. If the distance each protuberance stuck out from the knob shaft varied, that would have provided the appropriate offsets.

During the 13th century, these large 2-player symphonies faded in popularity as they were replaced by small 1-player box symphonies which sounded about an octave higher (like other fiddles). This parallels the rise in popularity of 1-player portative organs. It was either suspended against the player's chest or resting on the lap. Instead of turning knobs, the strings were stopped by some mechanism worked by pressing 13 to 15 keys that were evenly spaced along one box edge, from corner to corner.

The box edge with the keys could be along the top, either the edge close to or away from the player, or along the bottom, away from the player. If gravity was involved in key return, he basic design of the stopping mechanism when the keys were along the bottom must have been different from when the keys were along a top edge. If key return was accomplished by the springiness of wooden strips, the mechanisms could have been the same. The two cases when the keys were along the top could have been mirror images of each other.

One possibility is that the string was stopped from the side, as with later hurdy gurdies. Another is that it was stopped from below, as with the earlier large symphony. I will dwell on the opposite possibility as was suggested by Jeremy Montagu\(^7\) that the mechanism pressed the melody strings from above onto the wheel as well as stopping them. It is possible that the strings were pressed against a fretted fingerboard to prevent pressing too far, to make the stopping more secure, and to improve intonation on uneven strings by fret adjustment. In this design, when the wheel is turned, only drones sound when no keys were operated. The keys could have stopped different melody strings tuned in unison, so more than one string could have sounded simultaneously. With two melody strings and appropriate assignment of which is operated by each key, all fifths, most fourths and some thirds could be played together. With more melody strings, one can get more intervals. If a key was swiftly but lightly pressed, the mechanism would make a weak sound by stopping the string (like on a clavichord) before the string touched the wheel. This could be used for very private playing. If we remember that all consonant chords could be played over three octaves on the early clavichord with only seven string pairs all tuned in unison\(^8\), it is not difficult to imagine such a box symphony, with no wheel (which could make the stopping mechanism simpler) and more strings, to be placed in a different-shaped box, and called by a different name, perhaps something like 'chekker'. When drawn iron wire for strings became available at the end of the 14\(^{th}\) century, which were very strong, durable and stable, this encouraged the development of amateur instruments with many metal strings, and one of them, the clavichord, borrowed much from the box symphony. It seems that afterwards there was some influence the other way, as seen in the symphony with a chromatic fingerboard in the stained glass (1501/2) of the north transept at Great Malvern Priory in Worcester, England.\(^9\)

The simplest mechanism for stopping the box symphony strings that I can think of has each key on top of a shaft confined to sliding in a track in the box side, with a rigid arm extending from the key end of the shaft straight to the string stopping position. There would have to be a spring arrangement (acting on the bottom of the key shaft) to keep the key and the string stopper raised when the key is not pressed, which makes this possibility to have two moving parts per note. What bothers me about this design is that for the keys on the wrong side of the wheel, the reaction force from the stopping twists the shaft in the track, promoting sticking.

A different mechanism, with one moving part for many notes and two moving parts for the others, involves a springy strip of wood for each note with a blob on it to press the string in stopping. One end is wedged into a channel cut near the top of the box side opposite to the side with the keys, and it is suspended above the strings, roughly perpendicular to them. On the other end is the key if the offset is not too great, and if the offset is too great, that end is pressed down by a stick with the key on one end and with the other end confined to a groove (acting like a hinge) near the top of the small end of the box that is remote from the end with the wheel shaft. The stick has a blob on it to depress the free end of the strip for the appropriate note. The sticks are at enough of an angle to the box side to make room for the shorter sticks associated with lower notes.

A consideration that must be kept in mind is that the player needs access to the strings to change them, to the fingerboard to tune the frets and to the mechanism for adjustment. In the mechanism design given above, it seems wise to have the springy strips of wood and the channel that their ends are wedged into in an independent frame that lifted out when the box lid was opened. On top of it would be another independent frame that has the sticks and the grooves that confine their ends.

There are many other possible designs of the box symphony stopping mechanism. They may include clavichord-like key shafts with sliders on the end and a balance rail with a pin to counter the twist of the offset. Then there would need to be other components to convert the upward motion of the key end to a motion to stop a string. Other ideas on this would be very welcome.
Some design considerations in making flattened lute backs

Lute body design usually starts with given outline shapes of the [a] soundboard, of the [b] back along the central symmetry plane, and of the [c] back along some planes perpendicular to both the symmetry plane and the soundboard plane, at least the one at its maximum width. The obvious way to generate the shapes of the ribs of a lute back is to [1] make a mould, [2] pick a number of suitable sectional planes perpendicular both to the symmetry plane and the soundboard plane, and [3] mark a line on the surface of the mould where it meets each of the sectional planes. If the mould is of the toast-rack type, each bulkhead is at one of the sectional planes. Then one has to [4] divide up each of these surface lines into short lengths to mark the widths of each rib. Steps [2], [3] and [4] will usually need to be done before [1]. After that, one [5] lays a flexible strip over where the rib should lie over the mould and transfers the rib-edge marks to it. Then [6] one connects the marks into smooth curves and transfers this shape onto the rib blank.

After the ribs are cut to shape and bent to fit at their rightful places on the mould, their edges need to be fitted to one another and glued. It is advantageous for the joints between staves of a lute back to lie on planes, because it is then easy to make them fit closely by planing (or abrading) the edges of each stave on a flat surface after it was bent to its final shape to fit on a mould. It is likely that this was a common practice in early lute making, and it is the usual way in modern lute making. This note discusses the considerations involved in defining these planes (step [4] above) in the usual case when the back's section is not circular, and in the extreme case when the back is seriously flattened.

The plane of symmetry of the back cannot be one of the joint planes because a central stave is needed (resulting in an odd number of staves) to balance the instrument without tipping when resting with its back on a horizontal surface. Perpendicular to the symmetry plane (and to the soundboard) are the sectional planes. The simplest choice of where the joint planes are located is perpendicular to the sectional planes. Then these joint planes can be represented as a set of lines on a drawing showing all of the back sections chosen. Each section in the drawing is a symmetric polygon of rib lines.

The sections are those given (that one starts with) and others that need to be generated. Those that need to be generated are initially represented on the drawing by a point on the symmetry line for the body depth from shape [b] for that section and points on the soundboard edge line from shape [a] for the width there. If one has started with two back cross sections, the joint lines go between the corresponding rib joints (polygon corners). If one starts with more than two cross sections, and the corresponding joints don't fall close enough to be along lines, this method doesn't work.

If one starts with only one cross section, the choice of joint planes is arbitrary. If one wants to try to make each rib symmetric, one can draw perpendicular bisectors1 to each of the rib lines on the initial section, and the joint lines go from each joint (corner in the polygon) to the intersection of the perpendicular bisectors of the rib lines on each side. Another approach is to find a few circular arcs that go through the polygon corners closely enough, and the joint lines go to the arc centres. If one wants to avoid having to twist each rib as one fits it to the mould, each rib line must be parallel to the corresponding rib line in the initial section. If one also wants the rib widths to be equal in each section, unless the initial section is near circular, one or both of the above conditions would have to be violated. Compromises will have to be made. Of course, don't worry about serious variations in rib widths that are covered by the end clasp.

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1 To construct the perpendicular bisector of a line between two points, set a compass for a radius somewhat greater than the distance between the points and mark the four arcs centred on the points on each side of the line. The perpendicular bisector is the line joining the arc intersections on each side.
In the case of a flattened back, an appropriate shape for an envelope for the polygon in the maximum section is an ellipse. An ellipse has a major axis (along the soundboard bottom of the polygon), and let us call half the soundboard maximum width 'a'. It has a minor axis (going to the maximum depth), and let us call the height at the maximum 'b'. The radius of curvature of an ellipse at the ends of its major axis is $b^2/a$, and at the ends of its minor axis is $a^2/b$. A traditional method of approximating an ellipse with four circular arcs (two with centres along one axis and two with centres along the other axis) leads to arc radii of $b^2/a$ centred along the major axis and $(a^2/b)(1+c/a)/2$ centred along the minor axis. With this shape, the body depth falls off as we go away from the polygon centre faster than is seen on surviving lutes. This fault can be corrected, becoming closer to a true ellipse, if we use arc radii centred along the major axis of $b^2/a$ (as above), and arc radii centred along the minor axis of $a^2/b$, plus a further arc between each of these, tangent to both of them. The additional arc has intermediate radius, and can be found by eye and trial and error in picking its radius and its centre.

The point of tangency between the arc and the section polygon is at a corner of the polygon at the section width, but not at the section height. The central stave parallel to the base has first to be drawn, and where it intersects the arc is the appropriate corner. Then the stave polygon is drawn with the corners at the arcs. Each line representing joint planes intersects with others at one of the three arc centres. A systematic way of finding the centre of the intermediate arc starts with deciding where the transition point between the large-radius and intermediate-radius arcs should be at a particular corner of the polygon. Draw a line from the chosen transition point on the large arc (with radius of $a^2/b$ and centre on the minor axis) and that centre. On that line, mark a point at the distance of $b^2/a$ from the transition point. Draw a line between that marked point to the centre of the small arc on the major axis ($b^2/a$ from its end). The perpendicular bisector of this second line intersects the first line at the centre of the new intermediate arc.

The transition point from the new intermediate arc and the small-radius arc should be on the line between the centres that those arcs were drawn from. If that ends up in the middle of a stave, try starting again with the first transition point one stave away from the first choice, and see if that improves the situation.
Tromba Marina Notes:  

Name:  
The name “Tromba Marina” initially may suggest relationship with a trumpet and further that it had maritime connections. Other names applied to it are Trumscheit, Marin(e) Trumpete, Trompette Marine, Marien Trompete and Trompetengeige. While its body shape resembles a trumpet its function appears to have little if anything to do with the sea.

Era:  
Emerging around the early 1500’s with an apparent use to c.1700 and occasionally beyond that date, it is a further category of bowed instruments contemporary with the development of viols, and a little later with the violin family. Crwth, rebec, trumscheit monochords and mediaeval fiddles in different regions were among its predecessors. From a maker’s point of view expression of symbolism, and to a large extent its function, seems to rank before acceptance of the acoustic outcome of such a bowed instrument. Relative standardisation, as we know it in terms of a modern violin with little scope for individual expression, was not a prominent issue facing tromba marina makers and their patrons scattered over Europe. Few examples exist to provide indications of variations in styles, sizes, and design features. Suggest referring to writings of Adkins and Dickinson. View instruments in Munich-Deutsches Museum, Wagner’s House Tribschen Lucerne, and Basel Musikmuseum.  
(See below: Dimension variations noted from Basel Musikmuseum display)

Production/application:  
Initially makers appear to have produced trombae marinae principally in a region between Lucerne and Landschut in S.Germany, the application being for use in convents, monasteries and cathedrals. This use in religious context would be the prime motivation in making such instruments. Hence the name “Tromba Marina” – (Mary’s Trumpet), having a body shaped like a trumpet and Mary’s role associated with intersession do tie together. Propagation in using and the making such instruments apparently spread and extended in a corridor approximately 250Km wide between Lucerne and Breslau in Poland. Regional styles inevitably arose with no single size or pattern emerging. However the body is always conical with a flat front as sound board, while the rear shape was constructed from a series of staves some 3 to 7 or more in number. Some of the bodies were flared, while others were less elaborate, all sides being straight and all edges straight. Such variations would relate to a maker’s preferences and skill as well as to the wishes of patrons and to financial provision.

Function and symbolism:  
Whether the outcome was intended to sound like a trumpet or not is difficult to say, when symbolism in an ecclesiastical context is a potent factor. Only one visible main string is the active ingredient in play. This one string is bowed so as to evoke the harmonics of that string. Scope exists to introduce sympathetic strings inside the body as well and to give credence to placing a rose window (or two) in the front of the instrument as it were to let the sound out. A neck piece was topped by various emblems such as a monk, gargoyle, bird, shield, orb, or some meaningful embellishment. To take account of how to tension the single string, a worm was suitable for vertical screw arrangements as in some Bavarian examples. Many others used a ratchet wheel and pawl, some made from hard timber. Later instances employed a metal ratchet wheel and pawl, and later still worm versions emerged with the advantage of possible finer tuning.
Size Variations: Noted from 8 in Musikmuseum Basel

<table>
<thead>
<tr>
<th>Instrument</th>
<th>Basel Ref:</th>
<th>LOA</th>
<th>H</th>
<th>Decke L</th>
<th>Decke B/B</th>
<th>Corpus T</th>
<th>Hals L</th>
<th>Decken L</th>
<th>String L</th>
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<tbody>
<tr>
<td>1. Schwyz</td>
<td>Uri Jh ?</td>
<td>1855</td>
<td>1155</td>
<td>80/290</td>
<td>275</td>
<td>525</td>
<td>965</td>
<td></td>
<td></td>
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<td>2. Schwyz, Klosters</td>
<td>end 16Jh</td>
<td>1667</td>
<td>1043</td>
<td>70/587</td>
<td>365</td>
<td>500</td>
<td>725</td>
<td>1225</td>
<td></td>
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<td>3. Schwyz</td>
<td></td>
<td>1763</td>
<td>1105</td>
<td>87/365</td>
<td>254</td>
<td>548</td>
<td>872</td>
<td>1420</td>
<td></td>
</tr>
<tr>
<td>4. Schwarzwald</td>
<td>17 Jh</td>
<td>1985</td>
<td>1236</td>
<td>183/374</td>
<td>250</td>
<td>589</td>
<td>930</td>
<td>1519</td>
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<td>5. Schwarzwald</td>
<td></td>
<td>1810</td>
<td>998</td>
<td>112/370</td>
<td>200</td>
<td>640</td>
<td>815</td>
<td>1455</td>
<td></td>
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<tr>
<td>6. Deutschland</td>
<td></td>
<td>1985</td>
<td>1160</td>
<td>131/365</td>
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<td>7. Deutschland</td>
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<td>1650</td>
<td>993</td>
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<td>878</td>
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<td>8. Unknown</td>
<td>9 Jh</td>
<td>1380</td>
<td>918</td>
<td>113/210</td>
<td>?</td>
<td>295</td>
<td>620</td>
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TM. By L.A. 2003: $H = 1940\text{mm}$
Personal Background:

Having made members of the violin family – violins, violas, cellos and swell-back double bass, there arose as attempt to follow the origins of bowed sound. Thus ensued ventures into making viol-da-gamba, and earlier bowed mediaeval instruments. Crwth, rebec, early and late mediaeval fiddles were duly produced while the next instrument to seize on personal attention was the tromba marina following visits at various times to Munich, Mittenwald, Wagner’s house at Tribschen Lucern, and to Basel. Seeing examples of these unusual instruments fired curiosity sufficiently to think how one could be made and what sort of sound they could possibly emit in relation to the other bowed instruments.

Mock up:

Thus followed a time making a mock-up version of a body in 2mm MDF board, together with endless contemplation re the practicalities of making a complex instrument such as this, and as it would be pursued in the 1500’s. In that connection one’s present day predisposition can easily be misleading by thinking like a violin maker who feels obliged to design a mould around, or in which the body shape could be built. So in a naïve way considerable effort was spent making a suitable series of cross section panels and constructing an external mould. It was a framework, considerable in size, made in a way that it could be easily dismantled and stored. Attention was devoted also to making an internal mould if that should be required.
Later the real task began. Time was available, but surprisingly no form or mould was needed either now, nor presumably in the 1500’s! Procedure was committed to video tape to confirm steps of one suitable method. Making an unfamiliar instrument for the first time of course means that craftsmanship is hesitant and extended, not as might be in the case of making subsequent editions. Nevertheless sloppy work is ruled out. In spite of all cerebration and effort, the puzzle that kept haunting my dreams was – how do you glue in the final panel, place it correctly, and fasten it soundly as with all the others? Again a simple method was found.

**Materials:**

The aim was to use local timber as nearly akin to those used by makers of the period in question. Thus sycamore, maple, willow, apple, beech, pear and laburnum woods were all to hand from local sources. An exception was Swiss or Bavarian spruce for the sound board. This being resin free, grown slowly at considerable altitude and with closer grain, it would be likely to produce better sound results than resinous spruce grown in Scotland, of rapid growth at modest heights and in an acidic soil. Thus after some searching I was able to obtain from David Dyke, some Swiss spruce of around 50 inches in length, likely to be more suitable. Work could proceed. All gluing employed traditional animal skin/hoof/horn glue as used by instrument makers who wish their jointing work to be reversible without damage to the wood.

**Symbolism adopted:**

Feeling one’s way in unknown territory led to considerable emphasis on symbolism. A rose window of apple wood with maple and laburnum surround was backed by a cloth of blue, Mary’s colour, to lend an ecclesiastical setting. This accommodated internal arrangements for sympathetic strings and emergence of sound.

The flared body has six sides, stimulating thought for the longitudinal joins of the wooden staves. Many Trombae Marinae in the past have had lacing or banding in parchment along the joins for appearance and to pre-empt separation and splintering.
Purbeck stone applied in this instance would serve little purpose. Decoration by parchment strips prompted finding manuscript copy for 1605 viol music showing some decorative treatment achieved by wood engraving. Strips of print which could be interpreted as a series of faces was intriguing and consequently I made a half face drawing. So from this element inversion to a half design, and again by inversion, a four quadrant whole gave a suitable unit. Some effort was made to engrave this in boxwood end grain with sandbag and appropriate tools. Results were obtained, but brought home the fact that the art and craft of wood engraving calls for a high degree of experience and skill to produce fine work.

So I surrendered and relied on the computer to convert the half face drawing into full face, and then by inverting again to produce the quadrant whole. In turn this was printed out in as many strips as required. Symbolically speaking it can convey an assembly of interceding humanity on this instrument's edges presenting one and a half thousand faces.

Similar effects could undoubtedly be achieved using lengths of lace as an edge binding strip. Meantime, surmounting the whole is a head carving in gargoyle form. This attempts to show an individual pleading for intersession and sure salvation. Perhaps this may seem appropriate to the period in question an age of pilgrimage seeking salvation by some special endeavour.

Neck-block

The neck-block in turn has a collar perhaps in Bavarian – Mittenwald manner with a shoulder akin to a style expressed on Nuremberg instruments.

Internally the block has a large slot into which the neck can be set or removed if required.

Anchor points are also arranged upon the block anchoring upper ends of internal sympathetic strings.
Mechanics:

Simple mechanics naturally call for attention and imply issues concerning:-

a) The back stave, which will be in tension due to a forward pull on the main string, should thus be straight rather than convex or concave.

b) Main string tension is achieved by a ratchet and pawl system of hard wood, being laminated to give cross-grain toughness. Later examples employed metal systems.

c) Sympathetic strings have an internal bridge pressing on the front. This counter-acts to some extent main string pressure from the outside.

d) Bridge construction which is asymmetrical has the main string resting above one foot and thus it is unlike a violin family bridge. A subtle equilibrium then exists allowing the second foot to touch the front only lightly enabling it to tap the front so providing secondary vibrations, a unique characteristic of this form of bowed instrument, and making its characteristic sound resemble that of a trumpet or trombone.

e) A Guidon peg is provided near the edge of the front to carry a gut strand attached to the main string between bridge and saddle. A player can thus adjust the tapping ingredient of the second bridge foot to acquire its desired function.

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Scientific Interests:  

(A note from the writings of Adkins and Dickinson).

"Aside from its fluctuating career as a performing instrument, the trumpet marine earned an honourable place in the history of acoustics as the experimental vehicle of a number of 17th century scientists engaged with the phenomenon of harmonics. The considerable interest shown through the century in the nature of the vibrating string often focused on the trumpet marine because of its unique capability of reproducing the pitches of a natural trumpet."

"Mersenne had discovered that a vibrating string would yield at least four other tones beside its basic pitch, and these tones were the same as those produced on a trumpet and its string counterpart. His failure to comprehend the nature of the object vibrating simultaneously at different frequencies, resulted primarily from his lack of knowledge of the organ and other wind instruments."
Playing Method:

Playing a tromba marina differs from play on members of the violin family. Bowing the string with one hand takes place a short distance below the peg-box nut, while fingering with thumb or finger of the other hand is the matter of touching the string lightly below at successive nodal points to elicit the harmonics of the string. The string is not stopped on a fingerboard as with normal play on violin family instruments. (i.e. there is no fingerboard as such.) The implication is that every change of pitch calls for a shift of thumb or finger position. This is unlike the violin experience where fingers of the hand do help to locate positions of notes and thereby limit the number of shifts required. Its role would largely be to supply ground harmony notes rather than a melodic line. Hand shift characteristics infer that it is not a particularly easy instrument to master.

The back of the peg-box and carved head will rest on the shoulder of a player in standing position while the instrument tilts and rests on the bottom edge of its rear stave.

Bows:

Bows in general do not survive as long as do instruments. Information as to the types of bow used at different periods is not always clear. The likelihood would be that around 1600 a gamba type bow could be put to use. The bow made for this particular instrument is akin to a bow for a viol-da-gamba but echoes an earlier period where no screw tensioning was employed. Instead the frog is slotted into the bow shaft and held there by a hook projection. The bulk shape of the frog thus creates tension on the hair which can be supplemented by finger pressure where the bow is held underhand. The bow shown here is of timber grown in a curve while later bows were bent to shape by heat.
Woodwindwords Online Dictionary

A while ago, while FoMHRI was hibernating, I created a dictionary on the Internet that lets you translate woodwind terminology between English, German, and French. It is free for anybody to use.

I started it out of a personal need, but it soon grew out of hand and now contains around 5800 terms, covering instrument parts, materials, making and repair techniques, tools ... it should contain most of your woodwind workshop's inventory.

It is not an annotated dictionary – it will not explain what a flute is. It is simply meant to translate, in order to facilitate communication between people of different languages, who know what they are talking about in their own language.

There are three search modes:
- a word search mode
- a category search
- a browsing facility

The database contains tables of the words in their three languages, respective grammar columns, a table of scientific names (e.g. for wood species, metals, acids...), and one for view symbols that you can click to pop-up pictures.

This project is a work in progress – please do submit new words, new translations, regional expressions, copyright-free pictures, and let me know of any mistakes you encounter.

May it be of use – here's the link:
http://www.woodwindwords.com
For Information - Diderot et d'Alembert, L’Encyclopedie - Free!

For those researchers and historians who may not be aware of it, the famous and monumental reference work by Diderot et d'Alembert - “L’Encyclopedie ou Dictionnaire Raisonne des Science, des Arts et des Metiers”, published between 1751 to 1765, is available on-line, as a free download, at:-

http://diderot.alembert.free.fr

The entire text of the Dictionary (in French) has been transcribed into a clear and easy to read modern format, separated alphabetically into files convenient for downloading.

Additionally, all 11 volumes of the 2,600 or so original plate engravings and explanatory texts are available in pdf format. The file size of each volume is quite large (from about 30 to 60 MB) so downloads can be fairly time consuming for those on a dial-up Internet connection.

To access and download files of the engravings, click on the main header at the top of the ‘Home’ screen entitled “Planches”. Then click on the “Page Index” (the button with four squares) located on the left side of the title window. Select the volume required on the scroll down menu, click on the “Telechargez” button and wait. After each file has completed loading – save it to a convenient location on your computer. Note that volume 2 is in two parts i.e. 10 volumes for downloading.

The Encyclopedie is a valuable reference source for those interested in early technologies in general but also contains a lot of material about musical instruments of all kinds and their manufacture as well as related technologies. For example, there is a lot of information about rope and string construction and manufacture - made from materials like hemp, silk, sinew, gut etc.

Modern editions of L’Encyclopedie can cost between $2000 and $5000 from antiquarian booksellers – dependant upon the edition and condition of the books.
‘Mortua Dolce Cano’ – A Question.

“Viva fui in silvis; Sum dura occisa secure; Dum vixi tacui; Mortua dolce cano” (I was alive in the forest; I was cut down by the cruel axe; In life I was silent; In death I sweetly sing.). This well-known and popular verse is said to be inscribed inside a lute but I have been unable to find information about the lute or its location. An Internet search was not helpful, various sources suggesting that the inscription was “written; carved; engraved; scrawled in pencil; inscribed on the face frets of - a 15th C lute; an Elizabethan lute; a 16th C. Italian lute, and on a harpsichord (but very similar verses have been found on instruments much older)”. Take your pick!

Edgar Hunt, in his article “Inscriptions on Harpsichords” (The English Harpsichord Magazine, Vol. 1, No 8, April 1977), notes that the verse is associated with luthier ‘Gaspar Tieffenbrucker among others but gives no other details. Hunt also notes that the second half of the couplet appears on the name board of a restored octave spinet in the Russell Collection, Edinburgh (S2-PO1710.11 by Petrus Michael Orlandus, dated 1710). However, limited information about the spinet, currently posted on the Russell Collection website, makes no mention of the inscription.

Does this lute exist and, if so, is the inscription original?
Mortua Dolce Cano: two more instances

While I cannot answer John Downing's question about whether a lute with such an inscription survives actually carved or written on it, I know of one other instance of an English rendering of the Latin text, from Timothy Kendall, *Flowers of Epigrams* (1577)

In forest when I lived,  
I had no sound nor voice,  
But made a lute, with silver sound  
Men's hearts I do rejoice.

And William Drummond of Hawthornden expands on the theme in his *Poems* (1616):

My lute, be thou as thou wast when thou didst grow  
With thy green mother in some shady grove,  
When immelodious winds but made thee move,  
And birds on thee their ramage did bestow.  
Sith that dear voice which did thy sounds approve,  
Which used in harmonious strains to flow,  
Is reft from earth to tune those spheres above,  
What art thou but a harbinger of woe?  
Thy pleasing notes be pleasing notes no more,  
But orphan wailings to the fainting ear,  
Each stop a sigh, each sound draws forth a tear:  
Be therefore silent as in woods before.  
Or if that any hand to touch thee deign,  
Like widowed turtle, still her loss complain.

Inscribed lutes are not unknown; there is a gilded copper lute in Stuttgart, described by Stephen Barber in *Lute News* 78 (June 2006) which bears two Latin inscriptions, but not surprisingly wood is not mentioned. The literary trope was evidently in general circulation.

For anyone interested in lute poetry, by the way, the Lute Society publishes an anthology, which I edited. Enquire if you would like a copy.
Further to Comm. 1818, the new Mary Rose fiddle bow

I now have a good drawing of the bow from the Mary Rose, with my thanks to Andrew Elkerton. The top picture shows the back of the stick, with the remains of the retaining plug in the hole.

The middle pic is, obviously, the profile. As I said before, it is said to be one piece of wood and if so, maybe the cord lashing is simply to prevent any risk of the frog splitting off the stick. However, the bow is still in conservation and when that is complete, maybe we will know more.

The bottom pic shows the stick with the frog facing us, and again the hole with its plug. What it doesn’t show is the curved groove on the face and back of the frog, which is clearly a channel for the hair and which is what made me say, the moment I saw it, ‘yes, it is a bow’. The top surface looks like this: ~, but I can’t find any symbol on this machine that would show the vertical sides!

![Drawing of the new Mary Rose fiddle bow](image)

Drawing courtesy of and copyright The Mary Rose Trust, 2008.
Further to Comm 1819, a large-scale drawing has been prepared to verify the geometry and relative proportions derived from the oud engraving. The representation of the neck in the engraving is only a rough approximation, as an oud having the same proportional neck geometry would scale to a string length of only 325 mm - assuming an overall spacing of five double courses at the nut of 32 mm.

A traditional feature of many ouds is that the length of the neck is a third of the string length - as is the distance from neck joint to the center of the main sound hole and the distance from the sound hole center to the bridge. Applying this relationship, Fig. 1 shows the revised geometry drawn to scale as an overlay on the engraving.

The following geometric relationships were also derived from the large-scale drawing.

1. The diameter of each small sound hole is a fifth of the maximum soundboard width (measured at the sound hole centerline) with the centers located a distance of one diameter from the edge of the soundboard.
2. Taking the distance FG from the centerline of the small sound holes to the bar above as X then the distance from the bottom of the bowl L to the first bar (shown dotted) is X and the distance LJ to the front edge of the bridge is 2X. Furthermore measuring from inside the bowl at K, the distance to the center of the ‘imaginary’ large sound hole KE is 7X, distance KM is 9X, the distance to the inside edge of the neck block KC is 11X and the distance to the neck joint KB is 12X. This arrangement also agrees very closely with the barring geometry given by Mersenne. In addition, the distance LJ to the bridge is a sixth of the bowl length LB – a proportion given by Arnault de Zwolle.
3. The revised barring arrangement shown in Fig 1 includes a bar below the bridge and one midway between points C and D (shown dotted in Fig 1). The below bridge bar is a feature invariably found in traditional oud design and might also have been found in early lutes, as previously proposed in Comm. 1237 (Q. 74, Jan. 1994).
4. The five fret positions shown in Fig. 1 are those given by the early Arab theorists that conform to Western scaling. Intermediate fret positions are not shown.

Given a string length of 56 cm and assuming KL to be 4 mm (i.e. the width of the soundboard edge banding), the overall dimensions of an oud according to this construction are width of bowl 34 cm, depth 17 cm and length 45 cm. The small sound hole diameter is 68 mm. Neck joint width is 52 mm and depth 26 mm and width of fingerboard at nut 40 mm.

For information, the overall relative dimensions of a Turkish oud (see Fig. 2) compare quite closely (although the soundboard profiles and bridge positions differ). String lengths of 54 cm (girl’s size); 55.5 cm (woman’s size); 57 cm (small man or boy’s size); 58.5 cm (large man or boy’s size) are the four traditional sizes.

(Note that Arabic ouds are generally larger than Turkish ouds with string lengths of around 62 cm and with more elongated bowl geometry)
Fig. 1
Oud Revised Geometry
Fig 2
Turkish Oud - Relative Dimensions.
Wood Fit for a King – A Case of Mistaken Identity? – Comm. 1826 Updated.

Following the preliminary testing of wild grapevine wood samples as reported in Comm. 1826, a quantity of wild grape vine has now been collected for further processing. The largest of the stems is about 6 inches (152 mm) in diameter, the smallest about 2 inches (50 mm) in diameter. Vine wood is covered with a thick layer of ‘shaggy’, loose bark easily stripped off by hand. On removal of the bark many faults in the wood have become evident including extreme spiral growth, kinks and bends, undeveloped branch ‘nodules’, and open crevices running to the heart of the stem. Many of the stems have heart rot. The oldest and largest diameter stem – grown at ground level, the centre rotted and hollow - is twisted like a rope (see Fig. 1). Even the most promising looking stem, found growing up a large cedar tree and measuring about 3 inches (76 mm) in diameter – is significantly curved along its entire length.

It is now apparent, from this experimental work, that it is unlikely any of this wood will be suitable for instrument making. It would seem, therefore, that the observations in the Old Testament are factual – that vine wood is only ever good for burning!

In the meantime, a fellow researcher, located in the Middle East, has recently identified two other meanings of ‘Mais’ one being the Southern Hackberry or Nettle-Tree (Celtis australis – a species closely related to Elm) and the other the Cedar of Lebanon (Cedrus libani – a true cedar). In Lebanon, the Hackberry tree is ‘Mais’ in Arabic. The tree is indigenous and widely planted, often seen beside Moslem shrines. People consider the trees to be sacred and rarely cut them down. It is a tree bearing small black, sweet, edible fruit, growing to a height of 20 to 30 feet (6 to 9 metres) and yielding a close-grained timber taking a high polish. A village in Southern Lebanon, Mais Al Jabal (Mountain Mais) is famous for its ancient Mais trees and many other places in mountainous Golan are named with the prefix Mais.

‘Mais’ or ‘Abu Al Mais’ is Cedar of Lebanon in Palestine. Cedar is another sacred tree representing eternal life to earlier civilizations. Sneferu, the first pharaoh of the 4th Dynasty in Egypt (2613 BC to 2589 BC) tells of 40 shipments of cedars imported from Mount Lebanon – used among other things for palace doors and funeral caskets for royalty. Cedar resin (called ‘life of death’) and sawdust from the cedars was used in the royal embalming process. Support timbers surviving to this day inside Sneferu’s so called ‘Bent Pyramid’ are of cedar. Phoenician king Hiram of Tyre supplied cedar logs from the forests of Mount Lebanon for the construction of the temples and palaces of David and his son Solomon – kings of Judah and Israel. Solomon’s temple, completed in 960 BC, had support columns and beams as well as panelled walls and ceilings made from cedar. The altar was also made of cedar. One cedar-panelled room in Solomon’s palace was named “The Hall of the Forest of Lebanon”.

The recently excavated tomb of king Midas at Gordion Turkey, built in 718 BC, is constructed in part from cedar logs – and contains the king’s funeral casket carved from a massive log of cedar. Following a military invasion of Syria, Assyrian king Shalmaneser III (858 BC – 823 BC) demanded tributes from one prince that included two hundred cedar logs. A second prince was required to provide two hundred cedar logs and one hundred logs annually thereafter. A third prince had to supply three hundred cedar logs annually.

Assyrian king Sargon II (722 BC – 705 BC) accepted cedar logs in payment for taxes.
Of the two proposed alternatives, Cedar of Lebanon would seem to be the most likely choice as it not only fits the description of a wood “only to be found in the treasury of kings” but also is a wood that until quite recently was traditionally used for oud soundboards. Today the ancient cedars of Lebanon are in decline – a threatened species – due to over harvesting and other poor forestry practices as well as deteriorating environmental conditions.

Given these alternatives, how did Dr. Farmer come to conclude that ‘Mais’ meant ‘Vine’ in his translation of the Kitab kashf al-humum? Hard to say but ‘Mais’ in old Syriac language now means ‘Zbib’ (modern Arabic). Zbib can mean raisin or dried grape but more generally applies to any dried fruit, from any plant (such as the fruit of the Mais tree) So, it is suggested that Farmer may have concluded that ‘Mais’ was ‘Vine’ by the translation sequence Mais = Zbib = dried raisin = grapevine.

Dr Farmer’s translation of the other woods preferred by the ancients for oud construction, is Zan = Beech; Dardar = Elm; Saz = Shiz = Sasam = Walnut.

For information, and with reference to modern Arabic botanical sources, the following may also be possible alternatives:
Al Zan is Beech (although it sometimes means Fir).
Al Dardar, originally a Persian word, means Elm today but in old Arabic (current at the time Kitab kashf al-humum was written), it means Ash or more specifically Flowering Ash (Fraxinus ornus L) – a tree very common in the Middle East.
Sayam (also known as Saj or Sag) always came from India. This may be the same as Sasam, identified in Comm. 1826 as the rosewood variety, Dalbergia Sissoo from India. Another possibility might be that ‘Saz’ was the wood originally used for making the Persian Saz instrument (whatever that wood may have been) or perhaps, it was in some way associated with the Takht-e Soleiman (Throne of Solomon) at Shiz in the ancient Azarbaijan province of Persia?

![Fig. 1](image)

Severe Spiral Growth in Wild Grapevine.
Reply to Segerman's Comm. 1830

By misquoting his source in Comm. 1830, Eph's argument about probabilities likely do not mean very much.

Eph's description of Alexander Rakov's silk lute 6th course string indicates that the construction does have historic validity according to at least two surviving sources, the earliest dating to the Chinese Song dynasty, 960-1279. The construction of these early Chinese bass strings was 'over spun' - smooth silk windings on a silk core - consolidated and 'loaded' by boiling the completed strings in a fish glue concoction.

What I wrote in Comm 1805 over six years ago (memory fades so I took the trouble to check), clearly refers only to some modern lute strings of roped construction, not to modern gut strings in general. My opinion about these strings comes from experience having tried all gut stringing of a lute some years previously.

Eph does not say if professional lutenists, reputed to have used gut strings at that time, were using all gut or partial gut stringing. It would be interesting to know if those who may have used all gut stringing did so regularly for 'live' recitals, or only for studio recordings (where the quality and balance of the final result may be readily edited and 'enhanced' by the recording engineers).

Eph's nautical rope theory, originally used to promote his idea about how 'Venice Catlines' were named and constructed, would now seem to have been just speculation unsupported by historical evidence. Although the proposal did sound convincing at the time, I do not believe that this disappointing outcome undermined any trust I had, as a member of public, in the disciplined contributions that Eph's 'proper historians' may have made to the advancement of knowledge.
Reply to John Catch's Comm 1827 on frets and temperaments

Since I have never before been charged with "corruption and gratuitous fabrication of evidence" (John Catch's wording in Comm 1827, p.51), I hope you will forgive me for responding. Readers of FoMRHI can check up on the origins of this argument by reading the interesting and detailed discussions in The Viol (referenced in John's footnotes), and everyone will no doubt have their own views on the validity of much of the evidence presented there, both by me and by a number of others. I have no interest in fuelling a quarrel with John, nor will I attempt to speak up for the other contributors to this discussion. Rather, in the interest of brevity and clarity, I shall focus on what seems to be the core of John's accusation against me: my comments on Praetorius as historical evidence. Here I will focus on just three main points:

(1) Accurate reading of the historical source. John takes me to task for asserting that Praetorius described equal-tempered fretting on lutes and viols as if (for Praetorius) it was a matter of opinion. John quotes from his translation of Syntagma Musicum to support his contention that "the original reads as a plain statement of current practice without a hint of 'opinion'". John even claims that he has checked his Blumenfeld translation against the German original. All the more remarkable, then, that John has carefully omitted the phrase "Sintemal meines erachtens" ("Since in my opinion"), which in my facsimile copy of the original text (vol.2, 1619, p.66) introduces the sentence, right in the middle of John's quoted extract, that each fret marks off 4½ commas. If you are going to criticise someone else about accurate reading sources, would it not be worth checking your own accuracy first?

(2) Reading in context. One might argue that in this particular passage Praetorius's phrase "in my opinion" applies just to his estimate of the difference between major and minor semitones, not to the appropriateness of actually spacing the frets in accordance with such a compromise. Maybe so, but in the passage of Praetorius leading up to this statement, describing these tuning problems and possible solutions, I have so far found at least two other occurrences of the phrase "in my opinion". This does not seem to me to be the way an author would present an argument if he was sure it amounted to obvious or incontrovertible truth.

(3) Appropriate application of historical evidence. In fact John's accusations completely miss the point of my argument. In my article in The Viol, I argued that a description of the way frets of lutes and viols were tuned in Germany in order to accompany voices or play in larger ensembles (which is clearly what Praetorius is talking about in the passage John quotes), is neither helpful nor reliable if you are trying to ascertain normal practice, in England, for intimate viol consort playing (especially if accompanied by a pipe organ). Praetorius had only second-hand information regarding England. Neither his, nor the English context, of course, will provide any meaningful evidence on the standards expected at a performance of Marais at the court of Louis XIV nearly a century later.

No one doubts the self-evident mathematical analysis that tells us that equal-temperament fretting is theoretically the simplest compromise for coping with modulation into all tonalities. But as I also argued in my article, such a mathematical analysis was not available during the golden age of the viol, and cannot be used as historical evidence. John asserts (p.49) that "historical information - what happened in the past - is matter of fact, prosaic objective fact...". Whether you have such a touching faith in the factual accuracy or truthfulness of any historical evidence, or not, the historical evidence discovered so far has failed to provide anything like a clear answer in respect of instruments with movable frets. No doubt the argument will rumble on, but we might make better progress if we refrain from obstinately misreading or misrepresenting the arguments of others. And for the record, FoMRHI readers may be reassured to know that the whole point of my article in The Viol was NOT to claim that ¼-comma (or any other unequal temperament) was the only historically correct answer - on the contrary, my point was to emphasise that there is good reason why performers might want to experiment with several of them, so that they can make an informed choice to suit both the cultural/historical context and the tonalities of the music itself.
Re: Comms 1810 and 1815 - Angled Bridges, Tapered Strings, Frets and Bars

It is my impression that angled bridges were much much more prevalent on baroque lutes than on Renaissance lutes. In the baroque the right-hand little finger was usually located on or around the bridge tail (as seen from wear marks on old lutes) and a thumb-over hand position was usual (as Dowland preferred). There are indications that low bass strings were at lower tensions than higher courses, and if projection was to be maintained, they had to be played close to the bridge. Angling the bridge could then have been a way to reduce the amount of thumb excursion needed between strongly playing the low basses and playing on higher strings.

There may be other good reasons for the bridge angling but I very much doubt whether tapered strings is one of them. We make gut strings from cleaned whole intestines, and haven't noticed any taper in the resulting string diameter. Perhaps that is because we follow the traditional procedure of winding the raw gut strands between two hooks to build up the required thickness before twisting, and whatever taper there was in the original strands tends to be cancelled out by it being in the opposite direction with respect to the string each time it goes around a hook. It is quite possible that the strings Capirola used were deliberately made to be tapered for fishing purposes. Those made for musical instruments would then not deliberately be tapered. Modern nylon fishing line is also different than instrument nylon, having more inelastic stretch before breaking. Vitali wrote that his way of handling tapered strings was a secret of Capirola's, so it is not likely that it was common practice at the time. What might have been common is that if a string did not fret truly enough, it could sometimes be improved by reversing the way it lay. There is no problem with angling the frets slightly to compensate for an angling of the bridge.

Gut strings do not wear out quicker with a harder fret material. In the early period, gut frets were used because they did their job very well, they were readily available (mostly from broken strings), and they were easily replaced when faulty. Metal fretting at the time was much more complicated, involving hammering the brass thin to harden it, folding it to get it thick enough, and carving a backing strip of wood and a channel in the fingerboard to accommodate the strip and fret. Guitars in the 19th and the first half of the 20th century happily used gut strings with metal frets. There is no practical physical objection to using metal frets on modern lutes. Nevertheless, there is a large cultural objection since early musicians highly value every difference there is between their instruments and the nearest modern equivalents, and metal frets smell strongly of guitars. Though historical accuracy is much less of an issue now than it was some decades ago, many lute players are still uncomfortable with the usual rationalisations for nylon stringing given when asked about its anachronism by an audience member.

A great advantage of gut frets is that it is very easy to vary their heights to make the action as low as possible (see Comm 1168 Q71 p.42). I can make a better fit than I did in that Comm to the required logarithmic spiral for Dowland's prescription for graded frets.

Bars on modern lutes don't get loose as readily as Mace's one did, probably because the glue he used was not as strong as the high quality animal (or fish) glue that is available now, and probably was available to professional instrument makers at that time. The most susceptible bar is the one just above the bridge, which is pushed inwards by the tilting of the bridge caused by the tension of the strings. That force usually bends that bar inwards, creating an embarrassing dip in the soundboard above the bridge, which if too deep, see-saws over the main bar just below the rose, raising the rose so it can interfere with the string vibrations. Some makers prevent the dip by shaping the top of the bar above the bridge into a convex arc, so the top goes straight when the bar bends. With no dip in the soundboard, and with the bar securely glued to it (and extending all the way to the ribs), there should be no tendency for the soundboard to split irrespective of the scalloping at the ends of the bar. The tilting force of the bridge tends to raise the region of the soundboard below it into a dome. That does stretch the soundboard laterally, tending to induce splits. The bass or 'J' bar is not deep enough to seriously effect the soundboard mass or stiffness, and its main function is probably to inhibit splits. Thicker soundboards may be good solutions to some problems, but not these.
The Lute in Renaissance Spain: Comment on Comm. 1833


In this article Rey argues about some statements from Alton’s book: the disappearance of the lute in Spain, its moorishness and the prosecution by the Inquisition.

Rey thinks that the lute was not considered a Moorish instrument in Spain, in fact the dictionaries of the time, Nebrija (1495) or Covarrubias (1611) give a Greek origin to it. Some even think it as coming from the Rhine valley.

He also considers that the lute did not disappear nor did stop being used, even in the strongholds of Christianity such as the royal palaces.

Cites as examples the inventory of Queen Isabel (The Catholic) made in 1503, where the chapter that deals with ‘musical’ objects is called “Laudes e cosas de musica” (Lutes and music things) and counts six lutes of different sizes. In that same court there was a musician called Lorenzo Suarez de Figueroa, lute player.

There is also an important amount of lutes in inventories from other royal houses like the one of Maria de Hungria (sister to Charles V) and in the inventory of goods of Felipe II, at the end of the XVIth century.

Many literary works make reference to the lute, in courtly atmospheres, in hands of knights or in idilic pastoral settings.

In visual arts, miniatures, paintings or engravings there is a place for the instrument, usually in hands of angels. Painters as El Greco, Zurbarán, Murillo, Cotán, de las Roelas or Pacheco, to name a few, depicted it in their works.

Rey does not think that the Inquisition nor anyone did persecute the lute in Spain. On that respect there is no work done about the relation of the inquisition and music. As an example says how in 1616, Francisco Pacheco (one of the above mentioned painters) finished his work ‘Cristo en el desierto servido por los ángeles’ (with one of the angels playing the lute for Christ) and in 1618 is named ‘censor y veedor de las pinturas’ (censor and watcher of paintings) by the Inquisition, a job that is given to someone with proved morality.

All this is just a short reference to an extensive article written in Spanish in the above mentioned journal which can be obtained from the Vihuela Society in http://www.sociedaddelavihuela.com
Modern Clavichord Studies: DE CLAVICORDIO VIII

The Eighth International Clavichord Symposium Magnano, 5 - 8 September 2007

An enterprising festival known as Musica Antica a Magnano has been held in that picturesque municipality in Piedmont since 1986 and, from 1993, clavichord studies have had a significant place. Their proceedings published as de Clavidordio, now in its eighth edition, include articles of wide musical and historical interest. For us in this country the text of this most recent Symposium report has been made very accessible, all the papers being in English with brief summaries in Italian. The proofreading and typesetting are of an excellent standard and the finished result is very pleasing. The volume retains the A4 perfect-bound format of previous Magnano reports but does not look at all institutional: indeed it is very attractive with its Indian red and gold cover and mildly cream-coloured paper. The pictures are not particularly large, but most of them have sufficient resolution to enable details to be seen through a magnifying glass.

It is no easy matter to handle publications of multiple international authorship with the added complications of illustrations, tables, footnotes and index. Very well done, the editors and layout team: Bernard Brauchli, Alberto Galazzo, and Judith Wardman. (This review is in narrative form: details of the full titles of the reports and their contributors can be found in the reproduced table of contents at the end.)

The Ends of Europe

Instruments of Portugal

A special emphasis in this symposium was on the Iberian Peninsula so I will begin this tour of the conference proceedings there. On page 81 can be seen a square piano with English characteristics built by Tataros in Coimbra. This is from Gerhard Doderer’s particularly well illustrated paper on the clavichord in Portugal after 1800. He catalogues the rather sparse surviving clavichords and traces the monastic tradition of instrument building. We are reminded of the ominous gap in the chronological table of Iberian instruments in the first decade of the 19th century when Napoleon’s armies set all Europe ablaze.

Coimbra, itself, was not spared the horrors of war. That quiet university town was abandoned in flames in the autumn of 1810 during the retreat to the lines of Torres Vedras. A glimpse of conditions in that retreat can be found in a letter which Colonel Colborne, then commanding the 66th Regiment in Wellington’s army, wrote to his sister. He described the sad sight of keyboard instruments piled with other furniture in the streets as a barrier to the advancing French. In view of what happened at Coimbra and other towns, it is a wonder that any clavichords survived outside Lisbon but there must have been a recovery after the War. No doubt there were generous English imports of pianos, but the new instrument did not have total dominance: there were clavichords being made by Coimbra makers at least up to 1855, the longest overlapping of the production of the two keyboard instruments recorded anywhere. Indeed there is one instrument which appears to have been begun as a clavichord but completed as a piano, the necessary strengthening being obtained by a second bottom board two inches thick!
An Iberian Manuscript  Ilton Wjuniski has explored a 16th century manuscript collection from Oporto in which the original music was noted in ‘four-part organ score’ with the ‘original clefs’. We may imagine this, with its emphasis on horizontal movement rather than vertical harmonic congruence, but a facsimile would have been valuable. He has made a special consideration of what might be done to interpret the music on a triple-fretted clavichord (on which of course, among other restrictions, so many of the characteristic suspended seconds are unplayable). Wjuniski’s suggestion is to change the polyphony into a ‘style luthe’. Again to some slight extent one may imagine the effect, but musical examples would be so very helpful to see exactly what he means. I know that pushing the boat out by being specific in this way exposes one sometimes to unsympathetic criticism but it does so focus the discussion.

Not just on a triple-fretted clavichord, polyphonic music is always a challenge for the solo player to perform and make clear to the listener in all its richness. Apart from the mental gymnastics of pretending to be two or more people at once, there may just be too much going in the music: the part writing may open out beyond the range of the hand or the compass of the instrument, notes may be written to be held but there are no fingers available to play them and parts may cross, without any means of indicating to a listener what is happening. All sorts of resources occur automatically to the experienced player when what is written is impossible to perform literally – octave transposition, omission of inessential notes, alteration of figuration, judicious adaptation of ornamentation, etc. Surely this must have been to some extent always true, especially in periods when improvisation was so much more a feature of professional music-making, and instruments were not standardized. There is, moreover, a craft of composing as well as a craft of performing. In the Renaissance artists often delighted in hidden subtleties and even codes not immediately apparent to any but the cognoscenti. There are also of course many examples of composers making arrangements of their own works for different instruments, and these sometimes involve quite substantial rewriting. Perhaps in our residual submission to the tyranny of the text we may make too heavy weather of preparing our performances.

The vexed problem of the interpretation of tablature was not raised in this paper, but the treatment of long notes did come up. Wjunisk mentioned the judgement of Macario Kastner that many long notes were there on paper simply to satisfy the pedant and were not expected to be played, but there is always the problem of exactly at what point unmanageable notes should be shortened on the various appropriate instruments. Some of this Iberian music seems laid out for vihuela da mano, harp (Kastner was a harpist) or one of the keyboards. Wjunisk said that he personally often favoured the organ for this polyphonic music but on that instrument, above all, the sudden silencing of one of the parts is most noticeable.

Land of the Tsars  From one end of Europe to the other, from war-torn Portugal to the fantastically opulent world of 18th-century St Petersburg. Russia, then as perhaps now, was a broken society of the starkest contrasts which affected instrument makers like everyone else. Native workers were serfs at the control of their masters and could be bought and sold at will. Very different, however, was the status of fashionable immigrant craftsman.

On page 115 is illustrated an instrument of an organized square piano of 1783 by Johann Gottlieb Gabram, one of two German builders prominent in that era. It is covered in sumptuous veneers featuring what appear to be tortoiseshell sharps and mother-of-pearl naturals. This is a reminder of all the splendidly decorated instruments which have survived but which are perhaps more relevant to artistic and social rather than strictly musical history.
All the same, no less an authority than Abbe Vogler was very impressed by the standard of organ building in St Petersburg at this time so presumably this instrument was worth playing. Maybe, however, it was never much more than a showpiece.

**Silbermann and Sweden** After a turbulent history of military adventuring in mainland Europe, Sweden managed, as its great neighbour, Russia, eventually did not, to remain out of the Napoleonic War — although Bonaparte did manage to foist one of his marshals on the country as heir to the throne. Earlier in the 18th-century The Royal Swedish Academy of Sciences (founded 1739) had interested itself in the manufacture of clavichords and harpsichords — they were, for instance, enthusiasts for long-scaling. In its proceedings it published articles by Nils Brelin who had travelled extensively in Germany and Italy studying and working in keyboard-making workshops.

Gottfried Silbermann, one of the great names among 17th-century keyboard makers, comes very much into the Swedish story. His journeymen worked and travelled extensively and their influence merits a study on its own; there is some fascinating detective work recorded here exploring Silbermann's influence on the making of the large Swedish clavichords. Records in many countries will still yield further details of his instruments and those made by his apprentices and disciples. By the way, at this date covered bass strings are recorded as being wound in white (silver?) wire — *wirade mid hwit trä*.

That complicated instrument the *cembal d'amour* came to Sweden as early as 1728. A fascinating Swedish description of this instrument is printed on page 190: also a eulogy from Saxony some few years earlier of Silbermann, its inventor, "after great trouble, time and indefatigable thought, and contrary to all expectation, so happily accomplished that for construction and amiable sound, sweetness, and easiness to play, and a totally new style except for the lute and the *viola d'amore*, so that we, in short, never heard any more agreeable sound from all kinds of percussion or bowed instruments. One can imagine.

**Swedish Clavichords** Long after the clavichord began to be eclipsed by the fortepiano, there was in Sweden a fantastic late flowering of clavichord manufacture. This was centred on the work of Pehr Lindholm who was active with his partners from 1774 to 1813. During that time he produced many clavichords subtle in design and excellently crafted for long and active life. Many of these came to be built of unusual size — with a compass of up to six octaves. This manufacture overlapped with the production of fortepianos for many years; one cannot help wondering what the particular attraction was. Who bought them, and what did they play on them?

Hans-Eric Svensson has studied twelve of these instruments in detail and enlarges on the problems involved in mapping them even in two dimensions. For instance, each of these big clavichords has 500 pins and that would mean plotting 1000 measurements. Fair enough, but what would you take as your point zero, the inside of the top left-hand corner or what? And then how would you compare various instruments? Svensson comes up with interesting suggestions for suitable techniques but really one cannot believe that the makers in the Lindholm workshop (and there were apparently several specialist craftsmen) worked to such specifications. That of course is not to say such measurements are unimportant, and this thorough paper is well worth studying by anyone undertaking copies of historic keyboards. Of relevance is Peter Mole's approach to the same problem in dealing with English bentside spinets recorded in the current Galpin Society Journal (LXI pp 325-331).
Other German builders

An interesting oddity is to be found on page 229 — a double-manual octave spinet with a range of about three octaves but with B-flat as the solitary black note below “tenor” C. Such a short octave is occasionally recorded on early Renaissance organs so presumably this was made as a practice instrument — but why so short a compass, and just what strings did the two manuals control? The builder was certainly an organist of acknowledged skill (but, we are told tantalizingly, with a reputation as a malicious gossip). Who was he?

He was Israel Gellinger, a member of the corporation of Carpenters in Frankfurt am Main. Only three examples of his work survive, this odd little spinet, an organ case in Sulzbach and a clavichord dated 1670 in a sad condition to be found in the museum of Namur. Pierre Verbeek has made a thorough examination of the clavichord with a view to making a reconstruction. Quite obviously the instrument has had a hard time: for some reason the rose has been torn out, various screws had been carelessly put into the casework and an attempt was made perhaps 100 years ago to string it with piano wire. In spite of all this the casework has maintained its integrity with a remarkable absence of distortion, a testimony to the craftsmanship of the maker. All sorts of fascinating details are recorded, not least the suggestion of a convincing module of measurement, a Werkzoll of 25.36mm (63/64ins). Replicas are now being made and one can be heard on Youtube. Look up Teafruitbat.

The work of another maker comes under scrutiny. The organ book of Johannes Creuzburg of Thuringia dated 1724 has survived. It opens a window on his life with details of the construction of organs, clavichords and harpsichords with measurements carefully written out in full. It also tells us about health-giving teas and how from a local dealer he obtained ivory, ebony, and hardwood from Brazil. There are copious details of wire gauges and the quantities required but unfortunately the gauge system he used has yet to be determined.

English Interlude

For a fascinating glimpse of musical life in Georgian England one can turn to Derek Adlam’s study of the correspondence of the Granville family. Mary Granville was a neighbour of Handel’s in Lower Brook Street from 1734; he was a regular visitor to her house and frequently played on her instruments. (She refers to her clavichord later on in her correspondence.) Adlam gives us an excellent précis of English history from the Civil War to the accession of George I in 1714 showing us how Handel came to be in England. Johann Matheson writes in 1722 that there are “people in Hamburg regularly sending clavichords to England” and mentions one being played at the home of a German organist there. It is impossible not to think of the names of Haas and Handel as we read this. The suggestion is made that the Haas clavichords now to be found in England may well have come here at this period rather than being more recent collector’s imports. The provenance of the 1763 J.A.Haas in the Russell Collection seems almost certainly Georgian. One wonders about the largish anonymous clavichords of a German pattern which have survived — could any of these have been made in England? Not apparently by the established makers, but it is a tantalising thought.

A neglected Composer

Daniel Gottlob Türk is best known nowadays for the teaching material he wrote for keyboard players; his other compositions are virtually unknown. He himself esteemed the keyboard sonata as a high form of musical composition. Between 1776 and 1792 Breitkopf published his sonatas in the usual 18th-century pattern of batches of six. They were immediately popular and came out in eight sets altogether — a rich repertoire to explore for amateur and professional alike. The style is baroque-galant with many marks of
expression. Turk himself favoured the clavichord but this is excellent fortepiano music. Michael Tsalka has had access to the complete collection which has yet to find its modern publisher. A representative sonata is published here on page 163 in a contemporary manuscript copy, the elegance of which matches the music. The facsimile print is a bit small to play from easily out of the bound book – and, for non-specialists, the soprano clef is not always as easy as it looks! Pleasant expressive music, dedicated to Kenner und Liebhaber but much less wayward than the works of CPE Bach.

**Iconography** There is no specifically iconographical paper in this collection but there is a supplement to Bernard Brauchli’s 2001 Magnano report. It includes a nice clear picture of a mediaeval rectangular clavichord from an Iberian monastery but, as with similar representations elsewhere, one can’t really tell how it would have worked. An engraving from Nuremberg in 1640 shows a party of richly dressed musicians engaged in chamber music. The beruffed lady at the clavichord would have been, one would have thought, totally overwhelmed by two violins, a bass viol, a lute and an archlute. Such somewhat improbable groupings, which are not rare in mediaeval pictures also, raise obvious interesting questions of practicality. Did such situations ever occur, and if so how commonly? If they did not occur, why were they depicted? Sometimes processed pictures were reused unthinkingly — remember the absurdly long keyboards of early 18th-century harpsichord prints. Here there is a mid-century picture of a lady showing off at a clavichord with improbable details and a piece of chinoiserie painted by someone who had presumably never seen such a thing as a clavichord in use. Of much more interest is a lesser-known Venus by Lambert Sustris (ca 1550) where the goddess of love is portrayed against a background of a domestic scene which includes a player at a clavichord on a large table. There is much symbolism here and also probably some organological information. For those who don’t go to Holland, we can visit http://www.rijksmuseum.nl/assetimage.jsp?id=SK-A-3479. More down to earth is a 1795 Dutch engraving of a pedal clavichord in a large case — perhaps intended to include pipework? The pedal compass is three octaves from FF to match the keyboard, presumably operating a set of pull-downs.

**Doing the Maths**

**Database** One remembers reading of suggestions for the preparation of a database of early keyboard instruments and having a general sense of approval coupled with the hope that someone else would get on with the job! I suppose most of us will have tried to organise our organological notes and come across difficulties concerned with deciding what to record and how to record it. Simon Field’s paper on building such a database is an eye-opener to what is really involved in the collection of data, subjective as well as objective, and its reduction to XML format. This is clearly beyond one worker on his own, although Simon Field has had a distinguished career in government statistics and has been supported by members of the British Clavichord Society, so it is good news that the Edinburgh University Collection of Historic Musical Instruments is now involved. When it comes to the actual computer nitty-gritty the database has to be structured with great foresight before the meticulous work of preparing it in computer language is undertaken. Then it all has to be made user-friendly — Web-ready in fact — so that it is able to be used by those with no special computer skills. All manner of filters, cross references and other aspects have to be anticipated before such a
system can be put to bed. (This is hard enough when dealing with fixed data, but where there
are conflicting observations or diverse opinions it becomes even more complicated.)

Much is owed to the zeal and forethought of Charles Mould for his systematic work in the
preparation of Boalch III; important credits are also ascribed to Arnold Myers, Darryl Martin
and Peter Bavington. At the time of writing the database contains details of 482 historic
clavichords and 855 harpsichords. To find out how things are getting on now, have a look at
http://www.clavichords.org/.

Citterns and Clavichords  The relationship between the frets on a lute-type instrument and
the sounding lengths on a monochord is immediately apparent, but determining the exact
positions of the frets is a matter for both scientific skill and practical compromise particularly
if some unequal temperament is proposed. In extant instruments it is the normal rule that
when the frets are fixed the bridge can be moved and when the frets are adjustable the bridge
position is fixed. This is not always the case, however. Alfons Uuber has made a detailed
study of the fingerboards of five early citterns and one *lira attiorbata* to record how the frets
lie. They clearly do not conform to exact equal temperament: what system therefore do they
favour? On page 108 he has most usefully calculated the fret positions for a single string
according ten known 16th and 17th century tuning systems. His conclusion is that the most
common candidates would be modified mean tone systems, such as that of Arnold Schlick —
some sort of 1/5 or 1/6 comma meantone. This comes as no surprise but it is most valuable to
have it so meticulously documented. This result will be of general tuning interest in setting
up many types of instrument but it has of course a special importance in the case of fretted
clavichords. (Incidentally, in view of the highly convincing work of David Hockney on the
use of optical instruments in Renaissance art, it would be well worthwhile examining the
artists' favourite virtuoso subject of lutes to see how the frets conform to this conclusion. I
duly take note of the caveat in Peter Forrester's Comm. 1817: perhaps in future art historians
will learn to consult organologists as a matter of course when instruments are portrayed.)

Measuring, Comparing and Assessing  Our work with musical instruments has a
foundation in observation, classification and comparison. We observe all that sight, touch and
hearing can give us from actual historical or modern instruments and reinforce all that by
evidence from written records, iconography and relevant social history. We have a special
interest in this as of members of FoMRHI because all this has to be processed with the aim
either of restoring original instruments or of making and maintaining good ones.

When it comes to assessment we are heavily dependent on comparison. We all know
perfectly well that there is no ideal Platonic clavichord, for instance — not even an ideal of
any particular area or period — but we nevertheless use such an imaginary standard perhaps
more than we realise. The influence of our individual experience, training and temperament
can make this supposed ideal not a little subjective. We can objectify and measure as much as
possible but for so many things we are at the mercy of our language. Even the simplest terms
we use may seem crystal clear to us but convey very little to others — for instance just what
is "round/tubby/brittle tone"? Just how rapid/positive/resistant is "good response"? Even
more, what do we mean by "a period sound"? To add to the confusion, words which are used
merely descriptively can be interpreted as quite pejorative by a third party.

A good start can be made towards accurate measurement by treating clavichords as
impersonal sound generators as in the study *Acoustic Portraits of Four Clavichords: Tangent
Velocities, Loudness, and Decay Times*. Although the work described in this chapter is only
the beginning of an enormous potential field of Bruel & Kjaer, whose instruments have played so valuable part in modern acoustic development, have their outing here in a detailed study of four modern clavichords of quite different styles. It is impossible to do justice to all the mathematics involved in a summary but this research should be of interest to specialists in many different instruments.

In order to give shape to these experiments it was necessary to use environments in which the instruments would not normally ever be heard. A soundproof, acoustically neutral room insulated from outside sounds was essential. A microphone was suspended somewhere over the bridge point for middle C, a good position for measurement but one that no player or listener would ever adopt. This is not to say that useful data cannot be collected or comparisons be usefully made, it is just a reminder of the necessary limitations of laboratory practice. For the tests, the clavichords were played with single notes, a rare event in actual music. Instead of using a mechanical striker the same player was used for each instrument and a great deal of information was gained about sound pressure level, tangent velocity and decay times. An enormous amount of valuable hard work and meticulous recording of data has been done by Christophe d’Alessandro, Charles Besnainou and Luc Ginieis. It is no easy job to take all this data about a single note but these four clavichords have 204 between them.

No subjective tests were recorded, but it is reasonable to assume that a listener in a blind test would have quite readily distinguished one clavichord from the other. One longs for more work to be done, such as the analysis of tone waveform of notes played at different volumes. Above all one would like it to be correlated with subjective criteria — for experiments to be listened to blind by a panel of experienced aficionados.

These four clavichords were, as I have said, of quite different types. It would be interesting to make direct comparisons between instruments apparently similar such as two copies of the same historical instrument (ideally with reference to the original but I can see that might be hard to arrange) or instruments of a standard pattern by a modern maker. Would there be any significant correlation between the objective data and the subjective impressions? When people make collections they do not normally want to have two the same and so it is quite rare to be able to hear pairs of instruments together and compare them.

Simple work on a single instrument would take a minimum of organisation and would be instructive. If an instrument has rose or a mouse-hole (a hole in the bottom board), tests could be made to show the difference in volume and waveform with such openings open or sealed; a temporary over-rail could be devised to show the effect of such a device at different pressures; a strip of cloth could be inserted under the distal ends of the keys to shorten the swing of the tangent onto the string with, again, revealing results. Coming back to an instrument after a time would make it possible to see how it has changed with time:just what has changed when an instrument has been said to be ‘played-in’? Other suggestions for further experiments will occur to readers to add to the burdens of these splendid workers.

Giants of Early Music

Arnold Dolmetsch  The opening chapters of this book take up the invitation of ben Sirach: “Let us now praise famous men, and our fathers that begat us”. Generations before early music became a sort of technical term the pioneering work of Arnold Dolmetsch began. His unbounded enthusiasm, wide sensitivity, single-mindedness and scholarly energy bore remarkable fruit. The myth might have been that he would discover an instrument one day,
practice it overnight and perform upon it in a concert the next day. Then he would build a whole consort of such instruments and have his family performing in costume on them by the end of the week. Utter rubbish of course, but he did get on with the job and achieved an amazing amount. Like many children, we may feel we have outgrown our father — and of course not only fashion but also scholarship has moved on — but we must give honour where honour is justly due.

The first article gives us a good review of Dolmetsch's life. Arnold was born in 1858 (the same year by coincidence as Francis Galpin) and, by the age of 14 he was hard at work in his father's workshop learning the skills of piano making. Next door his grandfather was still making organs and so he put in time there too. His father died two years later so he took on the family business. At the age of 21 he went to Brussels to study violin with Vieuxtemps and spent four years at the Conservatoire. While he was there, he heard period instruments being played but was not satisfied with the performance. Later he went to London where he studied at the RCM. After that he was engaged as violin teacher at Dulwich which gave him the leisure to spend time in the British Museum looking at the old music publications and manuscripts. From this study came the determination to perform it on suitable instruments and the rest followed, as it now seems, inevitably. His education had been a wonderful preparation of a type hard to come by today: the family tradition, the craft apprenticeship, and the first-class musical teaching and fluency in other languages.

The author of this paper, Uta Henning, had the job of cataloguing Dolmetsch's large musical library and has had continuing contact with the Dolmetsch family who were all trained in the tradition of player-craftsmen — or should it be craftsmen-players? Over the years there have been generations of such people who have been through the Haslemere experience either directly or indirectly and have made in their turn beautiful music on beautiful instruments, the Dolmetsch ideal.

Reading the Dolmetsch story is always fun because of the splendid cast of characters including the great, the good, and the ghastly. Dolmetsch is here credited with having no less a person as Ferruccio Busoni as a pupil, but in fact it seems that the pianist had no great respect for the clavichord — although he was very grateful for the elaborate harpsichord that Dolmetsch made for him while at Chickering's and for his instruction in its use.

The article promises iconography but has just two well produced photographs and a cartoon. Plenty of pictures are readily available, however, in Mabel Dolmetsch's memoir or on www.dolmetsch.com. Further images are on the National Portrait Gallery website:
http://www.npg.org.uk/live/search/person.asp?search=ss&sText=Arnold+Dolmetsch&LinkID=mp01325

**Dolmetsch Clavichords before 1914** It seems that the very last clavichord to be made in the unbroken historical tradition was built in Portugal in 1858, the year Arnold Dolmetsch was born. In 1889 Dolmetsch acquired and restored a five-octave instrument by Hoffman. Five years later he built his first clavichord closely based on one by J.A. Hass and so the modern revival began. It is no small achievement to have one's very first instrument revered over a century later. Dolmetsch's first clavichord was not the first known instrument of someone who had worked his way up in the studio of a famous maker, this was a the work of a man whose master craftsmen had been dead for 100 years.

In all he built about the 50 clavichords in the period before the Great War. He worked first on his own in London and then with piano workshops in America and France (Chickering in Boston 1905-1910 and Gaveau in Paris 1911-1914). Both these firms survive after mergers: Gaveau merged with Erard in 1960 and from 1971 has become part of Pianofortefabriek
Wilhelm Schimmel; the Chickering name cont.
brand name of Baldwin Pianos. The firm of Dolmetsch keeps Arnold’s family name.

In his fascinating article on Dolmetsch’s earlier clavichords Peter Bavington has this
insight on the attitude that Arnold Dolmetsch brought to his work. "Makers showed their
quality in careful execution and good materials rather than in creating new designs. The
philosophy expressed here by Dolmetsch was very much that of the English Craft Movement
in which William Morris was prominent. Copying was shameful, other than for beginners,
and creativity was obligatory."

Dolmetsch’s remarkable productivity of course owes much to his assistants. Bavington is
concerned to give “honour to whom honour”. Nils Ericsson is fairly well known but W.
Neame remains a shadowy figure who worked with Dolmetsch from the very first harpsichord
and, probably, clavichord.

Bavington’s article sets out to catalogue the 50 or so clavichords that Dolmetsch made
before 1914. Drawing on standard sources and original research, he tells us where they all are
and whatever can be known about them. His remarks, as one of our well-known builders, are
particularly valuable. Some instruments can be fully written up, a few are in obscure private
ownership, just a few are lost — one hopes not permanently. I wonder what happened to the
one Dorothy Swainson was obliged to abandon in Moscow in 1914.

Macario Santiago Kastner The second giant of early music is commemorated in a
biographical article by two of his relatives, a short memoir by one of his students, an
interview with the founder of this Symposium and an excellently presented bibliography. He
was born in London in 1908 into a musical family and was playing the piano before he could
read and write. The Kastner firm was known all over the world especially for its player
pianos. Its various branches enabled the family to get on its feet again after the Great War.
They lived briefly in Holland (where Macario added fluent Dutch to what would eventually
become a repertoire of eight languages) and then settled in Leipzig where he joined the old
established firm of Julius Feurich who are still making handcrafted pianos to this day.

He studied at one time with a pupil of Wanda Landowska and fell in love with the
clavichord after visiting the de Witt collection in 1925. He was much more interested in
playing harpsichord, clavichord and also romantic and modern piano music than in the
mundane business of actually selling pianos. He went to Barcelona for the 1929 EXPO and
while there his talent earned him a place in the Conservatoire; that was the end of his
business career. By great good fortune, a few years later he was invited to teach at the
Conservatoire at Lisbon, thus avoiding the horrors of the Spanish Civil War. Thereafter he
made his home in Portugal. He was an indefatigable seeker and copyist of old Iberian music
which he made available to all of us through no less than 45 publications. Indeed
‘indefatigable’ is an adjective that comes to mind when reading the bibliography printed here
listing 53 books and articles and seven contributions to standard reference works. An
inspiration to so many people in the early music world. Incidentally, in all his long career he
seems to have had no contact with the Dolmetsch tradition except in quite trivial overlapping
circumstances.

He was a kind man and the most generous of teachers with vast experience and a large
library. Nothing was alien to him, insights were to be found in many disciplines. He said,
“One must have an extremely wide cultural perspective; limited specialisation seems
dangerous to me.” As a pianist he was still playing Brahms and Hindemith in recitals up to
1945. He had many academic interests including an extensive practical knowledge of botany.
He was decorated by the ever-to-be-honoured University of Salamanca, and in 1984 he received an honorary doctorate from the University of Coimbra which brings our story full circle.

Conclusion  This Symposium is past, the next is on prospect: 16th-19th September 2009. Themes will be Haydn and the Clavichord and the Clavichord/Fortepiano transition. As I look forward to the next publication of the Proceedings might I ask for a brief note about each of the contributors: not all of these accomplished scholars are yet household names.

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Dietrich Kessler was an outstanding violin and viol maker and restorer. The viol and its music were always a special interest. During the course of his long working life he collected a few exceptional viols that were used both in performance and as a rich source of research which proved groundbreaking. The trust, called The Kessler Collection aims initially to raise £900,000 to buy the instruments from the estate and house them in an appropriate institution where they can continue to inform performers, researchers and makers.
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